THE POPULAR SCIENCE MONTHLY

THE POPULAR SCIENCE MONTHLY

EDITED BY

J. McKEEN CATTELL

VOLUME LXIX

JULY TO DECEMBER, 1906

NEW YORK
THE SCIENCE PRESS
1906

PRESS OF
THE NEW ERA PRINTING COMPANY.
LANCASTER, PA.

THE
POPULAR SCIENCE
MONTHLY

JULY, 1906

THE RELATIONS OF EMBRYOLOGY TO MEDICAL PROGRESS[1]

By CHARLES SEDGWICK MINOT, S.B., S.D., LL.D., D.Sc.,
JAMES STILLMAN PROFESSOR OF COMPARATIVE ANATOMY IN THE HARVARD MEDICAL SCHOOL, BOSTON

EMBRYOLOGY is the most complex subject in the domain of science. Living beings are the most complex objects which nature offers us for study, and of this great class the higher animals exceed all others in complexity. The anatomist who studies the structure of the adult has a finished apparatus to investigate; a machine which has been perfected, in which, to be sure, nature may still make repairs, but in the pattern of which she makes no radical changes. The physiologist deals with this machine at work. The embryologist, on the contrary, has for his theme the history of this machine and of its gradual production from a single cell and the progeny thereof. During the period of development the machine at every stage is a different machine from that which it was in the stage before and which it will become in the stage after, and yet in every stage it is actively at work performing its proper physiological functions. We have to deal not with a condition, but with a series of conditions, each of which is at once the consequence of that which went before and the cause of that which is to follow. The final problem of embryology is to determine the origin and cause of the structure of the living body, and incidentally it has to deal with the associated problems of teratology, growth, heredity and sex.

We acknowledge the immensity of the questions for which embryological science must seek answers, but it is far from my intention

[1] Oration delivered before the Maine State Medical Association, June 14, 1906, at Portland, Maine.

to imply that embryologists therefore are an order of scientists superior to all others. Embryology is so vast and varied that it offers problems adapted, I might almost say, to every size of mind, and persons of moderate capacity, as well as those of the highest gifts of genius, can find adequate opportunity to gratify successfully in the field of embryology any demon of research which may possess them.

Let us first consider some of the conditions upon which the progress of embryological science has depended. Of course the first and all-essential thing is the amount and quality of human ability which has gone into it. This is true of every science, as goes without saying. It may, however, be interesting to pause a moment, since contemporary events are directing so much of the interest of the world towards Russia, in order to point out that modern scientific embryology had its birth in that country, for the first step was the publication of the articles by C. F. Wolff on the 'Theory of Generation and the Development of the Intestine in the Chick'; and the second and more important step was the publication of the great work of Carl Ernst von Baer, which may be said without exaggeration to have created by itself a new science. Von Baer's treatise on the 'Entwickelungsgeschichte der Thiere' is one of the greatest works in the whole history of biological science, and established the author's reputation as a genius of research. By the aid of improved methods a tyro in embryology may now verify von Baer's discoveries, but there has been no one since von Baer, who could have approached with his scientific resources the magnitude of his achievement. Let us then honor his memory. Although Wolff and von Baer, both, were Russian subjects, they were of German descent, and we find indeed that throughout the greater part of the last century the advance of embryology was due chiefly to German investigations.

How recent this knowledge is we are apt to forget. From 1800 to 1840 the seminal animalcules were universally regarded as parasites. The fact that they are normal products of the testis and the true male sexual elements was first discovered in 1841 by the Swiss anatomist von Kölliker, who was a leader in microscopical research for sixty-five years, and whose death occurred last year. Of Kölliker it may be asserted safely that he knew more by direct personal observation of the microscopical structure of animals than any one else who has ever lived. He was much honored in Europe. The last time I met him was at the International Zoological Congress at Berne, in 1894. It was most impressive to see all the members of the congress spontaneously rise to their feet when the handsome old man unexpectedly entered the meeting. The fact that the spermatozoon enters the ovum and produces the so-called male pronucleus, the union of which with the female pronucleus completes the act of fertilization, was finally demonstrated only in 1875 by Oscar Hertwig. These two additions to our knowledge are so fundamental that we have become rapidly familiar with them, and easily forget how recently they have been added to our science. Many other illustrations of the newness of embryological knowledge might be given.

The methods used by von Baer and by all his successors in embryology down to 1860, or even later, were exceedingly simple. They worked almost entirely with fresh material, hand lenses, and sometimes with acetic acid to render the objects a little more transparent. The embryologists of that period were few in number, but they made many fundamental discoveries. I fancy that if the researchlings of our present luxuriously installed laboratories were put back into that time of lean resources, their publications would cease. As you know, the fundamental procedures in modern microscopical technique are the making of sections and the staining of them. The introduction of section cutting came about so gradually that its history seems to be lost to us. Many persons in the middle of the last century appear to have made sections by hand of various tissues. This was especially a practise among botanists. At first only fresh material was used, but it was learned that preserved material, especially that which had been properly hardened in alcohol, could be cut to greater advantage, and gradually the process of 'hardening' before cutting became more and more common. So long as the cutting was done only by hand with that favorite unsuitable instrument of old days, the razor, no very fine sections were possible, save occasionally by some person of exceptional dexterity. The first microtome, so far as known to me, was that devised by Professor His and employed by him, about 1866,[2] for making serial sections of chicken embryos. Since then many inventors have contributed to the perfection of the instrument, and we now have the rather complex but very accurate and convenient automatic microtomes which are in such general use.

With the aid of microtomes, we can make perfect series of sections, and by mounting the entire series from a given object, it becomes possible to examine every part of it under the microscope. In the case of embryos serial sections are invaluable. We have been forming in my laboratory at the Harvard Medical School a collection of such series of sections of vertebrate embryos. The total number of series at the present writing is 1,106, of which forty-nine are from human embryos. The total number of sections is probably over 100,000. This collection has already served as the basis of forty-two embryological investigations and we trust that it will serve in the future for very many more. So far as I know the collection is unique in plan and extent. As soon as we are established in our superb new laboratory, into which we are about to move, we shall be glad to have

[2] Described in the *Archiv für mikroskopische Anatomie,* 1870.

you inspect our embryological museum. We value especially a fine human embryo which is in the youngest stage yet recorded by actual observation in America.

The history of staining is more definite. I have had the pleasure of hearing from Professor Leo Gerlach, Sr., himself the story of the introduction of coloring matters in microscopical technique. He was interested about 1857 in studying blood vessels, and wishing to trace them out by injection, applied to a local apothecary at Erlangen for a suggestion of some red coloring matter, and the apothecary proposed that he should use an ammoniacal solution of carmine, the pigment extracted from the cochineal insect. Professor Gerlach employed it, and in examining some of his preparations later found that the color had soaked through the blood vessels into the surrounding tissues, and had stained them so that they were much more distinct, and he also noticed that the stain had especially affected the nuclei. He at once recognized the importance of this coloration as a means of rendering more clear the character of cells and tissues, and to him we owe the introduction of carmine into histological technique, and it remains to-day the most important and valuable coloring agent for our purposes which we possess. The introduction of carmine marks an epoch in microscopical science. It was most fortunate that the accidental observation of the action of carmine was made by a man so thoroughly able to appreciate its great value. Since then many other staining reagents have been introduced by many different persons. I need not pause now to enumerate them, or hold up your attention in order to give a list of names and dates such as could be easily compiled. I will only recall to your minds that the introduction of chloride of gold, of osmic acid, of the aniline dyes, and of the Golgi method have each of them represented the beginning of a fresh advance, which without these added technical resources would undoubtedly have remained impossible for us.

Another class of methods are those by which we reconstruct from serial sections the anatomy of an embryo or an embryonic organ. To the late Professor His, of Leipzig, we owe practically the first recognition of the value and possibility of such reconstruction. He employed chiefly the method of drawing, by which many figures have been made. The process is laborious, for each section must be drawn and then the position of the parts measured and plotted off—but the labor is worth while as it results in accurate representations of the anatomy of parts which can not be dissected. I am in hopes that in our new Harvard laboratories that this method of reconstruction from sections will be applied to the adult, for I am sure that we can obtain by it representations of adult relations far superior to anything we now possess.

Doubtless to many of you the method of reconstruction from sections in wax models is also well known and its value appreciated as a means of giving us perfectly clear plastic conceptions of the arrangement of parts. The very ingenious wax-plate method was invented by the late Gustav Born, who conceived the happy idea of making wax models of single sections or parts of sections equally magnified in all three dimensions. It is only necessary to pile such wax plates in order, one on top of another, to get a correct model of the whole object. The method is very widely used and in my laboratory, for example, has been employed recently by Dr. John L. Bremer to model the anatomy of a human embryo and by Dr. John Warren to model the developing brain. Such models are truly revelations to one who has studied sections only.

This is not a suitable occasion to review the history of the technical progress of embryological science. I wish only to so far indicate it as may suffice to direct your attention to the dominant importance of method in scientific problems. It seems to me that the greater part of the advance which is made from time to time in modern science is the direct result of either an improvement of old methods or the invention of novel methods. I can see in my own science clearly that this has been the case, and from what I learn about other sciences, infer that it is equally true of them. Viewed from the psychological standpoint, the vast majority of methods have a common purpose, namely, to present the results to the eye, so that we can see what the facts are with which we wish to become acquainted. When we make sections, it is in order to see the cells in their natural relative positions, and with all their various characteristics. When we stain sections, it is in order to make things visible which were before indistinct or perhaps invisible. When we make reconstruction or models it is to furnish again an image to the eye which we can not get from the actual object itself. The eye, indeed, is the chief agent in collecting information for us from the objects by which we are surrounded. It is because they help out the eye that the microscope and telescope have counted for so much. The eye is almost the monarch of research, and reigns even more supremely over our relations with our surroundings than does the ear over our intercourse with our fellow men.

The results of embryology for a long time remained rather meager, and when as a young man I went to pursue some of my scientific studies in Germany, the principal text-book of the science was a modest octavo volume by Professor Kölliker. Since that time (1873) the activity in this domain has increased by leaps, and is now enormous, and the latest handbook of the science, that which is in course of publication at this time under the editorship of Professor Hertwig of Berlin, will comprise eight volumes, each of which promises to exceed a thousand pages when complete; yet the work is only a digest of the researches upon the development of vertebrates and does not deal with the invertebrates at all. This bald statement may give you some impression of the present vast extent of the science.

What I wish to attempt on this occasion is to select out of this huge accumulation of discovery some illustrations of the way in which embryology has made contributions of practical value to medical science and medical practitioners. I have on another occasion spoken of the relations of science and the scientific spirit to medical education and practise, and on yet another occasion have discussed embryology as a basis of pathology, so that it seems unnecessary to deal again and before you with these more general aspects of the situation, but I shall ask your attention rather to certain more detailed and specific examples.

Every science has its larger aims and purposes. Those of embryology may be classed not unnaturally under five heads. First, I shall group together those researches that refer to the general topic of generation, the production of the new being, the conditions under which it first develops, including, of course, for man especially, the relations of the fruit to the womb. Under this head are comprised phenomena of impregnation, problems of heredity, the origin of sex, the conditions of gestation and pregnancy, and the physiological causes of birth.

Secondly, under the head of cytomorphosis we can put all the work which has been accomplished in tracing out the development of cells. The conception of cytomorphosis is one which has only recently become clear to us, but it is one of the most fundamental notions of biological science, and one which every student of morphology, pathology or physiology must clearly grasp and keep constantly in mind. Cytomorphosis has been defined as the comprehensive designation for all the structural modifications which cells or successive generations of cells may undergo from the earliest undifferentiated stage to their final destruction. It starts with the history of the undifferentiated cell, considers all phases of differentiation, and in those cases where the process goes to its end, it follows out the final steps of the degeneration and destruction of the cell. The law of cytomorphosis is indeed the chief foundation of all anatomical and pathological science. The possibilities of modification within a cell are determined by the stage of cytomorphosis which is reached, and as it goes forward from stage to stage the possibilities of further change become more and more limited in accordance with the recently established law of genetic restriction. I have expounded my views on the importance of the laws of cytomorphosis for pathology in the Middleton-Goldsmith lecture for 1901 and need not now dwell longer upon the subject.

Third, I should class the studies which refer to the germ layers, those laminæ of cells which, as it were, occupy an intermediate place between the single cell and the organ. They correspond to the first orderly arrangement of cells which we have in the organism, and from this tripartite arrangement the organs are fashioned. Of course, the fundamental morphological fact in regard even to the higher animals is the cell, but next to that we may place the existence of the germ layers, the complicated interrelations of which dominate the entire history of every individual alike in health and in disease. The comprehension of the morphological importance of the germ layers and their relation to the production of tissues and organs and abnormal growths of all kinds is absolutely indispensable to every medical man who wishes to have an intelligent mastery of his subject.

Fourth, we may place the strictly anatomical aspects of embryology, which give the morphological interpretations of organs and the explanation, as you know, of many anomalies of adult structure. The anatomy of the adult offers to us many riddles, for numerous are the arrangements and characteristics of the body which we can not understand or explain from the study of the adult alone. The language of adult structure we often can not read unless we have first studied the Rosetta Stone of embryology which affords us the key of translation. As a teacher in a medical school, I have again and again been profoundly impressed with the value of embryology to the student of anatomy. Things which are obscure are illuminated by a knowledge of the developmental changes. In an embryo we encounter simplified conditions; secondary modifications coming in later in the course of development not only add to the complication of parts, but often also produce so great changes as to mask the fundamental and original relations. What student of adult anatomy alone could possibly discover that the thymus gland is a modification of the lining epithelium of a gill pouch which exists as a pouch in the embryo and is homologous with the gill pouch of a fish? Or what pure anatomist could ever have discovered that the spermiduct is the modified duct of a kidney present in the embryo, but which in the adult has as such totally vanished? If we pass from mere human anatomy to the larger and more scientific subject of comparative anatomy, we feel again the value of embryology, which establishes the real homologies of structure, proving exact homologies from the study of the early stages of parts, which in different types become so unlike that their fundamental identity of origin is completely hidden. For example, without embryology we never should have known that the little bone of the ear which we call the malleus is homologous with the upper part of the lower jaw of a cartilaginous fish. Indeed, the stories which embryology has to tell are the most romantic known to us, and the wildest imaginative creations of Scott or Dumas are less startling than the innumerable and almost incredible shiftings of rôle and changes of character which embryology has to entertain us with in her histories. I have been tempted to exclaim sometimes while pursuing my science that in embryology only the unexpected happens.

Fifth and last, I should like to gather under the head of morpho-physics a number of researches, nearly all of which are very recent, and which tackle the doctrine of the chemical and physical causes of development. These researches have been largely experimental in character, and though we are only at the beginning of this sort of work, yet the results already obtained are of the highest value and make us hope for far greater results to come.

There should be added logically a sixth heading for the physiology of the embryo, but so little has been done and so little is doing in this part of biology that only in the future can this logically correct sixth division correspond to a field of active research. Here poverty of achievement makes further present consideration by us superfluous.

It is unnecessary to argue in order to prove to you that the study of generation is of importance to the medical man. The results which embryologists have already offered in solving some of the problems of generation form part of the stock in trade of every practitioner, for every one must know something of the uterus and placenta, must know that there is no communication between the fœtal and maternal circulation—no passage of the blood from the mother to that of the child; that there is no machinery for the making of so-called maternal impressions; that conception depends primarily upon the fusion of two living elements, the ovum and spermatazoon, which arise as living and integral parts of the parental bodies, and must know thus that there is a continuity of life, an earthly immortality, and that from generation to generation life is uninterrupted. All these notions and many others derived from embryology are now-a-days part and parcel of every physician's information, and it is hard to realize that a short time ago many of these facts were unknown to us. I believe that in the course of the next few years many new discoveries concerning generation will be made which will in their turn become familiar to all. I expect especially in regard to the subject of heredity a great increase in our knowledge, because the subject has attracted many investigators and some notable results have already been achieved. I may instance the history of the germ cells in which I have been especially interested. Professor Moritz Nussbaum on the basis of certain observations which he had made, put forward in 1880 the theory of germinal continuity. He pointed out that there is noteworthy evidence in the development of various animals tending to show that the germinal cells from which the sexual products arise are separated off very early from the other cells of the embryo and undergo very little alteration until the time comes for them to be transformed into sexual elements, male or female, as the case may be. Dr. F. A. Woods, working in my laboratory upon the embryos of dog-fish brought the first conclusive demonstration that Nussbaum's theory is true for a vertebrate. He found that the germ cells are set apart, have a distinct history of their own throughout the embryonic period and do not contribute in any way to the formation of any of the organs of the body. Since then Mr. B. M. Allen has discovered that the history of the germ-cells in the turtle is strikingly similar, and Dr. Woods is now engaged in tracing out the history in birds. Every one present will, of course, immediately recognize the great importance of a discovery which tends to show that there is a permanent distinction between the reproductive cells and the somatic cells which belong to the body and do not serve for reproduction.

Concerning cytomorphosis I need not add anything to what has been said concerning its general value in pathological study, but I should like to refer briefly to the good results which we may anticipate from direct application of the notions supplied by embryologists to the investigations which are yet to be made upon what we may call the morphological diseases, in distinction to those which are of parasitic origin. Morphological diseases arise through intrinsic causes, abnormal conditions due to the body itself and its reactions. Parasitic diseases have extrinsic causes. The dramatic—I might almost say melodramatic—growth of bacteriology and the kindred sciences has caused us to give most of our attention to diseases of the infectious type caused by some vegetable or animal parasite. This tendency is to be so far regretted that it has rendered investigation one-sided and lured it away from the class of diseases which may be attributed to pathological cytomorphosis. In regard to these the fundamental problem is identical for the pathologist and embryologist. It is the question of what and how the change in the structure of the single cell may be. Here is a central problem about which a vast number of lesser problems revolve like satellites. In the solution of this and of allied problems our greatest hopes for the future progress of medicine seem to lie. If we can find out what are the conditions which cause a cell to change its structure and advance in its cytomorphosis, we may hope that that discovery will include the explanation of why certain cells develop abnormally and become, as we commonly say, pathological, and we are to have precise knowledge of the cytomorphic causes we may dare, even now, to hope that we shall learn to regulate them, and that some, at least, of the diseases which are now beyond our reach will come under our control. It was not long ago that the idea of conquering diseases like malaria, yellow-fever, diphtheria and tuberculosis seemed a mere dream, a beautiful dream, yet control of them is now a reality, and is becoming almost daily more assured, complete and beneficent. So too in regard to the strictly morphological diseases, knowledge may bring mastery; and even sclerosis, that disease from which we are all assumed to be suffering in varying degrees, may, ere long, find itself subject to man.

Of the services which embryology has rendered to medical science within the last twenty-five years, the best known and probably the most important, certainly the most spectacular and unforeseen, is the revelation of the structure of the nervous system and of the relation of the nerve cells to one another. This wonderful achievement has been due chiefly to the introduction of a single new method, named after its inventor Golgi, one of those brilliant modern men who prove that genius is still the gift of the Italian race. He was born at Corteno on the ninth of July, 1843. The method was first described in 1875 in an article on the fine structure of the olfactory bulbs. The method was so radically different from anything known at that time that it was treated with scornful incredulity, and no attention was paid to the new invention which was destined to revolutionize our knowledge until it was introduced in Germany by Professor Kölliker in 1887. This marvelous method has been found to work best with embryos and has enabled us to trace out the form, including their ramifications, of the nerve cells and neuroglia cells of the brain and spinal cord throughout their whole period of development. As you know, all our contemporary teaching in regard to the structure and functions of the central nervous system, our conceptions of the nervous mechanism within the central nervous system and in the ganglia, are based upon the results obtained through the application of Golgi's method by embryologists. It is pleasant to note that in 1903 the completion of twenty-five years of teaching, and, by a happy coincidence, the anniversary of his silver wedding, were celebrated by Professor Golgi's pupils by the publication of his complete works, 'Opera Omnia,' in three magnificent quarto volumes. Copies of this publication ought to be in every pathological and histological laboratory in the world. Indeed, every text-book of anatomy, embryology or pathology published now-a-days is a memorial of Professor Golgi, for they are all abundantly supplied with figures of Golgi preparations. We may see in this history an illustration both of the value of the embryological data and also of the almost creative power of a new method.

Degeneration has long been regarded as essentially a pathological process. This is the view which we have inherited. Nevertheless it is incorrect, as has been demonstrated by the more exact study of normal cytomorphosis during recent years. We now know that degeneration is to be looked upon as a normal end to a complete cytomorphic cycle. Instances of normal degeneration have long been familiar to us. Our mistake has been in overlooking their interpretation, their significance as part of the normal life. Thus the horny layer of the skin is made up of degenerated cells. Cartilage when it is replaced by bone undergoes a normal degeneration. In short, we must regard pathological degeneration very much as we regard those plants which we call weeds, things which are growing from our human point of view at the wrong time and in the wrong place, but which are not of themselves wrong or diseased, though they become so in man's nomenclature by their mode of occurrence. This broader conception of degeneration affords a new foundation for further investigation, and by the hearty cooperation of the embryologist and the pathologist we may expect new enlightenment.

The fourth head under which we classed the work of embryologist corresponded to the field of anatomical research. We all know that the embryological explanation of the anatomical disposition in the adult is a real and clarifying explanation. Certainly no teacher of anatomy to-day, competent to his work, will undertake to teach the structure of the brain, of the urogenital system or of the heart, except on an embryological basis. But there are a great many other anatomical conceptions which may best be made clear if we start with an examination of the conditions in the embryo. My experience as a teacher has afforded me many examples of this. Let me mention a few. The arrangement of the great cephalic nerves is a subject of peculiar difficulty to the student, but by the examination of a few properly chosen sections through the head of mammalian embryos all the essential topographical relations can be made easily understandable, and these essential relations are never obliterated by any further development. The disposition of the peritoneum is one of the greatest bugbears to the first-year medical student. But let him study the peritoneum in its relation to the viscera in the young embryo and he easily overcomes his difficulties and gets a clear and correct conception of the topographical relations of the peritoneal membrane and is able thereafter to comprehend the secondary modifications by which the adult topography is so much complicated. So too in regard to the thorax, a few sections from embryos give definite and exact conceptions of the fundamental relations of the heart and lungs, the mediastinum, and of the pleural and pericardial membranes. A good student may obtain from such a section a visual image which he will carry with him throughout life and which will always serve to make clear in his mind all these anatomical relations. One more similar instance may suffice. Students are always perplexed by the nature and mutual connections of the three membranes of the spinal cord and brain. Here also experience convinces us that sections of embryos reveal the facts so perfectly that they are readily comprehended and not easily forgotten. But I think I need not argue further to convince you that embryology as an aid to anatomical study is of incalculable value, and ought, if we are to do our anatomical teaching conscientiously, to be included in every medical curriculum.

Not infrequently the study of embryos establishes entirely new anatomical conceptions. An instance of this is offered by the study of blood vessels. We have learned in recent years that in addition to the long recognized arteries, veins and capillaries, there is another class of blood vessels of great importance. These vessels are called sinusoids and to a certain extent resemble capillaries, but their development and their relation to the parts of the organs in which they occur are entirely different from those of true capillaries. A sinusoid is developed by the subdivision of a single large vessel. It consists of endothelium, but that endothelium rests directly, or almost directly, upon the cells of the organ in which this type of vessel occurs. Capillaries, on the other hand, are developed as small buds from preexisting vessels and are always found in connective tissue. The most important organ in which sinusoids occur is the liver, and the peculiar circulatory arrangements in that organ, which have so long seemed singular and puzzling, have become comprehensible, and have acquired greater significance since the conception 'sinusoid' was established. This newly formed conception has unlocked the mystery of the portal circulation, and has explained the supply of venous blood to the liver. A morphological explanation of the portal vein had previously remained impossible.

To the fact that embryology explains many anomalies of the adult structure, we have already referred. Let us leave out of consideration the true monstrosities and confine our attention for the present to the anomalies, which are due to arrest of development. These are comparatively frequent, and many of them are so definite we may fairly call them typical. Such, for instance, is the preservation in the adult of the foramen ovale between the auricles of the heart, or of the open ductus arteriosus by which blood of the pulmonary and body circulation may mingle. In case of the veins also an arrangement of vessels is often found in the adult which is due to the persistence of a truly embryonic condition. The urogenital system seems to be peculiarly subject to arrests of development. When it starts the rudiments for both sexes are complete and the two sexes become differentiated largely by the obliteration in the individual of one sex of those structures which are characteristic of the other, but not infrequently it happens that this law of suppression is disturbed, and we then get very interesting and significant anomalies with which the physician often has practically to deal. Such cases do not produce a true hermaphroditism. That is a condition which apparently may occur in the human species, but is of the utmost rarity. Among other of the most frequent and familiar illustrations of arrested development I will mention cleft palate and hair lip. It is quite unnecessary to prolong this list, for it is evident that the anomalies we are considering are of a definite prescribed nature. They are all of practical importance to the physician, and unless he knows something of embryology he can not know what these probable anomalies are. If he does know something of embryology he can understand much of what may be expected in this class of variations of structure.

We will turn now to the fifth and last of our headings, that of morpho-physics. It is only of recent years that methods of experimentation, as distinguished from methods of observation only, have been applied to embryological problems. Naturally under the circumstances many crude experiments have been undertaken, many of doubtful validity; but there have also been many others, soundly planned, rightly executed and fruitful in results. Already the new conclusions constitute an increment both large and precious to our stock of embryological knowledge. One important class of these experiments has been based upon the discovery of the possibility of grafting parts of amphibian embryos on to one another; or to get two large pieces of two distinct embryos, or even two halves of two embryos, to grow together. The grafting experiments which have already been made are very numerous. Let me present one or two examples of the sort of results that these experiments yield. If the halves of two species of frog in a very early stage are grafted together, they will unite perfectly, but it is found that the epidermis of the species which forms the anterior half of the graft will spread to a certain extent over the posterior half, thus showing that the skin can actually crawl over the underlying tissues. It is probable, indeed, that the migration of epithelial cells along the surfaces upon which they rest is a very general phenomenon, and plays a very important part in the animal economy. In another series of experiments the embryonic optic vesicle has been removed and grafted on to a new part of the larva. Where the optic vesicle comes in contact with the epidermis it causes the epidermis to form a typical lens for the eye. Thus it is proved that the formation of the lens is not a specific function of that part of the epidermis from which it is normally produced, but is a potential function of the entire embryonic epidermis which may be called forth into activity by contact with the growing optic vesicle. I believe that we have in this an illustration of one of the fundamental principles of the establishment of structure and that much depends upon the interaction and mutual stimulation of parts.

Another class of experiments has been conducted by those who have been somewhat jocosely named the 'egg shakers.' An egg during an early stage of segmentation is divided artificially into its natural segments, or into groups of such segments, as the case may be. In many cases this division can be accomplished by shaking the eggs somewhat violently so as to break the segmentation spheres apart; hence the name above quoted. Now it has been demonstrated that in some cases fragments of a single egg will develop into an embryo perfect apparently in structure, though only of say half the normal size, whereas in other cases half an egg will develop only into half an embryo. Investigators are still busy studying out these results, the final interpretation of which has as yet by no means been reached. The experiments have opened to us a new realm of inquiry full of astonishing surprises.

Experiments on artificial parthenogenesis have been much written about in the daily press, and many absurd things concerning this topic have been printed in the newspapers. Ordinarily the ovum requires to be fertilized in order to develop, but it has long been known that certain ova, of bees, of plant lice, of some crustacea and of other animals will develop without being fertilized. To this process the term parthenogenesis has been applied. Artificial parthenogenesis designates the development of unfertilized ova which normally would not develop at all and which are stimulated to development by placing them under artificial chemical conditions. Doubtless many of you have seen in the newspapers these experiments referred to as if they gave the actual creation of life. Of course that is nonsense. The life is there in the ovum. What artificial parthenogenesis accomplishes is to supply a stimulus, chemical in nature and capable of replacing the fertilization by the spermatozoon, which would otherwise be necessary. The possibility of artificial parthenogenesis was first partially demonstrated by Richard Hertwig, but has been perhaps more studied by Professor Loeb, now at the University of California, than by any one else. Hertwig produced artificially only a development of very limited degree, but Loeb by treating the eggs of a sea-urchin for about two hours with a weak solution of magnesium chloride succeeded in 1899 in producing larval sea-urchins (so-called *plutei*) from unfertilized ova. He concludes from this that fertilization is a chemical process, and that it is distinct and separate from hereditary transmission. No words of mine are needed to emphasize the importance of such investigations, for they are basic.

A line of work combining experimental and observational methods in which I have been especially interested deals with the problem of growth. It can be shown statistically that the growth of the embryo in early stages goes on at an enormous rate, and also that during the period of fœtal development that rate is constantly declining, so that something over 98 per cent. of the growth power is lost by the time of birth. After birth decline in the growth power continues, but gradually the decline becomes slower and slower, so that though growth is slight in rate, the growth power is long continued. A study of the condition of cells while this decline of the growth power is going on reveals to us that while the growth power is rapid the nucleus of the cell is active and well developed, and that the protoplasm of the cell is but slightly developed. As the proportion of protoplasm in the cells increases the power of growth diminishes, and as differentiation of protoplasm goes on the power of growth diminishes. I consider it probable that the growth and differentiation of protoplasm is the direct cause of the diminution of the growth power. The observations on growth bring out clearly to our minds the conception that the decline is by far the most rapid in the very early periods of embryonic development or, better expressed, that the rate of decline is at its maximum during the earliest periods. The older the individual becomes the less is the power of growth, but also the less rapid is the decline in that power. Thus we reach the paradoxical conception that the period of most rapid development is also the period of most rapid decline. This view, it seems to me, applies to all development, at least in the higher animals. As I have spoken on this subject more fully elsewhere, I will not pursue it longer now, but it seemed to me desirable to refer to it as an illustration of the far-reaching character and wide scope of embryological investigation, which inevitably allies itself with every other biological science.

It would be no difficult task to extend my discourse by multiplying illustrations of the beneficial influence of embryology upon other departments of medical science. It is one of the institutes of medicine—a part of the foundation of knowledge upon which medical practise is erected.

Embryology supplies facts which are directly valuable to the practitioner. It supplies explanations and interpretations of many anatomical structures and relations which would otherwise remain incomprehensible. It supplies the clues to many common and rare anomalies, and it supplies to pathology a series of fundamental conceptions, without which our actual present pathological knowledge could not have been upbuilt. These claims of embryology to recognition are very great, but nevertheless they do not include her greatest claim to a preeminent place among the medical sciences. That greatest claim is established in my opinion by the contributions of embryology to the solution of the problem of organic structure.

Structure is the only distinctive mark of living bodies, by which we know them to differ from inanimate objects. In the final discrimination between living and dead all other distinctions fail us or at best are utterly uncertain. In the higher forms we see differences of function always correlated with visible differences of structure. From such evidence, together with much other, we have established the hypothesis or theory—for at best it is only a theory—that all living functions are dependent upon organic structure. It is quaint, we may remark in passing, to read in recent essays by a learned German botanist the announcement of this theory, which the vast majority of biologists have long adopted, as a *new* foundation for biological philosophy, because he terms the ultimate unknown facts of structure 'Determinanten.' How often has science been impeded by the intrusion of a pedantry which mistakes the invention of a new term for the introduction of a new idea!

To find out what structure really is is the goal of all biological science. When we discover this secret, we may hope to discover also how structure functions and why it exists. The problem of structure—of organization—is double; there is first the question, what are the essential qualities of the structure of living matter as such, and there is, second, the question of the variations and specializations, which structure may undergo. With both of these questions embryology is confronted and both of them it is seeking to answer. The first is the riddle of life. Embryologists are bravely attacking it and have, I believe, already made a little real progress towards its solution. To them it presents itself as a series of queries concerning the germ-cells and the fertilized ovum. Searching analyses of the details with the highest powers of the microscope and the most refined technique coupled with experiments have indeed increased our knowledge of the organization of the germ cells. America, thanks to the brilliant work of E. B. Wilson and E. G. Conklin, and of their associates and followers, occupies a leading position in this difficult exploration. The importance of knowledge of the fundament of organic structure can hardly be exaggerated, and when it is obtained it will, I may prophecy, have profound far-reaching and enduring effects upon all medical science.

Even more intimately is embryology occupied with the second part of the problem of structure, namely, the question of differentiation, *i. e.*, of the gradual production of the varied organization of the adult with almost innumerable unlike parts. To-day the central problem of biology is that of differentiation, and the main purpose of cotemporary embryological research is to attack that problem. The problem is three-fold, for we must learn what differentiation is, how it is produced, and why it is produced. Embryology might almost be termed the science of organic differentiation. Now all that you do, as practising physicians, is to deal with differentiated organs and tissues. You deal with a function, normal or diseased, which is rendered possible by the differentiation of cells. You deal with pathological states, every one of which has its special differentiation. Every phenomenon which you encounter in your professional work is conditioned by the differentiation of the organic living substance. To regulate that differentiation, to set it right when it has gone wrong, is the brightest vision of future human power which I can conceive, and I can not but think of embryology, which strives unceasingly to discover the laws of differentiation, as that Institute of Medicine which is to be the foundation of a greater practical medical progress than any yet achieved. The physician's knowledge is the mother of mercy.

THE LARGEST AMERICAN COLLECTION OF METEORITES.

By L. P. GRATACAP, A.M.,
AMERICAN MUSEUM OF NATURAL HISTORY.

IT requires very little imagination to picture to our eyes the astonishment of the inhabitants of the older portions of the earth at the fall of meteorites in days before scientific knowledge had reduced them to ordinary phenomena. What could be better calculated to excite admiration and reverence than a luminous missile suddenly passing athwart the sky, accompanied by detonations, and almost simultaneously reaching the ground? Was it remarkable that superstition quickly enclosed them in its mesh of fable and fancy? Believing that the gods were accustomed to descend upon the earth, these visible apparitions of flame might not unnaturally seem to them the vehicles, or at least the portents, of their descent.

In fact, a series of interesting medals or coins struck off to commemorate these unusual visitations has been found amongst Roman and Grecian antiquities, which have been styled 'Betyl Medals,' from a supposed reference to the Hebrew 'El Bethel,' the house of God, thus implying that the meteorite was indeed, by contemporaries, regarded as a supernatural object.

Science and observation have long ago determined their cosmic nature, and while opinions may still vary as to their exact origin, their actual constitution is well understood, and their source, in extra-terrestrial streams of moving matter, recognized.

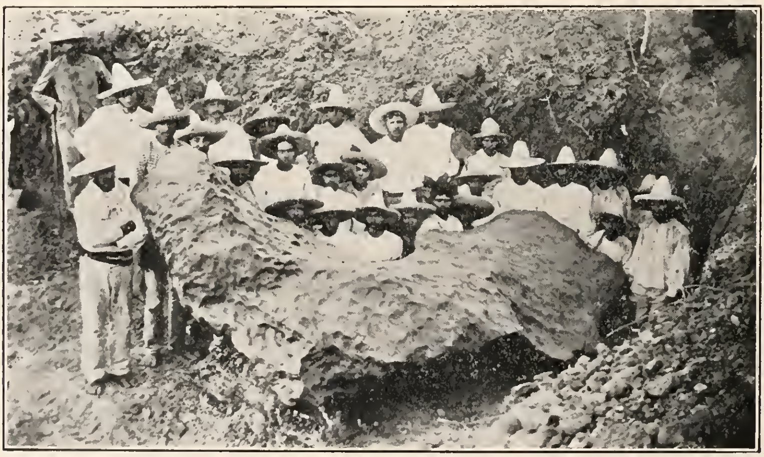
These strange objects have not diminished in interest because their miraculous origin has become a myth. On the contrary, science, by its exhaustive research, has placed them in the very front rank of objects that excite most vividly the imagination of the investigator. When science, in a perfectly rational way, speculates upon the possibility of one of these celestial visitors having passed through stellar space at measureless distances from our planet, exposed to inconceivable degrees of cold, and again at another time and place in its long transit to have encountered the most intense heat in the neighborhood of the sun, it requires little more suggestion to make it clear why they are to-day placed amongst the most prized specimens in mineralogical collections. The little further suggestion required to awaken the lay mind to a vivid realization of their interest is contained in the modern conception of their origin in dismembered comets, or even disrupted worlds.

Museums and individual collectors have vied with each other in

Cuernavaca, Mexico, Section of Iron showing Figures.

securing meteorites, and the details of falls, their date and locality, the comparison of types of meteorites, their mineralogical components and their many physical peculiarities have been so carefully recorded and studied that a special science has resulted, and a special group of students, whose rivalry is more animated than that generally discovered elsewhere amongst collectors of one kind of natural object.

The collection and the collector of meteorites have, with few exceptions, been features of the nineteenth century only. Previous to that these wonderful objects elicited surprise, and perhaps superstitious regard. Only within the nineteenth century did their careful comparison and study begin. All such collections as the great cabinets of Vienna, London, Paris, Berlin and the many smaller ones distributed through Europe have been slowly formed, the actual supply of material being dependent on meteorological incident and the results of travel and observation. In America, the larger collections at Yale, Harvard,



Bacurbito, Side View.

Amherst and Washington were very gradually brought together. There was no concentrated effort made to extend them; the scientific thought of the last century, except towards its close, had not keenly awakened to a realization of the almost marvelous connotations implied in these strange aerial vagrants, and the opportunities for their discovery had not been actually availed of.

No one in the United States has exhibited greater perseverance and a more boundless, almost reckless, enthusiasm in this work of collecting meteorites than Professor Henry A. Ward. His audacity and zeal have gone hand in hand with a very keen scientific sense of the meaning of meteorites, and an admirable acquaintance with the literature and the results that have developed in their study.

He has himself been an explorer in this field, and it would be safe to predict his first arrival at the scene of any new meteorite's fall to-day. His correspondence is extensive, and the merest mention of a meteorite occurrence flies to his desk, and is very quickly subjected to his pertinacious system of verification or exposure.

The Ward-Coonley collection of meteorites now exhibited at the American Museum of Natural History represents his tireless work through many years, and stands to-day first amongst the collections of meteorites in this country. In the possession of large, unique masses, other collections may at points excel it, but in its representative character and in the actual number of 'falls,' it surpasses all others. The reader unacquainted with the peculiar pride of meteorite collectors may, perchance, welcome a little elucidation.

Meteorites are named from the locality in which they fall, or are found. But few meteorites have ever been seen to fall, and hence the meteorite mass, when discovered, is given a name (by which it is ever afterwards distinguished) that is derived from its exact locality or neighborhood. Thus Cañon Diablo, Arizona, Mincy Taney Co., Mo., Brenham, Kansas, Mocs, Transylvania, Estherville, Emmet Co., Iowa, are familiar labels in these collections. These designations sometimes of necessity assume a curious character, as the Vaca Muerta meteorite, or 'dead cow,' so named in the desert of Atacama, Chili, from its proximity to the corpse of that quadruped, the only, or at least a striking, physical feature in an otherwise featureless waste. Such a name remains after its origin has disappeared.

A certain number of localities, however, have frequently proved to be but representative of a prolonged fall. A meteorite mass, meeting the atmosphere of our earth, becomes, through friction, enormously heated, disruption takes place, and the separated parts, instead of falling at one spot, are dropped in succession at widely removed points, and thus a series of names becomes synonymous. Long examination and careful comparison, such, for instance, as Fletcher, of the British Museum, has devoted to the Mexican falls, or Dr. Brezina, of Vienna, to those of Europe, only can correct erroneous nomenclature of this kind.

Again it is entailed, in the trials of a collector's work, to find some of his 'falls' spurious, that the specimens are illegitimate or are terrestrial; an accident which may awaken irreverence in the lay mind, but which has sent a shock—often salutary—throughout the community of 'star-gatherers.'

All falls or finds are recorded—generally in a description authoritatively made by the finder or a scientific acquaintance—and the objective goal to be reached by collectors is to have a representation of all such occurrences. Obviously size or weight is significant, and it is not

Cuernavaca, Main Mass.

unnatural for a layman to insist upon the superiority of a smaller collection with handsome examples over a larger collection where the specimens are diminutive or insignificant. The masses of various localities differ also greatly in size, the amount of material falling varying enormously in different falls, and so the value of a particular kind of meteorite is conditioned upon its initial size. It is quite evident that a mass of one thousand grams will not admit of such attractive subdivision as one of five thousand, and, in the case of the Angra-dos-Reis aerolite, one of the rarest of meteorites, there is not enough to 'go round.' The Angra-dos-Reis unit also possesses peculiar lithological features, which naturally enhance the value given to it by its physical diminutiveness.

Colossal intruders from space, such as the Anighito, brought by Peary from Greenland and weighing (calculated) over forty tons, the Mexican Bacubirito of similar weight, and the large Chupaderos monster, weighing over fifteen tons, while easily distributed to collectors, will eventually weigh more significantly as unique features in their entireness in the museums destined to receive and install them.

It is desirable to call close attention to the admirable results of Professor Ward's labors, and to emphasize the preeminence the Ward-Coonley collection now takes in American cabinets.

Professor Ward has taken every possible pains to perfect and enlarge his collection. He has purchased and exchanged, and has traveled the world seeking almost inaccessible masses to obtain new examples. An instance of the latter was his exploit in reaching and

WILLAMETTE, LOWER SURFACES SHOWING CAVITIES DUE TO DECOMPOSITION.

unearthing the Bacubirito iron in Mexico, and a more recent venture in studying the anomalous features of the Willamette iron in Oregon under severe meteorological drawbacks.

This collection has been signalized by the most striking compliments from original workers and directors of museums in Europe. Professor Carl Klein, the state counselor and director of the Royal Mineral Collection at Berlin, has referred to it as 'one of the finest and richest meteorite collections in the entire world'; and Dr. Brezina, the most famous student of these objects, writes: "Professor Ward has succeeded in reaching the highest number of localities now united in any collection. I count up to this day 689 localities described or mentioned, twelve of which are dubious for want of precise dates. From the remaining 677, I find 603 represented in his list, while the great Vienna collection gives but 557, counted in the same manner. The average weights are likewise highly satisfying, the total average, viz: the whole weight of the collection divided by the number of localities is over four kilograms (now 4,138 grams) for the locality."

This is the analysis of a technical expert, and might perhaps but poorly reflect the impressions of one less solicitous about percentages and exhaustiveness. The ocular view must be considered, the sensible visual effect of interest and wonder. In this respect the Ward-Coonley collection is eminently adequate to extort praise. It is now arranged in seven beautiful cases at the north end of the Hall of Geology at the American Museum of Natural History, and the specimens classified in their three groups of iron (Siderite), iron and stone (Siderolite), and stone (Aerolite), present their extended, yet thick and close ranges, most effectively to the spectator. Superb polished slabs, etched and developed, follow one another in the stepped series; and the bewildering number of aerolites, many large, and showing that invaluable and (to the eye of the collector) most exquisite test of certainty, the dark glistening crust of fusion, succeed, installed upon attractive mahogany standards; while a supplemental section of casts, reveals the original form and appearance of many celebrated or singular meteorites.

One of the Seven Cases containing the Ward-Coonley Meteorite Collection, American Museum of Natural History.

The assemblage is a striking and forcible tribute to Professor Ward's enthusiasm and persistency. A moment's reflection upon the order of events in the history of this great American collection of meteorites is not without interest. Professor Ward had been known as the indefatigable explorer of the continents and seas of the earth for all kinds of natural objects by which our American museums have so largely profited, but it was not until he had disposed of the great collection of natural history specimens which he exhibited at the International Exposition at Chicago in 1893 that he felt free to seize an individual field of study and enterprise. His mind had been deeply moved by the appeal made to it by these mute messengers from space, and a certain challenge offered by them to his ingenuity and skill to find and possess them. He gave himself over literally to this pursuit with a single-minded persistence that only success could reward. Professor Ward has played the part of *exchanger* to its fullest limits. In this he has acted upon a very acute design. In all large museums and with most active collectors, there are specimens that, as Professor Ward puts it, 'money could not dislodge.' Only the actual offer of pieces as rare, or more rare, and to which all approach was absolutely closed, except by the avenue of exchange, could displace them. Under such pressure Professor Ward has happily gathered into his possession many a piece of celestial iron or stone, which otherwise would have remained as immovable as Fitz-James before his less capable assailants.

N'Gourema Meteorite, Soudan, Central Africa. Fell June 15, 1900.

But practical and effective as has been exchange, as a means to an end, the infallible efficacy of money has been no less powerful. The actual expenditure has been very great, and in this pursuit, as in war, the cost for a successful issue must not be counted. Professor Ward enjoyed two unusual opportunities for suddenly increasing the size of his collection. The Gregory collection in England and the Siemaschko collection in Russia, vying with each other as the largest private collections in Europe, fell into his hands at the death of their owners, and from these treasuries he filled the gaps, and enriched the precious contents of his own. His collection is one of increasingly rapid growth, and illustrates the quantitative results of single-minded effort, precision of attack at vulnerable points and prodigality of purchasing power in a scientific warfare where the victor only receives the congratulations of his generous competitors.

Looking over Professor Ward's recently published catalogue, some concluding facts in this summary are interesting and convincing. In an introductory paragraph of his catalogue (1904) the author says: "The geographic sources of the collection are world-wide. Australasia and Asia, Africa and South America, are represented each by 95 per cent. of all their known meteorites, while North America and Europe bring up the train with 99 per cent. of the former and 97 per cent. of the latter. No collection in the world can say of itself more than this. Attention is particularly drawn to the series from Japan, Australia, Russia and Mexico. It is only within the last decade that the rare and interesting meteorites from these countries have been largely distributed. To-day it is true that in no collection in any one of these four countries are there so many kinds from that country as are represented in this collection." The catalogue further notices that in this remarkable collection thirty falls, irons and stones, represent the largest single piece of that fall to-day known.

The final impression left from an inspection of the Ward-Coonley collection is one of admiration and of natural astonishment that so complete an assemblage of these valuable and coveted wanderers from space could have been gathered together by the activity and industry of one individual. Their acquisition places the institution that receives them among the four most important meteorite depositories in the world.

Summary of the Collection.

Total number of falls and finds	603
(Siderites 241; Siderolites 28; Aerolites 334.	
From North America	229
From South America	31
From Europe	231
From Asia	77
From Africa	27
From Australasia, Sandwich Islands	26

Total weight of entire collection 2,495,429 grams (= 5,509 pounds).

Average weight of each kind 4,158 grams (= 9 1/9 pounds).

Average weight, counting nothing over 50 kilograms to a kind, 1,746 grams (= 3 4/5 pounds).

Total number of specimens, about 1,600.

THE PLANE OF ETHER

(Being Further Extracts from the Records of the Astral Camera Club of Alcalde)

By President DAVID STARR JORDAN

STANFORD UNIVERSITY

AT the December meeting of the Astral Camera Club, through the courtesy of Madame Yda Hhatch, of San Diego, vice-president of the American Chirological Society, the club received a rare treat, direct from the fountains of the Orient.

Madame Hhatch is an adept in the science of palmistry and therefore a person of wealth and culture. She is pleased to exercise her gracious prerogative of patronage to scholars of all lands and of all beliefs. By her kindly interest the club was favored with an address by the Swami Ram Telang, of Bombay, from the Congress of Religions in Chicago, the substance of which, omitting the Hindu words I can not understand, I shall try to transfer to these records.

The Swami Ram Telang is a slender, dark-skinned Brahmin, with a delicate moustache and a complexion of varnished leather. His finely cut mouth bears an impress of sweet patience, while his dark soulful eyes have a deep inward expression, as though earthly matters were but a veil, half hiding the light of the inner vision. He wore a white turban after the manner of his class, and his white and purple robes were very becoming to his gentle but manly figure. Madame Hhatch impressed upon us the importance of refraining from all contact with these robes, for a profane touch would soil his aura, besides impressing the severest pain upon his sensitive Nirvanic nature. For like reason he must be sheltered from the odor of cooked meat, while even the slightest approach to a butcher's shop on the street caused him the nausea and shudders. As he himself confessed, the perfection of being which he had attained was not an unmixed blessing, for the proofs of sorrow and suffering were ever in his sight. "Why hast thou cast Ram," he said sometimes to his Lords of Karma, "into the time of the ever-blind to proclaim thine oracles with the opened sense?" But he was very kind withal and very patient and accepted with kindness our offerings of adulation.

In this, my report, I can do but scant justice to his spoken words, for though I am not without literary taste and facility (though I say this, who should not) there was something in the lofty ideas and perfect expression in the measured words of this gifted young Brahmin which is beyond any effort of mine.

He began by an account of the five usually accepted planes of existence, the physical, the astral, the devachanic, the shushuptic and the nirvanic. But to limit these planes to five was, he said, 'a great error,' introduced into our philosophy by the too practical British mind which even in esoteric matters is crowding to the wall the finer understanding of the Hindu. The sole perfect number is seven and there must be seven planes and in his own experience as a wayfarer to Devachan and Shushup, he had always and ever found it so. Above the astral plane, already familiar to us at Alcalde lay the etheric plane, as the ether lies beyond the stars, while still beyond is the omnic plane, the Loka of the Perfect Silence.

Of the etheric plane, he chose to speak to us. He first emphasized the fact that all these planes and the things they contained are real, 'as real,' he said, 'as the American Hotel on the main street of Alcalde.' As all dreams came from emanations or excursions into these higher planes, all dreams and their contents are real also. In fact, there is no apparition which is false or illusory. The only illusion is the denial, and denial is the essential characteristic of that western philosophy, which is blighting the earth and changing it from a sphere of dreams and happiness to a world of war and commerce washed by a sea of aimless discontent. This is the natural effect of life on a physical plane. It leads to idle strife and constant struggle, as its greatest exponents have freely admitted, and its only hope of progress is in the killing off of all those who are useless in war and unskillful in making trades.

The scenery, inhabitants and actions in each of these seven planes are in part peculiar, each to its plane. In part they are the doubles or phantoms of the objects found in the plane next lower. For the finer matter of the higher planes permeates and penetrates the coarser objects of the planes below. Hence it transpires that to one in the plane below, the higher object, if he is aware of it, seems like a shadow or a phantom. Because he can pass through it is his argument for its unreality. But in like measure, to the astral or ethereal being the physical man seems quite as unreal, for with equal ease the being can walk through and through him, injuring him or helping him, just as he may elect to choose. By such means an evil-minded shadow may work dire revenge for injury done in another plane.

The higher the plane the more illusory the impressions we derive from it, not because of their unreality, but because of our own lack of training. For on the higher planes, objects change their forms with protean swiftness, casting a dazzling glamour from their aura as they change; again, sight on the higher plane is very unlike physical vision. Even so low as the astral plane the inside of any solid object is as plainly open to the eye from the outside, and all objects near or far are seen alike without perspective. The fourth dimension, the dream of geometry, is plainly visible and in the omnic plane is clearly seen a fifth or sixth dimension. With these chances for error, it is not strange that the Feringhi, or English writers, on the higher planes have made frequent errors, while even the Hindu adept is never quite infallible. As a preparation for such investigation crystal-gazing has been found desirable. Still better is the formation of circles of silence when men and women gather around a white lily or other creation of purity and beauty and, clasping one another by the hand, endeavor to think Ether and Om. After many years of these exercises, gathering around perfect objects under his direction, the Swami said, a few devoted women had even risen to think Devachan and Shushup. No Englishman had, however, come to this and in our Caucasian race not even a woman of English birth had ever been able to think Nirvana. For in Shushup all desire to act is lost, as in Om is all desire of speech. In Nirvana alone is the absolute extinction of all desire—a thing impossible to you Americans.

The scenery of the etheric plane is much as in the physical existence, only more wavering, more delicate, more enchanting. Its objects of fine matter, finer than anything in the atomic or molecular way, freely interpenetrate all merely physical matters. It is therefore not necessary to seek it far from home. In India and Thibet, this scenery is peopled with its multitudes of beings, for these are the oldest lands of man, peopled for ages with him and his creations. California, on the other hand, with the exception of a few areas, as Point Loma and Chinatown, is virgin soil in its astral and ethereal aspects, its sole abundant life being the nature emanations and the essential spirits of animals. One may wander in its verdant etheric shades for weeks and never encounter a human creature. When one meets such a being there on the etheric plane, she is most gracious and friendly, her company a welcome recompense for months of loneliness. The appearance of a friend in these wastes is signalized by the glimmer of his aura which, as a most learned occidental adept has pointed out, appears "as an oval mass of luminous mist of highly complex structure, from its shape sometimes called the Auric Egg." This writer maintains with apparent truth that these auras are not mere emanations but the actual manifestation of the ego on their respective planes. "It is," he says, "the auric egg which is the real man, not the physical body which on this plane crystallizes in the middle of it." This is certainly clear if we keep in mind the difference between this ego-aura and the physical health-aura which is the first purely astral object seen by the untrained and seldom enters the etheric plane. For the sake of clearness we should call the contents of the auric egg the 'etheric double,' which is merely a new name for a very old idea, for it has long been known that the mold on which the physical body is made up is held in perpetuity by the agents of the Lords of Karma. Thus, however complicated and unusual a man's Karma may be, these agents, or Lipika, are able to give a mold in accordance with which a body exactly suiting it can be formed.

The inhabitants of the etheric plane are as varied as those of the astral and physical planes, with this distinction, that certain types and races of men never rise above the last-mentioned level. For example, English is scarcely spoken on the astral plane and never on the etheric, where for most part is heard only some form or derivation of the Sanscrit. Those who aim to make of the bluntly practical English a universal tongue must soon see the futility of such efforts. For a language which barely reaches the astral plane can never be universal, while at the upper limit of Devachan, as is well known, all language disappears. Only the Voice of the Silences, the vibrations of the perfect Om, the one word which closes the lips whenever it is spoken, is heard in the Crystal Vaults Superior.

The learned Swami proceeded to divide the inhabitants of the etheric plane into seven great classes, for the sacred number seven is involved in all these investigations. These are (using our inadequate English words): (1) The embodied, (2) the bodiless, (3) the shells, (4) the incubi, (5) the extra-planetary visitors, (6) the essences, (7) the artificials.

Those of the first class, or embodied entities, have bodies on earth, still engaged in operations on the physical plane. These may be adepts or Chilas, or they may be canny yogis, or Initiates, possessed of second sight, or again ordinary men fast asleep at home, whose etheric doubles are drifting about involuntarily, swayed through esoteric currents, or the so-called 'winds of dreamland' of your western mythology, or finally they may be magicians, white or black, seeking their own ends, some of them unfortunately evil. The investigator may meet also on this plane students of the invisible, quite unconnected with the great master and knowing nothing of the mahatmas. These men, Slavs, Lapps or Malays, are often most earnest and self-renouncing seekers after truth, and the wise wanderer on the etheric plane will greet them with the omnic kiss of esoteric brotherhood.

Of the bodiless class are likewise many, some of whom have wandered from afar. Even from Nirvana, perfect souls have descended, creating for themselves a perfect ethereal body for the purpose, since the more refined vesture of Nirvana would be invisible to ethereal sight. These souls never attempt anything in these lower planes, for in Nirvana is the final quenching of desire. Here too, the ego of adepts may await reincarnation, an unusual mode of procedure, it is true, but sometimes to the self-renounced such favor is granted by the great master of reincarnation, Gautama-Buddha himself. But here the greatest care is necessary while the matter is arranged, lest by a touch of the Devachanic plane the waiting ego be swept by irresistible current into the line of normal evolution. In some cases, to avoid this strange accident, incomprehensible to you who have never left the limits of Alcalde, and to escape the trouble of a new birth, with the pains of teething and the other woes of childhood, the adept will enter some adult body, abandoned temporarily or permanently by its former tenant. The advantages of remaining bodiless are, however, considerable, as this avoids the hampering possibility of fatigue while retaining perfect consciousness. The disadvantage is that as nothing can touch the being in question, it can touch nothing for itself. Here, too, gather many of those who have abandoned the physical life, to be returned to it, in reincarnation, not having perfectly achieved higher possibilities. Most of these remain, however, on the astral plane, never falling below the density of shades.

The third class, known as 'shells,' are soulless bundles of qualities fastened together by will or thought and so retaining a human resemblance though all vestige of humanity has departed. Such, when seen by the canny, are recognized as ghosts. In writings of adepts you will find accounts of psychically developed persons, that is, persons possessed of second sight, who have seen about cemeteries 'hundreds of these bluish-white misty forms, hovering over the graves where are laid the physical vestures which they have recently left.' These shells may be galvanized into a very horrible form of temporary life, but this is only done by the loathsome rites of one of the worst forms of the black magic, which you of Alcalde, most of you not even initiates, much less adepts, could never be made to understand.

Still more unpleasant are the incubi, or clusters of wicked qualities, wrenched by violence from their possessors as a stone is torn from an unripe fruit. These take a fiendish delight in exercising the arts of delusion which the higher plane puts in their power to lead others to the excesses which proved fatal to themselves. For such a one to meet a medium with whom it is in affinity is indeed terrible, for by such means its existence may be indefinitely prolonged. Still worse are the vampire and the were-wolf, which we of the Fifth Root Race now seldom encounter, but which, to Slavic and Germanic adepts of the earlier centuries were objects of fear and danger in the astral regions and even as high upward as Devachan. All these are human in their origin. Not so the fifth class, the occasional visitors from other planets. Of these wonderful creatures we know nothing, for only the highest of high adepts have the power of moving from planet to planet, and even I do not understand how it is performed. When these visitors appear, they choose a body temporarily created out of unused ethereal matter belonging to the earth. Over this they wear a distinctive badge, a ring indicating Saturn, a series of belts indicating Jupiter or a tiny flaming spear and shield for Mars and a silver mirror in a golden necklace for Venus. In Shushup, it is said, guests for all the signs of the zodiac are received—but of these I have seen but two, a charming young lady from Virgo and a mahatma from Sirius who bore the badge of a great dog or wolf.

The sixth class are elemental essences. These may be either mineral or monadic in their nature, this depending on their origin. Usually they begin as a thought, aspiration or association of ideas, permeated by its appropriate variety of the deva or life principle, hence capable of floating or drifting about through etheric or astral space, until at last crystallized as an ego and embodied as a man. The history of these monadic essences is still obscurely known, as few adepts have the patience to watch them continuously through their centuries of development and incarnation. These often pass through the stage of animal essentials, some of whom are at last incarnated as animals, and the learned author of the 'Secret Doctrine' describes his encounter with a number of these essences 'embodied in anthropoid apes, already individualized and ready to take human incarnation at the next round or even sooner.'

Then in the etheric plane are swarms of nature spirits, the tiny emanations of the sunny banks of moss, the foamy waterfall or the fragrance of the roses. Some of these are dim and gigantic, the products of the mighty cañon, the roar of the sea, or the awesomeness of the forest. These may assume all forms at will, but when at rest they take the shape that is most befitting their natures. Ordinarily they are out of human sight, but they have the power of self-materialization, or they can be formed into visual clearness by the effort of a powerful will. Such essentials are known to us of the east as djinns and sprites and peris, but in the west they have many names, fairies, gnomes, elves and imps, and the Greeks called them fauns and satyrs. Those which live in water are called undines, those which live in air are sylphs and those in fire are salamanders. The wild essential spirits do not like the presence of man, though they often try to help him or sometimes to play little tricks on him for their own amusement. They have no real dislike for humanity, but the constant rush of astral currents set up by the restless ill-regulated desires of Europeans disturbs and annoys them. In India, they are more at ease and lie about under the palms and the bulbul trees. In Olcott Sahib's beautiful gardens in Madras, Ram has spent many joyous evenings in commune with them. Similar creatures are ever present in California, giving your state the indescribable charm of which you all talk so much and feel so little. In a quiet stroll in the woods near Alcalde with Madame Hhatch, Ram found them in myriads, some of them lurking in the branches of the eucalyptus trees, others had burrowed in the dry ground to form fairy homes. In Ram's visit to Angels he found them thick under Abner

Dean's great pines. Around the abandoned mining shafts there were other essentials of an evil disposition with an unpleasant smell of sulphur which suggested human origin. When examined and unrolled by means known to initiates, these sprites were found to be mere bundles of oaths held together by the force of spent passions and evaporate spirits of rye. The highest of these essentials or non-human entities is the deva, a superhuman essence destined to become man, but which has remained fixed for a time in an intermediate or higher stage. Such creatures, according to the common expression in Devachan, 'have yielded to the temptation to become a god.' No blame attaches to this expression. The path to incarnation thus selected is not the shortest one, but it is a very noble one, and for some not yet well tempered for humanity it is the one best suited to their natures. It is of course impossible at this stage of our striving to tell when we shall have earned the right to choose our own future. We should not, at any rate, before we ourselves reach Devachan, be too ready to despise those who have never seen fit to drop below that level. These devas are the winged globes and fiery wheels of our secret doctrine. There must of course be seven classes of these as there are seven of nature spirits and seven of elemental essences. For each class there must be a devarajah or king of devas, seven in all, but outside the circle of initiation, little is known and less must be said of the higher three. The four we know are called, respectively, the north, south, east and west wind, or the kings of earth, air, water and fire, clad, respectively in green, yellow, blue and red. These words and garments are symbols only telling nothing of their names or duties. These are inner mysteries of the White Mahatmas, unknown even to the Black Magicians, or to the seers of djinns and wraiths.

The last, or seventh, class of etheric entities is by far the most important to man. The artificials are man-created and by their return influence they make and unmake man. To this class belong the creations of the poet, undying and tangible in proportion to the poet's creative power. In this class too are all good wishes and all anathemas, all hopes, fears, faiths, creeds, and embodied loves and hates. All these find in time a living shape felt or seen by all canny psychic children in the flesh, a menace or a shield not limited by space or time. Elementals of this shape are often utilized in the 'sendings' of living objects transmitted invisible through space for the delight of a friend or the confusion of a foe. To this class belongs the white bird of the Oxenhams, whose appearance ever since the time of Queen Elizabeth is a sure presage of the death of some member of the family. In the noble family of the Whistlehursts a spectral coach drives up to the castle gate calling 'What, ho!', whenever a similar calamity is impending. Strains of wild music, the policeman's rattle, the blood-curdling shriek of a trampled cat, the clank of chains, all these are familiar to those who have looked into the phenomena of haunted houses. These elemental artificials are embodiments of man's will and thought. Once wrought together by longing, fear or crime, they may last for ages. An elemental, it has been wisely said, is a perfect storage battery from which there is practically no leakage. After a thousand years, a conception carefully worked out and firmly wrought together exhibits unimpaired vitality. In a famous case, such an artificial still warns the direct descendents of Sir Godfrey de Gespensterheim of their approaching doom by repeating in their ears the strange wailing music which was the dirge of his beloved son, Sir Lienhardt, seven hundred years ago in Palestine.

When these artificials are formed consciously and purposely they may be made the engines of tremendous power. Occultists of both the white and the black schools use them frequently and no influence in all the psychical universe can be so potent. But the evil use is not so common as it would be were it not that the highest occult power is granted only to the virtuous, and the black magicians are often torn to pieces by fiends of their own raising. Thus cast back into Devachan, their former power is lost to them and their reincarnation as dogs or monkeys by way of purification is extremely probable. This accounts for the multitudes of these animals in the streets of Benares and Constantinople. It is their nature to haunt the scenes of their physical exploits. To make artificials of extreme virulence and power has been the work of the sect known as the Lords of the Dark Face. Among other things they formed 'wonderful speaking animals who had to be quieted by an offering of blood lest they should awaken their masters and warn them of impending destruction.' From creatures of this type, created for a special purpose and afterwards neglected by an overworked magician, the race of parrots is descended. The devotees of the ghostly goddess Kale once performed rites too horrid to describe, and the results of which were the submergence of the continent of Lemuria with the loss of sixty-five million, two hundred and eighty-five thousand human lives, besides several myriads of promising anthropoids only lately condensed from Devachan. Even Ram, a mere Swami who has been in Nirvana only as a mahatma's honored guest, could speak a word which could blast your mountains, blight your fruit or growing grain or flood your valley with the waters of the sea. Such mystic words there be and Ram knows them. Ram's finger could point unerringly to the limitless fountains of gold in your hills. But Ram stays his voice and withholds his hand, for his life is a life of meekness and self-renunciation and these things must not be.

It is in the formation of artificials that a man's real character appears, whether on the physical earth or in Astrum or Devachan. The true adept forswears all that may be harmful to others. He may not use his power for his own advancement, hence his vow of poverty is for the protection of his soul. Having no selfish end in view he is ready to believe and to worship. To the western mind, belief and worship are as yet undreamed of. Instead of the silence of Om and the perfect rest of Nirvana, you hope for more business, more action, more pain, more unrest. The physical plane is the goal of life and the six planes above it are valueless assets of dreamland, unless they can be laid out into city lots. Turn your faces to the East, O Europeans, and learn of the patient, restful millions whose dreams, daily and nightly, bring more truth than all your struggles and your science of two thousand years. The religion of the West has long since lost its hold on thoughtful men and soulful women. The only reality in your lives is pain. The light of your old altar-fires is growing dim and when again it is relighted it must be in the name of the master of renunciation whose servant and follower you behold in me. It shall be for the worship of the suffering unconscious to whom pain and pleasure are dreams alike, mere floating shadows which dim for the moment the perfect serenity of perfected being.

After the conclusion of this passage, Madame Hhatch asked the privilege of a final word. She spoke of the learned Swami so far from his home and drew a pathetic picture of his life of renunciation and his vow of poverty. His heart yearns for Bombay and the light of his own altar-fires where the sweet sandalwood burns in its temples of perfection, which are symbolized in the mouth-closing word Om. Yet he is forced to earn his bread on the other side of the earth teaching people who can not understand him and whose every contact raises blisters on his astral skin. It is our duty to open the way to his return to that which is dearer to him than life.

So at the instance of Mr. Abram Gridley, the schoolmaster, we took up a generous collection which the young Brahmin received in patient silence.

As he passed out, Miss Violet Dreeme, of Fidèletown, who is a poetess and suspected of jealousy toward Madame Hhatch, uttered the sole syllable of discord. "I read every word of that," she said, "in one of Mrs. Tingley's little books of Hindu Poetry." This Swami is the very man who was at the Midwinter Fair out at Golden Gate Park. He etches your portraits on cardboard with his fingers while you wait and he cheated me with a bad half-dollar. Why, Madame Silva, who told fortunes in the next booth, says that he got a reporter for the San Francisco *Clarion* to write this speech, and it was a whole month before he had it learned so that he could go through it straight.

"The fact is, I am told, the Hindu in America has but one article of faith. More precious than rubies is the woman of leisure seeking for a new religion. The real 'Secret Doctrine of the Brahmins' is this: 'So beg that you will seem rather to grant than to receive a favor.'"

ARE THE ELEMENTS TRANSMUTABLE, THE ATOMS DIVISIBLE AND FORMS OF MATTER BUT MODES OF MOTION?

By Professor S. L. BIGELOW
UNIVERSITY OF MICHIGAN

THE advance workers in chemistry and physics are constantly accumulating new facts and propounding new theories which must be digested and incorporated in the body of the sciences. The process of assimilation is often slow, and it is right that new and important facts should be vouched for by more than one investigator, and that a new theory should prove its usefulness before being placed beside old and tried facts and theories. But too often the effects of the advances are unduly delayed through a reluctance to revise old text-books or old lectures, perhaps not so much because of mere laziness, as because of a failure to appreciate the full force of the evidence in favor of new views, or of the advantages to be obtained by their adoption. The fact that the arguments for an innovation, for a time at least, are scattered through many journals, leads to an underestimate of their cumulative force.

It is the purpose of this article to gather the main facts, some old, many recent, most of them fairly generally known, which are compelling us to alter our old definitions, and to show what a strong argument they make in favor of believing in the transmutation of the elements, the divisibility of the atoms and that what we call matter is simply a mode of motion.

It is interesting to note the caution with which text-books express themselves when it is necessary to give definitions for these terms. By a careful choice of words most authors avoid making false statements, but they certainly do frequently lead their readers to unjustifiable conclusions. For instance, in Roscoe and Schorlemmer's 'Treatise on Chemistry,' issued in 1891, we find the definition, 'An atom is the smallest portion of matter which can enter into a chemical compound.' As is the usual custom, the ideas of the alchemists regarding the possibility of transmuting metals is held up to ridicule, and thus, by implication at least, the ultimate nature of the elements and the idea that the atom is indivisible are infallibly conveyed to the reader. A more recent instance is to be found even in the late editions of one of the most widely used texts on general inorganic chemistry. In this book, on page 4, we read, 'Molecules may be defined as the smallest particles of matter which can exist in the free state'; on page 5, 'Atoms are the smallest particles of matter which can take part in a chemical change'; on page 6, 'Molecules consisting of atoms of the same kind are termed elementary molecules, and substances whose molecules are so constituted are known as elements.' The numbers of the pages on which these statements occur are also significant. This reminds one of the methods of the old Greek philosophers, who pretended to solve all questions of science by pure deduction, positing some hypothesis, and then developing everything else by meditation in their closets, disdaining to disturb the order of their thoughts by experiments. But it is unworthy of the present age of inductive science, wherein every thought has, or should have, experimental evidence as its starting point. It can not be said that this particular author has made a false statement, but he has left the subject incomplete; cautiously reserving a loophole for his own escape, he fairly traps his readers. For it is inevitable that, with such didactic phraseology, and without having his attention called to the hypothetical, the tentative, nature of these definitions, the student should become convinced that the most fundamental facts of chemistry are that there are about eighty substances so simple that they can never be broken up into simpler things, and that all substances are composed of ultimate particles, called atoms, eternally indivisible.

A student started out with this hodgepodge of fact and theory thoroughly implanted in his mind as the basis for all his future knowledge is sadly handicapped, indeed he is intellectually maimed, and it may take him years to overcome the habit of confusing fact and theory, and to learn how to think straight; perhaps he never succeeds in overcoming it. This confusing of facts with theories is a vicious habit, which grows till it colors all one's thoughts, hinders the free play of the intellect, diminishes the power of right judgment and starts the ossification of the wits even before the age set by Dr. Osler.

It is not necessary to consider a student of chemistry as an infant in arms to be fed on predigested food. He may be assumed to have a digestive apparatus of his own. Give him the benefit of any doubt and ascribe to him at least a dawning intelligence, which, properly stimulated, may some day shed some light of its own. It is the characteristic course of a lazy teacher, and one pleasing to lazy students as well, to supply a lot of personal opinions in the shape of cut and dried definitions, so easy to memorize and, unfortunately, so hard to forget; phrases which do not require the intellect to bestir itself and exercise its faculty of criticism, to pass judgment for itself between alternative or conflicting views. Strictly speaking, nothing should be presented in the form of a definition except what is, in itself, a statement of experimental facts, as, for instance, we describe or define a unit of measurement in terms of other units. When dealing with a subject where more than one opinion is permissible, all should be stated, or at least the attention should be directed to the fact that others draw different conclusions from the same premises.

The average student is better able to face issues and weigh arguments than most of us realize, and it is more important to educate those falling below the average in this particular than in any other. We should state the facts and then reason in such a way as to teach students how to think. It is indispensable for them to learn to think for themselves. Great stores of chemical facts are of but little real use, unless accompanied by an ability to adapt and to apply them in new conditions, unforeseen by either teacher or student in school or university days, but surely coming in after life. It is the prime necessity for research work or for originality of any kind, and we all are willing to admit that originality is what should be cultivated.

There is a great difference between the phrases, 'elements are substances which can not be broken up' and 'elements are substances which we have not as yet succeeded in breaking up'; and we should mark well the difference. This caution, lest we slip into the error of stating as fact more than we really know, is the distinguishing difference between the chemistry of to-day and the chemistry of a few years ago. It is more than this, it expresses concisely the difference between the way in which any science should be taught and studied, and the way in which it should be neither taught nor studied.

This particular differentiation between two definitions of the term element has been more than justified by the results which have followed the last ten years' work in pure chemistry, spectroscopy, radioactivity and Röntgenology (a term which has been seriously proposed by one of that fraternity which seems to consider its main function in life to be the coinage of new words).

The main arguments which may be marshaled in favor of considering the elements as ultimates, and the atoms as indivisible consist:

First, of all those facts which Dalton condensed into the laws of definite and multiple proportions, and to which there have been as many additions as there have been analyses and syntheses made before or since his time.

Second, Dulong and Petit's law that the atomic heats of all solid elements are the same.

Third, the isomorphism of many compounds containing similar elements, a phenomenon discovered by Mitscherlich.

Fourth, Faraday's law, that equivalent quantities of the elements are deposited at the electrodes during electrolysis.

Truly, an imposing array of evidence, and more than sufficient to justify us in making the assumption that atoms exist. But curiously enough, there is not one item amongst all these facts compelling us to believe that these atoms are the ultimate constituents, or that they are indivisible. These latter hypotheses are purely gratuitous, tacked on by Dalton and retained by succeeding chemists and physicists for no good reason. Perhaps because imitation is a characteristic inherited from our simian ancestry, and is so much easier for us than originality.

Many a chemist looks askance at any tampering with the atoms, apparently fearing that it may hurt them, or even destroy them utterly and the atomic weights with them. Or he trembles for his spidery and tenuous structural formulæ, knowing full well that if deprived of these he will be irretrievably lost in a labyrinth, without a thread to guide him. While, if he is not permitted to think of the carbon atom as a little chunk of matter, tetrahedral in form, he thinks he is launched on a sea of troubles.

But all this apprehension arises from a misunderstanding. That the atomic weights remain unharmed and unaltered, as the units for chemical calculations, and that nothing which is good or useful about the atomic theory is destroyed or even assailed by the new ideas, that the trend of these new ideas is unmistakably constructive and not destructive, are best shown by a review of the arguments in favor of the hypothesis that the atom is divisible, and that our elements are not elements in the true sense of the word.

There is nothing new in this view; it formed the first article of the faith of the alchemists. It was unqualifiedly denied by Dalton, and fell into such disrepute that even within recent years one risked being called a dreamer, or even a fool, if he dared to consider it possible. Here again is an instance of the desirability of being as precise as possible in the use of terms. Many believe experimental evidence of the complexity of 'elementary atoms' and the existence of one 'mother substance' must be followed immediately by directions for transforming elements into one another; by the transmutation of baser metals into gold. But these are two wholly distinct propositions. An astronomer might locate a mountain of gold on the surface of the moon, but there would still be a goodly chasm to bridge before he derived much material benefit from his discovery!

The idea that there is one fundamental substance would not down. The hypothesis of the English physician, Prout, is a familiar one. When the atomic weight of hydrogen is set equal to unity, the atomic weights of all the other elements come out remarkably close to whole numbers. There exist numerous groups of three elements, commonly called Döbereiner's triads, the individual members of one group being similar in their chemical properties, and so related that the atomic weight of the middle member is the arithmetic mean of the atomic weights of the extreme members. These are the facts which led Prout to suggest that there was but one element, namely, hydrogen, the others being complexes containing different quantities of this ultimate substance. It followed that the differences between the atomic weights and whole numbers were to be ascribed to experimental errors in the determination of these values. The desire to test this hypothesis was one of the chief motives for some of the most careful determinations of atomic weights which have ever been made. These determinations resulted in proving that the divergences of the atomic weights from whole numbers were greater than could be accounted for on the basis of experimental errors. This precluded the possibility that the atom of hydrogen was the common ultimate unit, but did not dispose of the possibility that a half, or quarter, or some other fraction, of the hydrogen atom might play that rôle.

In 1901 Strutt[1] applied the mathematical methods of the theory of probabilities to the most accurately determined atomic weights, and calculated that the chance that they should fall as close to whole numbers as they do was only one in one thousand. The inference from this is that it is not a matter of chance, but that there is a regularity in the atomic weights which we do not understand; a regularity which points to the probability that our elements are complex substances, constructed according to some system, from some simpler substance.

All the facts comprised in that great generalization, the periodic law, which states that the properties of the elements, both chemical and physical, are functions of their atomic weights, and most of them are periodic functions, point unmistakably to the same conclusion.

The evidence from spectroscopic analysis is so abundant that it is not easy to compress it into a few general statements.

In the first place, the spectrum of each of our elements consists of numerous lines, a fact not exactly compatible with the notion of extreme simplicity of the particles emitting the light.

In the second place, one and the same element, contrary to common belief, frequently has two or three distinctly different spectra, the particular spectrum which appears depending upon the pressure and the temperature at which the element is while emitting the light. In fact the extraordinary spectroscopic results obtained when highly rarefied gases enclosed in tubes (variously called Plücker, Hittorf, Geissler or Crookes tubes) were made luminous by the passage of high potential electricity, induced Crookes to suggest in 1887 a theory that the elements were all built up by gradual condensation with falling temperature from a fundamental substance to which he gave the name protyl.[2]

In the third place, the lines in the spectrum of one element may be separated out into several series. Each line corresponds, as is well known, to light of a definite wave length. The wave lengths of the lines comprised in one series are related to each other in such a way that a general formula may be derived for them. This means that, given some of the lines, the wave lengths, and thus the positions, of

---

[1] R. J. Strutt, *Philosophical Magazine*, March, 1901, p. 311.

[2] 'The Genesis of the Elements,' W. Crookes.

other lines belonging in the same series may be calculated. In this way the positions of certain lines for certain elements were foretold. Search failed to reveal all of them in light emitted by the element at any temperature producible in the laboratory. But some of the missing lines have been found in the spectra of the hottest stars, stars far hotter than our sun. At the same time many of the lines obtained by terrestrial means are lacking in the spectra of these stars. We have ample experimental evidence that many complex substances dissociate, as we call it, into less complex substances within the temperature range readily controlled in the laboratory. The inference is right at hand that at extreme, at stellar, temperatures our elements themselves are dissociated into simpler substances. To these substances, our elements, in this other condition, have been given their customary names, but with the prefix proto. Thanks to the introduction of Rowland's diffraction gratings for the study of these spectra, we have observations indicating the existence of proto hydrogen, proto calcium, proto magnesium, proto iron and so on through a list of a dozen or more 'proto' elements.[3]

Continuation of the work upon which Crookes was engaged resulted in the discovery of the X-rays by Röntgen in 1895. This date may be said to mark a new era in many of our conceptions regarding the universe about us. To J. J. Thomson, professor of physics at Cambridge, England, we owe the greater part of our present knowledge of the cathode rays. He devised most of the experiments and the ingenious, but strictly logical, reasoning which justify us in supposing that these cathode rays consist of swarms of minute particles, which he called corpuscles (reviving an old term and an old theory of Isaac Newton's); particles moving with velocities approaching that of light, each one carrying a charge of what we call negative electricity. He, and those working with him, determined the quantity of this electrical charge to be the same on each corpuscle, and to be the same as the charge we have good reason to suppose is carried by any monovalent ion in solution. By several methods the approximate number of these particles in a given volume and the weight of the individual particle were estimated. This weight appears to be about one eight-hundredth of the weight generally ascribed to the hydrogen atom, the lightest of all the atoms. It may be objected that there is no positive proof of the existence of these corpuscles, nor do we know the weight or mass of one of them. That is very true, but neither have we positive proof of the existence of atoms, nor do we know the weight of one atom. We can only say that the evidence makes the existence of these minute individuals, atoms and corpuscles extremely plausible, and makes one as plausible as the other.

[3] The methods, facts and reasonings relating to this spectroscopic evidence are interestingly given in 'Inorganic Evolution' by Sir Norman Lockyer.

Grant that we have discovered particles—in round numbers one thousandth part the size and weight of the hydrogen atom—the argument is still not complete for the divisibility of the atom. Perhaps we have found a new element. But cathode rays were produced under circumstances where they must have arisen from the cathode itself, and it is hard to escape from the conclusion that the atoms of the cathode disintegrated to a certain extent to furnish these particles. Furthermore, rays have been studied having as their sources different metals under the influence of electrical currents, different metals heated to incandescence, flames of different kinds and ultra-violet light; and these rays appear to consist of corpuscles of the same weight, no matter what their source. This makes it difficult to escape from the further conclusion that atoms of a great variety of natures are capable of disintegrating and of furnishing the same product by the disintegration;[4] and this is as much as to say that instead of about eighty different elements we have one 'mother substance,' and Prout's hypothesis is once more very much alive, somewhat modified, it is true, and in a new garb, better suited to the present fashions.

It remains to rehearse briefly the evidence to be obtained from radio-active phenomena. In the first place, the rays incessantly sent out from these extraordinary substances consist, at least in part, of rays like the cathode rays, and are streams of the same kind of corpuscles, but, on the whole, traveling with greater velocities than the corpuscles of the cathode rays. It has been proved by Rutherford and Soddy that the emission of the radiations from these substances is accompanied by a disintegration, or decay, as they describe it, of the substances themselves. These investigators have caught some of the products of this decay and have studied their properties. These products themselves decay, some slowly, some rapidly, sending forth other rays and furnishing new products to decay in turn. Indeed each new issue of a scientific journal for the past few years seems to chronicle the birth, life and death of a fresh radio-active substance. The rate at which new offspring of radium, thorium and allied elements are discovered and studied during their fleeting existences reminds one of nothing so much as the genealogy of Noah as given in the fifth chapter of Genesis.[5]

---

[4] Experimental details, and also comprehensive treatments of the subject as a whole and of special parts, may be found in three books by J. J. Thomson: 'The Discharge of Electricity through Gases' (based on lectures given at Princeton University in October, 1896); 'Conduction of Electricity through Gases' (a larger book); 'Electricity and Matter' (lectures delivered at Yale University in 1903).

[5] It is an indication of the widespread interest in this subject, and of the activity of the workers in this field, that one journal, in the year 1905, contained no less than 167 abstracts of articles upon radioactive phenomena. E. Rutherford's book, 'Radio-activity,' 2d edition, 1905, is a masterly survey of the whole subject.

These products appear to be elements, and this idea that some elements may have existences of but short duration, from a few seconds to many years, is a decidedly novel one. It has been suggested that this may account for some of the vacant spaces in our periodic table of the elements, particularly in the neighborhood of thorium, radium and uranium. Perhaps these spaces never will be occupied except by transients. Indeed it is not impossible that all our elements are mere transients, mere conditions of things, all undergoing change. But there is no immediate danger of their all vanishing away in the form of rays and emanations. Rutherford has calculated that radium will be half transformed in about 1,300 years, that uranium will be half transformed in 6×10^8 years, and thorium in about 2.4×10^9 years. We may safely say the other elements are decaying much more slowly, so we may continue to direct our anxieties towards the probable duration of our coal beds and deposits of iron ore as matters of more present concern.

The objection may be raised that perhaps radium should not be classed as an element, but rather should be considered as an unstable compound in the act of breaking down into its elements. But the answer to this objection is at hand. The evolution of energy accompanying these changes is far in excess of that obtainable from any known chemical process, so far in excess that it is certain we are dealing with a source of energy hitherto unknown to us, with a wholly new class of phenomena. The following quotation from Whetham[6] will convey an adequate conception of the magnitude of the forces at work here:

> It is possible to determine the mass and the velocity of the projected particles, and, therefore, to calculate their kinetic energy. From the principles of the molecular theory, we know that the number of atoms in a gram of a solid material is about 10^{20}. Four or five successive stages in the disintegration of radium have been recognized, and, on the assumption that each of these involves the emission of only one particle, the total energy of radiation which one gram of radium could furnish if entirely disintegrated seems to be enough to raise the temperature of 10^8 grams, or about 100 tons, of water through one degree centigrade. This is an underestimate; it is possible that it should be increased ten or a hundred times. As a mean value, we may say that, in mechanical units, the energy available for radiation in one ounce of radium is sufficient to raise a weight of something like ten thousand tons one mile high.

Again,

> the energy liberated by a given amount of radioactive change . . . is at least 500,000 times, and may be 10,000,000 times, greater than that involved in the most energetic chemical action known.

The theory that the source of most of the sun's energy is a decay of elements analogous to radium, to disintegration of atoms, is acknowledged to account better than any previous theory for the great quantity

[6] 'The Recent Development of Physical Science,' W. C. D. Whetham.

of this energy which we observe, and for the length of time during which it must have been given off according to the evidences of geology.

There is no chemical reaction which is not hastened or retarded by a change in temperature. In general, the velocity of a chemical reaction is increased by an elevation of the temperature and diminished by a reduction of the temperature. But radium compounds emit their rays undisturbed, at an even, unaltered rate, whether they be heated to a high temperature or cooled by immersion in liquid hydrogen and, what is perhaps equally striking, whether they are in the solid state or dissolved in some solvent.

In view of such facts as these, it is idle to suppose that radium is an unstable compound decomposing into its elements, using the terms compound and element in their usual sense. Conflict as it may with preconceived opinions, we seem forced to concede, not only that the transmutation of the elements is possible, but also that these transmutations are going on under our very eyes.

As has already been pointed out, this does not mean that we shall shortly be able to convert our elements into each other. Far from it, up to the present time we have not the slightest idea how to initiate such a process nor how to stop it. We can not, by any means known to us, even alter the rate at which it proceeds.

Now how shall we fit all these new facts and ideas in with our old ones regarding the elements and atoms, and how many of the old ideas must be discarded? Brief consideration is enough to convince us that very few of the old ideas, in fact none of value, need be sacrificed. We must indeed grant that Dalton's fundamental assumption is false, that the atom, in spite of its name, is divisible, and consequently that our elements are not our simplest substances, but decidedly complicated complexes. But all the facts included in the laws of definite and multiple proportions remain fixed and reliable, as indeed must all facts, expressions of actual experimental results, no matter what else varies. And there is not the least necessity for altering the methods of using atomic weights in calculations, nor for ceasing to use structural diagrams and models for molecules. We must merely modify our ideas and definition of an atom, and this modification is in the direction of an advance. We know more about an atom, or think we do.

Assume the inferences from the evidence just reviewed to be correct, and how do they affect our conception of the atom? First of all, our smallest, lightest, simplest atom, that of hydrogen, becomes an aggregation of about eight hundred smaller particles or corpuscles, and the atoms of other elements become aggregations of as many corpuscles as are obtained by multiplying the atomic weight of the element by eight hundred. Thus the atom of mercury must be thought of as containing 800 times 200, or 160,000, corpuscles. Next, the methods by which we believe we can calculate the approximate size of atoms and corpuscles give us values which enable us to make such comparisons as the following, suggested by Sir Oliver Lodge: 'The corpuscle is so small as compared to the atom that it, within the atom, may be likened to a mouse in a cathedral,' or 'the corpuscle is to the whole atom as the earth and other planets are to the whole solar system.'

These corpuscles are probably gyrating about each other, or about some common center, with velocities approaching that of light. It seems needful to suppose this, for it is hard to imagine that the enormous velocities observed could be imparted to a corpuscle at the instant it leaves the atom to become a constituent of a cathode ray. It is more reasonable to imagine that the corpuscle already had this velocity and that it flew off at a tangent owing to some influence we do not understand.

This may appear, after all, to be little more than pushing back our questions one stage, so that the position occupied in our thoughts but yesterday by the atom is now occupied by the corpuscle. Quite true, but this is in itself a great step, for the advancement of knowledge consists of nothing else than such pushing back of the boundaries. We dare not assume the end is reached, for there is no proof that the corpuscles are ultimate. They mark the present limit of our imaginings based on experiment, but no one can say but what the next century may possibly witness the shattering of the corpuscles into as many parts as it now appears to take to make an atom.

The question is a legitimate one, do we know any more about these 'new-fangled' corpuscles than we did about the old atoms? The answer is, yes, we probably do. We can go further in our reasoning on the basis of the properties of the corpuscles, and arrive at results which are startling when heard for the first time.

Lenard[7] has shown that the absorption of cathode rays by different substances is simply proportional to the specific gravity of those substances and independent of their chemical properties. It is even independent of the condition of aggregation, *i. e.*, whether the absorbing substance be investigated as a gas, as a liquid or as a solid. This is another strong argument in favor of the view that there is but one 'mother substance' which consists of corpuscles. The corpuscles of the cathode rays must be considered as passing unimpeded through the interstices between the corpuscles of the atom. Lenard calls the corpuscles dynamides and considers them as fields of electrical force with impenetrable central bodies which then constitute actual matter. He calculates the diameter of this center of actual matter as smaller than 0.3×10^{-10} (= 0.000,000,000,03) millimeter. Applying these results to the case of the metal platinum, one of the most dense of the metals, one of those with the highest specific gravity, he concludes that a solid

[7] *Wied. Annal.*, 56, p. 255 (1895), and *Drudes Annal.*, 12, 714 (1903).

cubic meter of platinum is in truth an empty space, with the exception of, at the outside, one cubic millimeter occupied by the actual matter of the dynamides.

If we can thus reasonably and mathematically eliminate the matter of a cubic meter of one of our densest metals to such an extent, it should not be very difficult to make one more effort and eliminate that insignificant little cubic millimeter still remaining, and say, with cogent reasons behind us for the statement, that there is no matter at all, but simply energy in motion. This is exactly what has been done by many who occupy high and authoritative positions in the scientific world.

Long before experimental evidence of the existence of corpuscles had been obtained, it was demonstrated that an electrically charged body, moving with high velocity, had an apparent mass greater than its true mass, because of the electrical charge. The faster it moved the greater became its apparent mass or, what comes to the same thing, assuming the electrical charge to remain unaltered, the greater the velocity the less the mass of the body carrying the charge needed to be to have always the same apparent mass. It was calculated that when the velocity equaled that of light, it was not necessary to assume that the body carrying the charge had any mass at all! In other words, the bit of electric charge moving with the velocity of light would have weight and all the properties of mass.

This might be looked upon as an interesting mathematical abstraction, but without any practical application, if it were not for the fact that Kaufmann[8] determined the apparent masses of corpuscles shot out from a radium preparation at different velocities, and compared them with the masses calculated on the basis that the whole of the mass was due to the electric charge. The agreement between the observed and calculated values is so close that it leads Thomson to say: "These results support the view that the *whole* mass of these electrified particles arises from their charge."[9]

Then the corpuscles are to be looked upon as nothing but bits of electric charge, not attached to matter at all, just bits of electric charge, nothing more nor less. It is this view which has led to the introduction of the term electron, first proposed by Stoney, to indicate in the name itself the immaterial nature of these ultimates of our present knowledge. We have but to concede the logical sequence of this reasoning, all based on experimental evidence, and the last stronghold of the materialists is carried, and we have a universe of energy in which matter has no necessary part.

If we accept the electron theory, our atoms are to be considered

---

[8] *Phys. Zeitschr.*, 1902, p. 54.

[9] 'Electricity and Matter,' p. 48.

as consisting of bits of electric charge in rapid motion, owing their special properties to the number of such bits within them, and also, no doubt, to the particular orbits described by the electrons. If space permitted it would be interesting to show how admirably the periodicity of the properties of the elements, as expressed in Mendelejeff's table, can be accounted for on the basis of an increasing number of like electrons constituting the atoms of the successive elements. We have molecules consisting, at the simplest, of two such systems within the sphere of each other's attraction, perhaps something as we have double stars in the heavens.

A possible explanation of the puzzling property of valence is offered, in that an atom less one electron, or plus one electron, may be considered as electrically charged, and therefore capable of attracting other bodies, oppositely charged, to form electrically neutral systems. An atom less two electrons, or with two electrons in excess, would have twice the ability to combine, it would be what we call divalent, and so on. An electronic structure of the atom furnishes a basis from which a plausible explanation of the refraction, polarization and rotation of the plane of polarized light may be logically derived. Hitherto explanations for the observed facts have been either wanting or more or less unsatisfactory. For instance, grant the actual existence of tetrahedral carbon atoms, with different groups asymmetrically arranged at the apices, and yet we can not see any good and valid reason *why* such a structure should be able to rotate the plane of polarized light. Grant that the molecule consists of systems of corpuscles traveling in well-defined orbits, and we see at once how light, consisting of other electrons of the same kind, traversing this maze, *must* be influenced.

Adopting this theory of corpuscles or electrons, not a concept of any value need be abandoned. On the contrary, the theory furnishes us with plausible explanations of many facts previously unexplained. Its influence is all in a forward direction towards a simplification and unification of our knowledge of nature.

A few words must be said regarding the old, the threadbare, argument which, of course, is cited against the electron theory. The materialist says he simply can not accept a theory which obliges him to give up the idea of the existence of matter; he says the table is there because he can see it and feel it and that must end the discussion for any one with common sense and moderately good judgment. Now it is the reverse of common sense to let that end the discussion, and our materialist is pluming himself on precisely those qualities which he most conspicuously lacks. He assumes the obnoxious theory to involve consequences which it does not involve and then condemns it because of those consequences. As a rule it is because he knows little about it, and has thought less, that he assumes the electron theory to be pure idealism in an ingenious disguise, that form of idealism which asserts that there is no universe outside ourselves and that everything is a figment of the imagination of the observer. The electron theory postulates a universe of energy outside ourselves. It does not deny the existence of the table; quite the reverse, it asserts it and then offers a detailed description of it, and why it has the properties which it has. This is more than any materialistic theory can do. The electron theory affirms the existence of what we ordinarily call matter. It defines, describes, explains these things, ordinarily called matter, in a clear and logical manner, on the basis of experimental evidence, as a *mode of motion*. It opposes the use of the word matter, solely because that word has come to stand, not only for the object, but also for the assumption that there is something there which is not energy.

Another groundless objection is offered by the materialists. They say this electron theory is clever, perhaps plausible, but very vague and hopelessly theoretical. Of course it is theoretical, but it is a theory more intimately connected with experimental facts than any other theory regarding the ultimate constituents. One departs further from known facts in assuming the existence of a something to be called matter. What is this matter which so many insist that we must assume? No one can define it otherwise than in terms of energy. But forms of energy are not matter as the materialist understands the word. Starting with any object and removing one by one its properties, indubitably forms of energy, we are finally left with a blank, a sort of a hole in creation, which the imagination is totally unable to fill in. The last resort is the time-honored definition, 'matter is the carrier of energy,' but it is impossible to describe it. The assumption that matter exists is made then because there must be a carrier of energy. But why must there be a carrier of energy? This is an assertion, pure and simple, with no experimental backing. Before we have a right to make it we should obtain some matter 'strictly pure' and free from any energy, or, at least, we should be able to demonstrate on some object what part of it is the energy and what part the matter, the carrier of the energy. We have not done this, we have never demonstrated anything but forms of energy, and so we have no evidence that there is any such thing as matter. To say that it exists is theorizing without experimental evidence as a basis. The materialistic theory postulates energy and also matter, both theoretical if you will; the electron theory postulates energy only. Therefore the electron theory is the less theoretical and the less vague of the two.

From the philosophical standpoint, having deprived an object of all that we know about it, all forms of energy, there remains what may be called the 'residuum of the unknown.' We are not justified in saying that nothing remains; we can only say nothing remains which affects, either directly or indirectly, any of our senses through which we become cognizant of the external universe. If the materialist takes the stand that this unknown residuum is what he calls matter, although any other name would be equally appropriate, it must be acknowledged that his position is at present impregnable, and that sort of matter exists. But it is nothing with which experimental science can deal. A fair statement would appear to be: The electron theory accounts for, or may be made to account for, all known facts. Besides these there is a vast unknown within whose precincts matter may or may not exist.

Michael Faraday is acknowledged to have been one of the ablest of experimenters and clearest of thinkers. His predominant characteristic may be said to be the caution which he used in expressing views reaching beyond the domain of experimental facts. His authority rightly carries great weight, and it is therefore of particular significance that he expressed himself more definitely upon these questions than appears to be generally known. In an article published in 1844[10] he says:

> If we must assume at all, as indeed in a branch of knowledge like the present we can hardly help it, then the safest course appears to be to assume as little as possible, and in that respect the atoms of Boscovich appear to me to have a great advantage over the more usual notion. His atoms, if I understand aright, are mere centers of forces or powers, not particles of matter, in which the powers themselves reside. If, in the ordinary view of atoms, we call the particle of matter away from the powers a, and the system of powers or forces in and around it m, then in Boscovich's theory a disappears, or is a mere mathematical point, whilst in the usual notion it is a little unchangeable, impenetrable piece of matter, and m is an atmosphere of force grouped around it. . . . To my mind, therefore, the a or nucleus vanishes, and the substance consists of the powers or m; and indeed what notion can we form of the nucleus independent of its powers? All our perception and knowledge of the atom, and even our fancy, is limited to ideas of its powers: what thought remains on which to hang the imagination of an a independent of the acknowledged forces? A mind just entering on the subject may consider it difficult to think of the powers of matter independent of a separate something to be called *the matter*, but it is certainly far more difficult, and indeed impossible, to think of or imagine that *matter* independent of the powers. Now the powers we know and recognize in every phenomenon of the creation, the abstract matter in none; why then assume the existence of that of which we are ignorant, which we can not conceive, and for which there is no philosophical necessity?

There is a striking analogy between the present condition of our science and our discussions, and those prevailing in the latter half of the eighteenth century when the phlogiston theory was almost universally accepted. We all now believe that heat is a mode of motion and smile at the thought that there were those who considered heat as a material. The materialistic theory is the phlogiston theory of our day, and perhaps the time is not far distant when the same indulgent smile will be provoked by the thought that there were those unwilling to believe that matter is a mode of motion.

[10] 'Experimental Researches in Electricity,' Michael Faraday, Vol. 2, pp. 289–91.

PURE FOOD LEGISLATION

By ROBERT McD. ALLEN,

SECRETARY OF THE INTERSTATE PURE FOOD COMMISSION

THE facts and evils of food adulteration have been overwhelmingly established. They have been published in volume after volume of state and federal government reports, and have been sworn to again and again by competent experts. State courts have imposed fines, and in hundreds of instances manufacturers and dealers have confessed that their food is adulterated, and judgments are entered accordingly. Yet, the evil is so strongly entrenched in business systems that a proposition to put truthful labels on foods and drugs intended for interstate commerce has met continuous defeat for more than fifteen years at the national capital.

Most of the states have enacted laws to control the manufacture and sale of foods. Some of these laws are good. Others contain bad provisions in an otherwise good law, provisions intended to nullify the law as it may apply to the several practises which food legislation is needed to correct, and such provisions but serve to legalize some adulteration which would have been subject to prosecution at common law. The main principles of the state laws have been well established by the Appellate Courts of the states and by the United States Supreme Court in a long train of decisions. With this backing, some eight or nine of the states are thoroughly enforcing their laws, and, as a result, there is a marked betterment in the food supply coming into such states.

The State Food Control Officials have an organization known as the Interstate Pure Food Commission. The commission was organized in 1896 for the purpose of bringing about uniformity among the state laws and securing the passage by congress of a law to apply to interstate commerce. This commission has held annual meetings, and at each meeting resolutions were adopted setting forth urgent reasons for national legislation to supplement the state laws. In nineteen hundred and three at Saint Paul, Minnesota, the commission called a joint meetings of manufacturing interests, state officials and representatives from the Bureau of Chemistry and the Inspection Division of the Bureau of Animal Industry of the United States Department of Agriculture. The manufacturers were given the full privilege of the floor, the discussions were frank, and, as a result of the meeting, the officials were impressed with the fact that in the preservation of the large fruit and vegetable crops much of food adulteration comes from problems which the manufacturing interests are honestly endeavoring to overcome. As a further result of this meeting, it was impressed upon all that food-control legislation should be correct labeling rather than prohibitive, except where substances are positively injurious to health.

The Saint Paul meeting was followed by a similar and larger meeting at the Louisiana Purchase Exposition in the nature of an International Pure Food Congress, and an exhibit showing adulterated brands of foods. The Saint Louis meeting was the largest of its kind ever assembled, and was a week, day and evening, of frank, honest discussion among officials, scientists and representatives from the several manufacturing interests. The congress discussed antiseptics, artificial colors, fruit, vegetable, dairy and meat products, confectionery, baking powders, wines, beers, distilled liquors and drugs. Special committees reported resolutions on the various questions, and among the resolutions adopted was a unanimous endorsement of the Hepburn Pure Food Bill which had passed the United States House of Representatives the previous winter.

For more than twenty years the Bureau of Chemistry of the United States Department of Agriculture has thrown the weight of its influence to the investigation of food and drug adulteration and its effect upon health. This bureau has had the cooperation of the association of Official Agricultural Chemists in perfecting methods of food analyses and in collaborating on a set of food standards. It is the agricultural chemist who has detected and called the country's attention to the evils of food adulteration. Formerly laws regulating the sale of foods were left to the boards of health to be enforced, but it is only as the states have created divisions of chemistry in the Department of Health, or have turned the work over to their experiment stations, or have organized state food commissions and equipped them with laboratories, that results have been obtained under state laws.

Food and drug adulteration has grown up because interests have been permitted to violate certain principles of identification in the sale of their products. When purchasers know *where* a product was made, *when* it was made and *who* made it, and are informed of the *true nature* and *substance* of the article offered for consumption, it is almost impossible to impose upon the most ignorant and careless consumers. Trade-mark law requires correct labeling as to *who* made an article and establishes the principle that a man is not to sell his goods under the pretense that they are the goods of another man, nor can he use any means which will contribute to this end. This principle has been upheld in courts as not only necessary to secure to each man the fruits of his own toil, but also as a protection to the public against fraud. Only the one, however, whose trade-mark is infringed has a cause of action before the courts, and where there is a business arrangement, or where monopoly operates, to put the combined product of many factories into the market under that trade-mark which is most in favor with consumers the public have no remedy at trade-mark law. The large wholesalers oppose the proposition to have the label tell, under all circumstances, the name of the real manufacturer. The independent manufacturing firms strongly favor it. There is nothing so fatal to monopoly or so stimulating to the competition of individual merit as where law requires an article of merchandise to be always identified in the market with the name of the person or firm who made it.

Congress has passed several laws relating to inspection and correct labeling. In 1890, a law was passed authorizing the inspection of meats intended for export, and forbidding the importation of adulterated foods and drugs. In 1891, the meat inspection provided for in the act was extended to meats intended for interstate shipment. The provisions of this law were further extended in the Appropriation Act of the United States Department of Agriculture for 1905 to apply to daily products intended for export. In 1896 congress provided for the bottling of genuine whiskey in bond and its identification to the consumer by means of a tax stamp over the cork. In 1897 a law was passed prohibiting the importation of inferior teas, and providing for a board of experts to adopt standards by which to measure the quality of imported teas. The law and the provision authorizing this board of standards have been held to be constitutional by the United States Supreme Court. During the war with Spain a special tax was levied upon certain products, among them adulterated flour. The tax stamp served to identify the flour subject to this tax, and the business was at once destroyed. In repealing the war taxes the act relating to adulterated flour was not repealed.

In 1896 Congress passed an act providing for the taxing and labeling of filled cheese. Oleomargarine was a subject of federal legislation as early as 1885. This act was passed as a tax measure, and in connection therewith provided for the proper labeling of oleomargarine. This law was amended in 1902, fixing the tax on oleomargarine, colored to resemble butter, at ten cents per pound, and on the uncolored at one fourth of one cent per pound. It also taxes renovated butter, and requires it to be so branded.

The Appropriation Act of the United States Department of Agriculture for 1903 and subsequent appropriation acts have authorized the Secretary of Agriculture to put into effect the act of 1890 relating to the importation of adulterated foods and drugs, and to adopt and fix standards for guidance in the enforcement of the law. Appropriation acts of the Department of Agriculture have also authorized the study of the effect of antiseptics and artificial colors on the human system. It was under these acts that the chief of the Bureau of

Chemistry, assisted by details from the medical staff of the army, experimented with the 'poison squad,' and as a result has recommended against the use of salicylic, benzoic and boric acids to preserve foods.

Congress passed a good law in 1902 prohibiting the misbranding of foods as to the state or territory in which the product is produced. This law was passed primarily to keep western cheese producers from labeling their product as 'New York' cream cheese. The law applies, however, to all foods, and it puts into partial practise one of the important principles of identification, namely, *where* a product is made. Where a product is made is an important bit of knowledge. Foods sometimes have exceptional qualities by reason of certain conditions of climate and soil and skill in packing or preserving. And so it is that certain fruit, vegetable, dairy and wine districts are known for the superiority of their products. The producers in these districts have the right to an honest market, while consumers should have the means to identify the foods from such districts should they so desire.

Correct labeling as to the geographical place of production prevents imposition in another way. A man can pack cottonseed oil for interstate commerce and label it 'olive oil,' but if he is compelled to state that the 'olive oil' is packed in Georgia or Alabama, the public becomes suspicious that it is getting cottonseed oil. A 'New Orleans' molasses, packed in one of the glucose districts of Illinois, is open to the same suspicion. A can of 'salmon,' packed in Minnesota, is known by its place of production to be carp. Between products of the same class this law is effective; but between the imitation and the product imitated, it is of little avail, for raw materials can be shipped into a district famous for its cheese, jelly, syrup, wine or whiskey, and the label of the imitation then bears legally the name of that district which is in favor with consumers.

Federal legislation is incomplete. The Hepburn-McCumber-Heyburn Pure Food Bill proposes to complete it. Not by a tax, but by a law which will command all the principles of identification to be truthfully and fully represented before foods, drugs and liquors are allowed shipment from one state into another. Such a law will make it possible to follow fraud across the state border and to punish the person responsible for the manufacture of an adulterated or misbranded product. Such a law will tend to unify state laws, but it will not interfere with state laws nor will it protect that state which does not maintain equal inspection over its own commerce.

The attitude of the food interests toward pure food legislation is either passive or antagonistic. The meat packers represent to consumers that their meat is 'U. S. government inspected.' The act of 1890, which relates to physiological wholesomeness only, permits them to make this representation, although meats, 'U. S. government inspected,' may contain antiseptics, aniline dyes, cheap fillers and any other adulteration or misbranding which the packer may find profitable after the carcass has passed veterinary inspection. The packers are considered to be opposed to a law which will prohibit or make public these added adulterations. The butter people have secured all the special legislation they desire and of course take little interest in a general food law. In fact a general food law may in the end restrict the use of artificial color in butter.

The brewers support the bill. This is brought about by the influence of strict regulations in Germany. The wine growers favor the bill on account of the advantage which the port inspection gives foreign wines over the uninspected American wines. Wines, however, are largely distributed through the wholesale liquor dealers. The majority of wholesale liquor dealers are also rectifiers. The rectifier, either through lack of confidence in his product, or some misunderstanding regarding the purpose of the bill, is its strongest opponent. The wines of any firm openly supporting the bill are therefore liable to be discriminated against.

Some manufacturers of catsups and other condiments fear that the law will prohibit the use of antiseptics. Others announce that such articles can be put up without these preservatives. The manufacturers of imitation jams and jellies supported the law at first, believing that it would bring about a uniform system of labeling. The enforcement of the labeling provision under the state laws, however, is now proving that consumers prefer the genuine product. The independent firms which put up pure foods of all kinds are for the bill; but such firms are not organized, maintain no lobby, and with several aggressive exceptions write few letters to congressmen and do little of anything else to offset the influence which the organized interests array in opposition. The reasons for using antiseptics, artificial colors and flavors and otherwise adulterating foods may be controverted, but no one will deny the right of consumers to know it whenever such adulterations have been practised. Manufacturing firms realize this and they are preparing to meet what they know consumers will demand when law compels such labeling. The head of a large association of food interests well expressed this in saying: "I have lost sleep for several years trying to see some way around the movement, but there is only one thing to do and that is to prepare to meet it." The majority of the food interests are preparing. Firms which began several years ago to so shape their business are ready. Others will be ready, but they would like to see such laws postponed for one or two more years. When the expense of making the change is incurred and the pure food policy has been inaugurated, business itself will demand the enactment of strict inspection laws.

All practical reform must be financed. Pure food is a sentiment until put into practise in the dairy or factory. The practise does not continue without profit. The profit is uncertain, often impossible, until the laws of the state and of the nation command that every article of commerce shall be sold under its own name and upon its individual merits.

In the enforcement of the state laws, in the committee hearings concerning the proposed national law, the dominant questions have been, and, in the event of the passage of the national law, will be, artificial colors, antiseptics, standards and labeling. And at the present time the adulteration and misbranding of drugs and liquors occupy a prominent place in the pure food issue.

Artificial Colors

In a pinch of aniline dye there is all of the color which a cherry-tree can produce in one season. The cherry juice or the cherry jelly is refreshing and invigorating, while the aniline dye, whether harmful or harmless, is without food value, lifeless and dead.

Genuine color and flavor are the truest representations of quality and purity, and the artificial color or flavor is *per se* a deception. Even in confections when the product purports to be flavored with lemon, vanilla, cinnamon, etc., and is not, the artificial color or flavor works a fraud. With the aid of color every article of food has been in appearance successfully imitated. With artificial color to depend upon, there is little need for selecting the best suited feeds and treating and culturing cream in such a manner as to produce a delicious butter with sufficient natural color. Little attention need be paid to the growing of fruit and vegetables uniform in color and quality, or to the treatment of the wholesale lot so that it will be uniform when it leaves the process of preservation, since no care in production or preservation can produce a color which can compete with that added by the aniline dye.

The manufacturers claim, and it can not be disputed, that the use of a harmless color to restore the appearance of a product of otherwise good qualities is not concealing inferiority, but makes the material, which is standard in all other qualities, pleasing to the eye. But where can the line be drawn? Once throw the gate open and the imitator enters with his saccharin and glucose, starch and waste products from the fruit factories and artificial acids to color compounds for the market which are often worthless and sometimes harmful.

Color should no longer be a subject of class discrimination. The dairy interests defend its use to improve the quality of cheese and butter; the packers, to change the appearance of their oleomargarine; the vinegar factories to help them make cider vinegar without apples; the French, to protect their industry in coppered peas; and all the imitators as their *modus operandi* in deceiving the public. In each and every instance it either deceives as to the quality of the product or aids in the sale of that which has no value, or assists the sale of some product under the name of another, at, perhaps, double its market price.

Antiseptics

Antiseptic preservatives are substances to be restricted, if possible, prohibited. This is the conclusion reached in all legislation, in the reports of scientific commissions appointed by governments to inquire into the use of antiseptics in foods, and by the large majority of experts who have studied the effect of the long-continued use of minimum quantities upon the human system.

Those who employ antiseptics to aid in the preservation of foods do not defend this use because of *wholesomeness,* but because of *conditions*—honest problems—in the production and sale of soda fountain syrups, tomato catsup and similar articles put up ready to serve, and which remain open for a week or longer until the contents of the package are consumed. And in this defense the reasons come more from the *market*—'rough handling in shipping,' 'the hot grocery shelf,' and 'the careless consumer'—than from problems in *production.*

The antiseptic is the competitive foe of cleanliness and other hygienic practises which should attend throughout the production and sale of foods. The antiseptic is often used in foods of otherwise high standards, but it is more often found substituted for wholesome practises or ingredients. Its use discourages the perfection of healthful ways for keeping foods—chilling, sterilizing, ripening, curing and the combination of one food substance with another—which have not only given us food preservation, but have added delicious and wholesome variety to what we live on.

Some of the state laws specifically prohibit the use of antiseptics in foods. Other laws prohibit the addition of 'injurious substances' to food products, and in the enforcement of such provisions, as in Pennsylvania, for example, the court holds that such a provision applies to the use of a harmful antiseptic even in a minimum quantity. In Connecticut and Kentucky, while the law prohibits the addition of 'injurious substances' to foods, it also requires the use of any antiseptic to be made known to the purchaser or consumer. Under the enforcement of this labeling provision Kentucky, for example, has meats without boracic acid, milk without formaldehyde, jellies and fruit juices without salicylic acid, while some of the manufacturing firms are successfully putting up tomato catsup and soda fountain syrup without the use of benzoic acid or other antiseptic preservatives.

In a letter to the Kentucky department a manufacturer of tomato soup writes:

> During the season of 1905 and henceforward the use of coal-tar dye and benzoate of soda will be entirely discontinued from our product, for we have arranged to make it entirely from fresh tomatoes in the height of the packing

season which will enable us to produce a palatable looking article without the color, and relieves us from the necessity of putting away stock and preserving it with benzoate of soda to avoid fermentation.

In a letter from one of the meat packers it is brought out that one manufacturer is required to use boracic acid to maintain a competitive keeping quality with the other manufacturers, if there is no law or enforcement of law to prevent or make public the use of antiseptics in meats.

Writing of the experiment with tomato catsup without an antiseptic, the manufacturing head of one of the large firms says:

> I believe that within five years, if that long, we could create ideal conditions in this country, and the consumer could be educated to take better care of such goods as are perishable and liable to spoil on his hands if not consumed within a certain time. Smaller packages would help to a large extent in that direction. In fact, there are numerous ways by which eventually we may accomplish that which is desirable to be done.

The head of the market end of the firm, writing about the experiment later says:

> We are making a strenuous effort to have every variety of our goods absolutely pure and free from any antiseptic whatever. But you appreciate thorougly the enormous undertaking this is, and, further, the great interest which we have at stake which makes us proceed slowly. Starting several years ago with our experiments on tomato catsup, we put out at first five or six thousand dozen; the next year we doubled that; the next year we doubled that again; this year we are going to put out approximately four hundred thousand dozen catsup, which will be absolutely free from any coloring matter or antiseptic. With this season's work a success, we will have demonstrated beyond any possibility of doubt the putting up of catsup without any antiseptic. After that you will be free to say to every manufacturer who sells otherwise, 'Look at the thousands of dozens of catsup that . . . has on the market which stand the test of shipping, of climate, and, afterwards, the hot shelf of the grocery store, and still the consumer gets the goods in prime condition and is well satisfied with the flavor.'

Tomato catsup has been claimed to be the most difficult product to put out without some antiseptic.

Added or Otherwise

The provisions of the food laws and of the proposed National Pure Food Law apply principally to adulteration by *addition* and to adulteration by *taking away*. There is a third class of adulteration. Foods may be unfit for consumption by reason of inferior methods of production or preparation, carelessness in handling, inherent disease, and the spoilage to which foods are subject by their very nature. Many foods at certain stages of production or preparation are unfit for consumption—a green peach and new whiskey. Many of the fruits and fruit or grain products contain in their composition certain normal poisons, poisons which a food law would prohibit being added. It is said that in some instances these poisons are not active in the combinations in which nature puts them, but whether active or not active when people eat a natural product they know the nature of that which they eat, and when a food law requires this product to be put up in its best form and to be identified to the consumer, it has gone as far as is necessary.

The opponents of the Hepburn-McCumber-Heyburn Pure Food Bill argue that it is unfair, because it prohibits the addition of poisonous ingredients, and yet permits a poisonous ingredient when inherent or normal in the product. This argument is plainly invented to divert attention from the question of honest labeling. It seems at first plausible; but its fallacy and purpose are evident upon short analysis. The proposed law prohibits directly the sale of animal or vegetable substances which are diseased, spoiled, or otherwise unfit for food, and the majority of the other provisions of the bill apply indirectly to adulterations present without having been added. Adulteration by inferior methods of production or preparation necessitate the artificial colors and flavors, antiseptics and other added substances which the bill proposes to regulate. Imperfect natural food bears its own condemnation in its unpalatable flavor and inferior color, and such a food, therefore, must be supplemented and disguised by the added artificial before it will sell. When the artificial is added the law operates. Foods which possess a natural color and flavor pleasing to consumers are the result of the highest arts of production and preparation, and it is not for such foods that food control legislation is needed.

The whiskey rectifier or blender in particular has attacked the word 'added' in the following provision:

> If the package containing it (the article of food) or its label shall bear any statement, design or device regarding the ingredients or the substance contained therein, which statement, design or device shall be false or misleading in any particular, provided, that an article of food which does not contain any *added* poisonous or deleterious ingredient shall not be deemed to be adulterated or misbranded in the following cases.

No open argument can be put forward against the first part of this provision, but from the overwhelming evidence of such misbranding, not only in the sale of liquors but in the sale of all foods, it is evident that there must be a powerful secret opposition to it. This opposition manifests itself in charges of 'government bureaucracy,' 'the tyranny of standards,' 'differences of opinion between scientists,' 'the competency of the agricultural chemist *versus* the competency of the physiological chemist in determining adulterations,' 'added or otherwise,' 'the constitution,' 'the enforcement of law by an individual instead of by the courts,' as if it were possible under the state and federal constitutions to enforce any law in case of dispute by other than the courts.

Application of the Law to Whiskies

The pure food issue covers, and should cover, all substances intended for human consumption, and the fact that any subject covered in the issue is difficult and unpleasant is the more reason why it should be included.

Whiskey is ethyl alcohol and natural flavor. Brandy is ethyl alcohol and natural flavor. The difference is the difference in flavor. The flavor of genuine whiskey comes from the grain, secondary products—fusel oil—distilled over with the ethyl alcohol and ripened into the flavors of 'rye' and 'Bourbon' whiskey. The new whiskey with its unripened secondary products is like the green peach, unfit for consumption. The quality of the flavor of whiskey depends upon the quality of fusel oil and the method and period of aging. The quality of the fusel oil depends upon the quality of the grain and water used, the preparation of the mash and the methods of heating and distilling. The new product is ripened by putting into charred oak barrels and storing these barrels in warehouses. These warehouses are under the lock of government officials, primarily to see that none of the product is taken away until the tax is paid. Whiskies may be taken out of this warehouse at once, or they may be permitted to remain for a period of eight years before the government collects the tax and ceases its control. Most of the whiskey, however, is tax-paid and removed from bond before it is three years old. The rectifier or blender claims that he has a process for producing palatable whiskey without the expense and delay of the barrel-aging process. The rectifier, however, colors, beads and labels his product in imitation of the aged whiskey. If the process has the merit which is claimed for it, there should be no injustice and all advantage in a law requiring rectified whiskey to be labeled for what it is.

When the tax is paid on distilled spirits the government puts a stamp on the product to show this fact. Formerly these stamps were only put on barrels. Consumers do not buy the product by the barrel, and so in 1896, following the investigation of the whiskey trust and the adulteration of whiskies, congress passed an act permitting a tax-paid certificate stamp to be put over the corks of bottles. Whiskey to be so bottled must have remained in the bonded warehouse at least four years, and must be bottled without the addition of any substance except distilled water to reduce it to one hundred proof. This law is optional. The four-year period of aging which it requires should be made compulsory for all whiskey.

No such supervision is exercised, on the contrary, over the business or product of rectifying. In fact, the rectifier or blender holds a government license to 'spuriously imitate' as he pleases, and a law is needed to restrain the adulteration which it is possible to practise. The natural flavor in genuine whiskey and the government tax are the dominant costs in production. The rectifier seeks to lessen these costs by expansion, or by the addition of artificial essences to neutral spirits to make a product which will taste and appear like genuine whiskey. Sometimes more or less genuine whiskey is mixed with this neutral spirit to help the flavor, and when such is the case, and when the flavors and other imitations added are harmless, the product has all the rights of the market provided it is labeled for what it is.

New genuine whiskey is often taken from bond before it is sufficiently aged and syrups are added to make it palatable. Green whiskey is unfit for consumption, and this practise should be prohibited. In one class of rectified whiskey the mixer not only seeks to avoid the cost of producing the natural flavor, but also to reduce the tax cost of the ethyl alcohol by incorporating some one of the non-taxed intoxicants, like wood alcohol. There are no statistics to show to just what extent this practise is carried on. It is such stuff as this which is sold in the 'dives' of cities and the 'blind tigers' of prohibition districts, and its crazing effect upon human beings is a matter of common knowledge.

The people who do not drink alcoholic beverages know little and care less about the composition and labeling of these products. "They are all bad because they contain ethyl alcohol, and there can be little difference between the adulterated and the pure." Some of the prohibitionists even fear that the investigation might help to 'legalize part of the traffic.' But it would seem wiser to insist that the searchlight of chemistry and the law of the honest label shall be applied to all substances intended for human consumption, whether foods, drugs or liquors. And such a control for alcoholic beverages is the beginning of a far-reaching reform. Some things are worthy of the sentiment of state rights. The adulteration of alcoholic beverages is not one of them.

Standards

All agree on the general principles of pure food legislation, but a controversy arises when it is proposed to apply these principles to the sale of some special product. The name and describing terms given to or incorporated in the label of an article of food or drink have much to do with the price and supposed food value of the article so named or labeled. The imitation, where law does not prevent it, goes into the market under the name and trade terms of the product imitated, and is so mingled in the market with the general food that it is impossible for consumers to distinguish between the two.

It is the purpose of standards to determine and establish the normal constituents of each food substance and to so apply and restrict names and describing terms that consumers can at once identify the imitation from the genuine or the inferior from the superior. The interests which produce the genuine want this principle established; the interests which make the imitation do not.

The purpose of standards embraces the principle of uniform weights and measures. The law of weights and measures determines what shall constitute a pound or a gallon, and requires that when a pound or a gallon is contracted for the substance delivered shall weigh or measure according to the contract. The law of standards would require that a pound of butter and a gallon of honey shall be such, with all the qualities of good butter and good honey. This standard principle is well established at common law, but the statute and commission are necessary to put it into continuous effect. The impression has been created that it is the purpose of food standards to arbitrarily determine what people shall or shall not eat, when, the fact is, the purpose is not to prohibit but to identify. Food standards will prevent the arbitrary imposition upon consumers which dishonest labeling permits.

For protecting consumers and to meet the ends of justice in the enforcement of food laws it is not only needed to know what this man or that man or these two men consider the qualities of a pure product, but also, what the collaborated evidence from all of the scientists and of all practical experience establish these qualities and the correct method for determining them to be. This collaborated result is the standard, and is, from its very nature, a protection against error.

The legal status of scientific commissions appointed by the state and national governments to determine certain facts in order to intelligently enforce laws becomes an important question when the findings of such commissions affect large interests. In the enforcement of police regulations against ignorant offenders, and in matters of undisputed public importance or danger the rulings of executives and the findings of government scientists have been given the warrant of law without dispute. But in settling a question which concerns established business, the authority for the appointment and the powers of the commission to investigate such questions become a matter of great importance. Such a commission exercises, in the consideration and determination of technical matters, the combined functions of the legislative, executive and judiciary up to the point of putting its findings into effect. Its findings may go into effect by the mutual assent of the interests affected, as is, with but few exceptions, the case where the finding is correct. But if disputed, the finding can not go into effect with the warrant of law, except under the rules and as the weight of evidence.

In his great speech on the constitutional right of congress to pass legislation limiting the power of the inferior federal courts to grant injunctions against the decisions of a railroad rate commission, until after full review, Senator Bailey says in defense of giving such a commission of experts the fullest powers possible:

I do not believe that this legislation will lead to the end which some men fear. I believe that the ultimate effect will be to promote a better understanding among the railroads and the people. I believe that when it is made the railroads do the people justice, the people will feel less resentful toward the railroads; and we will deal with these questions with more of reason and less of passion. It will take the railroads out of politics and we will hear no more about railroad senators. I want to see every corporation driven out of politics.[1]

In opposing the pure food bill the senator says:

Thus it is that bureau after bureau is built up and we vest them with such extraordinary power, until the American people will become a bureaucracy instead of a democracy—a government in which the bureaus and not the people rule.

If the federal government has the power to pass an act regulating the use of adulterated, misbranded and imitation foods, it ought to stop when it writes upon the statute books that it will be a crime to commit such a commodity for shipment between the States and the foreign nations, and leave it to the integrity and efficiency of its judicial officers to vindicate the authority of its law.[2]

Why should not the senator's argument in favor of the rate commission also apply to the food commission? Why should not also the liquor corporation, the drug corporation and the packing corporation be 'driven out of politics'? Why should rate experts, with extraordinary powers, be trusted to make 'the railroads do the people justice,' and food experts, with no limitation upon the courts, be expected to build up a bureaucracy antagonistic to the people's interests?

But, aside from these questions and the 'efficiency and integrity' of the judicial officers, the district attorneys and federal judges can not enforce a pure food law without facts and these facts can only be secured through a 'bureau' or staff of trained chemists, working in well-equipped laboratories under methods of analysis which have been established beyond doubt to be correct. There may be some occasion to fear that errors will be made in securing this evidence. There is greater occasion to fear that the investigation will make public the deficiencies and adulterations which some interests know to exist in their misbranded products.

The pure food issue is not altogether an issue of 'fraud' and 'poison,' but it is more largely a question of scientific and business problems—problems attending the preservation, packing and distribution of what the people live on; problems which the colleges and universities have too long left out of their courses, and problems which the experiment stations and government departments have too long neglected to study in connection with the production of the fruits, grains and other products from which foods are made.

---

[1] Before U. S. Senate, April 10, 1906, *Congressional Record*, April 13.

[2] Before U. S. Senate, February 21, 1906, *Congressional Record*, February 21.

WOODEN FLOWERS

BY ORVILLE PAUL PHILLIPS, PH D.

BERKELEY, CAL.

OWING to the demand of the uneducated mind for any kind of a crude guess rather than an acknowledgment of ignorance, strange stories often spring up around natural phenomena, attributing, in most absurd ways, effects to causes which have no more connection than the barnacles and geese of Gerarde. Especially is this true of the savage who deifies everything beyond his knowledge and attributes to it influences for good or evil to himself, according to his first impressions of them. Such stories often find credence in the minds of more enlightened people upon the plea that 'the Indian lives so close to nature that he can not be far wrong in his estimate of natural phenomena.' These believers in the infallibility of the 'untutored races' fail to remember that the most superstitious person on earth is he who reads nature, as does the savage, only by the awe-inspiring phenomena that have forced themselves upon his attention most strongly by some accident, without any reference whatever to cause and effect. Cases in point might be cited from every stage in the life of native races, but the following will serve as an example, and at the same time may clear up in the minds of some as to what is the cause of the peculiar growths known as 'wooden roses' or 'wooden flowers,' they having frequently been described to the writer by different botanists as 'fungi,' 'galls,' 'knots,' 'disease swellings,' etc.

Volcanoes have ever been looked upon with fear by native races and the crater shunned as the doorway to the 'infernal regions.' Agua, in Guatemala, had, however, been inactive for so long that when peculiar forms of plant life, known nowhere else in the region, were found near its summit, they were supposed in some way to be connected through the extinct, though still feared crater, with the regions of fire beneath. They were, therefore, called by the euphonious title of 'roses of hell,' because they were believed to be the only flowers that grew in the 'lower world' and, having escaped through the crater from that region, were supposed to exist nowhere in the world except upon the upper portion of the sloping sides of this volcano.

Because of their supposed origin, these 'flowers' were feared as having great power for evil. They were supposed to be more poisonous than anything upon earth, and any person coming within the influence of their inodorous, though not unbeautiful, 'petals' was marked for sure destruction.

Though Agua had been quiescent for centuries and its rampages were known only by the dim recitation of Indian tradition, still that tradition was strong enough in the hearts of the natives to cause them to warn the Spaniards who came to that country in 1524 that unless they shunned the influences of this 'flower' they would surely be slain by the volcano, whose long quiescent throat had permitted the 'roses' to see the light of day as a warning to human beings that, though he slept, he was by no means dead, and if aroused the Indians would suffer as well as the white man. Despite this warning, the Spaniards called their council under the shade of one of the very trees which bore these 'flowers,' and there decided to found the city of Antigua which was to be the capitol of the new state of Guatemala. Everything flourished in the new city until 1541, when Agua suddenly burst forth in terrific defense of his invaded sovereignty, deluging the beautiful valley with fifty million cubic yards of water and mud, completely burying the city from view and warning the people to no longer trespass upon the evil ground. The warning was again unheeded, the city rebuilt upon the old site and apparent prosperity experienced until 1773, when it was shaken to ruins by the great earthquake. The capitol was then removed to the new town of Guatemala, beyond the influences of the fateful 'flower' and has therefore never been molested since. This

FIG. 1. A BRANCH OF *Citrus medica*, showing the cup-like, delicately carved 'petals of the 'wooden flowers.'

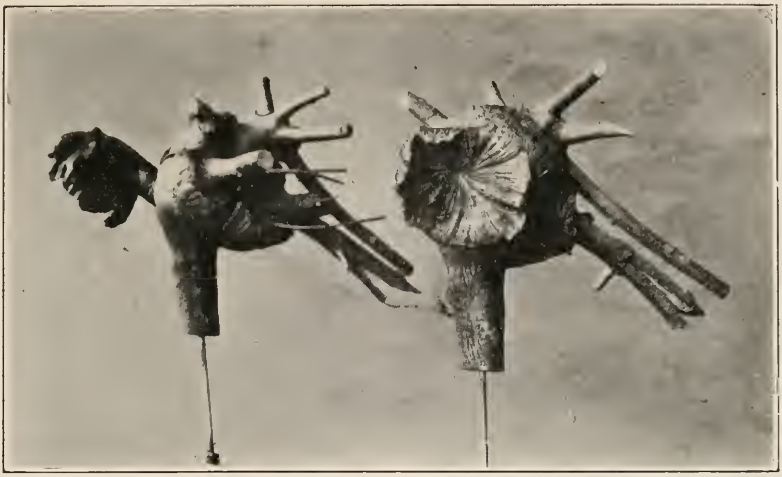


FIG. 2. A BRANCH OF *Citrus medica*, showing the parasitic *Lorenthus Ladebeckii* (Engl.) *in situ*, taken in front of a mirror so as to show both sides of the same specimen. The right-hand view also shows one of the cup-like 'flowers,' from which a plant of *L. Ladebeckii* has fallen. This photograph was kindly made for the writer by Dr. Voigt, of the Botanical Museum of Hamburg.

vindication and fulfilment of the natives' warning has fastened the tradition unalterably upon their beliefs, and no amount of enlightenment ever shakes their confidence in the direful results that will follow the too close inspection of these terrible 'roses of hell.'

This peculiar botanical formation, though strange in its gigantic size, is easily explained when the specimens are carefully examined. An examination of a number of them by the writer showed them to have a 'stem' of wood, upon the end of which was the enlargement or 'flower.' The outer or convex side of the enlargement is covered with a continuation of the bark of the 'stem,' the bark ending at the outer edge of the 'petals.' The concave or inside of the 'flower' is delicately creased like the veins of a petal, running from the center to the periphery, as shown by the photographs. Many of the flowers showed decided indentations in the periphery, as if divided roughly into four-parted 'corollas,' and varying from eighteen to twenty inches in diameter down to minute growths. They might, therefore, easily be mistaken for flowers by those who can not reason from effect back to cause.

The real cause of these peculiar growths is found in the biological law that every organism will protect itself against outside intrusion if it can. Thus when any foreign substance, whether living or inert, enters the living organism, the intrusions are resented by the organism, which tries to protect itself by either assimilating the intruder, ejecting it, or by building up a barrier around it. Thus when the seeds of the parasitic order Lorentheaceæ adhere to the bark of a tree by the gelatinous coating which surrounds them and there germinate, sending their parasitic roots down through the sap wood of the host to procure their nourishment, the host, not being able to eject them entirely, forms a ball-like excrescence around the juncture of the two plants by the irritable hypertrophy of the tissues thus caused in the host. If the invading plant be pulled out of the growth thus formed, a delicately carved socket will be seen, very much like that of these 'wooden flowers,' but upon a smaller scale. Thus such a 'flower,' though small and usually upon a large limb, is formed whenever a mistletoe grows upon an oak.

FIG. 3. A BRANCH OF *Pinus*, showing the 'wooden flowers' roughly divided into four petals.

These large 'wooden roses' are, therefore, nothing more than the protective hypertrophied tissue formed by the branches of some host tree when attacked by a parasite, which in this case is a gigantic species of mistletoe, *Lorenthus Ladebeckii* (Engl.), growing upon any one of several host trees, the principal ones being *Citrus medica* and several species of conifers. The *Ladebeckii* flourishes in isolated zones throughout the western coast of the American continent from northern Mexico to Terra del Fuego, but has never been authentically reported from any other part of the globe.

The one remarkable thing which attracts attention to these growths and causes them to be mistaken for flowers is the great proportions attained by them. The 'stem,' which of course is a limb of the host plant, rarely exceeds an inch and a half in diameter, the parasite evidently not being able to attack other than the younger and more vigorously growing shoots. As long as these can supply the nourishment for the *Ladebeckii,* it grows, the excrescence upon the citrus becoming larger and larger until the distal portion of the branch dies, leaving the small inner portion of the branch supporting a large ball from which grows the parasite. At the end of a few years, say four to seven, sufficient nourishment can no longer reach the parasite, either because the small supporting branch can no longer carry it, or the protective excrescence has shut it off from the intruder, which therefore drops out, leaving the open, delicately carved formation, which so resembles 'wooden flowers' as to give rise to the remarkable legend above recounted.

THE GEOLOGICAL PRELUDE TO THE SAN FRANCISCO EARTHQUAKE[1]

By GEO. H. ASHLEY

WASHINGTON, D. C.

SINCE the San Francisco earthquake, the reading and scientific public has become acquainted with the fact, if not already known, that the recent disaster was the result, not of volcanic activity, but of the activity of the ordinary mountain-making forces. In a large measure they have become acquainted with the further facts that mountain-making forces have long been, and still are, active in the immediate region about San Francisco; that as a result of these activities the rocks of the regions are folded and faulted; that the faulting is of major importance; that the recent disturbance is ascribed by the geologists to movements of adjustment along one or more of these fault planes; that investigation after the earthquake along some of these lines gave abundant evidence of differential movement visibly affecting the surface.

These facts, now widely known, start questions along several lines of inquiry. One of these lines, having an important bearing on the probabilities of future trouble, involves the geologic evidence as to the recency of the observed earth movements; the relative value of this last displacement as compared with past displacements, both remote and near; the character and amount of geologically recent movements; in short, as given in this paper, a résumé of the recent geologic history of the San Francisco peninsula and the observed evidence upon which the statement of that history is based.

As our interest increases with the recency of the events the earlier history will be passed over rapidly and increasing attention given to the later events.

California in Mesozoic time was the theater of profound geologic activity—the movements of subsidence, the vast volume of sedimentation, the intrusion of great sheets of igneous rocks, and the final folding, crushing and faulting were possibly not exceeded anywhere in the world during that period. In Tertiary time the same notable activity continued. The last expression of that activity in the immediate neighborhood of San Francisco consisted of a subsidence beginning apparently just at or before the end of Miocene time and continuing probably a little over into the Quaternary. Coincident with this subsidence was sedimentation that locally resulted in the laying down of over 4,700 feet of sediments. A remnant of these deposits, known as the Merced series, stretches from the city limits of San

Francisco southward along the coast for 4 miles, beautifully exposed in bare cliffs 700 feet high. A study of the geology shows that this fragment lies between two fault planes, beyond which no trace of it is found for many miles. Calculation shows that on either side of this depressed block there has been elevation during Quaternary time of about 1½ miles with *pari passu* removal of the Merced series and deep erosion of the underlying basement. The time interval is best measured by the erosion of the mile of Merced sediments. As exposed to-day upturned along Seven-mile Beach, these consist mainly

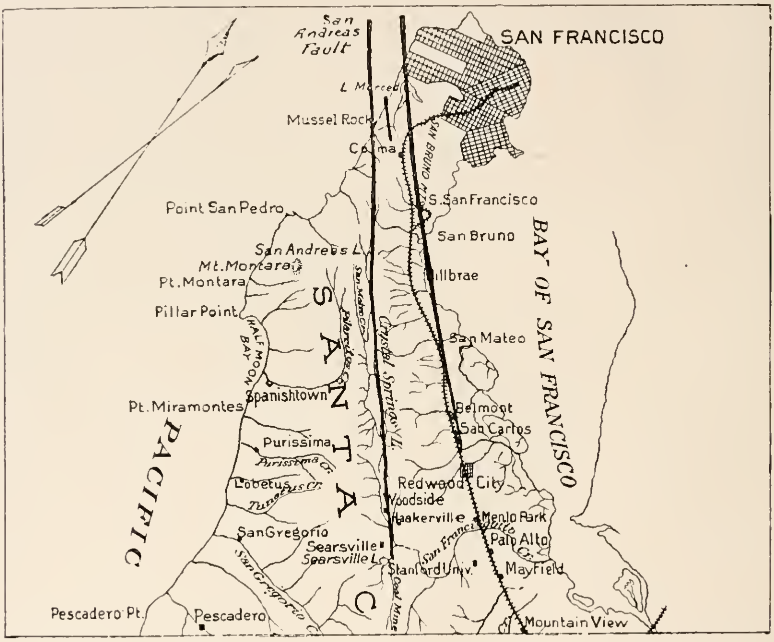


FIG. 1. SKETCH MAP OF NORTHERN PART OF SAN FRANCISCO PENINSULA, showing position of the three faults to which the discussion in this paper is confined.

of half consolidated sands, clays and hard conglomerates, ringing under the hammer. To have removed such a volume of rock would appear to have required most of Quaternary time, especially as the underlying hard pre-Tertiary sandstones, serpentines and cherts were also deeply eroded. During this movement there were volcanic outpourings of some extent. There also appear to have been interruptions to the movement which do not affect the main problem now, and will be passed by. The final result probably left the whole region about San Francisco dry land, except for the water courses. Sand dunes and wash deposits abounded then as to-day. The Monterey cypress and Douglass spruce grew abundantly near San Francisco, some of the latter reaching a diameter of four feet, or more. The mammoth or mastodon roamed the peninsula at that time.

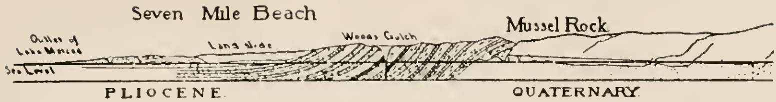


FIG. 2. SECTION OF SEVEN-MILE BEACH AND BEYOND; from mouth of Lake Merced, showing position and exposure of Merced series of rocks. This remnant of this series lies between the San Bruno fault plane at the north and the San Andreas fault at the south. Its entire absence from the San Bruno Mountains to the north and Mount Montana to the south is part of the basis for the theory that since its deposition there has been uplift along the two faults which lifted the terrritory outside of these faults nearly or quite a mile and one half above sea-level, and that erosion, not only planed down the folded rocks of this block, but entirely removed the Merced rocks either side of this block.

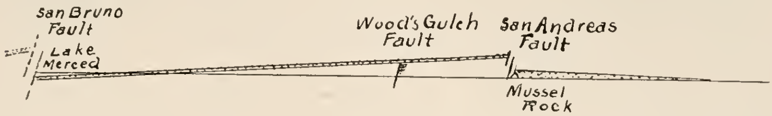


FIG. 3. FIGURE SHOWING POSITION OF RECENT MARINE DEPOSITS ABOVE SEVEN-MILE BEACH and the relative movement along the two fault planes.

Since then two events are clearly shown in the records; these are subsidence and differential uplifts. The subsidence carried most, if not all, of the San Francisco peninsula below its present elevation, flooding the valleys and leaving the hills of the city largely an archipelago. Marine deposits were laid down on top of the clearly recognizable sand dunes and wash deposits, containing in many places the trees mentioned above. Following that came local uplift, raising these marine deposits to elevations of over 700 feet above sea-level just

FIG. 4. MUSSEL ROCK FROM THE SOUTH, showing marine Pleistocene (*a*) overlying sand dunes (*c*) and igneous rocks (*b*).

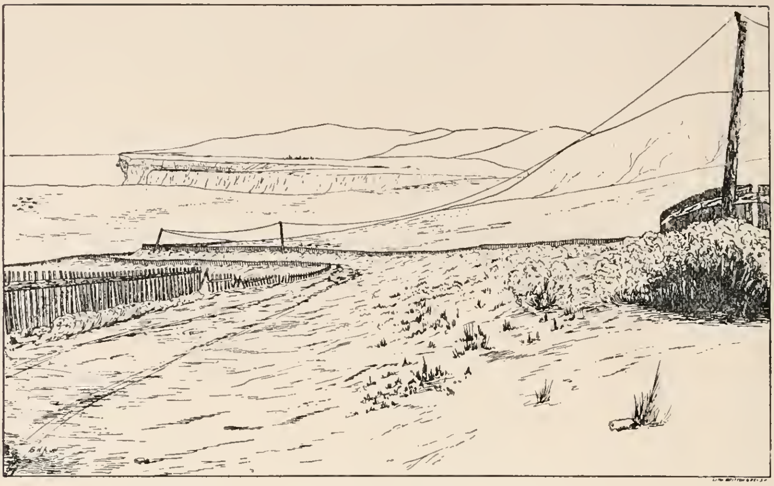


FIG. 5. RAISED BEACH NEAR LOBETUS, Quaternary overlying Merced Strata.

about San Francisco, and double that farther down the coast. These last movements were differential, and it is quite possible that in some cases the two took place together. In the same way, it is quite possible that while in parts of the area the evidence suggests elevation continuing at present, elsewhere subsidence is in progress, though not so evident. The point of special interest in this connection is the fact that this differential movement about San Francisco is clearly a movement of the fault blocks and reveals slipping along these planes of faulting of hundreds of feet in times so recent as to suggest that much of it may have taken place since the human occupation.

Considering first the evidence of movement, a good example is furnished by the San Andreas fault, where it reaches the ocean at Mussel Rock. At the foot of the parallel San Bruno fault a few miles to the northeast the recent marine deposits are about at sea-level. Coming south along the sea-cliff or southwest, these deposits are clearly seen rising, making distinct benches in the little stream valleys where they overlie the wind and wash deposits of the preceding stage, these wash deposits in places being full of half exposed spruce logs, which the neighboring inhabitants use for firewood. Before the San Andreas fault is reached, these deposits have risen to over 700 feet above sea-level, lying on the truncated upturned edges of the Merced strata. As soon as the trace of the San Andreas fault has been crossed, the top of these sediments is found at only 220 feet above sea-level, whence it descends until within a short distance it reaches the level of the beach. Here is therefore clear evidence of movement along the fault plane of several hundred feet since this last submergence. The question of most interest then becomes: How recent was this last uplift?

Farther south on the ocean side of the San Francisco peninsula the coast is fringed with a raised beach, such as occur so abundantly along much of the California coast. The top of this beach is 75 to 100 feet above sea-level. At Purisima, the creek of that name runs out across this raised beach through a slightly cut channel to the very edge of the plane where it drops as a water-fall directly into the surf. As the rocks through which it would have to cut to reach sea-level are the only partly consolidated Merced series, there is striking evidence of the recency of the raising of the beach. Again just south of San Francisco are many places in which the recently raised deposits and in slighter degree the underlying Merced rocks have been trenched to depths of as much as 75 feet since the production of the Coast and Geodetic Survey map of this region in 1869. The fact that so large a part of these recent deposits still remains in view of this rapid erosion impresses one in the field most profoundly.

There is another line of evidence to which the writer refers with some hesitation. Indian shell mounds abound along the California coast. In many cases these now occur spread out in thin sheets, apparently forming the surface layer of the raised beaches over such large areas that in his first study of them he was deceived and considered them in many cases as marine deposits. As he remembers them now, he can not help thinking that in many cases they have been reworked by water before the final uplift. In one case the writer found an Indian skeleton, evidently formally buried, half exposed in the side of a stream channel so narrow as to force one to the conclusion that the channel has been entirely cut since the burial of the body. This is only a fraction of the evidence that in the field leads one to consider this last uplift as a thing of yesterday, and in all probability of to-day also, or, in other words, that these differential uplifts are still in progress.

A final question of maximum interest is: Is there physiographic or

Fig. 5. A Portion of the U. S. Geological Survey Map from Mussel Rock southeastward nearly to San Andreas Lake, showing topographic environment of a chain of ponds and undrained basins believed to be due to recent earthquake movements.

other evidence that there has been sudden slipping along these fault lines in very recent years, comparable in importance with movements of two months ago, especially as expressed in fault scarps? According to the preliminary report of the earthquake committee, the rupture of April 18 shows a horizontal displacement averaging 10 feet, and a vertical displacement not to exceed four feet. How long such a surface disturbance can be subsequently recognized is a question. The San Andreas fault belt is well situated for the noting of any such displacement for several miles southeast of Mussel rock, in many cases the actual fault planes emerging in sublevel pasture land at the surface. Neither the writer's notes nor his memory now yield any evidence of such a scarp. On the other hand, the fact that where such evidence might be seen has long been subject to the tramping of cattle renders its absence of less value. In this connection he does not give any value to a small scarp noted just back of Mussel Rock. At the time, it was considered to be a land slip. A photograph taken of it suggests the possibility of its having a deeper meaning.

But if such scarps are lacking, there is abundant evidence of another kind bearing on this subject. I have spoken of the San Andreas fault belt. Such it appears to be rather than a single clearly-defined break. Along this belt between San Andreas lake and where the belt meets the ocean at Mussel Rock is a string of drainless depressions occupied with water part or all of the year. In one or two cases these can be clearly seen to lie directly in one of the lines of faulting. That they are the result of fault movements seems highly probable. When were they made? That they were made within the last few centuries can not be asserted, yet the fact that so many of these shallow basins still exist, neither filled nor drained, notwithstanding that in many cases it is but a stone's throw to the head of a drain with a high gradient, suggests such a possibility. The possible cause of these basins is suggested in what appears to have formerly been one, now trenched from two directions at the head of Wood's Gulch, a small ravine cutting the cliffs of Seven Mile beach, a mile north of Mussel Rock. The ravine follows a fault with downthrow of 800 feet. At the head a cirque-like cut exposed an overhanging fault scarp of 100 feet or more. Against this face there appears to have gradually filled in wash from the adjacent hills, wind-blown sands and detached fragments from the fault face, until the whole thing was buried and later covered with the marine deposits of the last submergence. Judged from what is left of this filling, it must at one time have strongly resembled the undrained basins just described. The evidence suggests that this fault scarp was produced by a single movement. An elephas tusk found about 75 feet from the top of the filled in deposit agrees with the other evidence in placing the time of this movement back to the land period preceding the recent submergence.

Summing up, the evidence seems to warrant the following statements:

1. Since the beginning of Quaternary time there have been differential movements of uplift along fault planes on the San Francisco peninsula amounting in several cases to nearly or quite a mile and one half.

2. That these movements have been followed by subsidence and subsequent uplifts, involving sliding of several hundred feet along the older fault planes.

3. That these last movements are geologically of extremely recent date, that possibly a considerable part of these movements have come since the occupation by the Indians, and probably have continued to the present.

4. That some of the movements along the fault planes, geologically, in fairly recent times, seem to have produced more striking physiographic results than any produced the past spring.

5. That while the recent movement may have relieved the stress which the rocks were under to such an extent that it will be many years, or possibly centuries, before another such a disturbance will take place, on the other hand, a comparison of the few feet of motion in April with the hundreds of feet of movement that have taken place in very recent time suggests that fault adjustments of equal or greater violence are liable to occur at any time in the future. And since similar conditions are known to occur all over the Pacific Coast region, no place in that whole district can claim immunity.

OUR GREATEST EARTHQUAKES[1]

BY MYRON LESLIE FULLER

UNITED STATES GEOLOGICAL SURVEY

PROBABLY few people are aware that the greatest earthquake our country has experienced since its settlement was not the destructive shock at Charleston in 1886, or even the recent terrifying manifestation at San Francisco, but was, on the contrary, the now almost forgotten earthquake of New Madrid, the first tremors of which took place on the sixteenth of December, 1811. Strange is that trait of human nature by which even the most appalling of nature's manifestations slip rapidly from the memory, so that only a hundred years later little but tradition remains of the earthquake which changed the configuration of extensive areas of the Mississippi Valley, raising some portions, depressing others, shifting the course of streams, draining old swamps at one point and forming new lakes at others. All this and more, however, took place during the successive vibrations which shook the New Madrid region almost continuously for a period of many months in 1811 and 1812.

The New Madrid Earthquake

The night of December 15, 1811, fell quiet and peaceful, and the settlers retired little dreaming of the impending catastrophe. At two o'clock in the morning, however, they suddenly awoke to find the houses over their heads groaning and cracking, chimneys falling, furniture thrown about, and the earth rocking and trembling. Groping their way to the open fields they huddled together until morning, the shock which succeeded shock at short intervals in the darkness keeping them from returning to their tottering houses. At New Madrid, on the Mississippi, the French population were dancing away the night when the shock came and instantly terminated the revelings, joy being replaced by terror as they rushed from the buildings to the open, where catholics and protestants alike knelt in supplication during the remaining hours of the night.

Daylight brought little relief. At seven a rumbling like distant thunder was heard and in an instant the earth was convulsed so that no one could stand. Looking at the ground the terrified people saw it rise and fall, as earth waves, like those upon the sea, rushed past, waving the trees until their branches interlocked and causing yawning

[1] Published by permission of the director of the United States Geological Survey.

cracks to open where the surface was bent and the swells burst. Giants of the forest were split for forty feet up the stump, half standing on one side of the fissure and the remainder on the other. In one instance a crack opened in a cellar, swallowing a large number of castings just received from Pittsburg and temporarily stored away there.

Some of the earthquake rents were of great size, having widths of thirty feet or more, while some are reported as many as five miles in length. Others were circular in form, making basin-like depressions up to several hundred feet in diameter. Into some of these cracks rushed the waters from swamps and bayous, while elsewhere small streams or even rivers left their old beds and made new channels through the cracks. In one instance, a settler living on a neck of land lying within a great bend or ox-bow started at daybreak the morning after the quake to go to his well which the night before had been in his yard. But no well was there! Instead the river was at his door. Glancing across the water, however, the well could be seen on the further side. During the night a crack had been formed between the house and the well and had been taken possession of by the waters, leaving both unharmed though on opposite sides of the stream.

Accompanying the cracking in many instances there seemed, according to one observer, "a blowing out of the earth, bringing up coal, wood, sand, etc., accompanied with a roaring and whistling produced by the impetuosity of the air escaping from the confinement . . . trees being blown up, cracked and split, and falling by thousands at a time. The surface settled and a black liquid rose to the belly of the horses." The atmosphere was saturated with 'sulphurous vapor,' due to the gases escaping from the decaying vegetation and coaly matter (lignite) deep below the surface in the deposits of the prehistoric Mississippi. These gases tainted the air for miles and so affected the streams and rivers that the waters, even to a distance of one hundred and fifty miles below, could not be used for several days. The intense darkness caused by these vapors in the night, and the murky purplish tinge imparted to the atmosphere by day, produced a vivid and never to be forgotten impression on every one who passed through the experience.

It was along the Mississippi that the destruction reached a maximum. A traveler on a flatboat, tied up to the bank about forty miles below New Madrid, speaking of the first shock, says that the men, wakened by the quake, sprang to the deck thinking the Indians had made an attack. After daylight, as they were preparing to depart, "a loud roaring was heard, sounding like steam escaping from a boiler. This was accompanied by a violent agitation of the shores and tremendous boiling up of the waters in huge swells which tossed the boats so violently that the men with difficulty could keep upon their feet. The sandbars and points of islands gave way, swallowed up in the tumultuous bosom of the river, carrying down with them the cottonwood trees cracking and crashing, tossing their arms to and fro, as if sensible of their danger, while they disappeared beneath the flood. From the check given to the current by the heaving bottom, the river rose in a few minutes five or six feet and again rushed forward with redoubled impetuosity, hurrying along the boats, now let loose by the horror-stricken boatmen, as in less danger on the water than on the land."

Whole islands disappeared. Captain Sarpy of St. Louis, with his family and considerable money aboard, tied up at an island on the evening of the fifteenth of December, 1811. In looking around they found that a party of river pirates occupied part of the island and were expecting Sarpy with the intention of robbing him. As soon as the latter found that out he quietly dropped lower down the river. In the night the earthquake came, and next morning when the accompanying haziness disappeared, the island could no longer be seen; it had been utterly destroyed as well as its pirate inhabitants.

Few scientists were in the region during the period of shocks, but we are fortunate in having handed down to us a realistic picture from the pen of the great naturalist Audubon.

> Traveling through the Barrens of Kentucky (of which I shall give you an account elsewhere) in the month of November, I was jogging on one afternoon, when I remarked a sudden and strange darkness rising from the western horizon. Accustomed to our heavy storms of thunder and rain, I took no notice of it, as I thought the speed of my horse might enable me to get under shelter of the roof of an acquaintance, who lived not far distant, before it should come up. I had proceeded about a mile, when I heard what I imagined to be the distant rumbling of a violent tornado, on which I spurred my steed, with a wish to gallop as fast as possible to a place of shelter; but it would not do, the animal krew better than I what was forthcoming, and instead of going faster, so nearly stopped that I remarked he placed one foot after another on the ground, with as much precaution as if walking on a smooth sheet of ice. I thought he had suddenly foundered, and, speaking to him, was on the point of dismounting and leading him, when he all of a sudden fell a-groaning piteously, hung his head, spread out his four legs as if to save himself from falling, and stood stock still, continuing to groan. I thought my horse was about to die, and would have sprung from his back had a minute more elapsed, but at that instant all the shrubs and trees began to move from their very roots, the ground rose and fell in successive furrows, like the ruffled waters of a lake, and I became bewildered in my ideas, as I too plainly discovered that all this awful commotion in nature was the result of an earthquake.

The vibrations did not cease for over a year from December sixteenth, the date memorable for the first shock. During the succeeding three months 1,874 shocks were recorded, of which eight were violently destructive, ten very severe and thirty-five generally alarming. In fact, this earthquake is famous all over the world as one of the few instances of almost incessant shaking for a period of many months in a region remote from the seat of any volcanic action.

Some, like the first, were accompanied by disruptions of the surface and by changes in elevation of the ground. The country which before the shocks was level, with occasional small prairies, was sadly changed. In places old bayou-lakes were drained so that corn could be planted in their bottoms, while elsewhere lakes of considerable size were created. The surface for hundreds of acres was covered by the sand thrown up with the water from the fissures. Even to this day this can be recognized in the forest, where it occurs as barren spots upon which little will grow.

A few more years and a century will have passed since the shocks so vividly described took place. From a wilderness with a few scattered settlements, the region has become, in the northern part at least, a populous farming region with numerous prosperous towns. The great city of Memphis has appeared near the limits of the earthquake region at the south, while St. Louis with its hundreds of thousands of people is but a little distance outside the area to the north.

Notwithstanding the development of the region and modification of the surface by nearly a hundred years of cultivation, the watchful eye can still detect evidences of the powerful forces which so strongly affected the area in 1811. Throughout all the country from New Madrid in Missouri southward to beyond the Arkansas line, and from the Mississippi river westward to the highlands of Crowleys Ridge, there is hardly an open field which does not show one or more low swells of light sand standing out in marked contrast with the dark soil constituting the ordinary surface. These are the well-known 'sand blows' produced by the actual eruptions of sand and water from considerable depths through cracks in the clayey surface deposits.

Some of the cracks were of considerable length, giving rise to the long narrow 'sand blows,' while others were very short, almost all the water and sand coming from a single point. In such instances little cones or craterlets, as they are called, consisting of low mounds of sand with depressions in the center were often formed.

Even more conspicuous, though less numerous, are the great cracks formed in the earth at the time of the quake. Few people have seen these in their full development, as they are hidden in tangles of vines in the as yet almost untouched hardwood forests on the bottoms of northeastern Arkansas. Turning northward from the lumber town of Parkin about thirty miles due west of Memphis and following the old 'De Soto trail,' in a few hours one reaches the southern portion of the earthquake area and is in the midst of earthquake features of surprising magnitude. The region is low and is frequently submerged for weeks in the spring by backwater from the sluggish rivers, while in summer the cane brakes in the more open spots and the thickets of poison ivy and other vines in the forests present additional obstacles to the explorer. Wild turkeys, deer, wild cats and even wolves are still

1. One Side of Earthquake Crack in New Madrid Region, showing scarp 4 feet in height. (Photo by Fuller.)

2. Landslide Scarps near Reelfoot Lake, Tenn., produced by New Madrid earthquake. (Photo by Fuller.)

found. There are no roads, and for many miles not even a solitary settler is to be seen, but with a good horse and a guide familiar with the cracks, blows and 'sand slews,' the region can be penetrated and the earthquake features examined. It is in the depths of these forests along the St. Francis river that the cracks reach their greatest development. How wide they may have been when first formed and how deep, no one can tell. The originally steep banks have crumbled and the fissures partly filled until at the present time they resemble a deep ditch more than a crack. Yet some of these ditch-like depressions are still thirty feet or more across and so deep that a man on horseback can not see over the top, even when he has succeeded in scrambling or sliding down the steep sides. From cracks of this size there are all gradations down to little ones of only a half a foot in depth, but all are still distinctly recognizable. Most of them are within a quarter or half a mile from some river and have a general north-south direction, as if the surface of the land shifted bodily towards the waterways, leaving great rents in the ground where the materials parted. One of the smaller of the cracks is shown by one of the illustrations of the present article (Fig. 1).

3. TREES TILTED BY NEW MADRID EARTHQUAKE. (Photo by Fuller.)

Fissures of another, but equally conspicuous type are the land-slide cracks formed where steep slopes, such as those along the east side of Reelfoot Lake in western Tennessee, occurred within the earthquake area. Here the bluffs, which are several hundred feet in height, were literally shaken to pieces by the shocks, the trees uprooted, overturned, or prostrated, and great masses of earth precipitated down the steep hillsides. Figure 2 shows some of the scarps thus formed, while another shows trees overturned at the same time (Fig. 3). Sometimes the original trunks are decayed and gone, all perhaps but a projecting stump, but shoots from the original have often taken their place as giants of the forest.

The features for which the New Madrid earthquake is most renowned, however, are the swamps and lakes which resulted from the warping of the surface. The former may be seen at many places in southern Missouri and northern Arkansas. In the view of such a swamp here given, the water growth in the foreground and the dead trunks of the old trees are in contrast to the dense growth of new timber which is gradually reclaiming the swamp (Fig. 4).

Reelfoot Lake, the most noted single feature resulting from the earthquake, is a shallow body of water between the Mississippi river and the Chickasaw Bluffs in western Tennessee. It has a length of about twenty-five miles, a width of about five miles, and a depth of twenty-five feet or more. Previous to the earthquake, it is said, no lake existed, the lands being of the ordinary type of fertile bottoms

4. View in Sunk Lands formed by New Madrid Earthquake in Southeastern Missouri.. (Photo by Fuller.)

characteristic of the Mississippi, and had early been granted by the Spanish to certain favored individuals. Through the land ran Reelfoot Creek, a little stream rising in the highlands on the east.

After the earthquake all was changed. A warping of the surface occurred across the course of the little Reelfoot Creek, the channel in the lower portion being lifted above its old level so water no longer flowed through it, while the upper part sank and was soon covered by the waters that collected behind the barrier. The old channel can still be traced by soundings across the lake and the landmarks bounding the early grants made out beneath the waters, while to the south the nearly dry bed of the unlifted creek may be traced to the Mississippi (Fig. 5).

Comparison with Later Earthquakes

In earthquake studies one of the most fascinating lines of research is the investigation of the relative intensities of the different shocks. Of the various evidences, that afforded by the resulting disturbances of the surface conditions is most reliable. Accounts of those who have felt the shocks are unreliable in determining intensities, since the feelings experienced at such a time are largely dependent upon nervous temperament, and upon previous experiences with earthquakes. One feeling a shock for the first time is often seriously disturbed by tremblings to which a resident of an earthquake country would not pay the slightest attention.

5. OLD AND YOUNG GROWTH OF CYPRESS IN REELFOOT LAKE, TENN. The large stumps without enlarged butts are the old trees killed by the New Madrid earthquake. The living trees with enlarged butts are mainly subsequent growth.

A comparison of the effects produced upon artificial structures and upon the earth's surface by our three great earthquakes, New Madrid, Charleston and San Francisco, seems to show that of the various types of phenomena associated with earthquakes, nearly all were more strongly developed at New Madrid than at either of the other localities.

The length of the period of marked disturbance at San Francisco was only a few minutes during the eighteenth of April of this year. The Charleston earthquake occurred, after a preliminary tremor a day or two before, but not felt in the city on August 31, 1886, the severe shocks being confined to a few hours, although not entirely ceasing for three months. In New Madrid, on the other hand, the vibrations, which began on December 16, 1811, continued almost unceasingly for several months, while for more than a year they recurred at frequent intervals. Even at the present time there is probably not a year goes by without a distinctly recognizable shock.

In San Francisco few of the better class of buildings were destroyed, and in Charleston, although the damage was great, few buildings collapsed completely, and the cabins were seldom more than shaken from their foundations. In the New Madrid region there were no high buildings, one story log or frame houses being the rule, but notwithstanding this many are said to have been shaken to pieces by the relatively intense shock.

Again, no progressive wavelike undulation of the surface of any magnitude was recognized at the time of the shock at San Francisco, but at both Charleston and New Madrid the surface rose and fell in waves several feet in height. In Charleston the forests were but little affected, but at New Madrid the trees were often thrown together upon the ground in confused heaps or snapped sharply off near the ground as by an axe.

The streams in the vicinity were little affected by the San Francisco shock, and even at Charleston few if any permanent changes resulted from the earthquake, but in the New Madrid region the effect of the disturbance was very marked. The courses of some of the streams were changed—the water following new cracks instead of the old channels. Others were deflected by warpings of the surface, and still others by sharp uplifts or faults, giving rise to swamps or bodies of open water.

[6. EARTHQUAKE CRATERLET NEAR CHARLESTON, S. C., as it appeared immediately after the shock.

Cracks were formed by the earthquake in each locality. In San Francisco except along the immediate line of faulting they were few in number, small in size, and limited to small tracts of especially soft ground or to the steeper hillsides. In Charleston they occurred over an area several miles in diameter, but were usually under an inch across, except near the rivers. At New Madrid, on the other hand, they extended over an area many times as great, extending from southern Missouri nearly to Memphis, a distance of over one hundred miles, and from one side of the Mississippi Valley to the other, and were often many feet in width. No sand is reported to have been thrown from the cracks at San Francisco except in rare cases, but at Charleston numerous craterlets such as shown in one of the accompanying illustrations were formed, from which large amounts of sand and water flowed out quietly upon the surface (Fig. 6). At New Madrid the sand and water not only came out more frequently and covered a larger area, but were ejected with violence, sometimes reaching, according to observers, to the very tree tops.

Little change of level occurred at either San Francisco or Charleston, but in the New Madrid region great areas sank and were covered by water, one of them now covered by Reelfoot Lake being over twenty-five miles long and more than five miles wide.

Cause of the Shocks

The shocks in each case have had their origin in the breaking and slipping of hard rocks underneath. All rocks of the earth's crust are subjected to stresses of different kinds, such as may be produced by the weight of overlying material, by the shrinking of the earth's interior, or by other causes, and the time comes when their strength is no longer sufficient to resist them, and a break occurs, usually accompanied by a crushing of the rock along the fracture or by a slipping of one part of the rock over the other. It is this slipping or crushing which gives rise to the vibrations known as earthquakes.

In the San Francisco region this slipping is constantly going on and minor shocks have been of frequent occurrence. It was only a slightly larger slip than usual which produced the recent disastrous shake. In Charleston the slipping was mainly at one time, no preliminary shocks of importance were felt and few occurred afterwards, except during a short period immediately following the earthquake, but in the New Madrid country the quaking has continued for several hundred years at least. Both the Charleston and New Madrid earthquakes occurred in regions where the earth's crust is being overloaded—in the one instance by the sediments brought down by streams from the Appalachian Mountains and in the other by the floods of the Mississippi—and the fracturing is believed to have resulted from the readjustment of the harder rocks to the increasing load.

A Look into the Future

As to what the future holds in store for the three regions we can only judge by the past. The Charleston earthquake, as far as shocks of any intensity in recent times are concerned, is unique in the Atlantic Coastal region. The equilibrium has probably been regained since the slip of 1886, and it may be ages before another occurs. San Francisco, on the other hand, is in an earthquake region, shocks are of common occurrence, and another of an intensity equal to or greater than the recent disturbance may occur at any time, although, on the other hand, the temporary adjustment brought about by the recent slip tends to decrease the danger of an immediate severe shock.

In the New Madrid area, however, the earthquake of 1811–12 was only one of a series. Cracks may be found with trees fully 200 years old growing in their bottoms, indicating early shakes of equal if not greater intensity than the last. Nor has the movement yet ceased. Every year there are one or more shocks, sufficient to shake objects from shelves, and to seriously affect wells and springs. Only last summer the newspapers were full of accounts of such a shock in southeastern Missouri and adjoining regions—the very area of the New Madrid earthquake. If there have been two or more strong shocks with an intensity far greater than the Charleston quake, and if the readjustment is not completed as is positively indicated by the recent shocks, then there is every reason to believe that disturbances of equal severity may occur in the future. Such quakes, it goes without saying, would be disastrous to such towns as Hickman in Kentucky, Caruthersville, New Madrid, Campbell and others in Missouri, all of which are in the area of disturbance. The larger cities of Cairo and Memphis, although outside the main area, would also probably suffer severely, as they are built on soft deposits overlooking the Mississippi in situations favoring easy slipping towards the streams. Such spots were often severely fissured by the early quake, large masses slipping into the river, and what has occurred once may occur again. St. Louis would also probably be severely shaken, but its buildings are less liable to destruction from a shock originating in the New Madrid area because of the remoteness from the point of disturbance.

SHORTER ARTICLES

THE EPIDEMIC OF TYPHOID FEVER AT PALO ALTO.

IN the spring of 1903, the university community at Palo Alto was startled to find that in about two days upwards of one hundred and thirty students and about a hundred other people—most of them living in the town of Palo Alto, but a considerable number also in fraternity houses on the university campus—were attacked by typhoid fever. The Students' Guild, the cooperative hospital association of Stanford University, immediately set to work upon the problem of furnishing hospital service, while the department of hygiene of the university and the board of health of the town of Palo Alto devoted themselves to the investigation of possible causes for the outbreak.

The university town, with a population of about four thousand, was entirely new and its health conditions were ordinarily of the very best, there being no slums, cesspools or foulness of the ordinary sort. Every sanitary precaution had been taken in the lodging of students. The water supply was above suspicion, being drawn from deep-driven wells. The whole difficulty was finally traced to a single small dairy, the milk of which had been the source of the infection.

A full account of all elements concerned in this case has been published in a pamphlet for free distribution, by Professor J. C. L. Fish, of Stanford University, president of the Board of Health of Palo Alto, together with analyses of reported cases by Dr. C. D. Mosher, and a discussion of the source of infection of the milk supply by Dr. William F. Snow. In view of the lesson to be derived from this case and from the nearly parallel outbreak at Cornell University which preceded it, an account of the method of infection may be found interesting and useful.

The report shows that there were no cases of typhoid fever in Palo Alto, so far as known, between 1894 and 1903. On investigation it was found that the one thing in common which connected the different houses in which cases were reported, was the milk supply. On further investigation it was found that the milk man got a portion of his milk from a Portuguese dairy about five miles from the university on a little brook tributary to Los Trancos Creek. Samples of the water used in washing the cans and cooling the milk were examined by bacteriologists and found to contain large quantities of the bacillus *coli communis*, the well-known bacillus of typhoid fever.

The investigation of the sources of infection at the Portuguese dairy reads like the plot of a tragedy. The scene is laid in the month of December, 1902, at Stanford University and Palo Alto and the immediate vicinity. The *dramatis personæ* are taken from homes all over the country. The Serpa house, where the trouble begins, is situated on the banks of the Madera Creek, three miles above Mayfield. A cousin, from San Francisco, comes to visit the Serpas. Soon after his arrival he complains of feeling ill, and Mrs. Serpa nurses him; she does not consider him sick enough to demand the services of a physician. One week later the relative is better, but Mrs. Serpa is quite sick herself and Serpa calls in a doctor, who pronounces the case one of typhoid fever. A few days more and two of the Serpa children are taken ill with symptoms identical with those of the mother and the cousin. Serpa now becomes thoroughly frightened and abandons what little cooperation in medical and sanitary precautions up to this time he has given to the physician. Confusion reigns in the Serpa house, and many friends come from the surrounding ranches to nurse the sick and to sympathize with distressed husband and father.

The San Francisco cousin, Serpa's wife and the two sick children of Serpa die of typhoid fever. Friends again come to console and remain to be consoled by food and drink. Among the guests are N. and N.'s wife from the N. dairy, P. and the family of P. from the P. dairy, an aunt and her daughter from San Gregorio (a seacoast town forty miles away) and a Portuguese family living on Los Traneos Creek on the road from the P. dairy to Palo Alto.

When the aunt, with her daughter, leaves the Serpa house, she takes with her two of the Serpa children. The first act ends with the general breaking up and dispersion of these solicitous friends to their respective homes.

The second act begins January, 1903. N. and the wife of N. lie sick at the N. dairy with typhoid fever. N. dies.

P. lives on the banks of Los Traneos Creek, a few hundred feet above the intake of the dairy water system; there are no buildings higher up on the drainage area of the system. Nineteen Filipino wood choppers and a hundred lumbermen are employed in the hills surrounding the P. dairy; they visit the dairy ranch. On the banks of the creek, near the pig pen and just above the dairy water supply intake, are primitive out-house facilities for these laborers.

P.'s child is sick with fever. P. complains of a general malaise. At the aunt's house in San Gregorio, the aunt and her daughter and the two Serpa children are ill with typhoid fever. In the Portuguese houses on Los Traneos Creek, on the road from the P. dairy to Palo Alto, are five cases of typhoid fever. At Stanford University and Palo Alto there are two hundred and thirty-six cases of typhoid fever. The black pall of death hangs over the university and Palo Alto. Parents all over the country sit in darkened homes with bowed heads and mourn for the dearly beloved son or daughter, while the health officers work with sleepless activity. "By the process of elimination and by the sequence of events connecting the typhoid fever at the Serpa house with the P. illness, the outhouse on the bank of the P. creek, the rains and the impounded water at the dam, the conclusion is reached that the P. milk was infected through admixture with the creek water used at the milk house of the P. dairy."

Dr. Clelia D. Mosher's contribution to the 'report' contains an exhaustive study of the symptoms, relapses and complications of the reported cases. The analysis is based on the detailed reports of the physicians, supplemented by statements from the families and the individuals affected, together with a careful investigation of the death records. The total number of cases reaches 236; this number includes 24 known cases for which no reports were obtained, occurring among students who had left Palo Alto after the outbreak of the epidemic.

The ages of the patients vary from two months to sixty years. Dr. Mosher shows that the most susceptible age, between fifteen and thirty years, is far above the average and explains the number of children infected by the fact that milk was the source of infection.

EDITH V. MATZKE.

THE PROGRESS OF SCIENCE

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

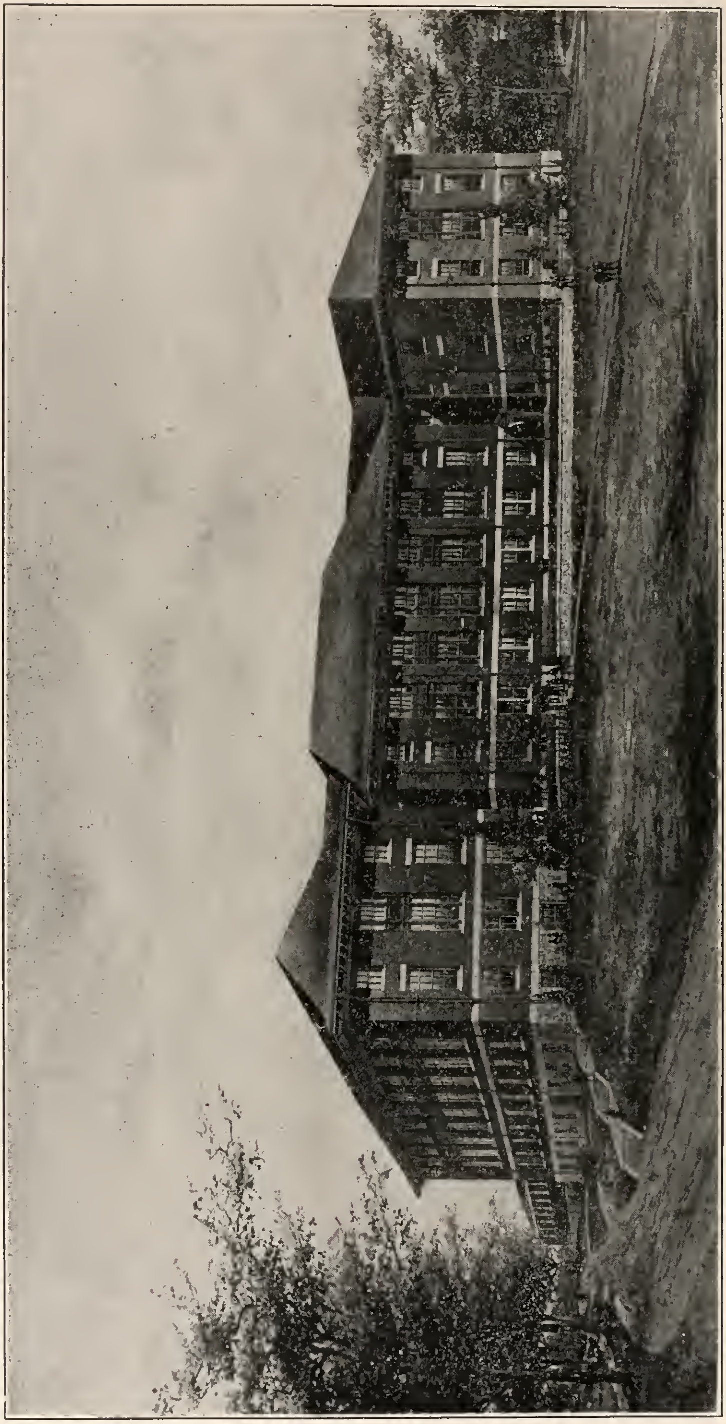
At the New Orleans meeting of the American Association for the Advancement of Science the council voted that in addition to the regular winter meeting, a summer meeting should be held at Ithaca, N. Y., from June 29 to July 3. For such an experiment, and the holding of more than one meeting a year is avowedly an experiment, the place is well chosen. Ithaca is a university town and well adapted by climate and situation for a summer meeting. The campus, where these sessions will be held, is green and well shaded. It is nearly 1,000 feet above sea level and overlooks from a height of 400 feet one of the most famous and beautiful of the lakes of central New York.

In the immediate neighborhood of Ithaca are many places of scenic as well as scientific interest. At Taughannock, about eight miles away, a water fall, 215 feet in height, plunges into an amphitheater the forest-topped walls of which rise vertically more than 300 feet above the bed of the stream. Lucifer Falls at Enfield about the same distance and numerous other cataracts in the glens formed by the tributaries of Cayuga Lake are also of great interest and beauty. The local committee is arranging for various short excursions to these places and also one to the widely known sociological colony—the George Junior Republic. Some of the sections are planning to devote their meetings exclusively to field work and excursions. Papers will be read and discussions held at the places visited during the excursions.

On Thursday evening, June 28, there will be an informal smoker at the Town and Gown Club of Ithaca. On Friday afternoon, June 29, the new Physics Laboratory of Cornell University—Rockefeller Hall—will be opened and several well-known men of science will speak. On Monday evening, July 2, a public address, by Professor J. C. Branner, of Stanford University, on 'The Great California Earthquake,' under the auspices of the society of the Sigma Xi will commemorate the twentieth anniversary of the founding of that organization. Other public lectures will be given by President David Starr Jordan, of Stanford University, on 'The San Francisco Disaster'; by Professor Henry S. Carhart, of the University of Michigan, on 'The South African Meeting of the British Association for the Advancement of Science.' and by Major General George W. Davis, U.S.A., on 'The Great Canals of the World.'

In addition to the usual meetings of sections, a number of special societies will hold sessions in conjunction with the American Association. Among these are the Society for the Promotion of Engineering Education, the American Physical Society, the American Chemical Society, the Society for Chemical Industry and the American Microscopical Society.

As has been said the holding of a summer as well as a winter meeting of the association is an experiment, but it is an experiment which should have the active cooperation of all those who are interested in the advancement and diffusion of science. Until 1902 the association met in the summer, and other scientific societies met in groups during the Christmas holidays. For a large and technical meeting, the winter is the best season, and a large city



Rockefeller Hall; the New Physical Laboratory of Cornell University.

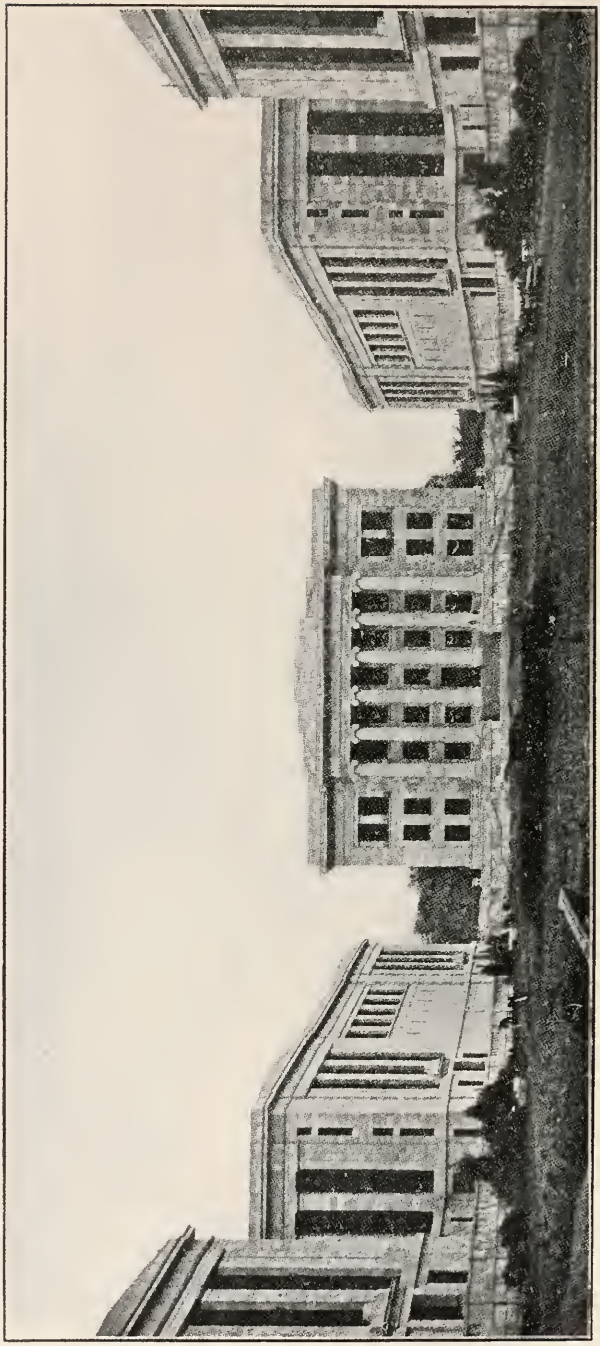
must be chosen. But many members of the association have regretted the abandonment of the summer meetings, which could be held in a university town or summer resort, when out-of-door life and excursions are pleasant, and where old acquaintances and friends may be met and new ones made. The American Association has now more than twice as many members as in 1900, and it should be able to increase its service by holding meetings that will fill the needs of all. It is to be hoped that those who believe that summer meetings are desirable or that the experiment should be tried will go to Ithaca. Whether the meeting is large or small, it will surely be interesting and enjoyable.

THE BOSTON MEETING OF THE AMERICAN MEDICAL ASSOCIATION.

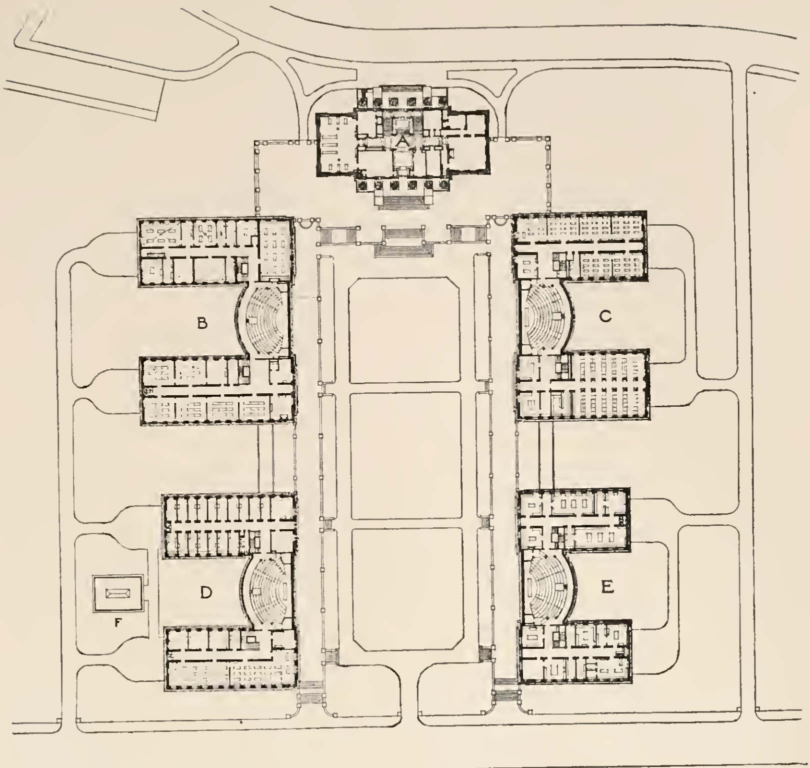
The fifty-seventh annual meeting of American Medical Association which began at Boston on June 5 was the largest and most notable in its history. There were about five thousand members in attendance; the scientific sessions improve from year to year, and the organization becomes more efficient and influential. Washington, New York and Boston are the three chief scientific centers of this country. Of the one thousand leading scientific men 119 are in Washington, 119 in New York and 85 in Boston-Cambridge. But historic continuity has been longest maintained at Boston, and it seems to lend itself better than any other city to a large scientific gathering. There the American Association for the Advancement of Science, the National Educational Association and now the American Medical Association have held their largest meetings. The governor of the state and the mayor of the city maintain the tradition of being gentlemen, while a welcome from President Eliot gives distinction to any gathering. The conditions in Boston are more nearly those of an English city, and the formal functions, the receptions and the garden parties pass off more smoothly and with less artificiality and aimlessness than in other American cities.

After the greetings of the opening session, Dr. Louis McMurtry, of Louisville, Ky., the retiring president, introduced the president elect, Dr. William J. Mayo, of Rochester, Minn., who made the annual address. It was concerned mainly with the organization of the medical profession and its relations to the public, emphasizing, though perhaps unconsciously, the trades union character of the association. Among the topics reviewed were: the need of union to promote not only the interests of the profession, but also the welfare of the public; the function of the medical profession in enlightening the public in regard to sanitation, the dangers from poisonous nostrums and the need of compulsory vaccination; the improvement of the army and navy medical departments; the supervision of medical schools and reciprocity in medical licenses; the relations of physicians to the insurance companies, contract practise, and hospital abuse by patients who are able to pay; the financial position of the physician and the evil of accepting commissions from specialists; the strained relations between medicine and pharmacy. In conclusion Dr. Mayo said: "The vital need of the medical profession is a harmonious organization—an organization that will encourage right thinking and good usage among ourselves, help to secure needed medical reforms, compel redress of grievances and promote and encourage the highest interests of its individual members: and in this lies the future usefulness of the profession as a whole."

The organization of the association has resulted in the 'house of delegates,' representing the medical profession through the states. The county medical societies unite in a state society and the state societies in the national association. The subjects dis-



The New Buildings of the Harvard Medical School



A - ADMINISTRATION BUILDING
B - ANATOMY AND HISTOLOGY BUILDING
C - PHYSIOLOGY AND PHYSIOLOGICAL CHEMISTRY BUILDING
D - BACTERIOLOGY AND PATHOLOGY BUILDING
E - HYGIENE AND PHARMACOLOGY BUILDING
F - ANIMAL HOUSE

PLAN OF FIRST FLOORS

cussed in the sessions of the house of delegates were largely those referred to in the address of the president. The strength of the association is indicated by the fact that it has nearly 25,000 members and an annual income of about $275,000. Its *Journal* is an important factor in organization and in the advancement of medical science.

For the presentation and discussion of scientific papers the association is divided into numerous sections. The programs at Boston were better than ever before, but the papers were very diverse in method and uneven in value. The scientific exhibits were unusually good, and were seen to much advantage in the new Harvard medical buildings, themselves an exhibit of unsurpassed importance.

THE NEW HARVARD MEDICAL SCHOOL

The new buildings of the Harvard Medical School are beautiful beyond illustration or description. They are a renaissance and reincarnation of the spirit of Greek simplicity, dignity and perfection. It is probable that there are no other academic or public buildings in America having equal distinction and beauty. This, at least, was the impression made on the present writer, in spite of garden parties and unkempt surroundings. This opinion, if conferred by competent judges, deserves special emphasis, because the laboratories and lecture-rooms have not been put into buildings designed to look well, but the buildings were made for their uses in accordance with plans of members of the Harvard medical faculty. The thanks of all men of science are due to the architects, Messrs. Shepley, Rutan and Coolidge.

Professor Minot proposed the 'unit system' and Professor Porter the arrangement of two wings with a common lecture-room and library. The unit adopted for the laboratories is 23 x 30 feet, accommodating 24 students. Each unit has three windows and can be subdivided into two or three rooms. The smaller rooms for individual research are also divided into mezzonine storeys. The windows extend to the ceilings, giving abundant light, and the architectural effects are in some measure due to the piers of pilasters between the windows. The buildings can be extended by adding new units, and would finally form courts.

The arrangement of the buildings is shown in the illustration and by the ground plan. The administration building contains offices, common-rooms, lecture-rooms and the Warren Museum with an area of 22,000 square feet. The laboratories are for anatomy and histology, physiology and physiological chemistry, pharmacology and hygiene and bacteriology and pathology, each pair having a common amphitheater and library. The arrangements for heating, ventilation, refrigerating, etc., are very complete.

The large cost of these buildings appears to be justified, as the money was given for them and might be charged to the city of Boston and the people of the country as well as to medical education. It is said that the gray marble added only three per cent. to the cost. The buildings do not, however, provide for clinical work, and as there are altogether only 287 students in the school, the rent to be charged to each student is in the neighborhood of $500. The number of students will, however, increase. The need of four similar amphitheaters, each seating 265 students is not clear. They may be built for the future, but the future may show the futility of lecturing to large audiences of medical students. Here the unit system seems to be lacking where it was especially needed. Still less evident is the desirability of four separate libraries which will apparently be both expensive and inconvenient. But the fundamental criticism which must be made is the permanent separation of the medical school from the rest of the university. It appears to the present writer that Harvard has done great harm to itself and to education by segregating both in time and space the work in medical science. It requires the bachelor's degree for entrance to the medical school, whereas if the sciences preliminary to medicine were carried on at the college, the liberal studies would become less aimless and the professional studies more liberal. The separation of liberal studies, professional work and research does injury to each.

MR. ADAMS ON THE AMERICAN COLLEGE.

The Phi Beta Kappa address given by Mr. Charles Francis Adams at Columbia University has been printed in the daily papers of more than one city, with abundant editorial comment and letters from correspondents. This means that the address was concerned with an interesting problem or, at all events, attacked a problem in an interesting way. Mr. Adams is alleged to have said when engaged in writing a book upon Puritan life, "I never have been so happy as during the last year; I have been destroying people's ideals." At all events he confesses in the present address to 'a decided lack of faith in ideals.' The iconoclasm is entertaining, and it may be profitable, but apart from the characteristically personal form of expression it is not new. Neither is the remedy new though it is claimed as such in Touchstone's words: 'An ill-favored thing, sir, but mine own.'

Mr. Adams became academically famous in 1883 by another Phi Beta Kappa address on 'The College Fetich,' namely, Greek. He now finds that there are things in colleges that are worse than Greek, namely, the elective system, which he calls crude, ill-considered, thoroughly unscientific and extremely mischievous.' The address may be fairly represented by the concluding paragraph, which reads:

"For him who graduated half a century ago, the game is now either won to a degree or irretrievably lost. But, reviewing his record, he is apt to see with great distinctness the nature of the game, and wherein his play was defective, wherein correct. For myself, thus retrospecting, I am constrained to say that, as a training place for the game in which I was to take a hand, the college of the period—and Harvard stood first among them—viewed as a mental gymnasium, was ill-adapted to existing conditions, unsympathetic, and, as respects organization, already distinctly outgrown. In the matter of intellectual training, it was a period of transition—the system of prescribed studies was yielding to a theory of electives. So far as it had then been developed and applied, the new system proved in my experience a delusion, a pitfall, and a snare. My observation, as I said in the beginning, leads me to apprehend that conditions in these respects have not since changed for the better. The old organization yet lumbers along; the implicit belief in the pursuit of aptitudes on lines of least resistance is in fullest vogue. Could I, on the contrary, have my way, I would now break our traditional academic system into fragments, as something which had long since done its work and is now quite outgrown; and I would somehow get back to the close contact of mind upon mind. I would to a large extent do away with this arms-length lecture-room education for the college period. I would develop an elective system based on scientific principles, and the study of the individual; properly regulated, it should be intelligently applied. I would prescribe one of the classic tongues, Greek or Latin, as a compulsory study to the day of graduation, the one royal road to a knowledge of all that is finest in letters and art. I would force every student to reason closely all through his college days; while no man not trained to observe, and equal to tests in observation, should receive a degree. Beyond this I would let the student elect.

THE CAUSES OF DEATH.

The Bureau of the Census has issued a special report on statistics of mortality from 1900 to 1904, which gives important information in regard to the prevalence of certain diseases.

Tuberculosis of the lungs and pneumonia were by far the leading causes of death. The average annual mortality from tuberculosis of the lungs, or consumption was 172.6 per 100,000 of population. The rate has shown a marked decline since 1890, when it was 245.4. The mortality from this disease in the registration area in the United States is lower than it is in Ireland, Germany, Norway, Spain and Switzerland, but higher than in England and Wales, Scotland, the Netherlands, Belgium and Italy.

Pneumonia was second among the principal causes of death, the average annual rate being 165.6 per 100,000 of population. In the registration states the mortality from pneumonia was about 50 per cent. higher in the cities than in the rural districts.

Heart disease was third among the leading causes of death, the average annual rate in the registration area for the five years being 120.9 per 100,000 of population.

Among the leading causes of death, diarrhoea and enteritis were fourth, the average annual mortality from this disease for the five years being 113.1 per 100,000. Over 80 per cent. of the deaths from these diseases were deaths of children under 5 years of age, and over 65 per cent. of children under 1 year of age. The average annual death rate from these diseases was about 75 per cent. higher in the cities than in the rural districts.

The average annual death rate from typhoid fever in the registration area was 33.8 per 100,000 of population. Of the ten European countries for which similar statistics are available Italy alone shows a higher. The mortality from typhoid fever was excessively high in Pittsburg, Cleveland, Cincinnati, Columbus, Louisville and Washington. The average annual rate was much below the average in New York City, St. Paul, Milwaukee, and Jersey City.

SCIENTIFIC ITEMS

Dr. A. Graham Bell, the inventor of the telephone, has been given the doctorate of laws by the University of Edinburgh.—The United States ambassador to Great Britain, Mr. Whitelaw Reid, has presented the gold medal of the American Geographical Society to Captain R. N. Scott, commander of the National Antarctic Expedition.—Dr. Joseph D. Bryant, of New York City, has been elected president of the American Medical Association.—At the recent International Medical Congress at Lisbon, the Moscow prize was awarded to M. Laveran and the Paris prize to Professor Ehrlich.

The International Congress of Applied Chemistry at Rome resolved that the seventh congress shall be held in London, with Sir William Ramsay as the president and Sir Henry Roscoe as honorary president.—The sixteenth International Medical Congress will be held at Buda Pesth in 1909, under the presidency of Professor C. Müller. It is likely that the following congress will be held in New York City.

The Prince of Monaco has offered to give his Museum of Oceanography and Laboratory for the Investigation of the Seas, now at Monaco, to the city of Paris, with an endowment of $1,000,000. The institution is to be under the charge of an international committee.—It is announced that Mr. David Rankin, of St. Louis, has decided to give $2,000,000 to found an industrial and manual training school in St. Louis.—Arrangements have been completed, under a plan outlined by Alfred Mosely, to send, between November and March, five hundred British teachers to the United States and Canada to study the educational systems of the two countries.

THE POPULAR SCIENCE MONTHLY

AUGUST, 1906

THE INVESTIGATION OF THE SAN FRANCISCO EARTHQUAKE

BY G. K. GILBERT

U. S. GEOLOGICAL SURVEY [1]

IT is the natural and legitimate ambition of a properly constituted geologist to see a glacier, witness an eruption and feel an earthquake. The glacier is always ready, awaiting his visit; the eruption has a course to run, and alacrity only is needed to catch its more important phases; but the earthquake, unheralded and brief, may elude him through his entire lifetime. It had been my fortune to experience only a single weak tremor, and I had, moreover, been tantalized by narrowly missing the great Inyo earthquake of 1872 and the Alaska earthquake of 1899. When, therefore, I was awakened in Berkeley on the eighteenth of April last by a tumult of motions and noises, it was with unalloyed pleasure that I became aware that a vigorous earthquake was in progress. The creaking of the building, which has a heavy frame of redwood, and the rattling of various articles of furniture so occupied my attention that I did not fully differentiate the noises peculiar to the earthquake itself. The motions I was able to analyze more successfully, perceiving that, while they had many directions, the dominant factor was a swaying in the north-south direction, which caused me to roll slightly as I lay with my head toward the east. Afterward I found a suspended electric lamp swinging in the north-south direction, and observed that water had been splashed southward from a pitcher. These notes of direction were of little value, however. except as showing control by the structure of the building, for in another part of the same building the east-west motion was dominant.

[1] Published by permission of the director of the United States Geological Survey and of the chairman of the California Earthquake Investigation Commission.

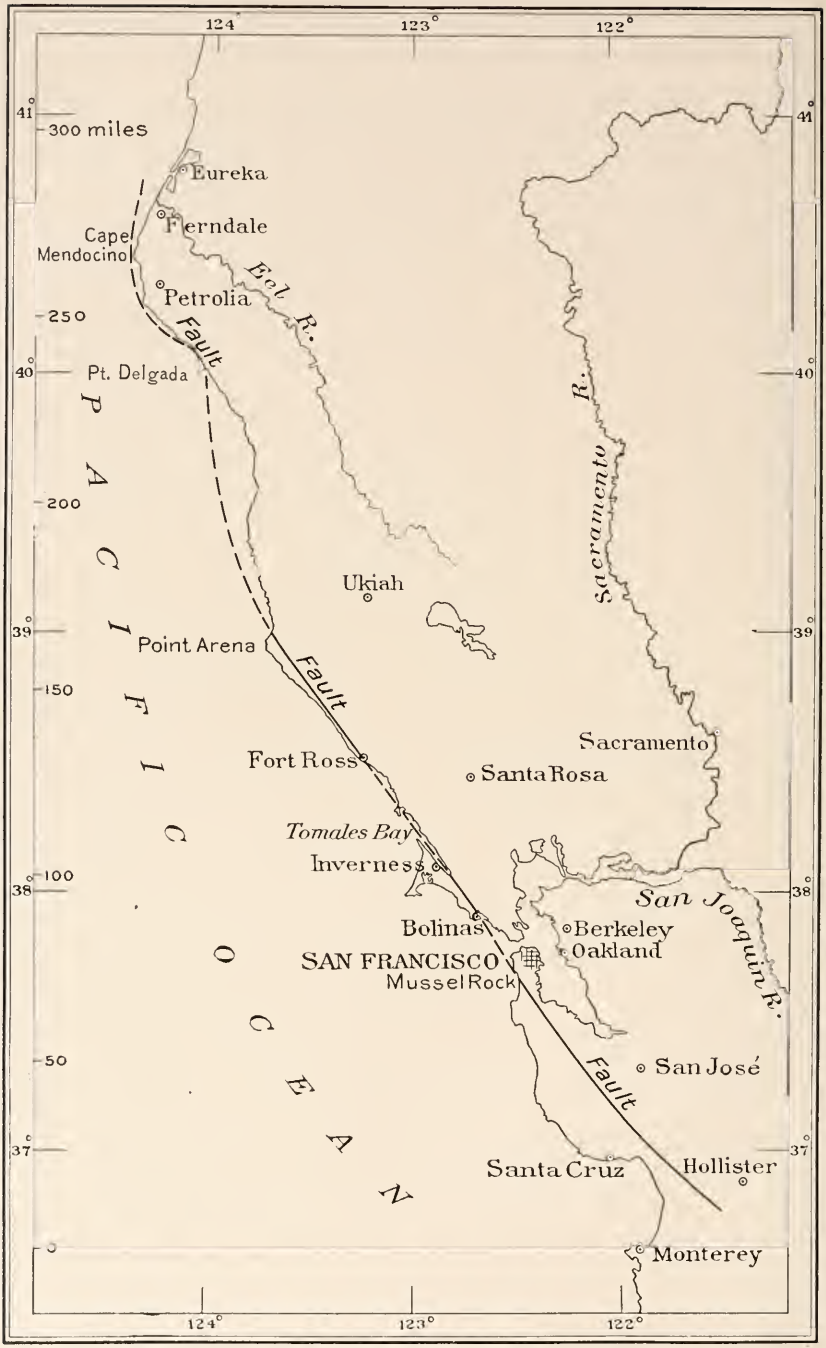


Fig. 1. Map showing the Position of the Fault which caused the San Francisco Earthquake.

In my immediate vicinity the destructive effects were trivial, and I did not learn until two hours later that a great disaster had been wrought on the opposite side of the bay and that San Francisco was in flames. This information at once incited a tour of observation, and thus began, so far as I was personally concerned, the investigation of the earthquake. A similar beginning was doubtless made by every other geologist in the state, and the initial work of observation and record was individual and without concert. But organization soon followed, and by the end of the second day it is probable that twenty men were working in cooperation under the leadership of Professor J. C. Branner, of Stanford University, and Professor A. C. Lawson, of the State University at Berkeley. At that time and for several succeeding days the ordinary means of communication were so paralyzed or overburdened that no messages passed between these two centers of organization; but as the needs of the hour were patent to all, the work was not prejudiced by the lack of intercommunication.

On the third day after the shock Governor Pardee appointed a State Earthquake Investigation Commission, naming as its chairman the head of the geological department of the State University, Professor Lawson, and including in its membership Professor Branner, of the Stanford University, Professors Davidson and Leuschner, of the State University, Professor Campbell, of the Lick Observatory, Mr. Burckhalter, of the Chabot Observatory, Professor Reid, of Johns Hopkins University and Mr. Gilbert, of the United States Geological Survey. The commission held its first meeting three days later, when the scope of its work was considered and defined, provision was made for circulars soliciting information, an announcement was prepared for the purpose of relieving certain groundless fears entertained by a portion of the community, and two committees were appointed for the general work of observation. To the first committee, with Professor Lawson as chairman, was assigned the determination and study of surface changes associated with the earthquake and the collection of data as to intensity, so that isoseismals, or curves of equal intensity, might be drawn upon the map. To the second committee, with Professor Leuschner as chairman, was assigned the collection of data for the drawing of coseismals, or lines connecting points on the earth's surface reached by the shock at the same instant. Some weeks afterward, when the main features of the earthquake had become known, a third committee was appointed, with Professor Reid as chairman, to consider the relations of the earthquake phenomena to certain problems in geophysics, or the science of the inner earth.

The work of these three committees is still in progress, and will not be completed for several months. The actual drawing of isoseismals and coseismals can not be performed until a large body of observations have been compiled and studied, and the geophysical problems are as yet only imperfectly formulated; but of the physiographic phenomena, or the disturbances of the earth's surface, so much is known that it has been thought advisable to prepare a preliminary report. This was submitted to the governor on the third of June, and has been issued as a pamphlet of twenty pages. The expenses of the commission are being met by the Carnegie Institution.

Architects and engineers were not less prompt and energetic. To

FIG. 2. FAULT TOPOGRAPHY BETWEEN TOMALES AND BOLINAS BAYS; looking northwest. The general slope toward the left has been interrupted by a slight uplift of the part at the left. The pond occupies a hollow thus produced.

the men who plan and direct construction in the earthquake district of California it was important to know what materials and what structural forms best withstood the shock, and they immediately began the study of earthquake injuries and of instances of immunity from earthquake effects. In that part of San Francisco where the earthquake injury was most serious the shock was quickly followed by fire, which destroyed much of the evidence, but many important observations were made in the brief interval. The study of structural questions, like the study of natural phenomena, was at first individual only, but afterward was aided by organization. Committees were appointed by various professional societies, national and local, and were charged with the investigation of specific structural questions, and the results of their labors will find place not only in the transactions of the societies, but in revised building regulations and in important modifications of municipal plants for lighting and water supply. Various bureaus of the national government have also taken part in the structural studies, sending experts to San Francisco and other localities of exceptional earthquake violence.

The Japanese government promptly sent to California a committee of investigation headed by Dr. Omori, professor of seismology in the University of Tokyo, and composed otherwise of architects and engineers. The first conference of these visitors with the state commission warranted the suggestion that we may find it as profitable to follow Japanese initiative in the matter of earthquake-resisting construction as in that of army hygiene.

The following sketch of the physical features of the earthquake is based chiefly on the body of data gathered by the State Commission:

An earthquake is a jar occasioned by some violent rupture. Sometimes the rupture results from an explosion, but more commonly from the sudden breaking of rock under strain. The strain may be caused by the rising of lava in a volcano or by the forces that make mountain ranges and continents. The San Francisco earthquake of April 18 had its origin in a rupture associated with mountain-making forces. A rupture of this sort may be a mere pulling apart of the rocks so as to make a crack, but examples of that simple type are comparatively rare. The great majority of instances include not only the making of a crack but the relative movement or sliding of the rock masses on the two sides of the crack; that is to say, instead of a mere fracture there is a geologic fault. After a fault has been made its walls slowly become cemented or welded together, but for a long time it remains a plane of weakness, so that subsequent strains are apt to be relieved by renewed slipping on the same plane of rupture, and hundreds of earthquakes may thus originate in the same place. From the point of view of the geologist the displacements of rock masses are the primary and important phenomena: the faults are incidental phenomena, of great value as indices of the displacements: and the earthquakes are of the nature of symptoms, serving to direct attention to the fact that the great earth forces have not ceased to act.

A faulting may occur far beneath the surface and be known only by the resulting earthquake; but some of the quake-causing ruptures extend to the surface and thus become visible. The New Madrid and Charleston earthquakes are examples of those having deep-seated origins, the Inyo and San Francisco of those whose causative faults reached the surface of the ground.

The general character of California earthquakes was so well known that when the dispatches told of a severe shock at San Francisco no American geologist had a moment's doubt that it was caused by a fault movement, and among those specially conversant with the structure of the affected district attention was immediately directed to several fault lines, with the expectation that one or more of them would show the marks of fresh dislocation. Mr. Ransome prepared a prophetic article

Fig. 3. This Fence, Previously Continuous and Straight, was broken and parted by the earthquake fault, the offset being 8½ feet. The line of fault, concealed by the grass, crosses the ground from left to right, touching both the dissevered ends of the fence.

in which he indicated the lines most likely to be concerned.[2] Professor Branner stated in an interview that he had immediately made a forecast of the locality of the origin and that it had proved to be correct, and Mr. Fairbanks went at once to a zone of 'earthquake topography' with which he was already acquainted, and found a fresh rupture in the expected place.

[2] *Nat. Geog. Mag.*, Vol. 17, 1906, pp. 280–296.

The San Francisco earthquake was caused by a new slipping on the plane of an old fault which had been recognized for a long distance in California, and in one place had been named the San Andreas fault. Associated with this fault is a belt of peculiar topography, differing from the ordinary topographic expression of the country in that many of its features are directly due to dislocation, instead of being the product of erosion by rains and streams. One of its characteristics is the frequent occurrence of long lines of very straight cliffs. Another is the frequent occurrence of ponds or lakes in straight rows. The tendency of erosion is to break up such cliffs into series of spurs and valleys and to obliterate the lakes by cutting down their outlets or filling their basins with sediment. Fig. 2 shows one of the fault-made ponds. This line and zone have been recognized by California geologists through a distance of several hundred miles. It was to this line that attention and expectation were especially directed, and it was on this line that the surface evidence of new faulting was actually found. The new movement was not coextensive with the line as previously traced, but affected only the northwestern portion: and, on the other hand, it extended farther to the northwest and north than the old line had previously been recognized. The accompanying map represents only the line along which the recent change occurred. From a point a few miles southwest of Hollister it runs northwestward in a series of valleys between low mountain ridges to the Mussel Rock, ten miles south of the Golden Gate. Thence northwestward and northward it follows the general coast line, alternately traversing land and water. The farthest point as yet definitely located is at Point Delgada, but the intensity of the shock at the towns of Petrolia and Ferndale probably indicates the close proximity of the fault and warrants the statement that its full length is not less than three hundred miles. South of Point Arena its course is direct, with only gentle flexure, but the data farther north seem to imply either branching or strong inflexion. Opposite San Francisco its position is several miles west of the coast line, and it nowhere touches a large town.

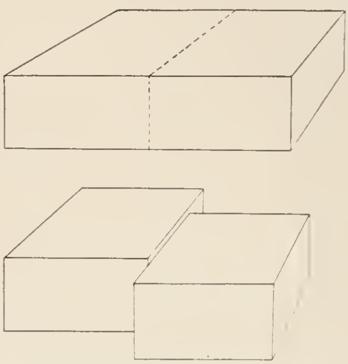


FIG. 4. DIAGRAMS ILLUSTRATING THE DISLOCATION CAUSING THE SAN FRANCISCO EARTHQUAKE. The upper represents an earth block 100 feet square and 25 feet thick, with indication of the position of the fracture. The lower shows the relation of its two parts after faulting.

That which occurred along this line was a differential movement and permanent displacement of the rock and earth on the two sides of a vertical crack. The principal displacement was not vertical, but horizontal. If one thinks of the land to the east of the crack as stationary, then the change may be described as a northward movement of the land west of the crack. If the land to the west be thought of as stationary then the land to the eastward moved toward the south. It is probable that both tracts shared in the movement, the eastern shifting toward the south and the western toward the north. Perhaps the nature of the change can be more readily understood by reference to Fig. 4, which represents an ideal block of the earth's crust, 100 feet square on the surface and 25 feet deep, before and after its division and dislocation by the earthquake-causing fault.

Wherever a fence, road, row of trees, or other artificial feature following a straight line was intersected by the fault its separated parts were offset, and an opportunity thus afforded for measuring the amount of change. The measurements range in the main from 6 to 15

Fig. 5. A Faulted Road near the Head of Tomales Bay. The nearer and more distant parts of the road were originally in one line—a continuous, straight road. The present offset is twenty feet.

feet and have an average of about 10 feet. At one place (Fig. 5) a road was offset 20 feet, but in this case the underlying ground was wet alluvium and part of its movement may have been due to a flowing of the soft material. There was also some vertical change, but this was not everywhere in the same direction and its amount was comparatively small. At many points the land west of the fault appears to have risen one or two feet as compared with the land at the east.

Fig. 6. Ordinary Appearance of the Earthquake Rift where it traverses Firm Turf.

The surface manifestation is not usually a simple crack, but a disturbed zone a few feet broad, the earth within the zone being split into blocks which show more or less twisting or rotation. In some places the zone is slightly depressed below the adjoining surfaces, and elsewhere slightly elevated. Other disturbances of the surface were associated with the earthquake, but the track of the central fault has a character of its own, a character with which the field workers soon became familiar, so that it could be clearly identified. It came to be distinguished in their conversation and note-books as 'the rift.' For considerable distances the rift is single, but elsewhere it is more or less divided, the parts lying within a few rods of one another and being approximately parallel. There are also branches parting from the main rift at various angles and gradually dying out in the adjacent country, and in some of these the belt of disturbance is broad and complicated (Fig. 7). There are also outlying cracks occurring within a mile or two of the central rift and having irregular courses, and these may probably be referred to the same general system of rock strains.

Other cracks are distinctly secondary in character; that is to say, they are not due directly to the stresses and strains by which the fault was made, but are results of the earthquake itself. The jar constituting the earthquake, or in technical language the earthquake wave, as it travels through rock and earth produces temporary compressions and other strains, and these often occasion cracks at the surface. Where the material is elastic such secondary cracks merely open and close, leaving the ground with its original form; but where it is inelastic and incoherent, as in the case of young alluvial formations and artificial fillings, some of the cracks opened by passing waves do not close again, but remain as permanent vestiges of the shock. Closely associated with these secondary cracks in soft ground are permanent changes in surface form. At the head of Tomales Bay, for example, a broad tract of soft ground between high and low tide was thrown into low ridges, with cracks along their crests, and these remained until destroyed by wind waves. In San Francisco considerable tracts of 'filled' land were shaken together and thus made to settle a few feet, and were at the same time slidden several feet toward the bay (Fig. 9).

Certain changes, very conspicuous to the observer who drove about the country, are closely associated with roads. A side-hill road is

Fig. 7. A Zone of Earthquake Fracture where it crosses a Road near Bolinas.

usually constructed by excavating a notch in the natural slope and piling the excavated material in an embankment at the outer edge of the notch. In course of time, and especially during rainy seasons, the embankment at the outer edge of such a road settles and has to be built up as a matter of repair. Portions of the bluff on the up-hill side of the notch are also apt to fall away, taking the form of small landslides, which have to be removed from the road as a rule after every rainy season. The earthquake precipitated many changes of this sort. Along all side-hill roads in the immediate vicinity of the rift a crack was developed between the embankment and the original soil against which it rested, and this crack often assumed formidable dimensions (Fig. 10); in fact its magnitude was found to be a convenient index of the local violence of the earthquake in regions where buildings are rare. Landslips from the bluffs margining the roads (Fig. 11) were also very numerous, in many instances stopping traffic until repairs could be made. And there were many landslides on a larger scale, the earthquake initiating movements which might otherwise have been delayed for years or even centuries. Some of these landslides fell into streams, dammed their waters and created temporary lakes.

FIG. 8. CRACKS CAUSED BY THE SHAKING OF MARSHY GROUND. The comparatively firm road embankment preserved the cracks better than the bog.

Other disturbances of water supply were more directly connected with the earthquake. At several points large volumes of water were squeezed from the ground during the agitation, causing brief but violent torrents, and one of these brought with it so much sand as to constitute a sort of sand eruption. There are reports also that certain springs have received a permanent increase in volume, a result which would naturally follow from the modification of underground circulation by the cracking of rock and earth.

Wherever the shock was specially strong there was considerable injury to trees; some were overturned, others broken near the ground, and yet others broken near their tops. A number of large redwood trees standing on the line of the rift were split from the ground upward, the basal portions being faulted along with the ground they stood on.

In the systematic survey of the earthquake area the relative intensity is being estimated by means of the records of various physical

FIG. 9. STREET SCENE IN SAN FRANCISCO, showing effect of the earthquake on filled ground. The distant p rt of the street probably retains its original level and position. Nearer by the ground has settled several feet and has also moved to the left.

effects. In the immediate vicinity of the fault road-cracks and cracks in alluvium are large and numerous; many trees were broken or overturned; there were many landslides; half of the wooden buildings of any village or hamlet were shifted horizontally, often with serious injury; buildings and chimneys of brick or stone were thrown down;

during the shock men, cows, and horses found it impossible to stand, and fell to the ground; and some persons were even thrown from their beds. In a general way all these evidences of violence diminish gradually with distance from the fault on either side. The rate of diminution, with exceptions to be mentioned presently, may be expressed by saying that at five miles from the fault only a few men and animals were shaken from their feet, only a few wooden houses were moved from their foundations, about half the brick chimneys remained sound and in condition for use, sound trees were not broken, and no cracks were opened which did not immediately close. At a distance of twenty miles only an occasional chimney was overturned, the walls of some brick buildings were cracked, and wooden buildings escaped without injury; the ground was not cracked, landslides were rare, and not all sleepers were wakened. At seventy-five miles the shock was observed by nearly all persons awake at the time, but there were no destructive effects; and at two hundred miles it was perceived by only a few persons.

FIG. 10. ROAD CRACK CAUSED BY THE EARTHQUAKE.

Fig. 11. Small Landslide on the Uphill Side of a Side-hill Road.

A number of exceptions to this gradation of intensity are connected with tracts of deep alluvial soil, especially if saturated with water, and with tracts of 'made ground.' The great destruction in the low-lying part of San Francisco, eight miles from the fault, is directly connected with the fact that much of the ground there is artificial, the area having been reclaimed from the bay by filling in with sand and other materials. The severity of the disaster at San José, twelve miles from the fault, has been ascribed to the deep alluvial soil on which the town stands, and many other local peculiarities seem to admit of the same explanation. It is necessary also to distinguish carefully between earthquake intensity and destructive effect, because injury to property was conditioned by mode and material of construction no less than by intensity of vibration. But after making due allowance for differences in natural foundation and for differences in the resisting power of buildings, there remain various anomalies for which satisfactory explanation has not as yet been found. The natural foundation of Oakland is similar to that of San José, and its distance from the earthquake

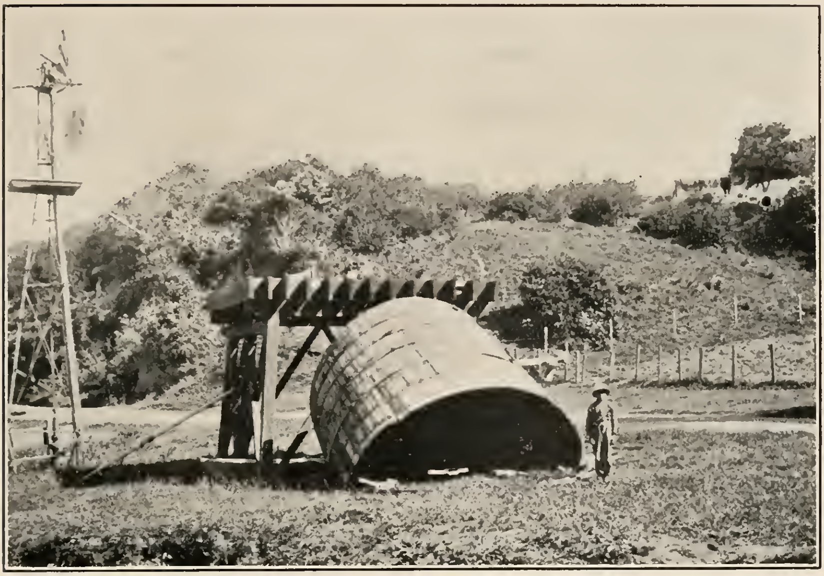


FIG. 12. WATER TANK THROWN FROM ITS PEDESTAL BY THE EARTHQUAKE.

FIG. 13. SCHOOL-HOUSE AT POINT REYES STATION, NEAR INVERNESS; shifted horizontally two and one-half feet by the earthquake. The corner here shown was slipped from the foundation and rests directly on the ground.

origin is about the same, but the injury to its buildings was decidedly less; and Santa Rosa, standing on ground apparently firmer than that at Oakland or San José and having a somewhat greater distance from the fault, was nevertheless shaken with extreme violence.

It is too early to discuss these anomalies. With the data now in hand it seems to be true that there are outlying tracts of high intensity surrounded by areas of relatively low intensity; and these features, if they shall be fully established, will doubtless affect in some important way the general theory of the earthquake.

One of the chief uses of time observations in connection with most earthquakes has been to determine the position of the origin. As the elastic wave travels outward in all directions from the initial point it reaches successively points on the earth's surface which are more and more remote. Coseismal lines, or lines of simultaneous arrival, are, therefore, closed curves circling about the region of the initial fracture. In the case of the San Francisco earthquake this particular function of the coseismals is not required, because the fracture is visible at the surface; but they are not therefore without value. It is not to be supposed that the yielding of the earth occurred at the same instant throughout the entire extent of the fault plane. We should assume, rather, that the fracture, beginning at some point, was extended thence to the remainder of the tract, a certain amount of time being consumed in its propagation. When the time data have been collected and studied, it may be possible to determine at what point the fracture began and at what rate it was extended. It is hoped also that when the time records and intensity records shall have been systematically discussed there may result some conclusion as to the depth to which the fault extended and also as to its subterranean form.

Mention has already been made of the question whether the permanent dislocation or change of absolute position involved in the faulting was divided between the tracts of land on the two sides or was confined to one or the other of them. At first sight it would appear that the only thing susceptible of actual determination is the relative displacement, and that the absolute displacement, or the real movement with reference to the earth as a whole, must remain a matter of theory only. Nevertheless, it happens that in this particular instance the real changes in geographic position are not only susceptible of determination, but are actually to be investigated. To illustrate the problem, let XY represent, in ground plan, a portion of the fault line, and let $ABB'C$ be the original position of a straight line intersected by the fault. Assuming for the moment that the dislocation was equal on the two sides of the fault, then the line AB was carried to the position DE, and the line $B'C$ to the position FG. We may think of the distances BE and $B'F$ as each equal to five feet. The dislocation of five feet pertains to every point near the fault line, but it is not supposable that the same dislocation affects points at a great distance from the fault. At some remote point, for example *Z*, in the direction *B′C*, there was no displacement. If *B′C* and *FG* were both produced in that direction they would be found not precisely parallel, but would eventually coalesce. How far the undisturbed region *Z* may be from the fault line is a matter of pure conjecture, but we may plausibly assume that the transverse dimension of the area affected by the displacement is of the same order of magnitude as the length of the fault line and is measured by hundreds of miles. If this assumption is correct, then throughout a great region in central and northern California all points have experienced a change in geographic position, the change in the vicinity of the fault being of about five feet and the amount diminishing toward the northeast and southwest. If the only determinations of latitude and longitude within this area were of the ordinary approximate character, it would be impossible to measure the changes in geographic position theoretically accomplished by the fault; but it fortunately happens that the region is traversed by two belts of the triangulation of the United States Coast and Geodetic Survey, one being a system of triangles for the control of the coastal map work, and the other the elaborately measured transcontinental belt. The region thus contains several scores of points whose coordinates have been determined with a high degree of precision, and it is possible by the redetermination of these positions to measure the dislocations which have taken place in connection with the earthquake. As all topographic and hydrographic maps of California are dependent for their latitudes and longitudes upon the positions given by this triangulation, and as there is reason to believe that many of these positions have been disturbed by a measurable amount, the superintendent of the Coast Survey has determined to repeat so much of the work of triangulation as may be necessary in order to redetermine the geographic positions. And it is proposed to carry this work far enough eastward to connect the redetermined points with stations that may safely be regarded as quite beyond the effect of the recent fault. When this has been accomplished much light will be thrown on the nature and distribution of the strains which were relieved by the dislocation along the fault line, and it will

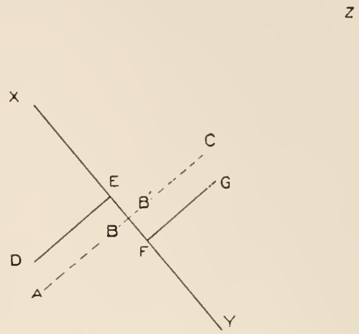


FIG. 14. DIAGRAMMATIC PLAN OF A PORTION OF THE EARTHQUAKE FAULT, illustrating changes in geographic position.

be possible to say definitely whether the original displacement involved the territory on both sides of the fault or on one side only.

A further check is to be afforded through the observations for astronomic latitude at Ukiah. The observatory at Ukiah is between 25 and 30 miles in a direct line northeast of the fault. In connection with the general dislocation it was presumably moved toward the southeast and its latitude diminished by several hundredths of a second. This is one of an international series of observatories established in approximately the same latitude but in different longitudes, for the purpose of determining variations in the position of the earth's axis of rotation. If the observations at Ukiah were studied alone it might not be possible to separate the result of a small change in the observatory's position from the effects of the migration of the axis; but by combining the Ukiah data with those furnished by the other observatories of the system, it is probable that the effects of the two causes can be discriminated.

The most important practical results of the various earthquake studies will probably be afforded by the engineers and architects, and will lead to the construction of safer buildings in all parts of the country specially liable to earthquakes; but the geologic studies of the State Commission are not devoid of economic bearings. In the city of San Francisco and adjacent parts of the peninsula on which it stands the underlying formations include several distinct types, and the district is so generally occupied by buildings that the relations of the several formations to earthquake injury can readily be studied. Such a study is being made with care and thoroughness, and one of its results will be a map of the city showing the relation of the isoseismals, or lines marking grades of intensity, to tracts of solid rock, to tracts of dune sand in its natural position, to upland hollows partially filled by grading, and to old swamps, lagoons and tidal marshes that have been converted into dry land by extensive artificial deposits. The information contained in such a map should guide the reconstruction and future expansion of the city, not by determining the avoidance of unfavorable sites, but by showing in what areas exceptional precautions are needed, and what areas demand only ordinary precautions.

Another economic subject to which the commission may be expected to give attention is what might be called the earthquake outlook. Must the citizens of San Francisco and the bay district face the danger of experiencing within a few generations a shock equal to or even greater than the one to which they have just been subjected? Or have they earned by their recent calamity a long immunity from violent disturbance? If these questions could be answered in an authoritative way, or if a forecast could be made with a fair degree of probability, much good might result; and even if nothing more shall be possible than a cautious discussion of the data. I believe such a discussion should be undertaken and published. Of snap judgments there has been no lack, and the California press has catered to a natural desire of the commercial public for an optimistic view; but no opinion has yet been fortified by an adequate statement of the pertinent facts. Among these facts are the distribution of earthquake shocks as to locality, time and severity in California, and also in the well-studied earthquake district of Japan; the relation of the slipping that has just occurred to the geologic structure of the coast region; the relation of other fault lines to the bay district; and the relation of the recent shock to a destructive shock that occurred in 1868. If a broad and candid review of the facts shall give warrant for a forecast of practical immunity, the deep-rooted anxiety of the community will find therein a measure of relief. If a forecast of immunity shall not be warranted, the public should have the benefit of that information, to the end that it shall fully heed the counsel of those who maintain that the new city should be earthquake-proof. In any case, timidity will cause some to remove from the shaken district and will deter others who were contemplating immigration; but such considerations have only temporary influence and can not check in an important way the growth of the city. The destiny of San Francisco depends on the capacity and security of its harbor, on the wealth of the country behind it, and on its geographic relation to the commerce of the Pacific. Whatever the earthquake danger may be, it is a thing to be dealt with on the ground by skillful engineering, not avoided by flight; and the proper basis for all protective measures is the fullest possible information as to the extent and character of the danger.

SEISMOGRAPH AND MAGNETOGRAPH RECORDS OF THE SAN FRANCISCO EARTHQUAKE, APRIL 18, 1906[1]

By Dr. L. A. BAUER

DIVISION OF TERRESTRIAL MAGNETISM, U. S. COAST AND GEODETIC SURVEY.

THE San Francisco earthquake was one of several large earthquakes recorded the world over since the beginning of this year. The writer's prime interest in it as a magnetician is in the record it left behind on the magnetographs at various magnetic observatories of the United States Coast and Geodetic Survey.

It has happened several times within the last few years that earthquakes have occurred in this country which were not recorded for one reason or another, on the existing seismographs, but were indicated by the record of certain magnetographs. The most notable instance was the New England earthquake of March 21, 1904, at about eight minutes after one o'clock in the morning, eastern time. Seismographs of the Milne type at Toronto, Canada, and Baltimore, Maryland, and of the Bosch-Omori type at the Weather Bureau, Washington, D. C., failed to give any record of this earthquake, which was appreciably felt throughout the New England States. The magnetograph at the observatory, Cheltenham, Maryland, sixteen miles southeast of Washington, gave a distinct record at $1^h\ 05^s$ to $1^h\ 17^m$ eastern time. So there have been a number of earthquakes recorded by the magnetograph at Baldwin, Kansas, which were felt in the middle states and reported in the papers. In fact, at this observatory, situated in a region where felt and unfelt local and regional earthquakes are comparatively frequent—note for example the many recent occurrences—more records of earthquakes are obtained on the magnetograph than at any of the other magnetic observatories.

This repeatedly authenticated fact made desirable a concurrent study of seismograph and magnetograph records and hence seismographs have been installed within the last two years at all of the magnetic observatories excepting at Baldwin, Kansas, which was omitted because of its probable early removal on account of the possibility of disturbing influences from electric-car lines. So it happens that the Coast and Geodetic Survey is able at present to contribute the principal portion of the accurate observational data of earthquakes obtained in this country. It was with the expectation that magnetic observatories would also be excellent stations for the installation of seismological instruments that the writer, while attending the Seventh International Geographic Congress at Berlin, 1899, as a delegate from the National Geographic Society, was made a member of the Provisional Committee of the International Seismological Association, just organized by the congress.

The instrumental seismological data referring to the recent San Francisco earthquake will be contributed from the following stations in Canada and the United States:

TABLE 1. LIST OF STATIONS AND INSTITUTIONS IN CANADA AND THE UNITED STATES CONTRIBUTING SEISMOLOGICAL DATA.

Station.	Institution.	Seismograph.	Magnetograph.
Toronto.	Meteorological Service.	Milne.	Adie.
Victoria.	" "	"	....
Albany.	State Geological Survey.	Bosch-Omori.	....
Baltimore.	Johns Hopkins University.	Milne.	....
Washington.	U. S. Weather Bureau.	Bosch-Omori.	....
Cheltenham.	U. S. Coast and Geodetic Survey.	" "	Adie and Eschenhagen.
Vieques.	" " " " "	" "	Eschenhagen.
Baldwin.	" " " " "	.........[1]	"
Sitka.	" " " " "	Bosch-Omori.	Eschenhagen.
Honolulu.	" " " " "	Milne.	"
Berkeley.	University of California.	Ewing.	....
Mt. Hamilton.	Lick Observatory, U. of C.	"	....
Manila.	Philippine Weather Bureau.	Vicentini.	Mascart.

The exceedingly sparse distribution of seismological stations in this country is made apparent by this list, there being none in the middle portion of the United States, where, as already stated, regional earthquakes are comparatively frequent. It is therefore fortunate in the study of the San Francisco earthquake that we may have recourse also to the data afforded by magnetographs, especially by those at Baldwin, Kansas, and Sitka, Alaska—the nearest magnetic observatories to the origin of the quake and situated, as will be seen from Table 3, at about the same distance from San Francisco. So also is it a fortunate circumstance that we have both magnetograph and seismograph data from the two magnetic observatories, Honolulu and Cheltenham, which are also practically equidistant from the origin.

Now a peculiar circumstance is that this earthquake, while giving a record on the seismograph at the Porto Rico Magnetic Observatory so large as not to be fully recorded, left no trace behind on a magnetograph of the very same pattern as at the other observatories. On the

[1] A Bosch-Omori seismograph procured for this observatory was temporarily installed at Baltimore by Professor H. F. Reid for a comparative study with his Milne seismograph.

other hand, the equally large earthquake of January 31, last, the origin of which was at sea off the west coast of Ecuador, besides recording itself on seismographs the world over was recorded on the magnetographs at Baldwin, Porto Rico and Cheltenham, but this time not at Honolulu. This seaquake was accompanied by a tidal wave twenty feet high which rushed in on the coast of Ecuador, causing great devastation; it set the Pacific Ocean in vibration, which according to the tide-gauge records of the Coast and Geodetic Survey at San Diego and Honolulu lasted for three days. The tidal wave, when it rushed in on the Hawaiian coasts, was several feet high, and the record of this quake of January 31, as recorded on the Milne seismograph at the Honolulu Magnetic Observatory, was among the largest since the installation of the instrument, September, 1903, and yet the delicately suspended magnets, as far as the magnetic records at this observatory would indicate, were not affected.

Why is it that an earthquake will at times be recorded by magnetic instruments and at other times leave no record? Or, to go back to the fundamental question, what do the magnetic instruments record—an actual mechanical effect due to the mechanical vibration of the point of support? If the observed effect is a purely mechanical one, then why is it that not every mechanical disturbance is recorded on the photographic records of the fluctuations of the magnetic needles? What is the characteristic of the mechanical vibration, the presence or absence of which in the earth movements is responsible for the presence or absence of the effect recorded by magnetic needles?

The solution of these questions may show the magnetograph to be a most useful adjunct to the present instrumental equipment for recording earth movements.

Is the possibility of any actual magnetic effect accompanying an earthquake entirely to be excluded? If so, in the case of the distant earthquakes, as seems probable, is the possibility also to be excluded for the less distant ones, or say for stations within a certain prescribed region about the origin of the quake? Are those cases where records are secured on magnetographs and not on seismographs to be attributed possibly to such a magnetic effect which has no influence on instruments responding merely to mechanical vibration? Or is it possible that the magnetograph is in certain cases a better micro-seismograph than the Milne or Bosch-Omori instruments used in this country?

We have thus some extremely interesting questions presented to us which, however they may be solved, will be a valuable contribution to our knowledge of earth movements. The possibility might also be mentioned that an approaching earthquake might through electric or magnetic effects give the first indication on magnetographs because of the much greater velocity of propagation of such effects than that of the mechanical vibrations. We know that a magnet subjected to strain undergoes changes in its magnetization and so the question arises whether the earth's magnetized rocks may not likewise give some indication of their state of strain during an earthquake by slight magnetic fluctuations. Or, an earthquake may be accompanied by a redistribution of the magnetic rocks or of the electric earth-currents known to exist, and thus give rise to a possible magnetic effect.

Enough has been said to show that a careful and exhaustive investigation of seismic effects recorded on magnetographs is certainly one that merits undertaking. The Department of Terrestrial Magnetism of the Carnegie Institution of Washington, in connection with the study of the magnetic effect recorded simultaneously over the entire globe coincident with the outbreak of Mt. Pelé, on May 8, 1902, is making a systematic study of the volcanic and seismic effects recorded by magnetic instruments with the cooperation of the Coast and Geodetic Survey and of the Canadian Meteorological Service. A paper by Mr. J. E. Burbank, published in Vol. X., p. 113, of the journal, *Terrestrial Magnetism*, brought the investigation up to the time of the installation of the seismographs at the Coast and Geodetic Survey Observatories two to three years ago; a second paper, to be published in the course of the year, will continue the research as based upon seismic and magnetic instruments in operation at the same observatory.

It had been noticed for some time that magnetic instruments responded to certain earthquakes, but the cases noted were of such a class as to convey the first impression at once that the effects recorded were mechanical ones. Milne in 1898 made quite an exhaustive investigation of this class of effects for the whole earth and covering the period from 1889 to 1897. He likewise found that these effects were not invariably recorded at every magnetic observatory. He considered the results inconclusive and deemed it necessary to await the time when both seismograph and magnetograph records could be had at the same place. A recent notable contribution to the subject based on magnetic records at one observatory, without, however, at the same time corresponding seismological data, has been made by Dr. Messerschmitt, in charge of the Munich Magnetic Observatory.

Previous magneticians, such as Eschenhagen, Wild and Liznar, had found that from a comparison of the effects recorded on magnetographs at various European observatories the effects, in certain notable earthquakes, progressed from station to station with the velocity of about three kilometers *i. e.* the rate of propagation of the long or surface seismic waves. This measurable difference in time between any two stations and its correspondence with the time interval required for the transmission of the surface waves was a very good indication that a purely mechanical effect had been recorded and not a distant magnetic effect, as the latter would have been observed simultaneously at all stations. Or, if it was a magnetic effect, then in each case it was due to one of the possible local causes enumerated brought into action upon the arrival of the mechanical disturbance at the particular station.

In our study, however, it has been possible to differentiate much more closely and at times to separate the effects on the magnetic records into the various phases—preliminary tremors and principal portions, etc.—in a manner analogous to usual treatment of the seismograph records. A notable instance was the destructive Guatemalan earthquake of April 19, 1902, which, as may be recalled, preceded the Antillean volcanic eruptions of that period. At that time there were no seismographs at the Coast and Geodetic Survey Magnetic Observatories; however, an inspection of the table below will show that with the records obtained on the magnetographs at Cheltenham, Baldwin, Sitka and Honolulu (the Porto Rico Observatory did not then exist), it is possible to study the seismic effects on them—even down to the preliminary tremors—equally as well as on the seismic records obtained at Baltimore, Toronto and Victoria. The earliest notice of this earthquake was received at Baldwin, the nearest station to the origin—Guatemala. Here then we have a notable case where the magnets were affected by even the preliminary seismic tremors, this being a different case from the European ones cited above, as these tremors travel with a velocity of about nine kilometers or more per second.

There have been many other similar instances and it has even occurred at times that the magnetic instruments have given a slightly

Table 2. Seismograph and Magnetograph Records of the Guatemala Earthquake, April 18, 1902.

Station.	Latitude North.	Longitude W. of Gr.	Greenwich Mean Time. Preliminary Tremors Begin.	Long Waves Begin.	Time of Maximum.	End of Effect.	Instrument.	Component or Magnetic Element.
	° ′	° ′	h m	h m	h m	h m		
Baltimore.	39 18	76 37	2 30.1	2 35.6	2 40.3	5 30	Milne seismograph.	N. W.
Cheltenham.	38 44	76 50	2 30.4	....	2 31.0	2 57	Eschenhagen magnetograph.	Hor. In.
"	" "	" "	2 30.4	....	2 31.6	2 55	" "	Decl'n.
"	" "	" "	2 33.2	2 36.7	2 40.5	2 59	Adie "	Hor. In.
"	" "	" "	2 29.0	2 35.1	2 48.4	3 00	" "	Vert. In.
"	" "	" "	2 29.0	2 40.7	2 42.3	2 58	" "	Decl'n.
Toronto.	43 40	79 23	2 30.5	2 35.5	2 38.0	5 24	Milne seismograph.	East.
Baldwin.	38 47	95 10	2 24.6	2 30.0	2 34.6	2 57	Eschenhagen magnetograph.	Hor. In.
"	" "	" "	2 29.1	2 31.2	2 34.8	2 53	" "	Decl'n.
Victoria.	48 27	123 22	2 31.3	2 37.2	2 50.7	5 36	Milne seismograph.	East.
Sitka.	57 03	135 20	2 40.4	....	2 59.6	3 05	Eschenhagen magnetograph.	Hor. In.
"	" "	" "	2 36.8(?)	....	....	2 49	" "	Decl'n.
Honolulu.	21 19	158 04	2 31.3	....	....	2 39	" "	Hor. In.
"	" "	" "	2 31.8	....	....	2 35	" "	Decl'n.

earlier record than the seismograph. The effect is such a characteristic one that when it has once been recognized it will not be mistaken for any ordinary magnetic effect. Four types can be distinguished: First, those in which the disturbance begins abruptly and quickly reaches a maximum, dying down gradually (usually the case in a near-by earthquake); second, those in which a small preliminary effect precedes the principal portion, and in which there are often two or more maxima; third—by far the most common—those consisting of a small diamond-shaped disturbance; and fourth, those in which the trace is simply blurred and broadened.

With these introductory statements as to the possible relation between seismology and terrestrial magnetism, let us now pass to the consideration of the recent San Francisco earthquake.

Table 3 contains the results of the records obtained up to date (May 17) at the office of the Coast and Geodetic Survey, both from magnetographs and from seismographs. It will be seen that the region embraced extends from Honolulu on the west to Hungary on the east, or about one third the way around the globe. All necessary data, such as latitude, longitude, distances from San Francisco along the surface, as well as along chord, chord depth, etc., etc., are found in the table.

Next are given the Greenwich mean times (0 to 24 hours, midnight to midnight) of the occurrence of the various phases of the seismic disturbance as recorded on the seismographs. For the preliminary tremors, phase I (longitudinal waves), next phase II (transverse waves), then principal portion (surface waves), etc., etc. It is particularly interesting to compare the times for Cheltenham, Washington and Baltimore and to note how closely they agree. Owing to the slight difference in distance of these three different stations from San Francisco the times should not of course be strictly the same, though the difference should not be more than a few seconds. Considering the totally different types of instrument (Milne at Baltimore and Bosch-Omori at Washington and Cheltenham), certainly the comparison is very satisfactory.

It will be seen that the preliminary tremors were recorded by the seismographs at Honolulu and Cheltenham at about the same time, these two stations being at about the same distance from San Francisco. The reader will follow without assistance the progression of the various waves from station to station as given in the table. [Since this table was prepared many more records have been obtained which are of interest, notably the seismograph and the magnetograph records from the Toronto Magnetic Observatory.]

In Fig. 1 we have a reproduction of the seismograph record obtained at the Cheltenham Magnetic Observatory. The recording cylinder of the Bosch-Omori seismograph, around which is wrapped the

San Francisco Earthquake, April 18, 1906. Results of Seismograph and Magnetograph Records.

Place—Country.	Honolulu, T. H.	Sitka, Alaska.	Baldwin, Kansas.	Chelten-ham, Maryland.	Vieques, Porto Rico.	Birming-ham, England.	Göttingen, Germany.	Ogyalla Hungary.	Zagreb Croatia.	Washing-ton, D. C.	Baltimore, Maryland.
Latitude (North)........	21°19′ N.	57°03′N.	38°47′N.	38°44′N.	18°09′N.	52°30′N.	51°32′N.	47°52′N.	45°49′N.	38°54′N.	39°18′N.
Longitude from Greenwich	153 04 W.	135 20 W.	95 10 W.	76 50 W.	65 26 W.	1 54 W.	9 57 E.	18 52 E.	15 59 E.	77 03 W.	76 37 W.
Great Cir. Distance in Degrees..	34.8	21.1	21.3	35.4	53.1	76.0	81.4	87.6	88.4		
" " " " Miles.....	2407	1455	1473	2447	3670	5251	5627	6056	6109		
" " " " Kiloms..	3874	2342	2371	3938	5906	8451	9056	9746	9831		
Chord Distance in Miles..........	2371	1447	1465	2409	3541	4876	5167	5484	5522		
" " " " Kiloms........	3816	2329	2358	3877	5699	7847	8315	8826	8887		
Greatest Chord Depth in Miles...	181	67	68	188	418	839	959	1103	1121		
" " " " Kiloms	291	108	109	303	673	1350	1543	1775	1804		
Earthquake (Seismograph)	Greenwich Mean Time 0–24 hours. First shock at San Francisco 13h 12m Gr. M.T.										
Preliminary Tremors I..........	13h 19.5m s	13h 17m 01s	h m s	13h 19m 24s	13h 22m 17s	13h 25m 03s	13h 24m 32s	13h 25m 20s	13h 25m 25s	13h 19m 20s	13h 19m 24s
" " II..........	13 25.8 ?	13 21 01?	....	13 25 04	13 29 55	13 35 07	13 34 42	13 36 08	13 35 32	13 25	13 25 12
Principal Portion; Begins.......	13 27.9	13 23 ?	....	13 30 13	13 41 29	13 45 13	13 51	13 53 38	13 53 16	13 30	13 31 36
" " Max..........	13 32.9	13 25 off	....	13 35 ?	13 50	....	13 53 }	14 00	14 01	13 32 } off	13
" "	....	off	....	....	....	....	13 57 }	14 06		13 35 }	
" " Ends....	....	....	....	13 44							
End of First Shock..............	....	16 55	....	16 35 N. 17 07 E.	16 12 N. 16 27 E.	15 29	19	....	....	17 35	
Earthquake (Magnetograph)											
Begins.......	13 27.8	13 22.9	13 24	13 30	} None						
Ends.........	13 37(D) 13 42(H) 15 07(Z)	13 31.9	13 31	14 04							
	Velocity of Seismic Waves in Miles or Kilometers per Second.										
Seismograph. Long. Waves, P.T. I...	5.3 miles	4.8	....	5.4	5.7	6.2	6.9	6.8	6.9	} along chord.	
	8.5 kms.	7.7	....	8.7	9.2	10.0	11.1	11.0	11.0		
Seismograph. Trans. Waves, P.T. II...	2.9 miles	2.7	....	3.1	3.3	3.7	4.0	4.0	4.1	} " medium path.	
	4.6 kms.	4.4	....	5.0	5.4	5.9	6.3	6.4	6.6		
Seismograph. Surface Waves, P.P.......	2.5 miles	2.2	....	2.2	2.1	2.6	2.4	2.4	2.5	} " surface.	
	4.1 kms.	3.5	....	3.6	3.3	4.2	3.9	3.9	3.6		
Magneto-graph. Surface Waves, P.P.......	2.5 miles	2.2	2.5	2.2							
	4.1 kms.	3.5	3.9	3.6							

smoked sheet of paper on which the record is made, makes one complete revolution in an hour, each sheet whether of the N.–S. or of the E.–W. component containing a whole day's record (24 lines). The distance between two of the dots represents one minute. In order to get a convenient size for the figure, it was necessary to omit about one third of the total length of the sheet, so that the distance from S. to S. or E. to E. represents about two thirds of an hour. On the original, the pointer or the recording stylus multiplies the motion ten times, hence in the reproduction the magnification is about three times. The maximum amplitude of motion was not recorded, the pointer striking the damping brushes. In deducing the actual displacement of an earth-particle at Cheltenham, it is necessary to take into account the period of the pendulum which for the N.–S. component was about 25 seconds and for the E.–W. component about 20 seconds and the period of the recorded earth-movement about 2 to 4 seconds for the preliminary tremors and about 10 to 20 seconds for the principal portion. A rough calculation would give the total recorded horizontal displacement of the earth-particle, back and forth, of about 1/5 of an inch, which on account of the comparatively long period would not be felt by the human being. These explanations will doubtless be sufficient for the elucidation of the figure; for a description of the instrument the reader is referred to Dutton: 'Earthquakes.'

Passing next to the times recorded by the magnetograph (*D* stands for declination, *H* for horizontal intensity and *Z* for vertical intensity), it is seen that the effect in this instance did not begin at the four observatories where a record was obtained—Honolulu, Sitka, Baldwin and Cheltenham—until the arrival of the principal portion (long or surface waves) recorded on the seismographs, and that for this phase the agreement between seismograph and magnetograph is most satisfactory. It will also be noted that the time at Baldwin is intermediate between San Francisco and Cheltenham, so that the record of this observatory is a most desirable acquisition. Note also that the time is nearly the same as at Sitka, Baldwin being just a trifle farther from San Francisco than Sitka.

Next are found in the table the velocities of the various transverse waves—longitudinal, transverse and surface—computed along the paths indicated in the column on the extreme right. For the region embraced it will be seen that the longitudinal waves, which were the first to arrive, traveled at an average velocity of six miles per second, the transverse waves at an average velocity of 3½ miles, whereas the surface waves had a velocity of about 2 1/3 miles per second according to seismograph and magnetograph. It takes about 3 hours and 20 minutes for these waves to pass around the earth completely, whereas the preliminary tremors, phase I (longitudinal waves) reach

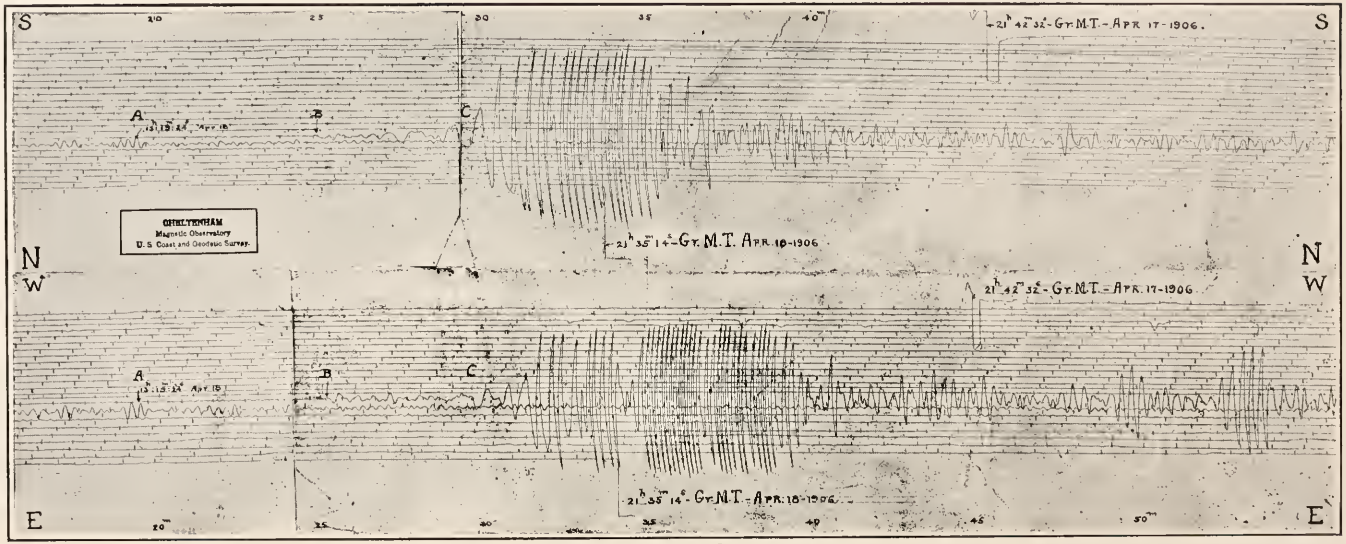


Fig. 1. Record of the San Francisco Earthquake, as obtained on the Bosch-Omori Seismograph at the Magnetic Observatory, Cheltenham, Maryland, reduced three and one-half times. [The records of the N-S component and of the E-W component were re-arranged so as to bring the points of beginning of the preliminary tremors, phase I (A) in the same vertical line. B marks the beginning of the preliminary tremors, phase II and C the beginning of the long or surface waves.]

a point on the opposite side of the earth from the origin in about 20 minutes; the latter are supposed to pass directly through the earth. In computing the velocities I have taken provisionally as the average

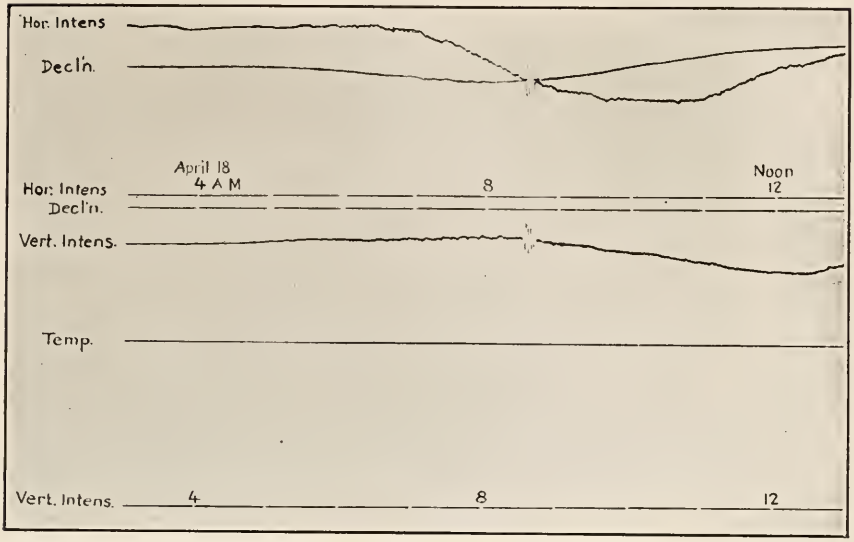


FIG. 2. RECORD OF THE SAN FRANCISCO EARTHQUAKE ON THE ESCHENHAGEN MAGNETOGRAPH at the Magnetic Observatory, Cheltenham, Maryland, reduced 2 times. [The hours as marked are approximately local mean time. The earthquake effect will be noti ed on the three magnetic elements, horizontal intensity, declination and vertical intensity in the shape of a trumpet formation between 8 and 9 a. m., local mean time. The range or double amplitude of the disturbance was about 1/1000 part of the horizontal intensity and about 1/3000 part of the vertical intensity. On account of the intersecting of the curves, the range in the magnetic declination cannot be given.]

time of the shocks on the Pacific Coast which gave rise to the effects recorded at distant places as occurring at 5^h 12^m Pacific time or 13^h 12^m, April 18, Greenwich time. There may have been earlier preliminary shocks.[2]

[2] *Professor George Davidson*, of the University of California, determined the time of first shock at his home in San Francisco by counting the number of seconds it took him upon awakening and going to his watch and noting the time. Owing to his large experience in the work of the U. S. Coast and Geodetic Survey, the time which he gives he deems to be correct within two seconds, *viz.*, 5: 12: 00 Pacific time.

Professor A. O. Leuschner, of the University of California, according to his article in the *Berkeley Reporter*, of Berkeley, Cal., April 20, 1906, says: "The best record of the beginning of the heaviest shocks is furnished by the standard clock of the Student Observatory, which stopped at 5^h 12^m 38^s Pacific standard time, while less severe shocks were recorded by Mr. S. Albrecht some 35 seconds earlier. The principal part of the earthquake came in two sections, the first series of vibrations lasting about 40 seconds. The vibration diminished considerably during the following 10 seconds and then continued with renewed vigor for about 25 seconds more. But even at this writing, about noon, the disturbance has not as yet subsided, as slight shocks are being recorded at frequent intervals on the Ewing seismograph, which has been restored to working order. [This seismograph was thrown out of action at the beginning of the earthquake; however, a fairly complete record was obtained with the duplex instrument.] The principal direction of motion was from south-southeast to

The amplification of this table to embrace the entire earth will be left to Professor Harry F. Reid, a member of the San Francisco Earthquake Commission.

Why is it that in this severe earthquake the magnets responded only to the long or surface waves and not to the preliminary tremors, and why did the magnets at Porto Rico give no record at all? These are the questions which I believe to be of concern not alone to the magnetician, but also to the seismologist and to the student of geophysics in general. Of the many earthquake records already obtained, there are a large number where the disturbance on the seismograph was considerably smaller than the San Francisco one and yet the magnetograph responded to even the preliminary effects. Evidently we must be getting a record of something on the magnetograph, not immediately evident from the present seismograph records, which causes this peculiar differentiation of seismic disturbances into the following classes: (*a*) recorded by seismograph and not by the most delicate magnetograph, (*b*) recorded by magnetograph and not recorded by seismograph, (*c*) recorded by seismographs and magnetographs partially (surface waves), (*d*) recorded completely by seismographs and magnetographs.

My present belief is that the effects recorded by suspended magnets are chiefly mechanical ones due to the vibrating motion of the points of support, though the possibility of a magnetic effect within a certain prescribed region of the origin of the earthquake, brought about as above explained, is not to be excluded. It is a notable fact that at the Baldwin Magnetic Observatory, where, as stated, so many seismic effects are being detected which are to be associated with the comparatively local earthquakes in the Middle States and which fail to make any record on seismographs as far distant as Washington, the effects corresponding in time to lightning discharges have also been found which in many instances resemble very closely the seismic effects.

In the case of the San Francisco earthquake, however, there can apparently be no question that what was recorded by the magnetographs was a mechanical effect (see Fig. 2). It is a matter of interest to note

north-northwest. The remarkable feature of this earthquake aside from its intensity was its rotary motion. The sum total of all displacements represents a very regular ellipse and some of the lines representing the earth's motion can be traced along the whole circumference." From this we deduce the time of the first shock 5:12:03.

At the Lick Observatory the first shock was recorded at 5:12:12.

At the Ukiah Latitude Observatory the first shock was recorded according to Dr. Townley at 5:13, correct within two or three seconds.

At Eureka, California, the first shock as reported to Professor Davidson was noted on a regulator owned by H. H. Buhne, who was awake at the time at 5:11.

As it is likely that the epicenter was somewhat west of San Francisco, but at no considerable distance, owing to absence of tidal waves, it is probable that the average time of the shocks at the origin which produced the records at the distant observatories was not far from $13^h 12^m$, Greenwich mean time, which is at present adopted. L. A. B.

that at Baldwin there was a slight actual magnetic effect 3–4 hours before the shock was felt at San Francisco, to which no counterpart has yet been found on the Cheltenham records, indicating that this effect observed at Baldwin was not a cosmic one, but was due to some local circumstances. To associate it with the San Francisco earthquake is not at present warranted.

Owing to the optical arrangement of the magnetograph, in order to produce an effect which will be evident on the recording sheet, it is necessary to have a *turning* movement of the suspended magnet. Any parallel displacement of the magnet—sidewise or up and down—will give no observable effect, an actual turning or rotary movement of the magnet must take place and for this purpose a turning couple must in some manner be introduced. Such a couple is produced when the magnet is drawn out of its normal direction with the aid of a bit of iron which is then quickly removed; the earth immediately acts on the magnet with equal and opposite forces applied near the extremities, and after performing a number of vibrations about its mean position the magnet settles down and then takes up the course pursued before the artificial disturbance. The effect thus produced is very similar to some of the earthquake effects. Were an earthquake accompanied by the generation of magnetic forces, the explanation of the observed effects would thus be very simple.

When the seismic motion is such as to produce a tilting or rocking of the support, it can readily be shown that because the suspended mass is a magnet, a turning couple is brought into play by the earth's magnetism causing the rotary, vibratory motion of the magnet about its mean position. Were the suspended material a non-magnetic mass of sufficient weight, no such turning would take place, but the mass would act more or less as a 'steady point.' However, it is quite possible that with the very light magnets weighing but ½ gram, and short suspensions used, we may also have to deal with a form of pendulum seismograph, in which the period of the pendulum is sufficiently small as to more readily respond to certain micro-seismic motions than either type of instrument at present in use in this country.

It would seem therefore that seismologists might be assisted in the solution of some of the problems as to the precise character of the earth movements recorded on seismographs by a careful study of the seismic effects recorded on magnetographs, especially if the effects both in the horizontal and in the vertical plane be considered, and if furthermore the record be obtained on a more open time scale, so as to be comparable in this respect with the best seismograms.

Whether the San Francisco earthquake caused a change in the distribution of the earth's magnetism within the affected region is at present under investigation.

REMINISCENCES OF YUKON EXPLORATION, 1865–1868

By WILLIAM HEALEY DALL
Smithsonian Institution.

OF the human flood which poured over the Chilkoot crest and inundated the drainage basin of the Yukon in the last years of the nineteenth century, Elizabeth Robins, Joaquin Miller and Jack London have given lurid pictures. The thirst for gold drew miners from every western camp, gamblers from every slum, dreamers from three continents, and human parasites from the whole round world. Ignorant of the climatic conditions, unprepared for the vicissitudes of life in the north, often burdened with preposterous machinery, unsuitable equipment and impossible loads—this motley horde invaded the Yukon territory in quest of fortune. With thousands exhaustion, exposure, disappointment, fear and panic dealt harshly in the end.

The interplay of human passions among those stripped thus of every conventionality offered an unrivaled opportunity to the observer. Greed, fear, suspicion, cruelty and selfishness revealed themselves, on occasion, as vividly as did the contrasted courage, kindliness, self-denial and heroic endurance of the nobler souls. On the just and the unjust, the strong and the weak, the coward and the courageous, indiscriminately, played the natural forces. The soft, white, clogging snow, the stinging cold, the searching wind, the claims of appetite—none might forego.

What with the fight for mere existence, the struggle for a paying location, the fitful gleaming of hope, fear, realization and disappointment, few in all that seething multitude may have had eyes for the beauty, the solemnity, the poetry of that wild north land. For most of them memory would picture the weary monotony of the trail, the buffeting of wind and snow, the penetrating rigor of the cold. These things so bit into their experience that all other impressions would seem trivial. Upon these factors fiction and romance would lean for local color, until, in the course of years, they would become to the average man essentially typical of the Yukon country. Under these conditions it seems possibly worth while, for one of the few who visited the Yukon region in its virgin prime, to put on record some of the impressions which it left upon his memory.

Like most great waterways, the Yukon itself has carved its kingdom out from the rude husk of mother earth. Before man was, its waters flowed in much their present channels. At times their flood was mighty, comparable only with such streams as Amazon and Nilus. Once its waters reflected the foliage of oak and plane-tree; the fig and the tulip tree flourished on its banks and the heights beyond were dark with forests of Sequoia. Later, its soft alluvial flats trembled under the ponderous tread of the hairy mammoth, while the wild horse grazed upon its verdant savannas. The bison knew its prairieland and the mazama its foothills. With the wane of the Age of Ice the musk-ox sought pasturage upon the Yukon tundra.

Strangely enough, during the height of the great Ice Age when the northeastern part of the continent as well as southern Alaska were buried deep under a continental ice-sheet, the greater part of the Yukon basin remained open to the sun. The traces of the glaciers are plain to see, about its head waters, on the Alaskan mountains to the south and the Yukon mountains to the north, but the terminal moraines are there to show where the deadly creeping of the ice was stayed, far above the present valley. During this time, perhaps, the muddy torrents bore to the river and the sea the alluvium which now composes the vast delta of the Yukon and the submarine flats, covering thousands of square miles, which are the characteristic feature of the eastern half of Bering Sea. With the shrinking of the glacier-sheet vast floods of water were let loose upon the alluvium of the lowlands, gradually shaping the features of the valley, concentrating the metallic contents of the gravels, and hurrying seaward the 'mountain meal' or impalpable white silt of the glacial grist.

From the volcanic craters of the mountain ranges to the south and west fine white ashes on one occasion poured in such volume as to cover the ground with a fleecy blanket, several feet thick, for many hundred square miles. Though covered by later accretions, this continuous layer of white ashes may still be traced for many leagues along the steep bluffs of the right bank of the river where it is under-cut by the current.

As the glaciers receded, the water supply became less profuse, the river settled between its banks, while the flats and prairies were invaded by willow and poplar, birch and spruce. The flora of the north, delicate and abundant, spread over the land, followed by the bee and the butterfly. Singing birds found nesting places, and with them all the small wild things which populate the wilderness, to gather sustenance from seed or berry, or seek refuge from the fox or hawk. And so at last the valley lay complete as first we knew it.

A brave domain, well defended, stretching some two thousand miles. On the north broad tundras hardly divided by low hills from the inviolate Arctic floes where they push upon the low sandy coast. To the northwest a turmoil of mountains, with hardly any game, kept off the explorer; while to the west, before the flatlands of the delta, lay many miles of mudflats and shifting sandbars, with no landmarks to indicate the channels of which only the salmon knew the secret. No invader, seeking a fairway for his vessel, might find comfort here. On the south the mighty rock walls of the Alaskan and St. Elias ranges, bristling with splintered crags, between which lurk the unconquered remnants of the Glacier Age, confront the would-be intruder. Lastly, on the east, mountains alternate with morasses for hundreds of miles; with streams unnavigable even by canoes, except at the price of hourly portages; tamarack thickets too dense to traverse, standing in bogs too soft to afford foothold, and so populated by black flies and mosquitoes as to be abandoned in summer by all the larger animals. Here a little band of Hudson Bay voyageurs, bent on reaching the great unknown river, some sixty-six years ago, were driven, through desperate starvation, to the last imaginable horror. Not till MacMurray flanked them by descending the Mackenzie far beyond the Arctic circle and forcing the Rat River portage to the waters of the Porcupine, were the eastern defenses of the valley carried by an explorer.

Even then, a quarter of a century should pass before the white man from the east met his fellow from the west, under the Arctic circle, at Fort Yukon, and the whole long river should know the stroke of their paddles and smoke of their camp-fires.

When the whites came they but followed on the trails of the Indian, whose far progenitors, lost in the mists of time, had penetrated to the valley, retreating, as legends tell, from massacre on the south at the hands of stronger tribes; or from starvation on the north, where, beyond the flats of frozen mud, lay only the barren floe. To them the Yukon gave of her caribou and salmon, and among her clustered spruce trees they found a safe refuge. There they prospered and begat other generations, who in the fulness of time came to call themselves Yukonikatana, Men of the Yukon. The ancient feud between Indian and Eskimo kept them from the coasts. Thus in a very emphatic sense the valley of the Yukon was their world.

To enter into the Yukon Valley one must scale its watershed or advance by the stream itself through the delta. The former was more difficult, the latter longer and more monotonous. Creeping along the coast in shallow water, one came finally to a branch where a loaded sloop might enter, and, by hard pulling against the current, finally gain the main channel. After leaving the sea one rowed between steep banks, hour after hour, the traveler seeing nothing but muddy water and scattered driftwood. If, in desperation, one scrambled to the level of the land, one saw on every hand an apparently illimitable plain, broken only far to the southeast by a single summit, the isolated peak of Kusilvak Mountain, blue in the distance.

Over the level surface lay scattered the worn and shattered trunks of driftwood fibrous with grinding in the ice of the spring freshet and stranded by the falling waters. Here and there were small patches of herbage growing rankly in the day-long sunlight of the boreal springtime. Everywhere rose the harsh cries of water fowl, hovering over their shallow nests hollowed in the warm sand. Ducks, geese and all the smaller waders, with here and there a sand-hill crane or snowy swan, all busy in the brief domesticity of spring, thronged the flats, covered the pools, or rose in dark extended myriads, as far as the eye can see. Violent cries and flapping wings called attention to some disreputable looking fox, with the rags of his winter coat still hanging to him, prowling in search of eggs of nestlings, but valiantly faced by the mother birds with loud vociferation. Now and then the great Arctic hare, looking as big as a deer in the absence of objects of comparison, lopes silently and swiftly between the tufts of succulent herbage; or a great black raven croaks hoarsely overhead, watching his chance to snap up a downy duckling in the absence of its defenders. The sun, low in the heavens, sheds genial warmth over the noisy congregation, and rich green patches of *Mertensia,* or forget-me-not, open a profusion of blue petals, basking in the radiance. Dotted over the sands little yellow poppies stand singly, spreading silky corollas over their slender densely hispid stems. A profusion of Saxifrage, Potentilla, sedge and Claytonia is found on every hand, except where the latest freshets have been scouring. Steadily between its low steep banks flows the turbid river, dividing into many channels most of which, when the floods are over, become dry.

After days of laborious tracking or rowing the main river may be reached. This for hundreds of miles flows steadily, with its current mainly hugging the right bank. This, if there be any high land about, is high, facing the stream with bold bluffs, which are gradually eaten away at the base by the gnawing current. At intervals a vertical slice of the bluff cracks, quivers and plunges into the water, carrying with it undergrowth and trees, which may remain as dangers to navigation or join the fleet of arboreal derelicts steadily moving toward the sea.

The left bank is usually low, with perhaps a blue line of distant hills dimly visible. Islands in the lower river are not numerous, though many sand-bars come to light at stages of low water. The scour of the river in spate is not favorable to permanent islands.

Ascending the river to the very center of the Alaskan territory, its width is suddenly contracted, its rate of flow increased, while high on either hand the banks rise steep and mountainous. This cañon received from the Hudson Bay men the picturesque name of 'the Ramparts.' Between the June water level and that of July, at the lower end of the cañon, there is a difference of seventy feet, and the maximum is even greater.

About one hundred miles of the river are comprised within the Ramparts, which do not rise as rocky walls, but rather as steep sparsely wooded slopes, formerly beloved of the mountain sheep. Above the Ramparts the river spreads out upon a wide alluvial plain, dividing itself amongst innumerable islands.

Just below the lower end of the cañon enters from the south the Tanana, River of the Mountain Men, a noble tributary. Here lay Nuklukahyet the neutral trading ground for many years.

On the border of Alaska, just above the Arctic Circle, enters the Porcupine River from the northeast, the channel by which MacMurray won his way into the Yukon valley in the early forties. Here the great river bends to the southeast, enters British territory, and carries its navigable waters further nearly five hundred miles. In this stretch its hitherto pellucid waters receive the milky flow of the White River, glacier-fed, which tinges their flood henceforward, to the sea.

The Yukon is the highway of all this land. When the frosts of October lock the streamlets and choke the outlets of the mountain springs, the wide stream is quickly ice-bound. At some points where the swifter current ripples, open water still remains, giving out feathery streaks of mist to the crisp air.

Migratory fishes hurry to the sea. Already the water fowl have departed. The first snow lies feathery soft amongst the seedling willows on the sandbars. The broad sheets of ice on either shore glisten in the enfeebled sunlight, and as the river falls, they sink, creaking and crashing until the early ice of the shallows lies unevenly on the gravelly river-bed. The turbidity of summer lessens and the current flows steely-dark along the open spaces. Sharper grows the cold; the heavy sun relinquishes more and more of its meridian arc. The skies turn gray, and presently comes the snow, steady, silent, soft, incessant, clothing the world.

Deep under the fleecy blanket nestle the little green herbs. The field mouse tunnels the drifts where he may roam unseen and nibble the sweet bark of the young birches. Stately, silent, vigorous, the ptarmigan cock treads pathways amongst the willows, in snow no whiter than his plumage. Here the admiring flock may pluck the spicy buds to their content, heedless of the fowler's snare, and hardly disturbed when the lesser hare, like them snow white, avails himself of their convenient runway.

The red squirrel chirrups in the branches of the spruce, nipping off the loaded cones. Around him chirp the winter redbirds, cheeriest of residents, while in a neighboring poplar the raucous voice of the whisky-jack declares that the world owes him a living. The yellow-headed woodpecker hammers busily away on some decaying alder and from the steep bluff among the rocks comes with solemn repetition the hoarse cry of the raven.

As the sun sinks early to the horizon the owls call to one another and issue from their retreats, whirring softly among the loaded branches. The squirrels are safe in their holes, but let the incautious snowbird beware, lest he be snatched incontinently from his perch. Snake-like the mink in his dark glistening coat winds among the willows by the waterside, on murder bent. The petulant bark of the dogfox is hushed as he too moves with stealthy tread in search of prey. The stars come out, the shadows blacken, hunters and hunted alike are still. Save for the musical twang of splitting ice, now and then, along the river, a measureless silence descends upon the world. As the cold strengthens, in the northern heavens the pale aurora lifts its quivering arch.

The extreme cold is felt always in still weather. As the wind rises, so does the temperature. When sixty-eight below zero of Fahrenheit implies a calm, a rise of thirty-eight degrees is probable as the wind rises. While it does not often snow at this temperature, the wind may carry so much fine loose already-fallen snow along the surface of the open tundra or the river that it has the effect of a blinding snow-storm, against which nor man nor beast may stand. This is the dreaded 'poorga' of the Russian, the 'blizzard' of the western prairies. Here the ignorant gold seeker, ill-clothed, ill-shod, wearing himself out by vain efforts to withstand the forces of nature, often meets his fate. But who has heard of a Yukon Indian perishing in a poorga? The man of the Yukon had adapted his dress, his snowshoes, his tools, his movements, to his surroundings. Like the beasts of the valley, whose skins he wore, he knew how to seek or build a shelter which would shield him from the blast and keep him safe, even if uncomfortable, until the elements wearied of their rage. The humming of the wind in the swaying spruces, the rattle of flying bits of ice or dead branches blown over the crusted snow, the complaining cry of the hawk-owl as his hollow tree quivered under the gusts, all told of the progress of the storm to him brought up to listen to and understand the voices of the wilderness.

And when at last the storm had spent itself, the traveler came forth from his temporary shelter to beat the snow crystals from his garments and look upon a world swept clean of litter, sparkling white under the winter sunbeams. The grouse from her tunnel in the snowdrift, the squirrel from his hollow log, the snowbirds from their retreats beneath the half-buried branches of the spruce, all issued forth upon their daily sustenance intent. The world was a good world, after all, and the singing gale merely a break in its monotony.

Where the tenderfoot, untrained, undisciplined and terrified, found only a demoniac nature striving to overwhelm a shivering victim, those to the manner born might feel a power, a majesty, an unswerving flight, as of the passing of a messenger of God.

As the days grow longer, while the trails harden, the deep snow settles, bearing a solid crust. There is a mildness in the air and in sheltered places little pools form on the ice about midday. The poplar buds are swelling. The raven's nest among the rocks of the bluff is no longer empty. The river ice whitens though it does not yield. Presently the cry of the wild goose is heard, the flocks are returning from the south. The ptarmigan forsakes the willow thickets, and the hare retreats from the edge of the river. They know what the creaking of the ice portends. The native fish-traps in the channel are dismantled, the snow on the beaches disappears. The river ice settles closer to the sand-bars; there is slush on the trail to the water-hole. The little brooks begin to trickle, and form pools where the grayling makes his retreat from the main river. The smaller migrant birds begin to appear, the wheatear, the American robin and a host of others, with phalaropes and sandpipers. The harlequin ducks arrive in pairs, silently making their way up the smaller streams, seeking secluded spots for nesting. The first mosquitoes appear, advance guard of the multitudinous pests of summer.

As the streams increase in volume the river rises, the ice becomes rotten and is lifted from the sand-bars; man and beast seem to wait breathless for the ice to go out. The sun pours down with a fervor not soon forgotten, though in the shade it is always cool.

The cry of the brant, northward bound, is continually heard, and myriads of smaller water fowl appear on every hand. All the minor forms of life, native to the region or migrants from the south, with startling suddenness people the copses and pervade the air. Vegetation springs into leaf and flower at a bound. The water creeps up on the beaches, the ice is shaken by tremors often accompanied by a groaning sound.

The tributary streams begin to run bank-high and flood the surface of the river ice; at last the crisis comes with the upriver rise. The ice breaks, great cakes are driven high upon the beaches or jam in the narrower channels between islands; at last it floods the lowlands; ice, débris, and driftwood pour, with a grinding noise, headlong toward the sea. Below the Ramparts at least a week goes by before the river is free enough from floating ice and broken timber to be navigable, even for canoes. With hardly a hint of spring, summer is upon the valley. Mosquitoes appear in clouds. Except in midstream or where a brisk air is blowing, life without a net and leather gloves is misery. The Indians smear their faces with a mixture of grease and charcoal, and paddle with a smudge on a square of turf, in the bows of their birch canoes. The cribou, moose and bear, driven from the thickets by the clouds of insects, plunge into the river for a temporary respite, where they are often slain by the hunter in his canoe. Whoso must travel will be prudent to sleep at noon and utilize the cooler hours when the sun sweeps low along the northern horizon and the insects are less active.

As the summer ripens the mosquitoes become less troublesome, though never entirely absent. The strenuous period of the spring floods being over, the great river settles down into its normal summer flow. Early in July it was the ancient custom for the Yukon Men, the Mountain Men from the Tanana River, and sometimes strangers from the Upper Yukon or the Koyukon tributary, to meet on a small flat island where the Tanana and Yukon come together. This was the neutral ground, Nū-klūk-a-yĕt′ in the Indian tongue. Here no man might bring his quarrel, no tribe its feud. The meeting was devoted to the peaceful barter of furs, and to festivities where food, the weird Indian music and Indian dances, were the rule.

Many years ago I was fortunate enough to be present at the annual meeting. My companion and myself were the first whites to have that experience. On arrival, after the usual harangue from the senior chief ashore and the spokesman of our party, and several salvos from flintlock muskets ashore and the shot guns afloat, we were allowed to land and a camping ground designated for us by the master of ceremonies, who held his office with dignity.

Later on shouts announced the arrival of the Mountain Men and we hastened to the beach to witness their reception. Dressed in his finest array the senior chief stood at the top of the bank, his followers all arrayed in their best, standing with loaded muskets ready for the salute.

Swiftly around the bend in the river came the little fleet of birch canoes elegantly fashioned, uniform in length and pattern, each holding one man with his bundle of furs and store of provisions. They were uniformly dressed in their purely aboriginal costume of dressed deerskin, ornamented with fringes, quill embroidery, and patterns drawn in red, derived from a soft argillaceous ore of iron. The trousers were continuous with the moccasins, and the upper garment bore a pointed skirt or pendant before and behind. Their long hair tied in two locks at the side of the head, wound with beads and polished with bear's grease, was sprinkled with the chopped up down of swans. Their faces were painted with red ochre, every man wore an ornament piercing the cartilage of the nose, and a belt of dentalium shell or caribou teeth. Their guns lay beside them. With military precision the paddles struck the water in unison, the canoes wheeled and came to rest, paddles uplifted, a short distance from the beach, while every gun on shore boomed its salute. The ceremonies of landing and camping once over, the interest felt in meeting these handsome athletic men, who had never before seen or been seen by whites, was very great. Although they possessed beads, guns and pipes, these had been acquired from other tribes acting as middlemen. It is probable that people so little touched by our civilization no longer exist in North America. The basin of the Tanana is now occupied by a large mining camp with all that that implies, and the dignity and glory of the Mountain Men have departed.

Midsummer brought all dwellers in the valley to the rivers, that the winter's supply of salmon might be secured, the real staff of life to these people. The banks near the fishing camps were scarlet with long lines of fish, split and cleaned, drying in the sun. On the lower river the salmon were mostly taken in traps. Six hundred miles up-stream only the larger and stronger species made their way. One of my most vivid recollections is of the sight, just after shooting the riffle at the lower Ramparts, of a fishing party provided with very large dip nets on long poles. The dusk was close upon us and the rank of birch canoes, arranged in line transversely to the stream, was already in the shadow of the cañon. Chanting a weird low chant in perfect time, at a given moment the broad nets were simultaneously plunged into the water while the frail birches rocked under the strain. Two canoemen were needed to lift one of the great king salmon out of his native element. The order, precision and silence, except for the mystical chant; the bronzed faces and sinewy arms half disclosed in the waning twilight, the swift water and towering heights of the cañon, left an ineffaceable impression.

The Yukon was good to her children. From her waters came the fish of many sorts, their staff of life. On her broad sloughs and amongst her thickets, the wild geese rested and the ducks raised their downy broods. The furs and skins which kept the native warm and dry, came from her banks. The stately spruce and silvery birch along her shores supplied houses, canoes, utensils, traps and fuel. Floored with ice or flowing yellow in the sun, she was her people's highway. In death their elevated tombs were placed where might be had the widest view of Yukon water.

The Men of the Yukon had, like other men, their careers, affections, tragedies and triumphs. The valley whose rim enclosed their world, since they knew none other, was as wide for them as our world is for us. It is certain that for their world they had worked out problems which we are still facing with puzzled trepidation in ours. No man went hungry in a Yukon village. No youth might wed until he had killed a deer, as token that he could support his family. The trail might be lined with temporary caches, yet no man put out his hand to steal. Men were valued by their achievements and their liberality. Any man might rise to eminence and leadership by showing his fitness in his community. That there were evil doers occasionally is probable, but there was an unwritten limit which might not be transgressed without condign punishment.

The stranger was welcomed without inquisitiveness, sheltered and fed without ostentation, and sent on his way without fee or reward. The dead were protected and remembered; their deeds of prowess handed down as examples for the young. Debauchery was unknown until taught by men of whiter skins.

They suffered from the dread of mysterious powers, and the shaman took his tithes of them. Their religion was vague and their politics mostly a minus quantity; but in practice they knew what was just and good, and in the main made it their rule of life.

Such were the Men of the Yukon, to whom civilization and the greed of gold brought drink, disease and death. The fittest has survived, but the fittest for what?

Time will restore their verdure to the Yukon placers, when the gold has been extracted and the prospector ceases from troubling. The graceful spruce will clothe her ravaged banks once more, and even the salmon exterminated by the canneries will replenish her waters in the fulness of time. The stern wheeler will pass away with the exhaustion of the mines, or at least become a rarity. The Arctic calm will rest once more upon her hillsides. But the Men of the Yukon trained to her ways by the experience of generations, wise in her capabilities, contented with her bounty, the true children of the river and its valley, these she shall know no more.

FACT AND FABLE IN ANIMAL PSYCHOLOGY

By Professor JOSEPH JASTROW,
UNIVERSITY OF WISCONSIN

MAN has ever been ready to show his esteem of animal ways, even to the veneration that in early times took the form of animal worship. The cunning and courage of animals, their passions and endurance, their keenness of sense and mastery of instinct, appealed to the man of nature as enviable qualities. The wolf that he feared, or the horse that be subdued was equally to him a fellow being. He was aware that the animal scent was truer, the animal sense of direction surer, than his own. Matching his wits against theirs, he knew that he might be outwitted by animal wile, might be overcome by animal daring. In his mythology he constructed beings endowed with superhuman qualities by fantastic combinations of the animal and the human form; and in his fables, from Æsop to B'rer Rabbit, he gave to his favorite animal the hero's part in his simple plots. He placed himself under the protection of some sacred animal as a totem, and held it as likely that the souls of an animal could be made to inhabit the bodies of a man, or that by some magic he could be transformed into their semblance.

It is quite possible that some obscure and disguised variety of this same instinctive feeling may still affect our estimates of what animals do, and of how they feel and think. We know so intimately how our domestic pets enter into the routine of our lives, share our moods and occupations, that it seems plausible to suppose that only a lack of speech prevents them from expressing a knowledge of our thoughts and sympathy with our feelings. But when we reflect upon the matter more soberly, we realize that we must not allow our prejudices to affect our judgment of what their behavior justifies us in concluding in regard to their intelligence. In considering what kinds of minds they have and how they use them, we must never forget how different are their needs from ours, how easily an action on their part may seem to be full of meaning to us (because if performed by us it would be done for definite reasons and purposes), and yet may be for them a rather simple trick to gain our favor. This, indeed, is the difficulty of the whole problem. We can judge what animals think only from what they do; yet what they really do may be wholly different from what they apparently do. It is we who unintentionally read into the action the meaning that it has for us. The way out of this difficulty is not very simple nor very direct; and it is the psychologist's business to determine by all the various kinds of evidence and reasoning that he can bring to bear upon the data, just what kinds of thinking the most favored animal can and can not master. The last particularly must be carefully considered; yet, both for animal capacities and animal limitations, is it of prime importance to note that, like ourselves, animals will only learn to do such things as enter profitably into the scheme of their lives. They will under ordinary natural circumstances acquire an intelligent appreciation of such of the goings on in the world about them as they can put to use; and even though we furnish our pets with decidedly different conditions of life and teach them much that they would have no occasion to learn for themselves, yet the manner of their learning will still remain of the same kind and require the same combination of powers as governs their natural behavior. So, in the end, the question of how animals think is one that psychology may hopefully consider. The answer will never be wholly complete; but there is no reason, so far as it goes, why it should not be sound and convincing—setting forth clearly and precisely what types of intelligent action animals share with us, and how much greater a range of even our simple thinking and doing lies wholly outside of both their interests and their capacities.

Such reflections are brought home to the psychologist whenever he observes how willing people are to be convinced that the multiplication-table and reading and spelling fall as readily within the powers of the exceptional animal as they do within those of an ordinary small boy. Let us consider a group of performances that within recent years have been triumphantly heralded as proving the vast possibilities of animal education, and have been accepted by the vast majority of people for what they pretend to be. A wise horse, 'Kluge Hans,' has mystified Berlin audiences; and 'Jim Key,' another equine sage, has done the same for the American public, by going through a program that includes adding and subtracting, and multiplying and dividing, reading and spelling, telling time and the days of the week, indicating people's ages, or sorting their letters, revealing their professions and their peculiarities, knowing the value of coins and bills, and even pointing out passages from the Bible or reasoning that a circle has no corners! In analyzing such performances, it is indispensable to remain undistracted by what the exhibitor asserts or pretends that the animal does, but calmly to observe what really takes place and to decide not necessarily how the trick is done, but what kind of thinking is concerned in the steps that the animal really takes. Such an exhibition may, however, offer an equally interesting study of the psychology of the audience as of the performer—a study of what people are ready to believe and why they are so disposed.

It does not require a deep psychological insight to make it clear that the calculating and spelling, time-telling and letter-sorting horse would be as much of a miracle as a Centaur, or a Pegasus, or a Unicorn. All these creatures belong, and with equal obviousness, to the world of fable; and the one falls as far outside the realm of actual psychology as does the other escape the ken of the zoologist. If one is inclined to regard that so obvious a proposition would at once command assent, he need only overhear the talk of those who come away from these 'marvelous' performances to be assured that the calculating horse and the unicorn are in popular estimation horses of very different colors. The latter is at once put aside as belonging to the world of myth; but the former, though not to be met with in every stable, is regarded as falling within the occasional possibilities of mundane horsedom.

If we forget for the moment that there is absolutely nothing in a horse's life that would supply the least occasion for developing so remarkable a talent as is needed for counting or spelling, we may bring ourselves to consider what kind of a miracle the calculating horse would stand for. An extravagant admirer of the Berlin horse, in maintaining that 'Hans's' education is about on a par with that of a boy (even a Berlin boy) of twelve years, has at least the courage of his convictions; nothing less would suffice to fit that genius of a horse to handle numbers and words and the abstract relation of things as his friends allege. And if a Zulu or an Esquimau were, after an equally brief schooling, to turn out a Newton or a Darwin, it would be rather less of a marvel.

To gain a common-sense view of the matter, observe a bright child of three years of age: note how it gives a hundred evidences for every hour of its waking existence, of a ceaselessly busy occupation with all sorts of ideas and little mental problems; how it sets up in its play one situation after another, sees new relations, devises a new use for an old toy, and creates a little world of its own imagining, for which it makes rules and breaks them, pretends that things are happening and gives reasons for their doing so; and so hour after hour proves itself possessed of a very acute little mind to which ideas and relations and situations are very interesting and familiarly handled mental tools or playthings. It is very true that much of this we know only because the child keeps up a constant chatter in its play, and speaks for itself as well as its toy or dolls, reveals its inventions in words, and thus tells the story, which without the explanation we could in our grown-up remoteness from such occupation but feebly understand. But the very possibility of learning all this language and of using it is itself a direct tribute to the intelligence that animates the little brain and reveals its finer quality, its greater possibilities. Language helps, most decidedly helps, the mind to grow in scope and power; but it does not create the capacity which its use requires. We have, moreover, some very interesting accounts of the cleverness of young children, who from early infancy were both deaf and blind, and who from their dark and silent world into which language could but sparsely enter, gave equally convincing proof of how busy their brains were with much the same kind of thoughts and purposes and interests as make up the mental lives of their more fortunate playmates. Naturally their doings were decidedly hampered, and their thinkings decidedly limited by the slightness of the bond—the single highway of touch—that connected them with their fellow beings. Such a child, in almost as languageless a condition as a dog and with far less chance of finding out what was going on in the world and of participating therein, develops into a rational creature of just that special kind of rationality that even in its simplest terms the brightest dog seems never to achieve.

And now consider what a slow and weary path this bright child, equipped with all its sense and senses, and at the expense of much patient teaching, must tread before it comprehends the message of the letters, and gets to look upon 'twice two is four' as something more than a rather stupid bit of memory exercise, that, like virtue, if persisted in, brings its own reward. With an inconceivably great start beyond the dog or the horse, with a tremendously greater aptitude for just this sort of mental acrobatics, the human child must await some years of ripening of its powers, and upon that favorable foundation expend some further years of initiation and schooling to exhibit a simple proficiency in getting meaning out of those crooked black marks on white paper, and in putting two and two together so as to comprehend the manner of its strange transformation into four. Surely, the accomplishment merits our profound admiration. To this understanding of how much is involved in bringing an apt mind to the point at which reading and calculating becomes a bare possibility, of how great a world is already conquered when the three R's begin to play even the most modest of parts, let us add one point more: When the child begins to show (and not wholly by language) that the letters and numbers have some meaning, it shows the fact so variously that we have constant means of testing how real its knowledge may be. We gain a pretty fair idea in each case, how far the accomplishment is a mere mechanical trick, or a really comprehended operation. Everywhere the limitations are conspicuously obvious; and we know how gradually we must add to the complexity of the business, how readily, by only a slight change in the setting of the problem, we sink the struggling mind beyond its depth. All this is a very sound lesson in psychology to take with us, when we attend a 'show' in which a horse or a dog is put through some steps which are supposed to prove for the star performer a real comprehension of the message of the letters and the operations of the multiplication-table.

With so much of preamble, let us look at the actual performance, first as it is presented on the show-bills, and then as it appears from behind the scenes. The program that advertises the learned performances of 'Jim Key' includes among its dozen numbers such items as these: 'Jim shows his proficiency in figuring, adding, multiplication, division and subtraction for any number below thirty.' 'He spells any ordinary name asked him.' 'He reads and writes.' 'Gives quotations from the Bible where the horse is mentioned, giving chapter and verse'; and in addition acts as a post-office clerk or handles a cash-register. When these problems are reduced to equine terms, they prove to be simple variations of a single theme. To aid the figuring, the numbers are placed in natural order on large frames, five in a row, and five rows; and the letters, in alphabetical order, are similarly displayed. The numbers to be added or subtracted are proposed by some one in the audience, and repeated by the showman. The horse then proceeds to the card bearing the number that indicates the result, takes that card between his teeth and gives it to his master. The same is done for words composed of letters, each letter being selected in turn.

This is absolutely the whole performance; and even when most generously interpreted bears a decidedly remote resemblance to what the posters describe. The interesting part of it all is that so many who witness this simple exhibition are quite ready to conclude that before 'Jim Key' chooses his card, he goes through those mental processes which each one of the audience performs when he works out the answer to the problem as announced. This assumption is not alone wholly uncalled for, but is actually preposterous. One of the elementary facts that students of mind, whether of human or of animal minds, clearly grasp, is that there are vastly different ways in this complex world of ours, of doing the same thing. The same result is reached by wholly different means. To neglect this distinction would be to conclude that because one man—or, if you like, a horse or a squirrel—avoids a certain mushroom on account of its unpleasant odor, and the botanist does so by recognizing it as a specimen of *Amanita muscaria,* that all have displayed the same kind of intelligence, have used the same reasoning, because in the end they reach the same result—the avoidance of the fungus. To the simple, but comprehensive statement that the horse gives not the slightest indication of going through any of these processes in order to select his card, it need only be added that he gives decided indication of going through a very different kind of process. It is not at all necessary to know precisely what special sign the horse observes in guiding his selections, in order to determine (which is the important thing) that it is some kind of simple sign, an operation that falls within this general type. The type of 'Jim Key's' operation is simply that of learning to go first to a certain one of five rows, that is either the middle, or the top, or the bottom, or the one between middle and top, or the one between middle and bottom; and then in turn to select one of five cards arranged horizontally that offer a similar choice. Whether the cards bear numbers or letters or Chinese characters or the Weather Bureau signals or any other markings, and whether these markings have any meaning, is as wholly indifferent to the horse as it is unnecessary for him to go through any reasoning process in order to select the card that he is to present as his answer. As to the precise association that an animal comes to establish between a certain sign and a certain action, and the number and complexity of such associations that he can master, there is doubtless some variation among animals, though again hardly as much as amongst men. It is also interesting to determine the nature of the signs, whether noted by the ear or the eye, that a dog or a horse most readily learns; but all these details do not at all modify the general nature of the operation, which mainly needs be considered. The actual indication that 'Jim Key' follows to reach first the right frame, and then the right row, and then the right letter, seems to be given by different positions of the master's whip. The ability to learn even this simple association is probably very limited, and in this case seems never to exceed 'five.' Upon this slender basis of actual achievement, does 'Jim Key' attain his reputation as a learned thinker.

The performances of 'Kluge Hans,' so far as they may be gathered from the printed descriptions, are of no more complex character. The method of response is simpler and consists of nothing more than in pawing continuously one stroke after another, and of stopping when the number of strokes corresponds to the answer of the arithmetical problem that has been set. Alphabets and 'yes' and 'no' must also be reduced to numbers before they fall within 'Hans's' repertory. Here again, as announced, the program is most versatile and startling. There is the same proficiency in multiplying and dividing and adding and spelling; and by an ingenious variation of the question, 'Hans' will tell how many of the admiring company are over fifty years of age, or are members of a certain profession, and will paw 'yes' or 'no' in answer to any question of which his master knows the answer. The claims put forth on behalf of the Berlin horse—and that on the part of men otherwise versed in scientific matters—is indeed remarkable, positively astounding; for one of these attributes to 'Hans' a perfect acquaintance with fractions, the ability of distinguishing colors as well as playing-cards, to tell the coins of the realm, to differentiate geometrical figures, to give the time upon any watch-face, to name musical tones and tell which are discords. The method by which these answers are indicated is never more nor less than that of pawing until the correct number is reached. The more complicated replies are in the form of words: for this purpose the elementary sounds are reduced to 42—allowing for combinations of vowels and consonants. Accordingly, any one of these sounds is indicated by occupying one of *seven* places on one of *six* rows; thus for '*j*,' 'Hans' stamps first *3* times and then *4*; and for '*St*,' first *5*, then *6*. Under this system, the horse is actually supposed to distinguish between the ordinary '*s*' and the '*long s*' at the end of the word, between '*äu*' (with the Umlaut) and '*au*' without it, and so on. Such, at all events, is the claim set forth for 'Hans's' miraculous intelligence. As a fact it is, of course, completely a matter of indifference to 'Hans' what the questions may be; they could with equal success be put in Greek or Sanskrit, so long as he can catch the right signal and stop pawing at the right time. And so again the gap between fact and fable is world-wide; and the assumption equally groundless that any measure of the human kind of reasoning intervenes to make possible the horse's replies.

Surely there is nothing in either of these performances, except the pretences of the showman, that in the least suggests the use of any of the powers that the developing child must first acquire to gain an actual knowledge of numbers and letters. And, if we look, we shall find many indications of the quite different processes that are really concerned. The best of these lies in the nature of the mistakes that are likely to occur. For 'Jim Key,' these take the form of selecting a neighboring letter—an 'x' for a 'y'—a kind of mistake which no mind that really was doing any spelling would be in the least tempted to commit; while 'Hans's' mistake consists in not seeing the signal quickly enough, and in pawing once too often or in anticipating through the getting ready of the signal, and stopping too soon, again a type of mistake that has no relation to the actual operation of those who calculate and read. So also the scope of the questions that these marvelous animals at once attack without preliminary training shows how unrelated is the finding of the answer to the consideration of the problem. If we add considerably to the difficulty of the problem that we set to a calculating child, we must be prepared to accustom its powers gradually to the increased difficulty and to take small steps repeatedly with much chance for mistake in the newer processes. But these calculating horses jump at once into fractions and square-roots, into propositions in geometry, and equations in algebra, when some enterprising questioner proposes them. This at all events is true for 'Hans's' master, who easily prepares the result; though in 'Jim Key's' case, one sometimes suspects that the calculating possibilities of the master are not immeasurably in advance of those of the horse.

And once more—it certainly seems strange that so exceptionally educated an animal should find no other occasion to exercise his remarkable powers, should not spontaneously exhibit some original evidences of his genius, that would distinguish him from the ordinary horse. We are even tempted to pity so talented an animal with no outlet for its vigorous mind, condemned to the monotonous round of oats and hay, varied only by the tit-bits of carrot and sugar which, however, seem to be appreciated as rewards of learning by these educated animals quite as keenly as by their untutored kind. It is also pertinent, though possibly unnecessary, to point out the inherent contradiction between the operations that a successful reply is supposed to involve and the absurdity of the failures or wrong answers that occasionally occur. Thus, this most intelligent Berlin horse, who is supposed to be acquainted with difficult mathematical relations, occasionally makes mistakes. Now when a child makes a mistake, it is in regard to some operation just beyond its capacity, while the simpler additions and subtractions are readily accomplished. On the other hand, Hans, immediately after giving an answer in square-root, fails to count the buttons on an officer's coat, and insists, until repeatedly corrected, that a man has three ears and not two; or again, after making the minute distinctions of German orthography, puts *K* for *J;* and further, if this miraculous horse really distinguished the sounds and converted them into letters, why should he not be phonetically misled and occasionally substitute, let us say, a *ck* for a *k,* which would mean all the difference between *2* pawings followed by *1,* and *3* followed by *5.* Yet such objections are indeed superfluous, or would be were they not so commonly disregarded by the prejudice in favor of taking such absurd pretences at their face value. In brief, it is difficult seriously to investigate these limitations in any other spirit than that of pointing out how unmistakably they indicate an unreasoning, unrelated method of reaching the answer through some system of signs.

This statement of the facts of the case does not at all imply that in this performance we have reached the limits of the horse's education. Very likely the intelligent horse may be taught to go very much farther than this in the direction of his natural ability to associate signs with actions. It would, for example, be very interesting to know whether 'Jim Key' could be taught, in selecting one after the other the letters that spell his name, to go of his own accord for the 'I' after he has been led to the 'J,' and then to the 'M,' and so on; that is, whether he could learn to perform a *series* of selections by associating each with the one following. This would still be a task of the same order, but a more complicated one; and in investigations of this kind earnest students of animal intelligence have obtained important evidence as to the capacities and limitations of animal thinking. Such psychological questions are asked in a different temper from that which prompts the stage performances, and lead to far more useful results.

And so we come last to the other side of our inquiry, why this kind of a performance is so generally accepted at its face value, why educated persons will attribute to the horse (as they do to the Berlin horse), the insight to recognize that 27 divided by 7 gives 3 with a remainder of six, that ¼ must be added to make a unit out of ¾, or that at 12:17 one must wait 43 minutes for one o'clock! Indeed, so wide-spread were the misleading accounts of this learned animal, that a commission of inquiry was appointed to investigate the whole affair; and upon this commission sat a professor of psychology of the University of Berlin. Though the foregone conclusion was reached that the performance did not exhibit 'a scintilla of anything that may be regarded as thought,' it certainly seems incongruous that so serious an inquiry should have become desirable. Only one point of interest seems to have been elicited, namely, that the horse's master or the bystanders may have frequently been honestly unaware of giving the sign which the keen senses of the horse caught as the indication to stop pawing. Perhaps we need not too pointedly raise the question as to how far these exhibitions intentionally deceive their audiences. Wherever systematic training enters, it follows that the trainer must realize how wide is the gap between what is done and what is pretended. Self-deception on the part of the showman can not be held accountable for more than a slight portion of this discrepancy. Yet still truer is it that if people were not ready to credit such remarkable powers to the horse or the dog, such exhibitions would find no favor. It is partly because animals can really do many things that are wonderful in themselves and, if performed by men, would require considerable rational powers, that we are inclined to credit them with capacities for learning similar to our own. This tendency can be held in check only by an appreciation of the complexity of even a simple piece of true reasoning, of how essential it is to appraise an action in terms of the process that led to it, and how indirect is the revelation of process that comes from the knowledge of the result alone. When this simple lesson in psychology is clearly recognized as furnishing a sound basis for judgment, there will be less tendency to believe that horses can take unto themselves brains with a capacity to multiply and read, as to believe that a horse has suddenly sprouted wings, even though such a Pegasus is pictured on the posters displayed in front of the exhibition hall.

People would also less easily succumb to such deception if they stopped to consider that in regard to these animal performances they must earn the right to an opinion by some simple measure of initiation into the arrangements of what impresses the uninitiated as a remarkable exhibition. The first attitude is naturally that of wonder, and in lack of any detailed knowledge of what the trick may be, the tendency is strong to credit, at least in part, the explanations that are advanced. Once this attitude is overcome and the kind of training that prepares for the performance is understood, the whole affair loses its marvelous aspect and becomes a mildly interesting demonstration of animal training. A brief glimpse of the mechanism behind the scenes is quite sufficient to balance the glare of the footlights and leave the spectator in possession of his usual measure of human intelligence that enables him to appraise sympathetically but sanely the intelligent powers of animals.

THE WORLD STATE

By C. C. ECKHARDT
UNIVERSITY OF MISSOURI

IN the last decade certain disconnected movements have accomplished something in the way of bringing about a world state, and other movements are on foot to bring about a completion of this general movement. The Hague Peace Conference (1899) brought about the establishment of a permanent international court of arbitration or the international department of justice; the Interparliamentary Union is trying to effect the establishment of a world parliament or congress, or legislative department, and if possible a world executive department. The purpose of this article will be to deal with the history and nature of each of these movements, to point out the reasons why the present movement ought theoretically to succeed and to discuss the obstacles to a successful realization of such plans.

1. *The Hague Peace Conference and the Hague Tribunal.*—In the summer of 1898 the Tsar invited all the principal states of the civilized world to send delegates to discuss concerted action for the maintenance of a general peace and the amelioration of the hardships of war and to plan for the possible reduction of the military and naval armaments of the world. This conference met May 18 to July 29, 1899. It was composed of one hundred delegates representing states having standing armies and navies; twenty-six nations in all were represented. The Papacy, the Transvaal Republic, and the states of Central and South America were not represented. The results of the conference were twofold: (1) Certain arrangements concerning warfare were made and (2) a permanent court of arbitration was established.

The arrangements concerning warfare need not occupy our attention; suffice it to say that a number of resolutions were adopted which tend to make war less brutal and have in view the ultimate reduction of military and naval armaments. Besides these humanitarian efforts of the conference, it accomplished what will be historically much more important, namely, the establishment of a permanent international court of arbitration. The powers agreed to submit all serious cases of international dispute for decision to an international tribunal. The conference provides for the selection of persons who shall constitute a permanent tribunal for six years. Each power can choose not more than four members, who must be learned in international law. If a dispute occurs the disputants select two of these four from each country, or others not permanent members. These parties choose an umpire, and the settlement is to progress according to stipulated arrangements of procedure. So by this conference a *world tribunal* was established.

Strange to say, the nations did not submit any cases for arbitration in this court until the year 1902. To the present time five cases have been submitted. The first was the Pious Fund Case, 1902. This concerned a dispute over the disposition of a fund created for the support of certain missions in Mexico. In 1697 the Jesuits in Mexico had collected a fund for their monastic and missionary enterprises. After the abolition of the order of Jesuits in 1768, the government seized the property of the Pious Fund, but distributed its income among the missions concerned. After California became a part of the United States, these annual payments were not made to the missions in California. Our government protested on the part of the missions concerned, but to no avail. Finally, both departments of state concluded to let the matter be judged by the Hague Tribunal, and there the award was given to the claimants of the United States. Mexico was asked to pay the arrears and to continue the regular annual payments. Another case was between Venezuela, on the one hand, and a number of European countries and the United States, on the other. The chief facts of the case are the following: Owing to numerous revolutions in Venezuela, the government was heavily in debt and was unable to meet its financial obligations to European and American creditors. Venezuela disagreed as to the amount to be paid and wanted time to reorganize her finances. Several of the European countries brought pressure to bear on Venezuela with little success. Finally, it was decided to arbitrate the claims at The Hague. This was accomplished in February, 1904. The claims in the aggregate did not amount to very much, but the precedent was very important. Decisions in the other three cases have not yet been rendered.

Fully as important as the cases which have been submitted to the Hague Tribunal is the recent agreement of Norway and Sweden. In drawing up the final terms which provided for the peaceful separation of Norway and Sweden, those two powers agreed for a period of ten years to submit to the Hague Tribunal all matters of dispute, except those which affect the independence, integrity and vital interests of either. Similar treaties of arbitration have been drawn up by the other powers, but this treaty differs from the others in one important respect. In the other treaties either party decides whether the point in dispute is of vital interest or not. In the case of Norway and Sweden, the Hague Tribunal decides whether the point in dispute is of such vital importance as can not be decided justly by the tribunal.

So we see that the newly created department of justice has had very little to do—but the fact that it has had any disputes at all to decide is a very hopeful sign. The tendency will undoubtedly be to have more cases submitted. The example of Norway and Sweden may be followed by other states. A more valuable precedent would be a similar arrangement by two first-class powers.

2. *The Interparliamentary Union*, or the attempt to establish a world congress or legislature. At present there is in existence an organization called the Interparliamentary Union; it is composed of members of the various legislative bodies of Europe and America; its object is to have conferences periodically to discuss the means of bringing about an international legislative body—a world congress or parliament. This union was founded in 1888 in Paris as a result of the work of William Randall Cremer, M.P., an English carpenter and labor unionist. In 1889 the first regular conference was held in Paris, and since then meetings have been held at most European capitals and in some other important European cities and in St. Louis. It now has more than two thousand members, all of whom fill seats in some national parliament. There are two hundred from the United States. Membership is voluntary and lasts as long as the members retain seats in their respective parliaments or legislatures. In discussing this union we shall note its accomplishments to 1904, the work of the meeting at St. Louis in that year and its most recent efforts to create a world parliament.

The achievements to 1904 are rather difficult to state. The movement has grown gradually; it has had practical statesmen as organizers, leaders and members; their influence has been great in creating and stimulating a sentiment in favor of universal peace and the means of attaining that peace. From the beginning the conferences have attempted to bring about international arbitration, and it is interesting to note that at the Hague meeting in 1894 the conference declared in favor of a permanent court of arbitration, and a commission of six men was appointed to draw up plans for such. Thus it should be remarked that this conference anticipated the Hague Peace Conference by five years, and that the main work of the Hague Peace Conference was due in no small degree to the work of the Interparliamentary Peace Conference.

One of the most prominent leaders, at least the most prominent American leader, at present is Richard Bartholdt, congressman from St. Louis. It was due to his influence at the 1903 meeting at Vienna that the union held its conference at St. Louis in 1904. At St. Louis the famous St. Louis resolution was drawn up by Bartholdt and adopted unanimously by the conference. It declared in favor of the following things: (1) There should be a conference of nations to consider the universal execution of treaties of arbitration; (2) a congress of nations should be created in which every nation shall have representatives. Such sentiments had been expressed before; but this seemed to be greeted with more enthusiasm than any other similar proposal of the past. After having closed the session at St. Louis, the members of the conference visited Washington and requested President Roosevelt to invite the governments of the world to send delegates to a Second International Peace Conference. This he did. All of the powers replied favorably except Russia and Japan; their refusal was owing to the prosecution of the Russo-Japanese War. When hostilities ceased it was thought that Roosevelt would ask the Belgian government to issue a formal invitation to all powers concerned. But during the Portsmouth Peace Conference the Tsar, through his diplomatic representatives, expressed his desire to have the privilege of calling the Second Peace Conference, since he had called the first. Roosevelt gladly consented. Just recently the Tsar summoned the conference to meet in the fall of 1906.

Hitherto the work of the Interparliamentary Union has been merely preliminary, working up an organization, creating a desire for an international congress. In the past year important steps have been taken. In August, 1905, the union held its thirteenth annual meeting at Brussels. Here two things of importance were accomplished. In the first place, the South American and Central American republics were invited to send delegates to the next annual meeting of the union. Up to that time the Latin American states had not been asked to participate in this movement. Secondly, a commission of seven members was appointed to draw up a plan for an international congress. Mr. Bartholdt was the American member of the commission, which met in Paris in November, 1905. The plan of the commission provides: That an international legislature of two houses shall be established; that the lower house, or popular branch, shall represent the various legislatures of the world; that the upper house, or senate, shall be appointed by the various governments; and that this legislature shall meet periodically, probably at the Hague. This plan of the commission will be the basis of discussion at the next meeting of the union.

So far the Interparliamentary Union has merely created an organization which has been given no legal status or official recognition by the various governments. It has had no appreciable influence on international relations. Nevertheless, its work to the present time has been of considerable value and importance. It has secured the membership and earnest support of the highest-minded and most capable legislators of the world. Through its members and its own influence it has created and stimulated feelings in favor of universal peace, and is ready to contribute what is perhaps a very satisfactory means of maintaining this much-desired universal peace.

3. *The World Executive.*—To the present time no definite plan for a world executive has been proposed; no movement to establish this has been put on foot. Provisions for this department will no doubt be made later as the occasion requires. No one knows what form it will take. It will hardly be delegated to one man, or even a few men. The international parliament may appoint a committee for all necessary administrative work; this may be divided into a number of bureaus having charge of the various kinds of administration. If any armed coercion is necessary to enforce international laws, the combined fleets of a number of powers may be used, as has been the case in the last century when the powers coerced Turkey.

4. *The Purpose of the World State.*—We have been discussing the origin, organization and characteristics of the proposed world state—now a few words as to its purpose and the place it will fill. Its purpose will be: (1) To provide a definite recognized code of international law; (2) to establish a tribunal which will apply this law, which will arbitrate disputes arising between nations and prevent the disagreeable and disastrous clashes between the peoples of the civilized world. International law at the present time is unsatisfactory. It does not have the force of law as does municipal law; it is not uniform. There is need of a legally constituted body to weed out inconsistencies, to bring in uniformity, to make new laws for the numerous points which are still undecided and which are bound to appear as the intercourse of nations becomes more and more extensive. No one will dispute that the world state will fill a definite place.

5. *Why the Present Movement for a World State ought to succeed.*—A proposal to establish a world state will naturally have its critics—many will doubt the success of such an undertaking. But there are a number of reasons why success can be hoped for some time in the future. Immanuel Kant in his 'Perpetual Peace'[1] declares that the following things are necessary for a world state: (1) All nations should have representative government; (2) successful systems of federal government must have been established in part of the world; (3) there must be a moral force to support the effort. This statement is mere opinion, but the opinion of this writer may well be cited as worth consideration. It seems to us that these things are essentials. To what extent are these requirements fulfilled at the present time?

Representative government has been attained by all the christian states of Europe except Russia; and undoubtedly the Russian people will also secure some measure of political liberty before the present disturbances are permanently allayed. All important nations and

[1] 'Saemmtliche Werke,' Vol. VI., pp. 416–427.

communities outside of Europe have their affairs settled by representatives of the people—the United States, Canada, Australia, Japan. Even China has recently appointed a commission to travel in Europe and America to inspect the best forms of representative government. Federal government has been successfully tried in a number of cases—the United States, Germany, Switzerland, Canada and Australia. The moral sentiment which will support such a movement has been increased very noticeably in the nineteenth century. The idea of the brotherhood of man has gained great strength; there is a growing idea that moral law is fully as binding on nations as on individuals; there is an ever increasing number who think it is just as wrong to kill a man in battle as to commit murder. Then along with these forces there is the so-called 'Welt-Geist,' a cosmopolitan spirit, the idea of world-citizenship.

The hope that the powers may some time unite to establish a permanent international legislative body does not seem unreasonable. The past century produced what is called the Concert of European Powers. They have met at irregular intervals to discuss affairs of mutual interest; the enactments of some of these congresses have become recognized international law. A few examples will suffice. The Congress of Vienna (1814–1815) rearranged the map of Europe and undid much of the work of the revolutionary era. The Near Eastern Question has frequently occasioned united action on the part of the great powers of Europe. The navigation of the Rhine, the Elbe and the Danube has been regulated by European congresses, held at various times. In 1856, at Paris, the powers drew up rules concerning privateers, neutral goods, goods contraband of war and blockades; and these rules are a part of recognized international law to-day. In 1888 the Suez Canal was neutralized. In 1884 and 1885 the powers assembled in order to partition Africa peaceably and to make definite regulations concerning African affairs. In 1874, at Berne, was established the Universal Postal Union, whereby most of the nations of the world have secured a more adequate foreign-mail service. In the past few months the conference of Algeciras met to discuss international interests in Morocco. These facts show that in the past and present century the nations, not only of Europe, but of the whole world, have successfully cooperated in affairs of mutual interest. If they can meet irregularly for special purposes, it is not at all fanciful to think that at some future time they may meet periodically to make laws concerning all affairs of international polity.

There are also some practical reasons why the movement can and ought to succeed. The first is economic. The establishment of a world state will tend to secure peaceful intercourse between nations; there will be less and less occasion for war; international relations will be more definitely defined; more and more disputes will be settled by arbitration in the court of the world. Such a state will receive the undivided support of the commercial interests of the world, for commerce thrives best in times of peace. Since this is an industrial and commercial age, the business interests ought and will support a movement to bring about and preserve peace. The practical or mechanical obstacles to the establishment of a world state are fewer now than ever before; in fact, one might almost say they do not exist any longer. By means of past inventions and discoveries the world is more closely united, more thoroughly bound together, than ever before. Steam railways, steamships, the telegraph, telephone and wireless telegraphy and scores of other inventions have annihilated distance. The formation of a world union is easier than ever before.

6. *Obstacles to the Success of the Movement.*—Unfortunately such a movement as we are considering will have a number of serious obstacles to meet and overcome. Perhaps the most potent hindrance to the speedy establishment of a world state will be the existence of strong national feelings, the antipathy of the nations of Europe and the local patriotism on the American continent. It will be a long time before race hatred and national enmity will be allayed; it will probably be centuries before 'jingoism' and local patriotism will be supplanted by reason and a cosmopolitan spirit. But in connection with this obstacle, one ought to note that a great amount of this cosmopolitan spirit does exist now and will continue to grow. Happily this spirit or feeling is not to be found solely among the upper and educated classes, but among the working classes also, and especially among the working classes of Europe. With them, however, the feeling is not necessarily an end in view, but a concomitant of their great struggle. The workingmen of Europe, organized under the banners of labor organizations and socialism, declare that their fight against capitalism is an international struggle, that the capitalistic régime is omnipresent and everywhere opposed to labor.

Another obstacle to the establishment of a world state is the hostile attitude of the monarchical governments of Europe. Most of them are more or less jealous of their sovereignty and hate to have it curtailed in any way; they scorn being forced to arbitrate their disputes instead of fighting them out. This is not a mere assertion and can best be illustrated by the attitude of Germany. At the Hague Peace Conference in 1899, the German delegates declared that arbitration was incompatible with the divine right of kings to rule. The Emperor William has openly manifested his opposition to a permanent court of arbitration. When it was proposed that the German-Venezuelan question be submitted to the Hague Court, William II. proposed that the matter be left to President Roosevelt as arbitrator. But Roosevelt declined and advised recourse to The Hague. A world state implies republican government; it will necessarily be representative, not of monarchs, but of nations and peoples; this is not in harmony with the divine right of kings. But this opposition of the monarchs will not be insurmountable. If a strong sentiment in favor of arbitration or federation be manifested, the monarchs will very discreetly give way, as has been their wont in the past century.

7. *Conclusion.*—This article has been merely an attempt to give an account of a tendency, its history and characteristics. The information available is exceedingly scarce; what is available is rather scattered and isolated; the status and force of the movement at any given time are difficult to estimate; the very nature of the movement itself is vague and visionary, it seems too fantastic to be practical, the attempt seems premature; it is the work of idealists, of optimists; their efforts are naturally looked upon with suspicion and discredit by conservatives and realists. Nevertheless, idealists prepare the way for movements which quicken the pulse of humanity and which bring about reforms both small and great. No person living to-day may ever see the establishment of a world state with power to execute its laws. However, we have seen that part of the machinery of a world state has been constructed and that other parts are in the process of construction. It may take decades before this world state will exercise any influence. But it seems no small accomplishment for an age to create the machinery of a world state which mankind will use to good advantage when there exists a sufficiently strong feeling of the need for and desirability of such an organization.

Too much must not be expected of this movement. The world state will not bring about immediate disarmament, but if it succeeds it will bring about compulsory arbitration; it will establish a state of law among the nations. The more disputes settled by arbitration, by judicial decision in accordance with laws passed by a world congress, the fewer will be the occasions for war. If the need for war is decreased, armaments will be less necessary and possibly may be abandoned. Inasmuch as an object so worthy may ultimately be attained by this now rather visionary movement, it at least merits our attention and sincere hopes for its ultimate success.

THE MEASURE OF 'PROGRESS'

By Dr. EDWARD S. HOLDEN,

LIBRARIAN UNITED STATES MILITARY ACADEMY

NO words are more common in the mouths of orators than the phrases: The march of Progress—the growth of Civilization. When we say that the twentieth century is in advance of the sixteenth, do we mean that it is so in each and every respect? Do we mean that men in general have now a keener feeling for art than in the age of Michel Angelo; a finer knowledge of justice than in the century of Socrates; deeper religious feelings than in the days of Wesley, or of St. Thomas Aquinas?

It is not easy to answer such questions offhand in any large way. The modern feeling for art is perhaps more wide-spread, but certainly far less keen, than in the Italy of the sixteenth century. If we say that our sense of justice is finer than that of the Greeks who condemned Socrates to death, and of all the centuries before our own, how is it that successive generations of men have preserved the narrative of his last day with sacred care? What are we to say of the religious feelings of the day of Wesley compared with the ethical efforts of the day of Felix Adler? It is clearly not easy to give answers of real import to questions of the sort. We need a better insight into the meaning to be attached to words like progress, civilization and the like. Definitions taken out of dictionaries will not answer.

It has been my fortune, lately, to make a fairly thorough study of that wonderful renaissance of science which began in the days of Roger Bacon, in the thirteenth century, and to endeavor to connect it with its origins in Alexandria, its precursor in Mohammedan Spain and its successor in the century of Galileo. There is no space here to present even a summary of such a study,[1] but it may not be out of place to give a few paragraphs which bear on the general and important question: how are we to measure progress?

In comparing the view-points of different ages with our own we continually meet with surprises. The uncritical attitude of the men of the thirteenth century towards miracles and wonders is little less than astounding to us. Our thought seems to be ages in advance of theirs. On the other hand, we often meet with an insight that has what we call the distinctly modern note. An instance from literature will illustrate:

[1] See Popular Science Monthly, Vol. LXIV., pp. 316–342, and elsewhere.

A man's character is his fate

is a sentence that one would assign to Taine or to Stendahl in the nineteenth century if one did not know it to have been written by Heraclitus in the fifth century before Christ. In like manner, some of the scientific processes of Hipparchus, Archimedes and Roger Bacon are so 'modern' as to bring a glow of delighted wonder when they are met with. Their failure to draw certain conclusions that seem almost obvious to us is equally astonishing. A formal explanation of the differences and of the resemblances of ancient ages with our own might be somewhat as follows. We may suppose that a completely developed man of our day has educated his sympathies and intelligence to have outlets in a certain large number of directions—let us say, in the directions A, B, C, D, E, F, G, H, I, J, K, L, M, N, O, P, Q, R, S, T, U, V, W, X, Y, Z. It is possible, however, that some few of these outlets are absent, or nearly closed, E and O, for instance. The men of the eighteenth century may be supposed to have had fewer outlets, and those of the thirteenth still fewer; but the intensity and refinement of their sympathies in certain directions may not have been less, but far greater, than ours. The feeling of the thirteenth century for religion, and of the sixteenth for art, for example, were not only different in intensity, but very different in quality, from our own.

When we make a formal comparison of our age with that of St. Thomas Aquinas and of Newton, the table might stand thus:

A, B, C, D, –, F, G, H, I, J, K, L, M, N, –, P, Q, R, S, T, U, V, W, X, Y, Z, XX century.
a, b, c, d, e, f, g, h, i, –, –, –, m, n, ... XIII century.
a, b, –, –, –, –, g, h, i, j, k, l, m, n, o, p, q, r, ... XVIII century.

If in a comparison of the thirteenth century with the twentieth our discourse is upon the matters A, B, C and D we may find their insights, a, b, c, d, singularly like our own. The case may be the same for the matters G, H, I compared with g, h, i. But if, by chance, we are comparing their insight e with our absence of insight, or our X, Y, Z, with the blanks in their experience, we are astonished at the difference of outlook.

This formal and unimaginative illustration may not be quite useless in clarifying one's thought upon a matter easy to express in words and exceedingly difficult to realize. It is essential to admit the presence of blanks in the experience of past centuries; and also the presence of insights upon fundamental matters which are astonishingly different in intensity and in quality from our own. The experience of the thirteenth century was handed onwards to succeeding ages; it could be understood by the ages near to it; words continued to mean in the fourteenth very nearly what they meant in the preceding century. But as ideas changed, the signs for ideas changed with them; and we must be constantly on our guard lest we unthinkingly admit an old form as if it had the new meaning. Consider, for example, what astrology meant to Roger Bacon and what it means to us. He had no difficulty in reconciling the fateful influence of the stars with a scheme of salvation for men possessed of free-will. Words had different meanings to him and to us. His mind was conscious of no conflict between his religion and his science. His religion—that of the thirteenth century—is in absolute conflict with our science—that of the twentieth. This one example may stand as a type of many that might be brought forward.

The Greek architects long ago discovered that a cylindrical column looked at from a distance would not appear to have its two sides parallel, but that on the contrary these two sides would be hollowed in, convex towards each other. A long colonnade of cylindrical columns would exaggerate the unbeautiful effect. The Greeks *felt* the lack of beauty and afterwards proceeded to discover a rule for making the outer surface of a column convex, so that a colonnade of convex columns would appear to the spectator to be comprised of cylindrical, or conical, surfaces, beautiful to the eye. This increase of the middle diameter of columns was called *entasis*.

In the early part of the nineteenth century, an English architect, Mr. Penrose, visited the Parthenon, for the purpose of making accurate measurements of its principal dimensions. What was his astonishment to find that something of the nature of *entasis* had been given by the Greeks to the architraves, cornices and other members of the building. The long horizontal lines of the friezes were convexed outwards in order that they should not appear hollow to the eye. Other horizontal members were also convexed in order that they should not appear to tilt upwards. Similar measurements made on the Maison Carrée, at Nîmes, demonstrated that like rules were employed by the subtle architect for similar purposes. Measurements made on the temples of Egypt have shown that their floors are convexed in order that they may appear flat.

The Egyptians, the Greeks and even the Romans were possessed of eyes and senses so subtle that certain architectural devices were demanded by them in all edifices designed to give high pleasure. The entire western world was ignorant of these devices until a couple of generations ago. With the destruction of Rome, even the traditions of these changes were lost, so that all the Gothic cathedrals of Europe and every great building erected between the end of the fifth century and the middle of the nineteenth were constructed on geometrical lines, so to say, and not to satisfy the eye.

Mr. Penrose's discoveries were made with a foot-rule, *not* by a sensitive eye. They have borne fruit in our own time and in our own great city. The beautiful library of Columbia University is built on Greek principles. Let any one glance along the edge of one of the steps of the main approach and determine for himself how far it departs from a horizontal line. Our eyes will soon come to demand such curved lines. Straight lines will soon seem hollow to us as they did to the Greeks. But, note the difference. We have come to our comprehension of such forms by literary, by archeological, by mensurational, steps, while it was a matter of feeling to the Greeks, or to their predecessors. Certain insights and sympathies of theirs have been atrophied in our ancestors. Can we say, then, that the appreciation of beauty is as keen with us as it once was to other peoples less 'progressive,' less 'advanced'?

Instantaneous photography has familiarized us with the various motions of the horse. The horses on the frieze of the Parthenon and in the paintings of the Renaissance are depicted in attitudes which are impossible. Because the horses of Mr. Frederick Remington's pictures are recognized by us to be true to life, does this show a greater sense of the beautiful? We have gained our new knowledge by photographic and scientific methods, but can we say that our æsthetic sense in this regard has become more refined? Is our analysis more subtle than the Greek synthesis?

We are all so used to the admission of the high sense of beauty of the Greeks that we consciously form our standards by what we suppose to have been theirs. We praise the classic purity of the Parthenon, not only the purity of its lines, but of its unbroken color—the native color of its marble. But in doing this we forget that the Greeks covered almost the entire surface of this pure marble with thick coats of color—parts with vivid blues and reds. A model of the Parthenon painted in its ancient colors seems crude to modern eyes. But are we to conclude that our sense of beauty of color is more keen and refined than that of the Greeks, our acknowledged masters? Is it true that the rains of centuries were needed to wash off colors carefully laid on by the builders so that it is only now, and to us, that the Parthenon finally emerges the one perfect building of the world?

As with Greek buildings so with Greek statuary. We are used to praise the classic purity of their white marble gods and goddesses, forgetting that the most famous statues were made of gold and ivory, enameled with images of animals and flowers, with metal bracelets and ornaments fixed to marble, or again painted in parts like the Hermes of Praxiteles or the Athene at Elis. The ears of the grave, serene and august Venus of Melos are pierced for metal ear-rings. To us she seems all-sufficing and stands alone. It is more than likely that the original statue formed a part of a group—Venus placating the wrath of Mars. Do we, in fact, at all comprehend what the Greeks meant to depict by their images of divinities? Would a Greek, returning to earth, in the least understand the interpretations of the æstheticians? Have we then progressed beyond the comprehension of the men who made these marvels? Is it permissible to take refuge in that fatuous phrase—they builded better than they knew—when, in fact, 'they' knew exactly what they were about, and possessed a consummate technique to express precisely what they chose?

There is nothing more certain than that the Gothic cathedrals of Europe were built by architects who knew their business and whose plans were definite and precise. The architects' drawings for some of these cathedrals are extant and even the builders' model for one of them. It was intended that the towers of Notre Dame, for example, should be crowned with spires like those of the cathedral of Lichfield, for instance. But circumstances of various sorts worked to prevent the completion of the spires on most of the great cathedrals, and they were left as towers, as at Notre Dame and on the front of York. Our own standard of beauty has been fixed by what we actually have seen, and if Notre Dame were now to receive lofty spires as a crown to its towers, most of us would find its beauty marred. Are we then to conclude that we, and not the architects of old time, are the possessors of the truest standards? Shall we say that we comprehend the beauty of a Gothic cathedral better than the builder who designed it?

It would be easy to extend comparisons from the material objects of art to the immaterial institutions of society, to contrast our notions of justice and of government with those of the ancients, for example. We are just as sure that all our social institutions are better than those of old time as we are that the towers of Notre Dame are more beautiful in their present unfinished state. But should we not pause before unthinkingly accepting either conclusion?

There is no member of a ladies' culture club in the northwest who is not ready to declare that the very essence of classic purity is to be found in the unpainted Parthenon or in an uncolored Venus, and equally sure that the constitution of the state of South Dakota contains the quintessence of political wisdom. History throws a doubt on the first conclusion and suggests that it may not be amiss to reexamine the last from time to time. 'Progress' is something more than the difference between the state of affairs on a Tuesday, compared with that of any preceding Monday. The measure of progress is not the discrepancy between the inventories—moral or material—of one epoch and of a later one. There is no space here to attempt to say what a true measure of progress might be, but it should not have been quite useless to suggest that the measure in question is not so simple as we commonly assume; that the differences between races and epochs show retrogressions in many fields as well as progressions in many others. The lesson is simple—so simple that it may even be resented. Yet it is so difficult that there is no day that passes without a proof that it is not yet learned.

EFFECTS OF IMMIGRATION ON HOMICIDE IN AMERICAN CITIES

BY MAYNARD SHIPLEY

IN his recent report for the Bureau of Immigration, Commissioner-General Sargent again calls attention to the dangers arising from the inadequate immigration laws of the United States. He contends that the time has come when some more effective restrictions must be introduced than those that have so far obtained. Although protests against prevailing legislation on immigration have been heard for more than fifty years, real cause for alarm has, perhaps, existed only within recent years. The total number of immigrant arrivals had never exceeded one half of a million during any one year previous to 1881. Since 1820, we have received 22,932,905 immigrants, an annual average of 269,798. During the fiscal year ending June 30, 1905, the total number of aliens who entered this country (exclusive of Canadian and Mexican immigrants), was 1,026,499, the largest aggregate of immigrant arrivals in any one year of our history. At this rate, we should receive during the next twenty years the same number of aliens that flocked to our shores during the past eighty-five years. As most of the newcomers of recent years have belonged to a class having neither trade nor profession, and as many of them are totally illiterate, it would seem that some very grave consequences must ensue as the result of their congestion within an area of a comparatively few square miles of the Atlantic seaboard. The attempt is now being made to transport many of them to those sections of the United States which can more readily absorb them. In just how far the success of this movement would mitigate present evils the future alone can reveal. Meanwhile, the problems arising from the presence of these alien hordes 'loom so largely in the prospect of our country,' declares Mr. Sargent, 'that it may be said without giving just cause for the charge of exaggeration, that all other questions of public economy relating to things rather than to human beings shrink into comparative insignificance.'

The great danger from this increase of immigration, however, arises rather through the change in its character than from mere increase in numbers. Once recruited mostly from the United Kingdom, Scandinavia and Germany, the greater part of our immigrant population now comes from Russia, Poland, Austria-Hungary, Bohemia, Italy and the Balkans. During the decade 1881–90, the proportion of immigration from the foregoing countries was about 20 per cent. of the total; during the ten years following, about 50 per cent. In 1902 these countries furnished about 71 per cent. of the total immigration, a proportion exceeded during the year just ended.

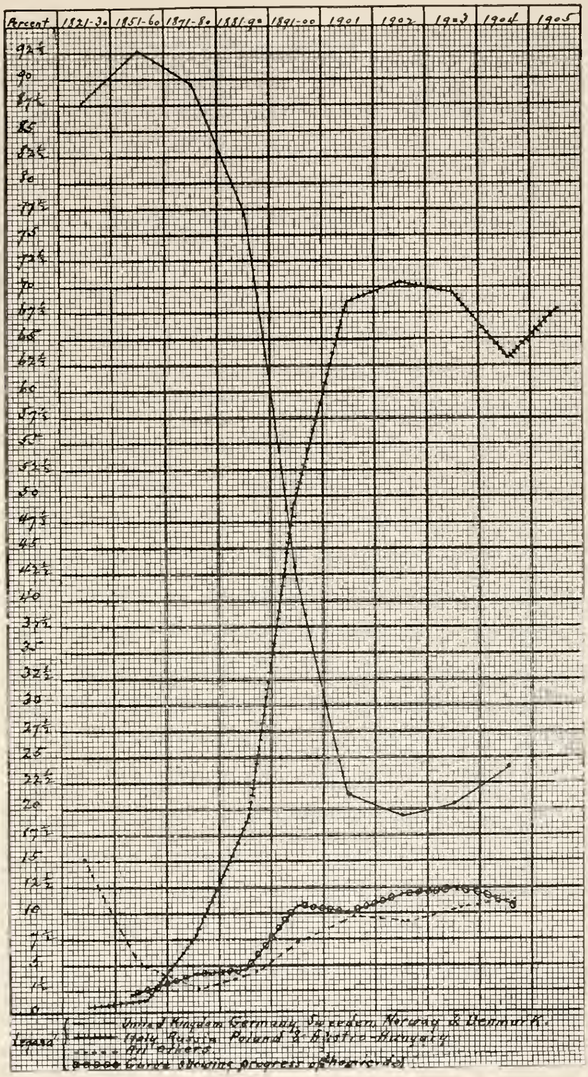


FIG. 1. DIAGRAM SHOWING CHANGING CHARACTER OF IMMIGRANT POPULATION INTO THE UNITED STATES

The accompanying figure (No. 1) shows graphically the precipitous decline of immigration from northern Europe, the vast influx from southern Europe, the proportionate increase from all other countries, and the deplorable rise in the ratio (per 100,000 of population) of deaths from homicide.[1]

The figure below (No. 2) shows graphically the source and extent of the annual immigration into the United States since 1880, the annual number of arrivals from Italy being given separately because of its special importance in the present inquiry:

The records of our penal institutions prove that it is not the alien as such who adds unduly to the number of homicides in this country; they show rather that the alarming increase in crimes of violence is due to certain particular elements of our immigrant population.

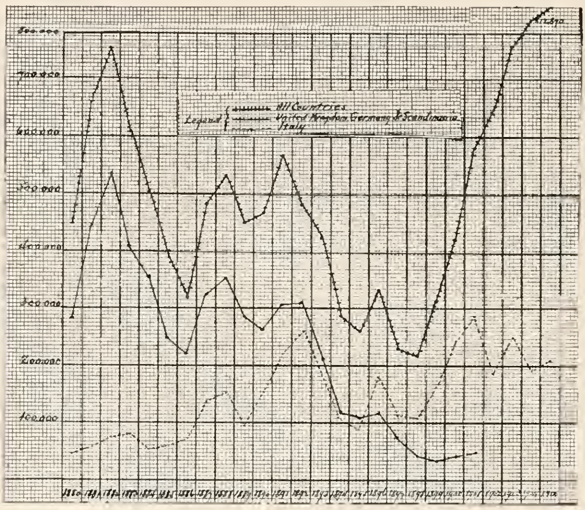


Fig 2. Graphic Curves showing Changing Character of Immigration into the United States.

Among the nationalities represented in our prisons, the sad pre-eminence for murder belongs to the alien Mexican, 121 of whom are now confined for deliberately killing a fellow man, though their total number in this country hardly exceeds one hundred thousand. Among an equal number of alien Irishmen less than three have been convicted on the charge of murder, and of German and English immigrants not more than four in each one hundred thousand are held for this grave offense. Among Scandinavian aliens the ratio is still smaller. Next to the Mexicans, the Chinese are the most murderously inclined of any foreigners present, 65 in each one hundred thousand of their

[1] The curves of immigration show the per cent. each group of race elements bears to the total influx of aliens; the curve for homicides shows the actual number of persons who met death by homicide in each 100,000 of the total population of the United States. The latter figures are based upon the mortality statistics of the United States Census Reports.

number being now held for murder. That there is danger in the vast increase of immigration from southern Italy is evidenced by the fact that of each one hundred thousand of alien Italians in this country 50.2 are held for murder. There is no question here of race prejudice, as may be seen by the following diagram (Fig. 3).

Unfortunately, the tendency of immigrants is to concentrate in large cities. The proportion of foreigners is about four times greater in the 161 cities of over 25,000 inhabitants than in the remainder of the country; hence the relation of increasing immigration to the increase of crimes of violence can be best studied through the police records of large cities. Naturally, such a task is fraught with many difficulties, owing to the want of adequate data. By confining the scope of the present inquiry to the effect of immigration on crimes involving the loss of human life, and by comparing prison statistics with the reports of chiefs of police, sheriffs and health officers, many obstacles, insuperable in any study of wider scope, are avoided.

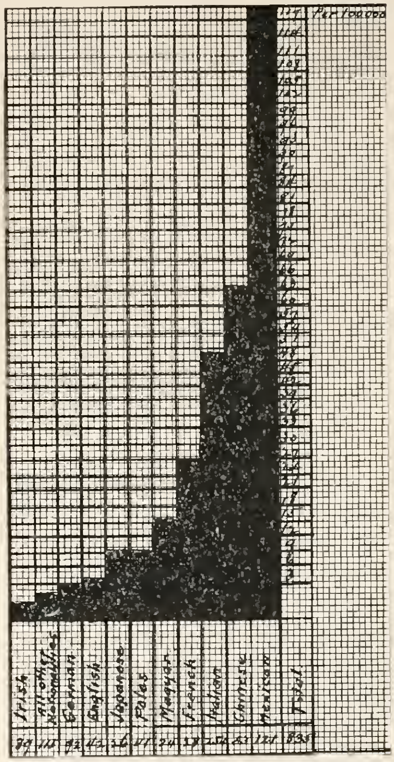


FIG. 3. TABLE SHOWING NUMBER OF FOREIGNERS HELD FOR MURDER IN AMERICAN PENAL INSTITUTIONS, WITH THEIR RATIO PER 100,000 OF POPULATION.

As the records of our penal institutions show which elements of our foreign population are most given to homicide, those cities should have the higher ratios of arrests for homicide which contain the greater proportions of aliens from the countries shown in the figure to produce the greater proportions of murderers. But there are many disturbing factors to be reckoned with, chief of which is the presence in many of our cities of large numbers of a socially inferior race of native-born citizens, the negroes. The United States Census Report on Crime, for 1880, shows that the tendency among negroes towards crimes against the person was 100 per cent. greater than among the native whites. The report for 1890 shows that the negroes had been convicted of three times more homicides in proportion to their number than had the foreign whites; whilst as compared with the native white population they appear to have been about six times more murderous. Nearly two thirds of the prisoners charged with murder were in the south.

Capable as many of the better-conditioned blacks are of attaining all the virtues of the average white citizen, and, in some instances, of transcending them, the American of African descent is, on the whole, fully as prone to homicide as are the native Indians, whose savage cruelty he does not hesitate, when excited by the lust for blood, to emulate. In Lexington, Kentucky, for instance, where 38 per cent. of the population are negroes, 13 per cent. of whom are illiterate, the annual average of arrests for murder and manslaughter during the four years 1901–04 was 40.07 per one hundred thousand of inhabitants, the highest ratio of any city of which statistics covering more than one year are available. Only 3.5 per cent. of the population are of foreign birth, hence the great number of homicides implied by these figures can not be attributed to the immigrant population.

On the other hand, in cities where the proportion of negroes is small, the higher ratios of arrests for homicide correspond with the higher percentages of immigrants from certain countries. Other things being equal, the lower ratios prevail where the foreign element is from northern Europe, and the higher ratios are seen where the foreign element is from the countries shown above to produce the emigrants who are most given to crimes of violence.

The following table shows the ratios of arrests for homicide per one hundred thousand of population in various cities, for the years 1880, 1890, 1900, and the annual average ratios for the last two or more years.[2]

Referring to the last two columns of the table above, it is seen that despite the fact that San Francisco has but a very small negro population (only 0.5 per cent. of the total inhabitants, in 1900), the annual average ratio of arrests for homicide is greater than in Louisville, Kentucky; or in Charleston, South Carolina, where the negro population is relatively large. This corresponds with the fact that San Francisco has a large number of Chinese (about 16,000) in her population; a large proportion of Italians (7,508, in 1900); about 1,500 Mexicans, and a large proportion of foreign born from various countries other than Scandinavian, Teutonic and Anglo-Saxon.

The latest report of the Sheriff of San Francisco County shows that of the prisoners committed to the county jails during the fiscal year ended June 30, 1904, 70 per cent. were of foreign birth, while the proportion of foreign born in the total population is about 34 per cent.

[2] The proportion of *homicides committed* would, of course, serve more accurately as an index of relative criminality, in respect to the crimes of manslaughter and murder, than would the number of arrests therefor. The former statistics not being available in many instances, the latter have been used as a basis for the present study. The writer's thanks are due the many chiefs of police, and others, who have so kindly assisted him in procuring the statistics of arrests for homicide in the various cities mentioned in this study.

TABLE I.

City.	Year 1880.	Year 1890.	Year 1900.	Annual average per 100,000 for the	Period.
San Jose, Cal.	15.91	55.37			
Springfield, Ill.		48.07			
Youngstown, O.	[illegible]0.10				
Lexington, Ky.	12.00	23.18		40.07	1901–04
San Francisco	13.25	19.39	25.09	19.69	1899–03
Kansas, Kan.		18.27		17.6[illegible]	1904–05
Louisville, Ky.	16.16		17.09	17.41	1900–05
Charleston, S. C.	1[illegible].00	9.0		16.12	1901–04
Trenton, N. J.		10.44			
New York	3.06	6.73	13.1[illegible]	13.23	1898–03
New Orleans	9.[illegible]5	1[illegible].3[illegible]			
St. Louis		7.0[illegible]		11.30	1904–05
Omaha, Neb.		12.10		9.7[illegible]	1901–04
Cleveland, O.		13.01		9.56	1903–04
Atlanta, Ga.		9.15			
Chicago, Ill.	2.79	4.45		9.30	1904
Memphis, Tenn.		9.30			
Newark, N. J.	1.46	1.10	6.90	9.16	1899–04
Denver, Col.				8.21	1903–05
Baltimore, Md.		6.90	8.21	7.74	1898–03
Des Moines, Ia.	13.38	9.98		7.6[illegible]	1901–04
Bridgeport, Conn.	3.61	8.18			
Jersey City	8.28	7.36			
Richmond, Va.		7.37			
Cincinnati, O.	6.66	4.01	4.29	6.00	1898–04
Hoboken, N. J.	6.45	6.94			
Washington		7.37	6.09	5.56	1902–04
Los Angeles, Cal.		13.88		4.86	1901–04
Springfield, Mass.		4.[illegible]2			
Philadelphia		0.76	2.74	4.93	1899–04
Indianapolis		4.74			
Patterson, N. J.	3.91	1.27		4.51	1901–04
Savannah, Ga.		4.6[illegible]			
Hartford, Conn.		13.15		2.17	1901–04
Buffalo, N. Y.	2.60	3.52		2.93	1902–04
New Haven	1.59	[illegible].4[illegible]		4.16	1901–04
Scranton, Pa.	2.18	2.65			
Albany, N. Y.		[illegible].10			
Boston	1.93	1.33		1.98	1904–05
Milwaukee, Wis.	0.86	0.97	1.0[illegible]	1.77	1898–04
Providence	0.95	2.[illegible]7		1.70	1901–04
Rochester	2.2[illegible]	0.74	0.00	1.53	1900–03
Minneapolis	2.13			0.49	1904
St. Paul		1.[illegible]0			

RATIO OF ARRESTS FOR HOMICIDE PER 100,000 OF POPULATION IN VARIOUS CITIES OF THE UNITED STATES.[3]

The table following, compiled from the mortuary statistics of San Francisco, shows how large a proportion of the homicides committed occur among foreigners:

[3] The annual average ratios of arrests for homicide during the last period given are based upon the population statistics of 1900. Due allowance must therefore be made for increase of inhabitants in some cases.

Table II.

Year.	Total Homicides.	Number of Certain Classes killed.			
		Foreigners.	Caucasians.	Mongolians.	Unknown.
1889	23	15	15	8	
1890	28	15	24	4	
1891	24	16	18	6	
1892	23	19	14	9	
1893	22	12	15	7	
1902	43	17	31	12	
1903	46	19	24	11	1

Number of Homicides reported by the Health Officer of San Francisco, during the Years given.

The figures above show that of the 199 persons who met death by homicide in San Francisco during the five years 1889–93 and the two years 1902–03, 111, or 55.6 per cent., were of foreign birth, while the total foreign born of the city form but 34.1 per cent. of the population. The undue proportion of murders among the Chinese is easily appreciated when it is stated that the Mongolians comprised 28.6 per cent. of the number who met death by homicide, while they form less than 6 per cent. of the population. The mortality statistics of San Francisco show that during the twenty-five years 1872–97, there were 469 Chinese murdered, an annual average of 18.76. From these figures it may readily be seen that the very high ratios of arrests for homicide in San Francisco are largely attributable to her alien Chinese population. Consulting the District Attorney's Report for 1897 (a report which happens to be available), I find that 41 per cent. of the charges filed for murder and manslaughter during that year were against Chinese.

The table following shows that the annual proportion of arrests for homicide and attempts at murder has varied but little during the years given (omitted years imply missing or unavailable reports).

That the ratio of arrests for homicide has not grown larger in San Francisco concurrently with the general increase of immigration from southern Europe, coincides with the fact that the proportion of Chinese in that city is growing smaller, and that the great influx of European aliens has not yet affected the Pacific coast to any considerable extent. Of the 1,025,000 immigrants who landed on our shores during the fiscal year ending June 30, 1905, at least 76 per cent. went into New York, Massachusetts, Pennsylvania, New Jersey, Ohio and Illinois. Only 431,571 were destined for the west, and but 46,343 for the south.

This tendency of the alien to congest in the great manufacturing cities of a few eastern states is gradually reversing the order of conditions heretofore existing. Twenty years ago the population of Pacific coast cities was much more largely foreign born than at the present time, while in the east the percentage of foreign born in urban communities is increasing. Ten and twenty years ago, as may be seen

TABLE III.

Year.	Population of City.	Murder.				Attempts at Murder.				Manslaughter.			
		No. of Arrests.	No. Released.	No. Held.	Arrests per 10,000 Population.	No. of Arrests.	No. Released.	No. Held.	Arrests per 10,000 Population.	No. of Arrests.	No. Released.	No. Held.	Arrests per 10,000 Population.
1850	34,776												
1860	56,802												
1870	149,473												
1874	183,267	21	9	12	1.1513	70	22	48	3.8195	6	4	2	0.3273
1880	233,959	29			1.2395	134			5.7275	2			0.0855
1881	240.462	22			0.9149	116			4.8240	12			0.4991
1883	253,470	23			0.9074	87			3.4323	22			0.8679
1885	266,478	35	10	25	1.3134	106	53	53	3.9777	24	23	1	0.9006
1886	272,981	20	3	17	0.7326	112	60	52	4.1028	13	10	3	0.4762
1887	279,485	36	9	27	1.2881	137	78	59	4.9018	24	22	2	0.8587
1888	285,989	30	6	24	1.0489	102	59	43	3.5665	17	13	4	0.5944
1889	292,4[illegible]3	33	16	17	1.1282	132	57	75	4.5129	25	22	3	0.8547
1890	298,997	44	9	35	1.4712	98	48	50	3.2776	15	13	2	0.5017
1891	303,375	26	5	21	0.8570	101	51	50	3.3292	16	16		0.5274
1892	307,754	19	3	16	0.6173	100	39	61	3.2493	10	9	1	0.2924
1893	312,132	30	11	19	0.9611	92	54	38	2.9474	11	6	5	0.1922
1894	316,511	34	13	21	1.0742	107	57	50	3.3806	19	15	4	0.4739
1895	320,889	22			0.6855	122			3.8019	21			0.6544
1896	325,268	32			0.9838	131			4.0274	17			0.5226
1897	329,646	26			0.7887	97			2.9425	17			0.5157
1900	342,782	41			1.1961	104			3.0339	45			1.3128
1901	347,161	19			0.5473	110			3.1685	36			1.0369
1902	360,000	29			0.8056					44			
1903		21								49			
1904	450,000	48			1.0666					52			1.1556
1905	467,500												

SAN FRANCISCO POLICE-COURT RECORD—1874–1904.

by Fig. 4, murders were far less frequent in proportion to population in eastern cities than they are now. In the west the reverse is true, because the immigration from southern Europe has had little influence on the growth of Pacific coast cities. In Los Angeles, where the proportion of native born of native whites is exceeded by only four cities (of 100,000 or over) in the United States, the ratio of arrests for homicide has greatly diminished, falling from 13.88 per 100,000 of inhabitants in 1890, to an annual average of 4.86 during the four years 1901–04.

Passing now to the Atlantic seaboard, we find in the city of Newark, New Jersey, a conspicuous example of the effect of immigration on homicide in a city which owes much of its recent growth to alien settlers. In 1900, 29 per cent. of Newark's population was foreign born, the foreign-born Italians alone numbering 8,537, since which year this element of her population has greatly increased.

In 1880, when the Italian population of Newark was very small, the ratio of arrests for homicide was but 1.46 per 100,000 of inhabitants; in 1890 the ratio was even lower, being 1.10 per 100,000 of population. During the decade following, the immigration from Italy, Poland, Russia, Austria-Hungary and the Balkans greatly increased, and Newark got her full share of all these elements. The police reports of the period following show an increase in the ratio of crimes of violence in general, while the ratio of arrests for homicide in 1900 was five times greater than in 1890. That this great increase was not merely the result of an unusual year is shown by more recent statistics. The annual average ratio of arrests for homicide in Newark for the six years 1899–1904 was 9.16 per one hundred thousand of population; this is on the basis of the population of the census of 1900, more recent statistics not being available.

In New Haven, Connecticut, where the Italian population has greatly increased since 1890, the ratio of arrests for homicide has about doubled during the past six or eight years. In 1880, the ratio of arrests for murder and manslaughter in that city was 1.59 per 100,000 of inhabitants; in 1890, 2.46; and during the four years 1901–04 there were 4.16 arrests per 100,000 of population on the charge of killing a fellow man. This ratio is still quite low, compared with some cities of the east, but the increase is very significant, especially in view of the fact that the second generation of foreigners is always more criminally inclined than the first.

The relation of immigration to the proportion of crimes of violence in urban counties is well exemplified in the various large cities of the Empire State.

In the city of New York, with its million and a half of foreigners, constituting as they now do more than 37 per cent. of the total population, and nearly half of whom are from countries other than those of northern Europe, we find very favorable conditions for a comparatively high ratio of crimes of violence. Consulting the police statistics of New York City, I find that the ratio of arrests for homicide has increased more than fourfold since 1880, rising from 3.6 per 100,000 in that year, to 6.7 in 1890, and 13.12 in 1900. The annual average ratio for the six years 1898–1903 was 13.23 per 100,000 of inhabitants. There are no statistics available showing the proportion of these homicides which were committed by New York's immigrant population. Many of the journals of the great metropolis charge the Italians (of whom there are now about 400,000 in that city) with a large share of the murders committed; and when it is considered that, as shown in a preceding table (No. II.), one out of every 1,906 Italians in this country is held for murder, it is apparent that the charge is not without foundation. Some idea of the problems arising from such a vast influx of aliens as the past few years have witnessed is derived from the records of New York's Children's Court. Of the 7,647 children arraigned during the year ending December 31, 1904, more than half were born in Italy or Russia or of parents born in those countries.

"Practically all of the material brought into the Children's Court," says Mr. Coulter, "is a gift from Europe."[4]

Passing now to the cities which have received few immigrants from southern Europe, we find that the ratios of homicides have changed very little, and that they are uniformly low.

In the city of Buffalo, where at least 73 per cent. of the foreign born population are from northern Europe, the ratio of crimes of violence has remained almost stationary for thirty years. In 1880 the ratio of arrests for homicide was 2.60 per one hundred thousand of inhabitants; in 1890, the proportion was 3.52; while the annual average for the three years 1902–04 was 2.93.

In Rochester, where 85 per cent. of the foreign-born inhabitants are from the United Kingdom, Canada, Germany and Scandinavia, homicides have decreased relative to the population, the ratio of arrests for murder and manslaughter being, in 1880, 2.23 per one hundred thousand of population; in 1890, 0.74; while the annual ratio for the four years 1900–03 was 1.22.

In Syracuse, where 77 per cent. of the population are native whites, and where about 82 per cent. of the foreign-born are from the countries of northern Europe, but six cases of homicide have come under jurisdiction of the police department during the past fourteen years.

Passing south now to the Quaker City, we find that an arrest for homicide in Philadelphia, fifteen years ago, was of very rare occurrence (0.76 per 100,000 of inhabitants in 1890). Since that time the population of Philadelphia has increased by about 250,000, a large proportion of which augmentation has been the result of the increased immigration from southern Europe. Of the 295,340 foreigners in Philadelphia in 1900, 17,830 were born in Italy, 7,554 in Poland; 28,951 were natives of Russia, while 8,209 were born in Austria-Hungary or Bohemia; aggregating in all, from these countries, 62,544 immigrants of a much lower type, industrially and educationally, and, by inference, morally, than formerly migrated to this country. The results are shown in the records of the police courts, the ratio of arrests for homicides having increased fourfold since 1890, the annual average for the six years 1899–1904 being 4.93 per 100,000 of inhabitants.

That crimes of violence increase with the changing character of the immigrant population is shown clearly by the police statistics of the cities of Cleveland and Cincinnati. In the former city the annual average of arrests for homicide during the two years 1903–04 was 9.56 per 100,000 of population. In Cincinnati the average for the six years 1898–1904 was 6.23.[5] In 1890 the disparity was still greater, the ratio being 4.04 in Cincinnati, and 13.01 in Cleveland. The annual average

---

[4] 'Alien Colonies and the Children's Court,' *North American Review*, November, 1904, Vol. 179, No. 5.

[5] The average for the same period in Cleveland is not available.

of arrests for murder and manslaughter in Cincinnati during the nine years, 1880, 1890 and 1898–1904 (as shown in the foregoing table), was 5.56; for Cleveland during the three years for which statistics are available, the annual average was 10.71 per 100,000 of inhabitants. The causes of this disparity may readily be found in the relative character and proportions of their foreign population. The United States census for 1900 shows that whereas in Cincinnati 35 per cent. of the inhabitants were native-born of native parents, in Cleveland only 23 per cent. belonged to this class. In Cleveland the foreign-born formed 46.1 per cent. of the population; in Cincinnati but 17.8 per cent. were foreigners. It is also worthy of notice, in this connection, that whereas about 37 per cent. of Cleveland's foreign-born population were from southern Europe, in Cincinnati the same countries furnished only about 11 per cent. of the foreign-born inhabitants. In 1903, 13,651 immigrants settled in Cleveland; and during the same year there were 23 murders, 34 stabbing affrays, 45 shooting affrays, 56 cutting affrays, while 131 persons were assaulted. In 1904, over seven thousand immigrants arrived and settled in Cleveland, among whom were 1,464 Italians, 1,098 Hungarians, 1,637 Poles, 826 Russians, 390 Slavonians, 133 Roumanians and 30 Croatians. During this year there were 30 stabbing affrays, 51 shooting affrays, 6 murders, 91 cutting affrays, and 121 persons assaulted. As to how many of these crimes were perpetrated by foreigners is not given in the police statistics, but in his report the Chief of Police remarks that—"While the records show a large number of the arrested to be of foreign birth, it by no means follows that our foreign-born element is a criminal element, but that our city is a cosmopolitan one and our extensive manufacturing interests have drawn a large laboring class to the city." In the report of the Chief of Police of Cincinnati, the number of foreign-born among those arrested is given. The report for 1904 shows that among the 7,135 white persons arrested in Cincinnati, 4,437, or 64.04 per cent. were foreigners, though the foreign-born in that city, in 1900, formed but 18.61 per cent. of the white population. The total number of persons arrested for murder during the year was 31. In respect to other offenses, 68 were arraigned for 'cutting with intent to kill'; 49 were arrested for 'shooting with intent to kill.' There were also 146 arrests for 'cutting,' and 61 for 'shooting' a fellow man (with what 'intent' in these cases is not stated). While a large proportion of these crimes of violence were undoubtedly committed by the foreign whites, it must also be taken into consideration that Cincinnati has a large negro population, numbering, in 1900, 14,482; and that while they comprised but 4.4 per cent. of the total population, the number of negroes arrested during the year numbered 2,822, or 19.60 per cent. of the total population of the city.

In the table given above showing the ratio of arrests for homicide in various cities, per 100,000 of population, it appears that the proportion of crimes of violence has not increased in Cincinnati during the past twenty-five years. The following figures show that they have at least increased in numbers. The police-court records show that during the twenty-one years 1884–1904, there were 335 arrests for murder in Cincinnati, an annual average of 15.95. During the ten years 1895–1904 the annual average was 17. The annual average for the four years 1901–04 was 21.

In Minneapolis, where nearly 90 per cent. of the foreign-born population are from northern Europe, and where there are neither Chinese, Italians, nor negroes, homicides are proportionately rare, the ratio of arrests for murder and manslaughter being in 1880, 2.13; and in 1904 less than one per one hundred thousand of inhabitants. In Milwaukee, where over 60 per cent. of the foreign population are Germans, the annual average ratio of arrests for homicides during the seven years 1898–1904 was 1.77 per one hundred thousand of inhabitants. This is more than twice the proportion occurring in Minneapolis; but Milwaukee has about twice the proportion of immigrants from southern Europe that Minneapolis has. In 1880, when there were comparatively few immigrants in Milwaukee from Italy, Poland, Russia or the Balkans, the ratio of arrests for homicides was 0.87 per one hundred thousand of population, and, as late as 1890, the ratio was but 0.97.

Although there is no city in the state of Kansas ranking in population with the cities here studied, for the sake of its instructive example the city of Kansas, as the largest city in the state, may be given special mention. During the two years ending October 31, 1905, the annual average of homicides (not the number of *arrests* therefor) was 17.64 per one hundred thousand of inhabitants. An inquiry addressed to Chief-of-Police Vernon J. Rose, evoked the reply that at least four fifths of these homicides were committed by the immigrant and negro population, who comprise, together, over 25 per cent. of the total population of the city. About 40 per cent. of the 9,000 employees of the six great packing houses are natives of Russia, Greece, Poland, Croatia, Bulgaria and Hungary, and Chief-of-Police Rose states that it is among these 'Bohunks' (the local term which comprehends these races as a group) that nearly all of the homicides among the whites occur.

In Providence (R. I.), where the Italians comprise more than ten per cent. of the foreign population, and where 38.1 per cent. of the total inhabitants are of foreign birth, the ratio of arrests for homicide is lower than in Boston, the annual average of arrests for homicide in Providence being, for the four years 1901–04, 1.70; and for Boston (during the two years 1904–05) 1.98 per one hundred thousand of inhabitants. But in Rhode Island, as a whole, the proportion of population held for homicide is greater than in Massachusetts, the ratio for the former state being 0.57 per one hundred thousand of inhabitants, and for the latter 0.39. In Rhode Island the Italians comprise 11.58 per cent. of the convicts held for homicide, and but 2.09 per cent. of the total population. In Massachusetts the Italians form but 1 per cent. of the total population, and 26.1 per cent. of the convicts held for homicide.

Next to Nevada, Colorado has the highest ratio of deaths from violence of any state or territory in the continental Union. But the ratio of arrests for murder and manslaughter in her chief city, Denver, is comparatively low, the annual average ratio for the three years 1903–05 being 8.21 per one hundred thousand of inhabitants. It is a significant fact that there are but five cities of her class in the United States which have a larger proportion of native white inhabit-

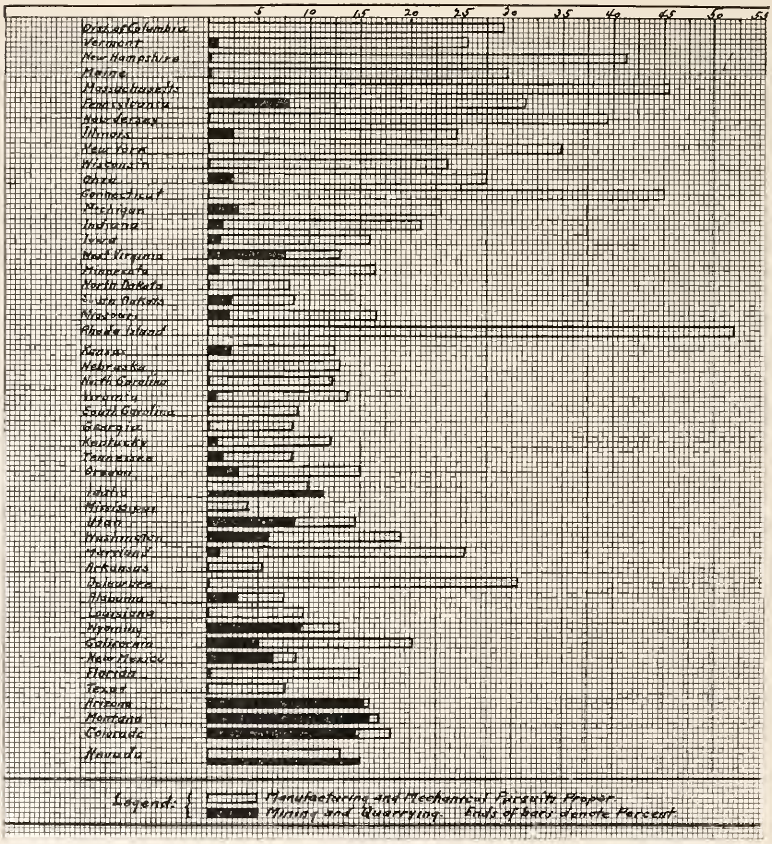


Fig. 4. Per cent. which the Total Number of Persons of Both Sexes engaged in Manufacturing and Mechanical Pursuits Proper and in Mining and Quarrying bears to the Total Number engaged in Gainful Occupations, by States and Territories, 1900. (States and Territories are given in the order of their progressive criminality in respect to homicidal crimes.)

ants born of native parents. Only 19 per cent. of Denver's population are foreign born.

The question may well be raised: Why has the state of Colorado so high a ratio of deaths from violence (52.5 per one hundred thousand of population), and Denver so few arrests for crimes of violence? It may be replied that homicide is not naturally frequent in urban communities, but prevails in sparsely settled regions, or in mining districts. In general, homicides decrease as the proportion of persons engaged in manufacturing increases, and increase as the proportion of persons engaged in mining increases.

The figure above illustrates this principle, the states and territories being arranged according to the annual average of deaths from homicide during the decade 1890–99, the District of Columbia having the smallest number of deaths from violence, and Nevada the highest

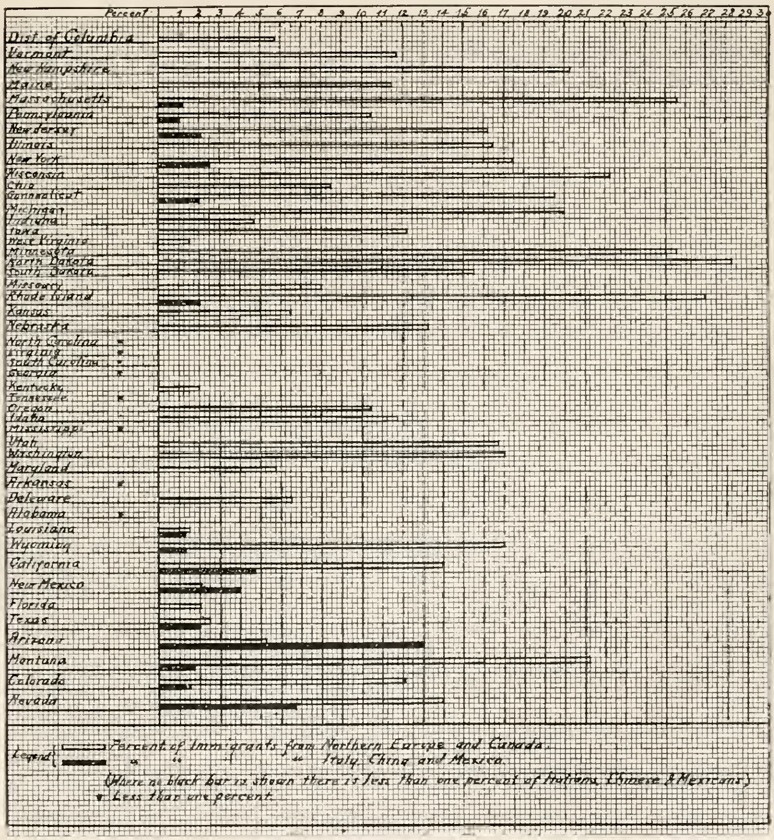


FIG. 5. SCALE SHOWING PER CENT. OF FOREIGN-BORN POPULATION (from Northern Europe, United Kingdom, Norway, Sweden, Denmark, Germany), and from the Dominion of Canada, by States and Territories, arranged according to their progressive criminality in respect to annual average homicides committed therein during the decade 1890–1899.

ratio per one hundred thousand of population. (The ratios were computed on the basis of statistics of crimes of violence compiled and published by the *Record-Herald*, of Chicago.) The white bars show the proportion of the population who, according to the United States census for 1900, were engaged in manufacturing pursuits, and the black bars indicate the percentage engaged in mining and quarrying:

It will be observed that Pennsylvania and West Virginia are the only states in the Union which are engaged extensively in mining, and yet have a comparatively low ratio of homicides. This is largely due to the fact that the mining districts in these states are adjacent to well-populated and comparatively cultured communities, whereas in the West, the mines are situated in states or territories which contain few or no large cities, and wherein the rural population is of a rather low order. Colorado, for example, has but one large city, and is one of the five most sparsely settled states or territories of the (continental) Union; whereas Pennsylvania, on the other hand, has more towns of over 4,000 population than any other state, giving it the highest per cent. of urban population of any commonwealth in the Union with the exception of New York. Again, whereas West Virginia has 38.9 persons to the square mile, Nevada has 0.4.[6]

Consistently with all that has herein been stated, we find the greater percentages of foreign-born who are most given to crimes of violence in the very states shown to produce the greater proportion of homicides, and most of which are engaged most extensively in mining, as may be seen by comparing Fig. 7 with the one preceding.

Not wishing needlessly to multiply examples and evidences, it may be said in conclusion that, however desirable the hundreds of thousands of ignorant immigrants annually landed on our shores may be from an economic standpoint, as 'much-needed laborers,' or, however charitably we may personally feel toward the hordes of hapless human beings who seek to better their condition by coming to this land of freedom and opportunities, such a vast addition of untutored and poverty-stricken people, unused to self-restraint, can not be absorbed without a material increase in crimes of violence throughout the United States, and especially in the large cities, where the recent immigration has for the greater part congested. It is to be hoped that the evidences of the Children's Court of New York City, and of police statistics in general, are symptoms rather of conditions to be remedied than of evils destined to grow more portentous.

[6] The Report of Warden C. E. Haddox, of the West Virginia State Penitentiary, for 1903–04, shows that the five counties in which mining industries predominate, with a total population of 139,812, sent 419 persons; while sixteen other counties, whose population is engaged in agriculture or other equally stable pursuits, numbering in all 205,175 persons, are represented by 28 convicts. In the mining counties one person in every 333 was sent to the penitentiary; in the sixteen counties mentioned, one in each 7,327 of population was sent to prison—a difference as great as 300 is to 13.

THE STUDY OF THE VARIABLE STARS

By Professor SOLON I. BAILEY,

HARVARD COLLEGE OBSERVATORY

THE ancient philosophers taught that the celestial bodies were 'incorruptible and eternal,' not subject to change, as are all terrestrial objects. In more recent times the stars were regarded merely as convenient points of reference for the determination of the motions of the planets. In this way they became known as the *fixed stars.* Relatively, they are indeed fixed; absolutely, all are in motion. Their light remains constant, also, for the most part, so that, if Hipparchus or Ptolemy should come back to earth after 2,000 years, he would probably notice few changes in the positions or brightness of the stars.

Any one who observes the sky carefully, through a period of years, is sure to be deeply impressed with the absence of change. Nevertheless, there are many stars which undergo more or less regular changes in brightness, and such objects are known as variable stars. In some cases the whole cycle of change takes place within a few hours, while in other cases it consumes months, or even years. The amount of the variation, also, varies enormously, ranging all the way from zero to many magnitudes, how many is not known. It is possible, even probable, that at minimum the light of some variable may, for us at least, be entirely extinguished. Mr. J. A. Parkhurst found that the variable V Delphini was invisible at its minimum of 1,900 in the forty-inch refractor of the Yerkes Observatory. This, it is estimated, would make it fainter than the seventeenth magnitude. Since its light at maximum is of about the seventh magnitude, this implies a range of at least ten magnitudes. Other stars vary as much or more. A change of ten magnitudes means that at maximum its light is 10,000 times as great as that at minimum. To illustrate this we may imagine a room illuminated by 1,000 ten-candle power electric lamps, and that these are replaced by the light of a single candle. To reduce the light of our sun by ten magnitudes would be equivalent to increasing its distance 100 times, or to more than 9,000,000,000 miles. At such a distance its apparent size would be less than the present mean size of Jupiter or Venus. Fortunately our sun, if a variable star as seems probable, has a small range of variation.

The general problem of variable stars may be divided into three parts—the discovery of the variables, the observation of all the phenomena involved, and the search for the causes. The present generation, thanks to the powerful aid of photographic methods, may hope to bring near completion the first part of the problem, and to make good progress on the two remaining portions.

The existence of a variable star was probably first recognized by Holwarda of Franeeker, in 1639. The number was slowly increased, and some knowledge of their nature learned during the next two centuries. Their observation was placed on a scientific basis through the labors of various astronomers, especially Argelander and Schönfeld. The latter astronomer issued, in 1865, a catalogue of 113 variables, and later, one of 165 variables, which included all stars then known to be variable. The list was enlarged, in 1883, at the Harvard Observatory by the addition of forty-eight variables. In 1888 Dr. S. C. Chandler published his first catalogue of variable stars, 225 in number, which had been discovered by some thirty different observers in various countries, by visual methods. Many of these observers have continued their investigations till the present time—the most successful in the line of discovery being Dr. T. D. Anderson, of Edinburgh, who by visual means alone has found forty new variables, a result truly remarkable.[1]

About the year 1889, however, began a rapid increase in the number of variables through the introduction of photographic methods. The first notable addition was made by Mrs. Fleming, through the examination of the photographic spectra of the stars, while engaged in the work of the Henry Draper Memorial, a research carried on at the Harvard Observatory under the direction of Professor E. C. Pickering. By means of an objective prism, placed in front of the lens of a photographic telescope of large aperture and short focal length, photographs

[1] Although the subject of variable stars is now under investigation at many observatories, there is still a wide field in this line of research for amateur astronomers. It is true the light-curves of many variables are now fairly well known, but new ones are constantly being discovered, the study of which offers an interesting field of investigation. It is necessary, in order to accomplish results of scientific value, that the observations be made, not only with enthusiasm, but with an intelligent conception of the future use to which they must be put. The observations need to include only two things, a record of the time, and the most precise determination possible of the brightness of the variable. The estimate of magnitude is usually made by referring the light of the variable to that of one or more adjacent stars, whose light is constant. For this purpose a series of adjacent comparison stars is selected, forming a sequence from bright to faint stars, and their brightness is carefully determined. It is very important that these magnitudes be reduced to the photometric scale. For identification of the stars the star charts of Father Hagen are admirable. Marked photographs are also extremely useful.

The discovery of new variables offers, perhaps, a line of work even more fascinating than the investigation of the peculiarities of those already found. Brilliant work has been done in this direction by amateurs, but at the present time much more can be accomplished by photographic than by visual means. Among those who have done work of special value, in this country, may be mentioned Chandler, H. M. Parkhurst, J. A. Parkhurst, Sawyer and Yendell. Abroad, the number of amateur observers is large.

were obtained which showed well the characteristics of scores of spectra on a single plate. Variable stars of long period were found to have spectra in which the hydrogen lines were bright, when the variables were near maximum. By taking advantage of this spectral peculiarity Mrs. Fleming has been able to 'pick up,' as a by-product of other investigations concerning stellar spectra, some 200 variables of long period.

In 1895, the writer, while engaged in photographic work at the Arequipa Station of the Harvard Observatory, began an examination of photographs of the globular clusters of stars. By the use of improved devices for controlling the motion of the telescope, satisfactory photographs were obtained of the dense globular clusters. An ex-

The Globular Cluster, ω Centauri, containing 128 Variable Stars. To the naked eye this cluster appears as a single hazy star of the fourth magnitude.

amination of these led to striking results. It was found that while certain clusters contained few or no variable stars, other similar clusters were closely packed with them. Messier 3, a faint group, barely visible as a hazy star to the naked eye, was found to contain 137 variables out of 900 stars examined, or about one in every seven stars. This is by far the greatest proportion of variables yet found anywhere in the sky. Over 500 variable stars have been found so far in dense globular clusters, and, undoubtedly, these do not entirely exhaust the number.

Madame L. Ceraski, wife of the director of the astronomical observatory of Moscow, has found a large number of variables by an examination of photographs made by M. Blajko, of the same observatory. Madame Ceraski has been especially successful in finding variables of the interesting Algol type. Of sixty-seven variable stars discovered by her, no less than ten are of this class. This is remarkable when we take into consideration that of over 3,000 variables now known only thirty-eight are of the Algol type.

Through her generous gifts in aid of astronomical research, the late Miss Catherine W. Bruce, of New York, made her name widely known in astronomical circles. Dr. Max Wolf, director of the Astrophysical Observatory at Heidelberg, was presented by her with a photographic telescope, which has enabled him not only to find some seventy new asteroids, but also to increase materially the number of known

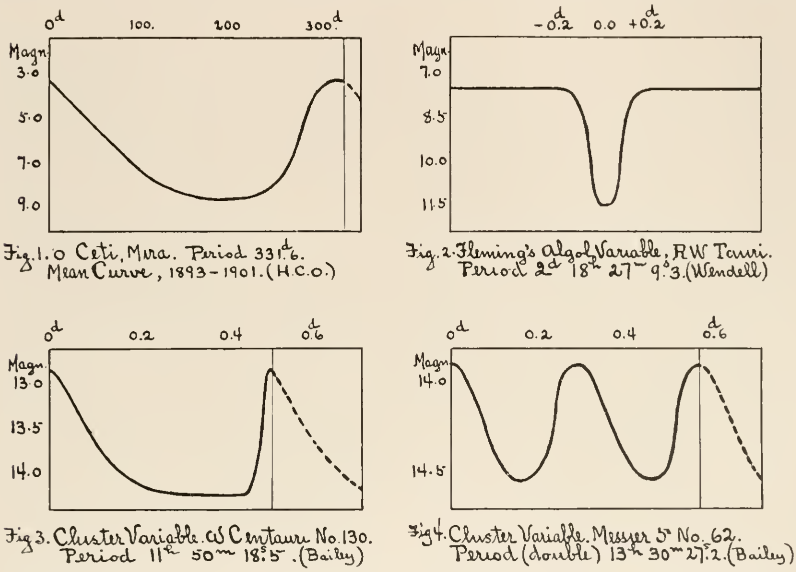
The Small Magellanic Cloud in which nearly a Thousand Variable Stars have been found. To the right is the globular cluster, 47 Tucanæ, taken at Arequipa in the bruce telescope.

variables. Dr. Wolf, recently assisted by Frau G. Wolf, has discovered about 200 new variable stars.

Nowhere else, however, has so large a collection of celestial photographs been made, covering so long a period of time, as at the Harvard Observatory. In 1903, Professor Pickering instituted, among other pieces of work, an examination of the Magellanic Clouds. This work was assigned to Miss H. S. Leavitt, who has shown rare talent for this line of investigation. The regions selected were very fortunate, also, since, aside from the dense globular clusters, no other region has been found as rich in variables as the Small Cloud, although the Large Cloud also promises to yield nearly as many. It should be noted that the Magellanic Clouds are by no means merely irregular extensions of the Milky Way. They appear to be as unique in structure as in position.

Altogether Miss Leavitt has added 1,500 new variables to the already rapidly growing list.

It may be asked, why it is necessary, or even desirable, to go on indefinitely with the discovery of new variables. The answer is that, aside from the value of adding any new fact about the universe to the sum of human knowledge, the problem is now so well advanced that it seems unwise not to render the search complete for the whole sky. A serious international attempt is about to be made, for the first time, to investigate systematically all the leading problems concerned in the construction of the universe, so that a scientific cosmogony may be possible. It will be of value in this greatest of all problems to find



VELOCITY CURVES AND LIGHT-CURVES OF VARIABLE STARS. (The light-curves are inverted.)

the discussion of the variable stars reasonably complete for the whole sky. At the Harvard Observatory, where variable stars have been given serious attention during more than twenty years, a new catalogue, compiled by Miss Cannon, is in course of publication, which contains reference to about 1,850 variables. This does not include the variable stars in the Magellanic Clouds. Also, a committee of the *Astronomische Gesellschaft*, consisting of the well-known astronomers, Dunér, Müller, Oudermans and Hartwig, have in hand the preparation of a catalogue, which will be an extension of the former catalogues of Chandler.

The work of discovery, however arduous, is but a small part of the whole problem of the variable stars. Long series of observations are necessary, in order to learn the amount, duration and rapidity of the light-changes, or, in other words, to determine the light-curve.

Recent advances in methods of research have also made possible the study of various other phenomena, in addition to the variability in brightness. All available information will be needed to assist us in finding the true explanation of the changes. Especially must we study the spectra of these stars, and the changes in the spectra at different phases of the light-curve, as well as the motions of the stars in the line of sight. For a long time it has been known that the radial motion of any bright body may be studied from the shifting positions of the spectral lines. This principle is proving of great importance in different branches of astronomy. Only recently, however, and in few cases has this crucial method been applied to the problem of variable stars; yet it appears that the true solution of the difficulties must await, in many cases, the application of this method of research.

The determination of these different phenomena—the light-curve, the velocity-curve and the spectrum—is often carried on without special reference to the physical causes which produce them. But it

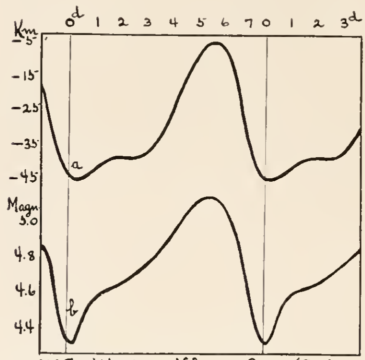


Fig. 5. W Sagittarii. a. Velocity-Curve (Curtiss) Period 7.5. b. Light-Curve (Pickering)

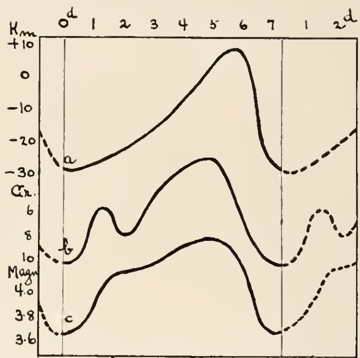


Fig. 6. η Aquilae. a. Velocity-Curve (Wright) Period 7.176. b. Light-Curve (Schur) c. Photometric Curve (Pickering)

Light-curves of Variable Stars.

will be convenient in what follows to refer to the phenomena and the probable causes together. No final classification of variable stars is possible at the present time, since such a classification would doubtless be based on the physical causes which underly the phenomena, and these are known in comparatively few cases. The division proposed by Professor Pickering, in 1881, is as convenient as any for our purposes. He placed them all in the following classes:

I. New stars.

II. Long-period variables, undergoing great variations in light.

III. Stars undergoing slight changes, according to laws as yet little understood.

IV. Short-period variables of the β Lyræ type.

V. Algol stars.

Reference will be made in what follows only to classes II., IV. and V. New stars may well be considered as a class apart. There is, possibly, no sufficient reason for including them among variable stars, technically so-called. The stars of class III. are few, doubtful, not well understood, and relatively unimportant.

II. Variable stars of long period and large variation in light are perhaps the easiest to observe and the most difficult to interpret of all. Many of them are bright enough to be observed, near maximum at least, by the naked eye, and the variations are so great that observations of the highest precision are not essential for the determination of the light-curves. The length of period ranges in general from 100

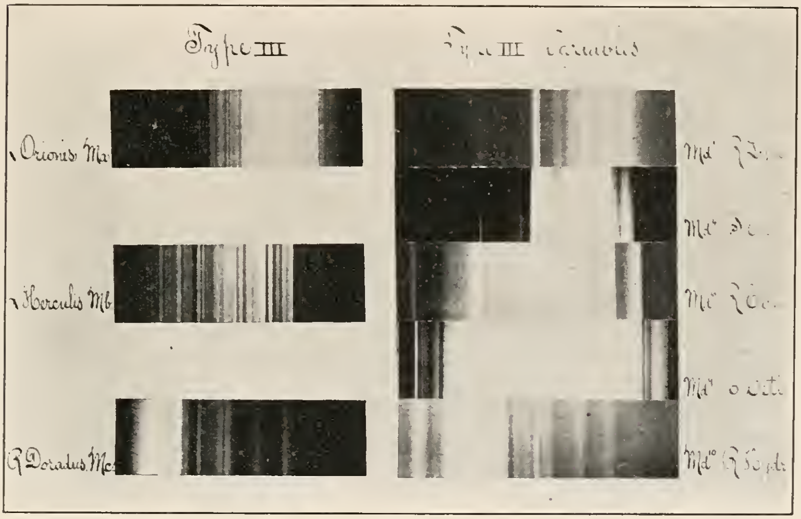


FIG. 7. STARS OF TYPE III. AND VARIABLE STARS OF TYPE III. WITH BRIGHT HYDROGEN LINES.

days to 400 days. Some of these stars have been observed by different astronomers during the last two centuries, and elaborate investigations concerning them have been made by Argelander, Schönfeld, Chandler, Pickering, and others. Omicron Ceti, or *Mira*, The Wonderful, has been studied more carefully than any other. Even here, however, much remains to be learned. The light-curve of Mira is shown in Fig. 1, and is fairly typical of the group. The variations in brightness are irregular and a single light-curve can only represent mean results. Irregularity characterizes all the phases; the exact time of any return of maximum is uncertain, and the brightness at different maxima, and at different minima, varies greatly.

The spectrum of stars of this class is in general of Secchi's third type, with heavy banded lines and flutings. A short time before maximum the bright lines of hydrogen appear, and persist till the star has grown somewhat faint again. At least, this is true of Mira, and of some others, and is perhaps true for all. These bright lines, due to incandescent hydrogen, undergo various modifications during the time in which they are present. The relative intensity of different lines varies greatly in different stars, and also in the same star at different phases. Mrs. Fleming has been able to arrange them in a series having ten subdivisions, with R Lyncis at one end, with the H β and H γ lines prominent and H δ wanting, and R Leonis at the other end with H β wanting, H γ faint, and H δ prominent. There are also corresponding changes in the distribution of the remaining light of the spectrum, a peculiarity which is shared by stars of the same type which are not variable. These characteristics are well shown in Fig. 7.

Their great range of variation makes many of these stars invisible when near minimum in telescopes of ordinary size. This may account for the custom which has been followed by many observers of measuring the light only when the star is near maximum. This is unfortunate, since the determination of the length of the period is not sufficient in itself for the solution of the problems involved. On this account special efforts have been made at the Harvard Observatory, where the observations are carried on by Miss A. J. Cannon and Mr. Leon Campbell, to get measures of the variables at all the different phases. Even thus it is doubtful if the secrets of the changes can be found, until the research is made to include a more detailed spectroscopic study than has yet been made. A systematic study of a large number of well-selected stars is much needed. This could probably be done best by a photographic reflector of the largest size. Such a scheme of work has been proposed by Professor W. W. Campbell, director of the Lick Observatory, and from it we may expect results of the highest value. It may be well, also, to study the radial motions of these variables, but it is more than doubtful if their variability is in any way associated with orbital motion, such as would be found in binary systems. The irregularity in the recurrence of the phenomena seems to preclude the possibility of such an explanation. The stars of this class probably contain within themselves the causes of their changes. They are, perhaps, at that critical stage of development where occasional internal disturbances cause tremendous outbursts, especially of incandescent hydrogen, resulting in an enormous increase of light. The commotion slowly dies down only to return again with more or less of regularity. For the details of these disturbances we must await further study.

IV. Of the 3,000 variables known at present probably the vast majority have short periods, that is, periods of a few days, or a few hours. The periods, also, are uniform; or, at least, if apparent irregularity exists at times, this is capable of being expressed by rigorous mathematical formulæ. η Aquilæ and β Lyræ are well-known examples of this class. Recent investigations have shown that such stars are binary systems, and that in some way the light phenomena are associated with orbital revolution. Belopolsky, and later Campbell and Wright have investigated the velocity-curve of η Aquilæ. From their investigations it appears that this variable is a binary, whose period of revolution is of the same duration as the period of the light-changes. The determination of the velocity-curve is accomplished by the use of a slit-spectroscope, which gives a comparison spectrum of some known element, which is also present in the spectrum of the star. Since the velocity-curve and light-curve are synchronous it might be suspected that the light variation was caused by an eclipse of the star by a relatively dark companion. This can not be true, however, in the case of η Aquilæ, for various reasons. In the first place, the light-curve is not that of an eclipsing star. An eclipse must occur when two stars are both in the line of sight, at which time the apparent motion would be small or zero. As a fact, the minimum of the star does not occur at such a time. The light maximum occurs noticeably later, and the minimum noticeably earlier than the periastron of the star. These facts seem not inconsistent with the theory that the variations in lights are caused by the close approach of the components of a double star moving in elliptical orbits, the outburst of light resulting from some tidal disturbance incited by the enormously increased mutual attractions of the two bodies. An objection to this explanation is that under these circumstances the outburst would probably manifest itself by the presence of bright lines in the spectrum at maximum, as is the case with long-period variables. Small evidence exists that this is true. Another difficulty is found by a comparison of the curves of velocity and light, as determined by Wright and Schur. The former is a smooth curve, while the latter has a secondary maximum. That this may be due in part to an error in the form of the light-curve as given by Schur, seems not impossible, if we compare it with the light-curve of the same star as determined by Pickering with a polarizing photometer. The latter curve shows merely an indication of a secondary maximum. It may be true, of course, that the secondary maximum is sometimes present, at other times absent. That the relation between the curves of motion and light may be most intimate, in some cases at least, is beautifully shown by the variable ω Sagittarii. The velocity-curve of this star was determined by Dr. Curtiss, of the Lick Observatory. As pointed out by him, the velocity-curve, and the light-curve determined by Professor Pickering, show a close resemblance even in the details, which proves conclusively that both phenomena are associated with the same underlying causes. Incidentally, a striking proof is furnished of the accuracy of these two widely separated investigations, thus critically compared. These curves are shown in Fig. 5.

β Lyræ represents a somewhat different variety of the short-period variable. This star has been studied for more than a century and still remains something of a mystery. The spectrum is complex, the lines showing displacement, apparently due to the motions of the bright components of a close binary. These displacements were explained in 1891 by Pickering as the result of the revolution of the unlike components of a binary system, having a relative velocity of 300 miles per second, and a radius of 50,000,000 miles. Belopolsky has also investigated this object, obtaining results which differ somewhat from those given above. Professor G. W. Myers has made a mathematical discussion of the problem, reaching the conclusion that the phenomena can be explained on the theory of a binary system, composed of two gaseous, scarcely separated, components of different masses, mutually eclipsing each other during their revolutions. Indeed the two components may not have separated, but exist still as a single body of unusual form, such as Poincaré's, or Darwin's figures of equilibrium. The problem is extremely complicated, and well illustrates the almost infinite diversity which is met with in the various problems about variable stars. The binary character of this type of variables seems sure in many cases, while in others even three bodies appear to be present; but the details involved are still in doubt.

The variable stars found in clusters have periods ranging for the most part from ten to fourteen hours. The elements of about 300 of them have been determined by the writer. The uniformity of the periods found in the same cluster is remarkable, pointing unmistakably to a common cause. What that cause is has not yet been found. The form of light-curve is shown in Figs. 3 and 4. Owing to the faintness of these stars, which generally vary between the twelfth and fifteenth magnitudes, it has not yet been possible to determine either the nature of the spectrum or the radial motion. The light-curve shows no indication of eclipsing phenomena. The uniformity in the period, traced in many cases through more than 5,000 returns of maximum, points to axial rotation or orbital revolution. Variability might result, undoubtedly, from the rotation of an elliptical, or unevenly luminous body; but the light-curves of cluster variables are difficult of explanation on this theory. They may be binaries with small, elliptical orbits, but even this is hardly consistent with the form of light-curve. The rejection of these hypotheses, nevertheless, seems to leave the phenomena without plausible explanation. A few cluster variables have been found where the maxima succeed each other at intervals of about six hours, one half the usual period. This indicates pretty clearly a double variable with alternating maxima, both components having the same period. These apparently accidental cases of duplicity may throw some light on the physical condition of all these stars.

V. Of the Algol variables Algol itself is a good example. Its light usually remains at a uniform brightness of the second magnitude, but once in a little less than three days it falls to the third magnitude, where it remains for some twenty minutes before beginning to regain its brightness. The whole time for the decrease and the restoration of light is about ten hours. The form of this light-curve points unmistakably to the eclipse of a bright star by a relatively dark companion. This explanation, first proposed by Goodricke, was developed by Pickering, and proved spectroscopically by Vogel. Dr. Alexander W. Roberts, of Lovedale, South Africa, has recently developed a method for determining the absolute dimensions of an Algol binary. The theory which underlies the determination is that light takes an appreciable interval of time to traverse the orbit of a binary system. For an accurate solution observations of the highest precision are essential. Precise photometric observations of such objects have been made by Professor O. C. Wendell, of the Harvard Observatory. The cause of variation is in general the same for all the Algol variables, though there are minor differences of importance. As might be expected, they show great regularity. Nevertheless, there are certain secular variations from causes not well understood. The period of Algol is believed to vary slightly, and Dr. Chandler explains this as due to the presence of a third body. M. Tisserand, however, has advanced a different theory. He assumes a slight flattening of the globe of Algol, and an elliptical orbit for the companion. These rival theories can be settled only by elaborate determinations of the light-curve during many years. According to Dr. Chase, Algol is at a distance of ninety-three light-years. Vogel finds the diameter of each of the components to be nearly a million miles, and the distance between them little more than three million miles. There are doubtless thousands of binary systems in the heavens, one component of which is more or less obscure. Such a system, and it holds true even if the components are equally luminous, becomes for us an Algol variable when the plane of revolution passes through or near the earth. Such systems are comparatively rare. At the present time only thirty-eight are known. The largest variation yet found is that of Fleming's Algol, R. W. Tauri, whose light at minimum is only one twenty-sixth as great as its usual amount. It would be possible for a dark companion of the same size as the bright component to completely eclipse it. In the case of υ Cephei, indeed, this probably takes place, so that the light while the eclipse lasts comes entirely from the dark companion. The companion is only relatively dark, however, so that its light alone is about one eighth as great as the combined light of both components. If the companion, in such a case, were completely obscure, there would be a total eclipse of the star's light, but no such case has yet been found.

The Campus of Cornell University, with Cayuga Lake in the Distance.

THE PROGRESS OF SCIENCE

CORNELL UNIVERSITY AND THE SUMMER MEETING OF THE AMERICAN ASSOCIATION.

When the American Association for the Advancement of Science decided to hold a special summer session between the winter meetings in New Orleans and New York City, it was well advised in choosing Ithaca as the place. There is probably no other university in the world with such a beautiful site and surroundings, and there are but few institutions whose buildings, equipment and work are of greater interest to students of science. Ithaca is not far from the center of scientific population, and Cornell is in many ways intermediate between the eastern private foundations, such as Harvard, Yale and Columbia, and the state institutions of the central west, such as Michigan, Illinois and Wisconsin.

Cornell owed its origin to state support combined with the generosity of the man whose name it perpetuates. One of the most beneficent acts of congress, notable for its wisdom and because it was proposed in the midst of the civil war, was the land grant for the establishment in each state of a college primarily for agriculture and the mechanic arts. The act, approved July 2, 1862, provided that there should be granted to the several states public lands, thirty thousand acres for each senator and representative of congress, from the sale of which there should be established a perpetual fund "the interest of which shall be inviolably appropriated, by each state which may take and claim the benefit of this act, to the endowment, support and maintenance of at least one college, where the leading object shall be, without excluding other scientific and classical studies, and including military tactics, to teach such branches of learning as are related to agriculture and the mechanic arts, in such manner as the legislatures of the states may respectively prescribe, in order to promote the liberal and practical education of the industrial classes in the several pursuits and professions in life."

New York state received about a million acres, and, thanks largely to the wisdom of Mr. Ezra Cornell, most of the land was held until ultimately it yielded over five million dollars. In accordance with Mr. Cornell's well-known words, 'I would found an institution where any person can find instruction in any study,' Cornell University was established in 1865 and opened in the autumn of 1868. Thanks to the wise administration of Dr. Andrew D. White, to a loyal group of able teachers and men of science, to state support and private beneficence, Cornell has become one of the great universities of the country and of the world, fulfilling as nearly as may be the dreams of its founder. There are now some five hundred officers and four thousand students divided among departments as follows: the graduate department, the college of arts and sciences, the college of law, the medical college, the New York state veterinary college, the college of agriculture, the college of architecture, the college of civil engineering, the Sibley college of mechanical engineering and mechanic arts.

At Cornell University a gathering of scientific men could not be other than pleasant and profitable. There were in all about 400 in attendance at the meetings which were held from June 27 to July 3. This is as large a group as is necessary or even desirable for the enjoyment of those who are present. It might, however, have been expected that a larger number would have taken advantage of the opportunity. The chemists, physicists and others who held technical meetings came in fair numbers, but there were not many who attended the meeting in order to see the university and the surrounding regions, to meet their colleagues in other departments and learn of the general forward movement in science, or to do their share in promoting the organization of scientific work and scientific men. Thus sections A and K held no meetings at Ithaca; the special societies whose subjects were included in those sections—mathematics, astronomy, physiology, anatomy, pathology, psychology, etc., did not hold meetings, and the registration in those sections was four members. The absence of those who are not professional students of science was also noticeable and probably regrettable. It should be one of the functions of the association to keep science in touch with the larger public and to increase scientific interest throughout the country. It was hoped that the resumption of summer meetings would be a step in this direction, but it does not seem that very much was accomplished at Ithaca.

Yet the character of the meeting, as well as its place, was as attractive as could well be. Dr. Welch, our leading pathologist, was an admirable presiding officer and gave two interesting addresses. Two addresses were given by President Schurman and one by Dr. Andrew D. White. The evening lectures, by Professor Carhart on the South African meeting of the British Association and by Professor Branner on the California earthquake, were particularly timely and interesting. The new physical laboratory of the university was dedicated, and Sigma Xi celebrated the twentieth anniversary of its foundation. The excursions arranged by the

THE LIBRARY OF CORNELL UNIVERSITY.

SIBLEY COLLEGE OF MECHANICAL ENGINEERING.

geologists, botanists, chemists, engineers and economists were in every way successful. Thus apart from the more special scientific programs, which in several subjects were very good, there was much to attract all who are interested in science, and those who were present will remember the Ithaca meeting as one of the most pleasant in the history of the association.

THE STATE VETERINARY COLLEGE.

BEEBE LAKE AND TRIPHAMMER FALLS ON THE EDGE OF THE CORNELL CAMPUS.

LEGISLATION AND SCIENCE

GOVERNMENT is becoming more and more an application of science. Politics are still largely a game and a trade; the kind of science at hand is crude and is applied by the rule of thumb. But if the proceedings of successive parliaments or congresses are reviewed, there is an evident tendency for legislation to rest increasingly on expert knowledge and to require continually greater scientific skill in its execution. When the constitution of the United States was written, the threefold division of the functions of the government—legislative, executive and judicial — was adequate. Now, however, it may be urged that the scientific or expert functions are coordinate with the others. Laws may be made by the congress, interpreted by the courts and executed by the president, but they should be based on scientific investigations and carried out by scientific experts.

We are told that municipal government should be divorced from politics, and this is doubtless true. A municipality is primarily a business or engineering corporation. Its main concern is with streets, sewers, parks and docks; with schools, hospitals and public institutions; with water, light and the means of transportation and communication. But there are equally sound reasons for keeping the government of a state or nation free from politics and conducting its affairs with such skill and efficiency as are attainable. There are certain questions that are quite outside the limits of such science as we now have, for example, the desirability of more or less centralization, paternalism, aristocracy, war or religion. The people may legitimately divide themselves into parties on such lines. Science may be unable to answer the question as to whether the government should conduct the postoffice, the express business or the railways, but when the government has undertaken to manage the mails, it makes no more difference whether the postmaster general is a republican or a democrat, than whether he is a catholic or a protestant, married or single. It would be well if we could separate those questions which must for the present be settled by party government from those which should be decided by expert knowledge, and if the latter could be settled by men having the necessary special training. And of course nearly all the executive work of the government should be done by experts, and a large part by those who are technically men of science.

The main questions before the first session of the fifty-ninth congress were concerned with the extension of federal control by the regulation of interstate commerce, and may be regarded as outside the scope of this journal. But the decisions of the congress rested, or should have rested, on statistical or other scientific data. In the execution of the laws relating to railway rates, meat inspection and pure food, a large number of trained scientific men will be required. The removal of the tax on alcohol which has been 'denatured' will have an important effect on the arts. While we should like to see the decimal system of weights and measures or even a duodecimal system made compulsory, it must be admitted that technical opinion is so divided that the house can scarcely be blamed for rejecting the measure. Of direct scientific interest were the bills protecting Niagara Falls, the Mariposa trees of California and the antiquities on the public lands. Although the main increase in the appropriation for the Department of Agriculture was for meat inspection, its scientific work was enlarged in several directions. The appropriation for rebuilding the Military Academy at West Point was increased to $6,500,000. A lock canal at Panama carried to the height of eighty-five feet was decided on, and the sum of $42,500,000 was appropriated for the work.

SCIENTIFIC ITEMS.

We record with regret the deaths of Dr. Henry A. Ward, president of Ward's Natural History Establishment at Rochester, and Dr. Fritz Schaudinn, recently appointed head of the parasitological department of the Institute for Tropical Diseases of Hamburg and well known for his work on the protozoa.

The Ordre pour le Mérite has been conferred on Professor Robert Koch by the German Emperor.—Dr. Ernst Mach, of Vienna, has been awarded the Bavarian Maximilian order for science and art.—Professor Simon Newcomb has been elected a member of the board of overseers of Harvard College.—The Society of Arts has awarded its Albert medal to Sir Joseph W. Swan, F.R.S., 'for the important part he took in the invention of the incandescent electric lamp, and for his invention of the carbon process of photographic printing.'

Announcement has been made of the resignation of Dr. William T. Harris, commissioner of education, and of the nomination of his successor, Professor Elmer E. Brown, of the University of California. Dr. Harris's retirement has been made possible by a retiring allowance from the Carnegie Foundation for the Advancement of Teaching. This action was taken by the trustees of the foundation under one of their rules which permits of such action in the case of extraordinary and unusual service to education. Dr. Harris has been the commissioner of education since 1889, and has, perhaps, had a larger and more intimate connection with the whole body of teachers than any other man. The offer to him of this retiring allowance was an act of the highest regard for his work and places his name at the head of the list of distinguished men who have accepted such retiring allowances from the Carnegie Foundation.—Dr. D. E. Salmon, from 1884 to 1905 chief of the Bureau of Animal Industry, has accepted the offer of the government of Uruguay to organize a Bureau of Animal Industry for that country. Dr. Salmon, who is at present engaged in scientific work in Montana, will start for South America about December 1.

The protocol providing for the establishment of an international institute of agriculture at Rome, Italy, has been adopted by the congress. There are about forty governments party to the arrangement. Studies will be made of all kinds of plant life and means of extermination of insects and other pests. The institute will receive the reports of the agricultural bureaus and societies of all countries. The Italian government will supply the buildings, and the cost to other governments will be about $5,000 a year each.

THE POPULAR SCIENCE MONTHLY

SEPTEMBER, 1906

THE VALUE OF SCIENCE[1]

By M. H. POINCARÉ

MEMBER OF THE INSTITUTE OF FRANCE

Introduction

THE search for truth should be the goal of our activities; it is the sole end worthy of them. Doubtless we should first bend our efforts to assuage human suffering, but why? Not to suffer is a negative ideal more surely attained by the annihilation of the world. If we wish more and more to free man from material cares, it is that he may be able to employ the liberty obtained in the study and contemplation of truth.

But sometimes truth frightens us. And in fact we know that it is sometimes deceptive, that it is a phantom never showing itself for a moment except to ceaselessly flee, that it must be pursued further and ever further without ever being attained. Yet to work one must stop, as some Greek, Aristotle or another, has said. We also know how cruel the truth often is, and we wonder whether illusion is not more consoling, yea, even more bracing, for illusion it is which gives confidence. When it shall have vanished, will hope remain and shall we have the courage to achieve? Thus would not the horse harnessed to his treadmill refuse to go, were his eyes not bandaged? And then to seek truth it is necessary to be independent, wholly independent. If on the contrary we wish to act, to be strong, we should be united. This is why many of us fear truth; we consider it a cause of weakness. Yet truth should not be feared, for it alone is beautiful.

When I speak here of truth, assuredly I refer first to scientific

[1] Authorized translation by Professor George Bruce Halsted, Ph.D.

truth; but I also mean moral truth, of which what we call justice is only one aspect. It may seem that I am misusing words, that I combine thus under the same name two things having nothing in common; that scientific truth, which is demonstrated, can in no way be likened to moral truth, which is felt. And yet I can not separate them, and whosoever loves the one can not help loving the other. To find the one, as well as to find the other, it is necessary to free the soul completely from prejudice and from passion; it is necessary to attain absolute sincerity. These two sorts of truth when discovered give the same joy; each when perceived beams with the same splendor, so that we must see it or close our eyes. Lastly, both attract us and flee from us; they are never fixed: when we think to have reached them, we find that we have still to advance, and who pursues them is condemned never to know repose. It must be added that those who fear the one will also fear the other; for they are the ones who in everything are concerned above all with consequences. In a word, I liken the two truths, because the same reasons make us love them and because the same reasons make us fear them.

If we ought not to fear moral truth, still less should we dread scientific truth. In the first place it can not conflict with ethics. Ethics and science have their own domains, which touch but do not interpenetrate. The one shows us to what goal we should aspire, the other, given the goal, teaches us how to attain it. So they can never conflict since they can never meet. There can no more be immoral science than there can be scientific morals.

But if science is feared, it is above all because it can not give us happiness. Of course it can not. We may even ask whether the beast does not suffer less than man. But can we regret that earthly paradise where man brute-like was really immortal in knowing not that he must die? When we have tasted the apple, no suffering can make us forget its savor. We always come back to it. Could it be otherwise? As well ask if one who has seen and is blind will not long for the light. Man, then, can not be happy through science, but to-day he can much less be happy without it.

But if truth be the sole aim worth pursuing, may we hope to attain it? It may well be doubted. Readers of my little book 'Science and Hypothesis' already know what I think about the question. The truth we are permitted to glimpse is not altogether what most men call by that name. Does this mean that our most legitimate, most imperative aspiration is at the same time the most vain? Or can we, despite all, approach truth on some side? This it is which must be investigated.

In the first place, what instrument have we at our disposal for this conquest? Is not human intelligence, more specifically the intelligence of the scientist, susceptible of infinite variation? Volumes could be written without exhausting this subject; I, in a few brief pages, have only touched it lightly. That the geometer's mind is not like the physicist's or the naturalist's, all the world would agree; but mathematicians themselves do not resemble each other; some recognize only implacable logic, others appeal to intuition and see in it the only source of discovery. And this would be a reason for distrust. To minds so unlike can the mathematical theorems themselves appear in the same light? Truth which is not the same for all, is it truth? But looking at things more closely, we see how these very different workers collaborate in a common task which could not be achieved without their cooperation. And that already reassures us.

Next must be examined the frames in which nature seems enclosed and which are called time and space. In 'Science and Hypothesis' I have already shown how relative their value is; it is not nature which imposes them upon us, it is we who impose them upon nature because we find them convenient. But I have spoken of scarcely more than space, and particularly quantitative space, so to say, that is of the mathematical relations whose aggregate constitutes geometry. I should have shown that it is the same with time as with space and still the same with 'qualitative space'; in particular, I should have investigated why we attribute three dimensions to space. I may be pardoned then for taking up again these important questions.

Is mathematical analysis then, whose principal object is the study of these empty frames, only a vain play of the mind? It can give to the physicist only a convenient language; is this not a mediocre service, which, strictly speaking, could be done without; and even is it not to be feared that this artificial language may be a veil interposed between reality and the eye of the physicist? Far from it; without this language most of the intimate analogies of things would have remained forever unknown to us; and we should forever have been ignorant of the internal harmony of the world, which is, we shall see, the only true objective reality.

The best expression of this harmony is law. Law is one of the most recent conquests of the human mind; there still are people who live in the presence of a perpetual miracle and are not astonished at it. On the contrary, we it is who should be astonished at nature's regularity. Men demand of their gods to prove their existence by miracles; but the eternal marvel is that there are not miracles without cease. The world is divine because it is a harmony. If it were ruled by caprice, what could prove to us it was not ruled by chance?

This conquest of law we owe to astronomy, and just this makes the grandeur of the science rather than the material grandeur of the objects it considers. It was altogether natural then that celestial mechanics should be the first model of mathematical physics; but since then this science has developed; it is still developing, even rapidly developing. And it is already necessary to modify in certain points the scheme I outlined in 1900 and from which I drew two chapters of 'Science and Hypothesis.' In an address at the St. Louis exposition in 1904, I sought to survey the road traveled; the result of this investigation the reader shall see farther on.

The progress of science has seemed to imperil the best established principles, those even which were regarded as fundamental. Yet nothing shows they will not be saved; and if this comes about only imperfectly, they will still subsist even though they are modified. The advance of science is not comparable to the changes of a city, where old edifices are pitilessly torn down to give place to new, but to the continuous evolution of zoologic types which develop ceaselessly and end by becoming unrecognizable to the common sight, but where an expert eye finds always traces of the prior work of the centuries past. One must not think then that the old-fashioned theories have been sterile and vain.

Were we to stop there, we should find in these pages some reasons for confidence in the value of science, but many more for distrusting it; an impression of doubt would remain; it is needful now to set things to rights.

Some people have exaggerated the rôle of convention in science; they have even gone so far as to say that law, that scientific fact itself, was created by the scientist. This is going much too far in the direction of nominalism. No, scientific laws are not artificial creations; we have no reason to regard them as accidental, though it be impossible to prove they are not.

Does the harmony the human intelligence thinks it discovers in nature exist outside of this intelligence? No, beyond doubt, a reality completely independent of the mind which conceives it, sees or feels it, is an impossibility. A world as exterior as that, even if it existed, would for us be forever inaccessible. But what we call objective reality is, in the last analysis, what is common to many thinking beings, and could be common to all; this common part, we shall see, can only be the harmony expressed by mathematical laws. It is this harmony then which is the sole objective reality, the only truth we can attain; and when I add that the universal harmony of the world is the source of all beauty, it will be understood what price we should attach to the slow and difficult progress which little by little enables us to know it better.

Intuition and Logic in Mathematics

I.

It is impossible to study the works of the great mathematicians, or even those of the lesser, without noticing and distinguishing two opposite tendencies, or rather two entirely different kinds of minds. The one sort are above all preoccupied with logic; to read their works, one is tempted to believe they have advanced only step by step, after the manner of a Vauban who pushes on his trenches against the place besieged, leaving nothing to chance. The other sort are guided by intuition and at the first stroke make quick but sometimes precarious conquests, like bold cavalrymen of the advance guard.

The method is not imposed by the matter treated. Though one often says of the first that they are *analysts* and calls the others *geometers,* that does not prevent the one sort from remaining analysts even when they work at geometry, while the others are still geometers even when they occupy themselves with pure analysis. It is the very nature of their mind which makes them logicians or intuitionalists, and they can not lay it aside when they approach a new subject.

Nor is it education which has developed in them one of the two tendencies and stifled the other. The mathematician is born, not made, and it seems he is born a geometer or an analyst. I should like to cite examples and there are surely plenty; but to accentuate the contrast I shall begin with an extreme example, taking the liberty of seeking it in two living mathematicians.

M. Méray wants to prove that a binomial equation always has a root, or, in ordinary words, that an angle may always be subdivided. If there is any truth that we think we know by direct intuition, it is this. Who could doubt that an angle may always be divided into any number of equal parts? M. Méray does not look at it that way; in his eyes this proposition is not at all evident and to prove it he needs several pages.

On the other hand, look at Professor Klein: he is studying one of the most abstract questions of the theory of functions to determine whether on a given Riemann surface there always exists a function admitting of given singularities. What does the celebrated German geometer do? He replaces his Riemann surface by a metallic surface whose electric conductivity varies according to certain laws. He connects two of its points with the two poles of a battery. The current, says he, must pass, and the distribution of this current on the surface will define a function whose singularities will be precisely those called for by the enunciation.

Doubtless Professor Klein well knows he has given here only a sketch: nevertheless he has not hesitated to publish it; and he would probably believe he finds in it, if not a rigorous demonstration, at least a kind of moral certainty. A logician would have rejected with horror such a conception, or rather he would not have had to reject it, because in his mind it would never have originated.

Again, permit me to compare two men, the honor of French science, who have recently been taken from us, but who both entered long ago into immortality. I speak of M. Bertrand and M. Hermite. They were scholars of the same school at the same time; they had the same education, were under the same influences; and yet what a difference! Not only does it blaze forth in their writings; it is in their teaching, in their way of speaking, in their very look. In the memory of all their pupils these two faces are stamped in deathless lines; for all who have had the pleasure of following their teaching, this remembrance is still fresh; it is easy for us to evoke it.

While speaking, M. Bertrand is always in motion; now he seems in combat with some outside enemy, now he outlines with a gesture of the hand the figures he studies. Plainly he sees and he is eager to paint, this is why he calls gesture to his aid. With M. Hermite, it is just the opposite; his eyes seem to shun contact with the world; it is not without, it is within he seeks the vision of truth.

Among the German geometers of this century, two names above all are illustrious, those of the two scientists who have founded the general theory of functions, Weierstrass and Riemann. Weierstrass leads everything back to the consideration of series and their analytic transformations; to express it better, he reduces analysis to a sort of prolongation of arithmetic; you may turn through all his books without finding a figure. Riemann, on the contrary, at once calls geometry to his aid; each of his conceptions is an image that no one can forget, once he has caught its meaning.

More recently, Lie was an intuitionalist; this might have been doubted in reading his books, no one could doubt it after talking with him; you saw at once that he thought in pictures. Madame Kovalevski was a logician.

Among our students we notice the same differences; some prefer to treat their problems 'by analysis,' others 'by geometry.' The first are incapable of 'seeing in space,' the others are quickly tired of long calculations and become perplexed.

The two sorts of minds are equally necessary for the progress of science; both the logicians and the intuitionalists have achieved great things that others could not have done. Who would venture to say whether he preferred that Weierstrass had never written or that there had never been a Riemann? Analysis and synthesis have then both their legitimate rôles. But it is interesting to study more closely in the history of science the part which belongs to each.

II.

Strange! If we read over the works of the ancients we are tempted to class them all among the intuitionalists. And yet nature is always the same; it is hardly probable that it has begun in this century to create minds devoted to logic. If we could put ourselves into the flow of ideas which reigned in their time, we should recognize that many of the old geometers were in tendency analysts. Euclid, for example, erected a scientific structure wherein his contemporaries could find no fault. In this vast construction, of which each piece however is due to intuition, we may still to-day, without much effort, recognize the work of a logician.

It is not minds that have changed, it is ideas; the intuitional minds have remained the same; but their readers have required of them greater concessions.

What is the cause of this evolution? It is not hard to find. Intuition can not give us rigor, nor even certainty; this has been recognized more and more. Let us cite some examples. We know there exist continuous functions lacking derivatives. Nothing is more shocking to intuition than this proposition which is imposed upon us by logic. Our fathers would not have failed to say: "It is evident that every continuous function has a derivative, since every curve has a tangent."

How can intuition deceive us on this point? It is because when we seek to imagine a curve, we can not represent it to ourselves without width; just so, when we represent to ourselves a straight line, we see it under the form of a rectilinear band of a certain breadth. We well know these lines have no width; we try to imagine them narrower and narrower and thus to approach the limit; so we do in a certain measure, but we shall never attain this limit. And then it is clear we can always picture these two narrow bands, one straight, one curved, in a position such that they encroach slightly one upon the other without crossing. We shall thus be led, unless warned by a rigorous analysis, to conclude that a curve always has a tangent.

I shall take as second example Dirichlet's principle on which rest so many theorems of mathematical physics; to-day we establish it by reasonings very rigorous but very long; heretofore, on the contrary, we were content with a very summary proof. A certain integral depending on an arbitrary function can never vanish. Hence it is concluded that it must have a minimum. The flaw in this reasoning strikes us immediately, since we use the abstract term *function* and are familiar with all the singularities functions can present when the word is understood in the most general sense.

But it would not be the same had we used concrete images, had we, for example, considered this function as an electric potential; it would have been thought legitimate to affirm that electrostatic equilibrium can be attained. Yet perhaps a physical comparison would have awakened some vague distrust. But if care had been taken to translate the reasoning into the language of geometry, intermediate between that of analysis and that of physics, doubtless this distrust would not have been produced, and perhaps one might thus, even to-day, still deceive many readers not forewarned.

Intuition, therefore, does not give us certainty. This is why the evolution had to happen; let us now see how it happened.

It was not slow in being noticed that rigor could not be introduced in the reasoning unless first made to enter into the definitions. For the most part the objects treated of by mathematicians were long ill defined; they were supposed to be known because represented by means of the senses or the imagination; but one had only a crude image of them and not a precise idea on which reasoning could take hold. It was there first that the logicians had to direct their efforts.

So, in the case of incommensurable numbers. The vague idea of continuity, which we owe to intuition, resolved itself into a complicated system of inequalities referring to whole numbers.

By that means the difficulties arising from passing to the limit, or from the consideration of infinitesimals, are finally removed. To-day in analysis only whole numbers are left or systems, finite or infinite, of whole numbers bound together by a net of equality or inequality relations. Mathematics, as they say, is arithmetized.

III.

A first question presents itself. Is this evolution ended? Have we finally attained absolute rigor? At each stage of the evolution our fathers also thought they had reached it. If they deceived themselves, do we not likewise cheat ourselves?

We believe that in our reasonings we no longer appeal to intuition; the philosophers will tell us this is an illusion. Pure logic could never lead us to anything but tautologies; it could create nothing new; not from it alone can any science issue. In one sense these philosophers are right; to make arithmetic, as to make geometry, or to make any science, something else than pure logic is necessary. To designate this something else we have no word other than *intuition.* But how many different ideas are hidden under this same word?

Compare these four axioms: (1) Two quantities equal to a third are equal to one another; (2) if a theorem is true of the number 1 and if we prove that it is true of $n+1$ if true for n, then will it be true of all whole numbers; (3) if on a straight the point C is between A

and B and the point D between A and C, then the point D will be between A and B; (4) through a given point there is not more than one parallel to a given straight.

All four are attributed to intuition, and yet the first is the enunciation of one of the rules of formal logic; the second is a real synthetic *a priori* judgment, it is the foundation of rigorous mathematical induction; the third is an appeal to the imagination; the fourth is a disguised definition.

Intuition is not necessarily founded on the evidence of the senses; the senses would soon become powerless; for example, we can not represent to ourselves a chiliagon, and yet we reason by intuition on polygons in general, which include the chiliagon as a particular case.

You know what Poncelet understood by the *principle of continuity*. What is true of a real quantity, said Poncelet, should be true of an imaginary quantity; what is true of the hyperbola whose asymptotes are real, should then be true of the ellipse whose asymptotes are imaginary. Poncelet was one of the most intuitive minds of this century; he was passionately, almost ostentatiously, so; he regarded the principle of continuity as one of his boldest conceptions, and yet this principle did not rest on the evidence of the senses. To assimilate the hyperbola to the ellipse was rather to contradict this evidence. It was only a sort of precocious and instinctive generalization which, moreover, I have no desire to defend.

We have then many kinds of intuition; first, the appeal to the senses and the imagination; next, generalization by induction, copied, so to speak, from the procedures of the experimental sciences; finally, we have the intuition of pure number, whence arose the second of the axioms just enunciated, which is able to create the real mathematical reasoning. I have shown above by examples that the first two can not give us certainty; but who will seriously doubt the third, who will doubt arithmetic?

Now in the analysis of to-day, when one cares to take the trouble to be rigorous, there can be nothing but syllogisms or appeals to this intuition of pure number, the only intuition which can not deceive us. It may be said that to-day absolute rigor is attained.

IV.

The philosophers make still another objection: "What you gain in rigor," they say, "you lose in objectivity. You can rise toward your logical ideal only by cutting the bonds which attach you to reality. Your science is infallible, but it can only remain so by imprisoning itself in an ivory tower and renouncing all relation with the external world. From this seclusion it must go out when it would attempt the slightest application."

For example, I seek to show that some property pertains to some object whose concept seems to me at first indefinable, because it is intuitive. At first I fail or must content myself with approximate proofs; finally I decide to give to my object a precise definition, and this enables me to establish this property in an irreproachable manner.

"And then," say the philosophers, "it still remains to show that the object which corresponds to this definition is indeed the same made known to you by intuition; or else that some real and concrete object whose conformity with your intuitive idea you believe you immediately recognize corresponds to your new definition. Only then could you affirm that it has the property in question. You have only displaced the difficulty."

That is not exactly so; the difficulty has not been displaced, it has been divided. The proposition to be established was in reality composed of two different truths, at first not distinguished. The first was a mathematical truth, and it is now rigorously established. The second was an experimental verity. Experience alone can teach us that some real and concrete object corresponds or does not correspond to some abstract definition. This second verity is not mathematically demonstrated, but neither can it be, no more than can the empirical laws of the physical and natural sciences. It would be unreasonable to ask more.

Well, is it not a great advance to have distinguished what long was wrongly confused? Does this mean that nothing is left of this objection of the philosophers? That I do not intend to say; in becoming rigorous, mathematical science takes a character so artificial as to strike every one; it forgets its historical origins; we see how the questions can be answered, we no longer see how and why they are put.

This shows us that logic is not enough; that the science of demonstration is not all science and that intuition must retain its rôle as complement, I was about to say, as counterpoise or as antidote of logic.

I have already had occasion to insist on the place intuition should hold in the teaching of the mathematical sciences. Without it young minds could not make a beginning in the understanding of mathematics; they could not learn to love it and would see in it only a vain logomachy; above all, without intuition they would never become capable of applying mathematics. But now I wish before all to speak of the rôle of intuition in science itself. If it is useful to the student, it is still more so to the creative scientist.

V.

We seek reality, but what is reality? The physiologists tell us that organisms are formed of cells; the chemists add that cells themselves are formed of atoms. Does this mean that these atoms or these cells constitute reality, or rather the sole reality? The way in which these cells are arranged and from which results the unity of the individual, is not it also a reality much more interesting than that of the isolated elements, and should a naturalist who had never studied the elephant except by means of the microscope think himself sufficiently acquainted with that animal?

Well, there is something analogous to this in mathematics. The logician cuts up, so to speak, each demonstration into a very great number of elementary operations; when we have examined these operations one after the other and ascertained that each is correct, are we to think we have grasped the real meaning of the demonstration? Shall we have understood it even when, by an effort of memory, we have become able to repeat this proof by reproducing all these elementary operations in just the order in which the inventor had arranged them? Evidently not; we shall not yet possess the entire reality; that I know not what which makes the unity of the demonstration will completely elude us.

Pure analysis puts at our disposal a multitude of procedures whose infallibility it guarantees; it opens to us a thousand different ways on which we can embark in all confidence; we are assured of meeting there no obstacles; but of all these ways, which will lead us most promptly to our goal? Who shall tell us which to choose? We need a faculty which makes us see the end from afar, and intuition is this faculty. It is necessary to the explorer for choosing his route; it is not less so to the one following his trail who wants to know why he chose it.

If you are present at a game of chess, it will not suffice, for the understanding of the game, to know the rules for moving the pieces. That will only enable you to recognize that each move has been made conformably to these rules, and this knowledge will truly have very little value. Yet this is what the reader of a book on mathematics would do if he were a logician only. To understand the game is wholly another matter; it is to know why the player moves this piece rather than that other which he could have moved without breaking the rules of the game. It is to perceive the inward reason which makes of this series of successive moves a sort of organized whole. This faculty is still more necessary for the player himself, that is, for the inventor.

Let us drop this comparison and return to mathematics. For example, see what has happened to the idea of continuous function. At the outset this was only a sensible image, for example, that of a continuous mark traced by the chalk on a blackboard. Then it became little by little more refined; ere long it was used to construct a complicated system of inequalities, which reproduced, so to speak, all the lines of the original image; this construction finished, the centering of the arch, so to say, was removed, that crude representation which had temporarily served as support and which was afterward useless was rejected; there remained only the construction itself, irreproachable in the eyes of the logician. And yet if the primitive image had totally disappeared from our recollection, how could we divine by what caprice all these inequalities were erected in this fashion one upon another?

Perhaps you think I use too many comparisons; yet pardon still another. You have doubtless seen those delicate assemblages of silicious needles which form the skeleton of certain sponges. When the organic matter has disappeared, there remains only a frail and elegant lace-work. True, nothing is there except silica, but what is interesting is the form this silica has taken, and we could not understand it if we did not know the living sponge which has given it precisely this form. Thus it is that the old intuitive notions of our fathers, even when we have abandoned them, still imprint their form upon the logical constructions we have put in their place.

This view of the aggregate is necessary for the inventor; it is equally necessary for whoever wishes really to comprehend the inventor. Can logic give it to us? No; the name mathematicians give it would suffice to prove this. In mathematics logic is called *analysis* and analysis means *division, dissection.* It can have, therefore, no tool other than the scalpel and the microscope.

Thus logic and intuition have each their necessary rôle. Each is indispensable. Logic, which alone can give certainty, is the instrument of demonstration; intuition is the instrument of invention.

VI.

But at the moment of formulating this conclusion I am seized with scruples. At the outset I distinguished two kinds of mathematical minds, the one sort logicians and analysts, the others intuitionalists and geometers. Well, the analysts also have been inventors. The names I have just cited make my insistence on this unnecessary.

Here is a contradiction, at least apparently, which needs explanation. And first, do you think these logicians have always proceeded from the general to the particular, as the rules of formal logic would seem to require of them? Not thus could they have extended the boundaries of science; scientific conquest is to be made only by generalization.

In one of the chapters of 'Science and Hypothesis,' I have had occasion to study the nature of mathematical reasoning, and I have shown how this reasoning, without ceasing to be absolutely rigorous, could lift us from the particular to the general by a procedure I have called *mathematical induction.* It is by this procedure that the analysts have made science progress, and if we examine the detail itself of their demonstrations, we shall find it there at each instant beside the classic syllogism of Aristotle. We, therefore, see already that the analysts are not simply makers of syllogisms after the fashion of the scholastics.

Besides, do you think they have always marched step by step with no vision of the goal they wished to attain? They must have divined the way leading thither, and for that they needed a guide. This guide is, first, analogy. For example, one of the methods of demonstration dear to analysts is that founded on the employment of dominant functions. We know it has already served to solve a multitude of problems; in what consists then the rôle of the inventor who wishes to apply it to a new problem? At the outset he must recognize the analogy of this question with those which have already been solved by this method; then he must perceive in what way this new question differs from the others, and thence deduce the modifications necessary to apply to the method.

But how does one perceive these analogies and these differences? In the example just cited they are almost always evident, but I could have found others where they would have been much more deeply hidden; often a very uncommon penetration is necessary for their discovery. The analysts, not to let these hidden analogies escape them, that is, in order to be inventors, must, without the aid of the senses and imagination, have a direct sense of what constitutes the unity of a piece of reasoning, of what makes, so to speak, its soul and inmost life.

When one talked with M. Hermite, he never evoked a sensuous image, and yet you soon perceived that the most abstract entities were for him like living beings. He did not see them, but he perceived that they are not an artificial assemblage, and that they have some principle of internal unity.

But, one will say, that still is intuition. Shall we conclude that the distinction made at the outset was only apparent, that there is only one sort of mind and that all the mathematicians are intuitionalists, at least those who are capable of inventing?

No, our distinction corresponds to something real. I have said above that there are many kinds of intuition. I have said how much the intuition of pure number, whence comes rigorous mathematical induction, differs from sensible intuition to which the imagination, properly so called, is the principal contributor.

Is the abyss which separates them less profound than it at first appeared? Could we recognize with a little attention that this pure intuition itself could not do without the aid of the senses? This is the affair of the psychologist and the metaphysician and I shall not discuss the question. But the thing's being doubtful is enough to justify me in recognizing and affirming an essential difference between the two kinds of intuition; they have not the same object and seem to call into play two different faculties of our soul; one would think of two search-lights directed upon two worlds strangers to one another.

It is the intuition of pure number, that of pure logical forms, which illumines and directs those we have called *analysts*. This it is which enables them not alone to demonstrate, but also to invent. By it they perceive at a glance the general plan of a logical edifice, and that too without the senses appearing to intervene. In rejecting the aid of the imagination, which, as we have seen, is not always infallible, they can advance without fear of deceiving themselves. Happy, therefore, are those who can do without this aid! We must admire them; but how rare they are!

Among the analysts there will then be inventors, but they will be few. The majority of us, if we wished to see afar by pure intuition alone, would soon feel ourselves seized with vertigo. Our weakness has need of a staff more solid, and, despite the exceptions of which we have just spoken, it is none the less true that sensible intuition is in mathematics the most usual instrument of invention.

Apropos of these reflections, a question comes up that I have not the time either to solve or even to enunciate with the developments it would admit of. Is there room for a new distinction, for distinguishing among the analysts those who above all use this pure intuition and those who are first of all preoccupied with formal logic?

M. Hermite, for example, whom I have just cited, can not be classed among the geometers who make use of the sensible intuition; but neither is he a logician, properly so called. He does not conceal his aversion to purely deductive procedures which start from the general and end in the particular.

(*To be continued.*)

DISCONTINUOUS VARIATION IN PEDIGREE-CULTURES[1]

By Dr. D. T. MacDougal

Director, Department of Botanical Research, Carnegie Institution of Washington

HEREDITY may be defined as that appurtenant function of living matter by which qualities, characters and capacities are transmitted through successive generations. The absolute identity, or measurable expression, of the inherited characters may be qualified by partial and individual fluctuations about a norm in a continuous series in the progenies, and these characters, singly or in groups, may be subject to dominations, recessions, integrations and resolutions in hybridizations, or to various forms of combination, actual or apparent. In the case of discontinuous variations or mutation, single or unit characters, or constellations of them, may be activated, or converted into a latent or perlatent condition.

Methods of Investigation

Owing to the stimulus of recent discovery, attention is focused at the present time upon the ultimate result of fluctuating variability as influenced by various agents, in the origin and fate of hereditary strains, species, races or physiologically unified groups of organisms, and upon the probable part played in the matter by the saltatory movements, which are being brought to notice so plentifully in all quarters. In connection with the last-named feature, the behavior of the individual qualities or unit-characters in hybridizations are being studied with enormous zeal as offering a ready analysis of the action of inheritance. The comparative ease with which hybrid combinations of plants are effected, and the simplicity of the subsequent resolutions in the progeny, render this phase of the subject easy of attack, and results are being obtained, which, if one may judge from recent literature, do not seem available to many writers.

It needs but a moment's consideration to bring the realization that the entire subject offers some of the most abstruse and difficult problems in the whole realm of biological science. Intricate and elusive in their physiological complexity, we may hope to uncover the main factors by perfected methods in research upon the ultimate mechanical basis of heredity coupled with a refinement of technique in dealing with the course of inheritance as we trace it from generation to

[1] Lecture given at Marine Biological Laboratory, Woods Holl, July 20, 1906.

generation, eliminating guesswork as to parentage, and prophecy as to offspring. The first is steadily yielding results of undoubted importance and is bringing about a renewed interest in the functional relations of the components of the protoplast with respect to the inheritance of characters. The second method, that of statistical observations and experimental methods in pedigreed cultures, has given such notable results in the hands of various investigators, that it may be truly asserted that it may not be outclassed in value by any form of research yet used in investigations in natural history.

As an explorer, do you wish to ascertain the source, direction, character, rate of flow and confluence of a river across your route? Surely, you do not reasonably attempt it by an examination of a single reach, or from a photograph of a single waterfall. Even so surely you may not gauge the possibilities of development, or estimate the potentiality or method of action of groups of characters, embraced in a hereditary strain, guided by dimly recognized forces for thousands of years, by the measurement of a preserved specimen. Physiological problems demand analytic methods of observation of living material.

What ridicule might we not heap upon a botanist who attempted to make a study of geotropism from consideration of the dried material in a herbarium. The existence of such a form of reaction might indeed be recognized, but what futile inferences might be drawn as to its mechanism or the nature of the results. It seems unnecessary and superfluous to call attention to a generalization so obvious; yet that the necessity is not lacking is shown by the material that crowds the pages of our technical magazines and popular periodicals.

Inadequate Treatment of the Subject

Before proceeding with the main thesis it will be profitable to notice some of the most glaring of the inadequacies of treatment which have been recently exhibited, and to call attention to certain unsupported statements which so far have gone unchallenged.

A vice-president of the American Association for the Advancement of Science, in a recent address, has taken occasion to call up the mutation theory, and assumes to have given it a test by "reexamining certain groups of birds and mammals, of which I had previously made systematic studies, for the purpose of discovering evidence, if such exists, of the formation of species by mutation." This author says that "for a quarter of a century I have been an earnest field student of plants in relation to geographic environment. These studies have convinced me that with plants as with animals the usual way in which new forms (subspecies and species) are produced is by gradual progressive development of minute variations." In regard to this comprehensive statement I may say that I have read practically everything that Dr. Merriam has put into print concerning his extensive and thorough field work in the west, and that I yield to none in my admiration of the wide inclusiveness of his results and the profitable manner in which he has treated incidentally the general features of the occurrence and distribution of plants. This appreciation is heightened by the fact that I have spent many seasons in the regions covered by him during the last fifteen years, and that I have carried on experimental work in the field and at the Desert Laboratory with many of the species which are included in his generalizations. I have not been able to come upon the evidence, or record of evidence, upon which his sweeping statements relative to plants are based, although detailed studies upon the relation of plants to environmental factors have been in progress for some time.

Dr. Merriam does not find any evidence to support the conclusion that species arise by mutation. It would be a matter of great surprise if he had. It would be as reasonable to have demanded of him the solutions of problems of respiration from his preparations and field notes. Once a mutant has appeared, no evidence of its distribution can be taken to account conclusively for its origin. Although I have had many mutants under experimental observation, I should not be able to speak with reasonable certainty as to the origin of any of them, had I not ascertained it by guarded pedigree-cultures. It also follows that the systematists who announce and describe new forms as mutants, simply from preserved specimens, or from individuals, the origin of which is not a matter of careful observation, are following a wholly unwarranted practise. Several months ago I had occasion to say "that the 'naturalists,' as some zoologists term themselves, having made the greatest number of essays to offer a universal interpretation of the problems of distribution, are to be credited with the greatest number of defenseless assumptions. In all genetic and evolutionary researches too much emphasis can not be laid upon the basal fact that the physiological and morphological natures of the two great classes of living things are so widely divergent that the derivation of universal biological principles from their apparently concurrent behavior must be made with the greatest caution." Nowhere does this find better exemplification than in the unedifying results of a recent discussion of isolation as a factor in evolutionary action. A number of zoologists have assumed to speak of the distribution of plants, with apparently no basis except 'general information' to the effect that closely related species do not have the same habitat. This has been variously put, but the general meaning is as given. Now such a conclusion is so widely inapplicable, and is so loosely guarded, as to be wholly without value as a statement of a principle in plant geography and floristics. That the actual mechanical contiguity of two forms competing for the same conditions of habitat would result in some stress is to be taken for granted, but this vegetative struggle would by no means be severe enough in any case to eliminate one from any region. If the advocates of the idea that closely related species do not occupy the same region take this ground on the assumption that hybridization disturbances would follow, here again would be an unwarranted assumption. Readiness of hybridization is by no means a measure of consanguinity, and any slight difference of habit, so small perhaps as not to be capable of description, might ensure pure fertilization. In the case of forms differing by one or a few characters, Mendelian splitting might operate to maintain the forms even if hybridization did occur.

The accompanying photograph of *Opuntia fulgida* and *O. mammillata* presents two forms so closely related that the latter has been taken as a variety of the former by some botanists, but it has been found to be a distinct and physiologically unified strain, and worthy of specific rank. These two forms are widely intermingled, though of course not many instances of actual contact such as appears in the illustration are to be found. The flora in the vicinity of the Desert Laboratory, at Tucson, presents scores of similar examples among other species representing many genera.

Aside from such misinterpretations, a prolific source of confusion lies in the widely different conceptions as to the nature of the taxonomic units used in zoological and botanical writings, as a consequence of which we have some zoologists calling attention to the supposed fact that certain botanists of differing views have no real conception as to the nature of 'species' and 'varieties.' Such statements serve the useful purpose of emphasizing the disadvantageous prejudices under which their authors labor.

While such misunderstandings contribute to hinder progress and confuse the subjects, the basal and underlying fault consists in the fact that taxonomic and geographic methods are not in themselves, or conjointly, adequate for the analysis, or solution of genetic problems. The inventor did not reach the solution of the problem of construction of a typesetting machine by studying the structure of printed pages, but by actual experimentation with mechanisms, using printed pages only as a record of his success. Likewise no amount of consideration of fossils, herbarium specimens, dried skins, skulls or fish in alcohol may give any actual proof as to the mechanism and action of heredity in transmitting qualities and characters from generation to generation, although from such historical data the general trend or direction of succession may be traced.

Misinterpretation of Results

Beyond such mistaken attempts at an analysis of the problems, there is another series of difficulties that interferes materially with the advancement of knowledge of the subject. This consists in careless, prejudiced or mistaken interpretation of results, having the force in some instances of actual misrepresentation. Such demonstrations do no final harm, yet they befog a difficult subject: with 'opinions' and 'beliefs,' they are quite out of place in any scientific discussion.

Since I have had *Œnothera Lamarckiana*, one of the plants which offer a favorable example of discontinuous variation in unit-characters, under cultivation for several years, I am disposed to regard Dr. D. S. Jordan's recent statement concerning this plant as of the above character. He says "it is not at all unlikely that the original *Œnothera Lamarckiana* found in the field near Amsterdam was a hybrid stock, a product of the florist, the behavior of its progeny being not unlike that which appears in the progeny of hybrids. It is moreover known that the seeds of hybrids of an American species, probably *Œnothera biennis*, the common evening primrose, with other American species produced by Mr. Burbank, have been in the past years sold in the cities of Germany." The well-informed botanist will be in doubt as to whether these statements are supposed to lie in 'the plane of ether,' or are to be taken literally. If seriously meant, carelessness as to the existence of records of the plant in question in several localities, long previous to the beginning of the activity of any living horticulturist, is shown. Moreover, material from these localities has been found to be in a mutative condition. It is unnecessary to cite facts so readily accessible in any well-appointed botanical library. The following will be of interest in connection with the statements quoted above:

1. Numerous and repeated hybridizations between *O. biennis* and other species have been made without obtaining anything resembling *O. Lamarckiana* in anatomy or behavior. Several unit-characters are exhibited by *O. Lamarckiana* not found in any other species.

2. *O. grandiflora* obtained pure in its original habitat is now giving off mutants in the cultures in the New York Botanical Garden, after a manner generally similar to *O. Lamarckiana*.

3. The close examination of the evening-primroses shows that the several species are extremely localized. *O. grandiflora*, discovered by John Bartram in 1778, was unknown except in gardens, until rediscovered in the original habitat by Professor S. M. Tracy in 1904. *O. parviflora* has been known in Europe at least since 1759 and has not been seen in America until 1905, when some undoubted indigenous specimens were found in Maine. During the interim the reasoning

shown above would have designated these two species as 'hybrid' or 'garden products.'

It is true, of course, that similar finality of evidence with regard to *O. Lamarckiana* has not yet been obtained, but it does not seem 'unlikely' that it may come to hand when we are able to organize a search for it as well-directed as for the species named. Collectors are few in the region to be covered, and it might be many years before it could be traced to its habitat, even if it occupied a large area. To those interested in suppositions, however, the following from Miller ('Figures of the Most Useful and Uncommon Plants described in the Gardener's Dictionary exhibited on Three Hundred Copper Plates Accurately Engraven after Drawings taken from Nature,' Vol. II., 1760) will suggest the need of caution in the matter. He says regarding the "Tree Primrose with oval spear-shaped indented Leaves, and Flowers proceeding from the Wings of the Leaves on the Upper Part of the Stalk":

> This plant is also a Native of *North America;* but was the first species of the Genus which was brought to Europe, so is more commonly seen in the Gardens than any of the other species. In some Parts of *Europe,* this is spread about from the Gardens in such Plenty, that it might be supposed a Native there. In a small Wood near *Haerlem* in *Holland* this Plant covered the Ground insomuch that many skilful Persons supposed it was a Native of that Place. But it may be easily accounted for; because the Gardeners who live near that Place, are chiefly Florists, and they annually change the Earth of the Beds in their Gardens; so by carrying out of their old Earth from their Beds, in which many of the Seeds were scattered the Plants came up there; and those being suffered to scatter their Seeds, had filled the whole Wood with the Plants.
>
> This differs from the first Sort, (described and figured as *O. biennis*) in having broader Leaves; the Stalks grow taller, and the Flowers are much larger. Both these Sorts will thrive in the Smoak of London better than most Plants.

The appended descriptions and the plate (No. 189, dated 1757) very fittingly characterize *O. biennis* and *O. Lamarckiana,* and as the descriptions were made before *O. grandiflora,* the only known species which might be confused with *O. Lamarckiana,* was discovered, this evidence is serious enough to give one pause in ascribing a hybrid origin to the last-named. Meanwhile the mutation-theory, based on the conception of unit-characters, will neither stand nor fall by suppositions or proof as to the ancestry of *O. Lamarckiana.* If this plant and all of its derivatives were obliterated from our records, the facts in our possession well warrant current conclusions as to unit-characters and their appearance and disappearance in hereditary strains in saltatory fashion.

A certain literary freedom of expression is well illustrated by the following citation from a recent article by Dr. Jordan in this magazine, in which he says:

In Mr. Burbank's cross of the English walnut (*Juglans regia*) with the California walnut (*Juglans Californica*) the first generation shows a certain blending of the traits of one species with those of the other. In the next generation appears every conceivable kind of variation in every feature of the plant and in every function of its organs.

The last sentence offers a fair example of the misrepresentation to which Mr. Burbank's horticultural work has been so profusely the object. A similar progeny of a hybrid oak is included in experimental cultures in New York, and the observer may readily see that the physiological possibilities are not exhausted in either case. To illustrate the possible variations in form would require many millions of individuals, as may be seen when a simple computation shows that seven single differentiations would require more than sixteen thousand individuals for their exemplification, if the characters behaved as indivisible units. If, however, qualities or characters are capable of modification or variation, as indicated by the quotation, the number of different forms of any organ of the entire plant would be so large as to make estimates useless. Then again by what extended experimentation have the conceivable variations in every function been ascertained?

A wider range of literary license prevails in some recent articles by Mr. E. A. Ortmann. Among other inaccuracies he says:

De Vries failed entirely to take notice of this fundamental principle (intergradation), and to show that his elementary species and his mutations are not connected by intermediate forms.

Although somewhat familiar with 'Die Mutationstheorie' and 'Species and Varieties,' no explanation occurs to me to account for this mistaken statement. A few combination forms were found and faithfully recorded by de Vries, but these were certainly not intergrades, whatever might be said of them.

Mr. Ortmann's discussions introduce a novel feature, in his estimate of the futility of experimental methods, which has the sole merit of boldness, coming at a time when the greater number of workers in the subject are turning from discussions and statements of opinion to actual observations. A mistrust is shown by him of experiments 'under artificial and unnatural conditions, as for instance in the botanical garden, or with domesticated forms.' Several months ago the following characterization of this attitude was given in a paper on the subject:[2]

Popular belief in the influence of environment and the inheritance of acquired characters finds its commonest expression in 'that plants have been changed by cultivation.' Domesticated races are spoken of as 'garden forms' by botanists and horticulturists, with the implication that they are specialized types resulting from the effects of tillage. Now so far as actual cultivation is concerned, this assumption is without foundation, since at the present time

[2] 'Heredity and Origin of Species,' *Monist*, January, 1906.

no evidence exists to show that the farm, garden or nursery has ever produced alterations which were strictly and continuously inheritable, or were present, except under environic conditions similar to those by which the alterations were induced, although vague statements and erroneous generalizations to the contrary are current. It it true of course that structural and physiological changes may be induced in a strain of plants in any generation, which may persist in a share to the second, or even in some degree to a third, but no longer.

So far nothing has been offered which would tend to disprove these conclusions.

It is by no means intended to maintain that the stream of heredity may not be altered by the action of external agents, and the possibility of having such changes ensue in experimental cultures beckons with alluring finger to the observer. So important do I hold this aspect of the matter, that a series of experiments, yet in progress, were begun previous to the mutation cultures, and these tests have been continued and expanded until one plant is now undergoing culture in New York, Jamaica, and in connection with the Desert Laboratory, at sea-level and altitudes of 2,300, 5,000, 6,000 and 8,000 feet, and under conditions widely different from those prevalent in its original habitat. If at the end of the decade, this, or any other of the species under test, shows any transmission of the characters induced by the various localities, the care and work necessary in the experiments will be richly rewarded. In this comment reference is had to factors presented by tillage, or entering into the environment of plants in their native habitats.

Announcement has been previously made that mutations, breaks, saltations or discontinuous action may be caused in inheritance by forces external to the protoplasts and cells, which are the true bearers of the hereditary characters (see p. 17).

The technique of the methods by which such changes are induced might be simulated by the action of gaseous emanations, from the soil, radio-action, foreign pollen, or by the stings and incisions of insects, but certainly these possible factors would be as potent with wild as with cultivated plants, as may be seen in the description of the manner in which such changes have been produced.

The Method of Pedigree-cultures

The importance of pedigree-cultures of plants as a means of tracing the course and action of heredity has been notably emphasized by recent investigations, and it will be profitable to go into a detailed statement of the manner in which experiments of this kind are carried out. One of the most striking developments of methods of research in botany has been the continually increasing extent to which taxonomists are having recourse to observations upon growing material in botanical gardens and experimental grounds. In the determination of difficult problems of relationship, it is becoming more and more customary to secure the individuals representing the doubtful forms and cultivate them under identical conditions, thus securing data for comparison and analysis, representing all stages of development of the sporophyte from the seedling to the mature fruit. The record of important questions which have been solved in this manner is a long one, and includes the investigations of Alexis Jordan on *Draba*. Sargent on *Cratægus*. Wittrock on violets, Britton and Rose on *Crassulaceæ* and scores of other less extensive researches.

If the observer becomes interested in the hereditary action of his plants in addition to a comparison of their anatomical and physiological qualities, it then becomes necessary to follow his plants from generation to generation to ascertain to what extent and in what manner variation may ensue.

The first step in this work is to secure purely fertilized seeds. Hybridization is not common among plants except in a few genera, yet in tests which must continue for a number of years every precaution must be used to ensure accuracy of results. The observer, therefore. covers the unopened flower buds of the individuals from which he wishes to procure seeds with bags of paper, or other suitable tissue, and then makes sure that pollination is secured spontaneously, or by hand, with no danger of admixture of any kind.

In due time the ripened seeds, with photographs, notes and proper herbarium material, are taken from the parental individuals. With the first lot of seeds on hand, the next step is to make a pure culture from them. To do this a quantity of soil of the proper consistency is secured, and while in a moist condition is heated to the boiling point of water in an oven on two succeeding days, or, better still, to a higher temperature in an autoclav for four or five hours. The treated soil may now be stored to be used as wanted, but at all times it must be guarded from possible contamination by the introduction of foreign seeds.

Seed-pans, of earthenware, or shallow wooden boxes are next secured and thoroughly washed in clean water and filled with sterilized soil, after which they are set in place in a cold frame, or in a greenhouse or germination chamber. As each pan is to be used it is taken to a special operating table, and the selected seeds are sown directly from the packet, so that from three to five hundred (in the case of small seeds) are evenly distributed over the surface. A thin even layer of earth is sifted over the seeds, a wooden label is affixed to the pan, giving all necessary data, and the pan is returned to its place in the culture room. If more than one species is being tested at the same time, the greatest care must be used to prevent admixture, and the remote separation of the pans may be necessary. The splashing of

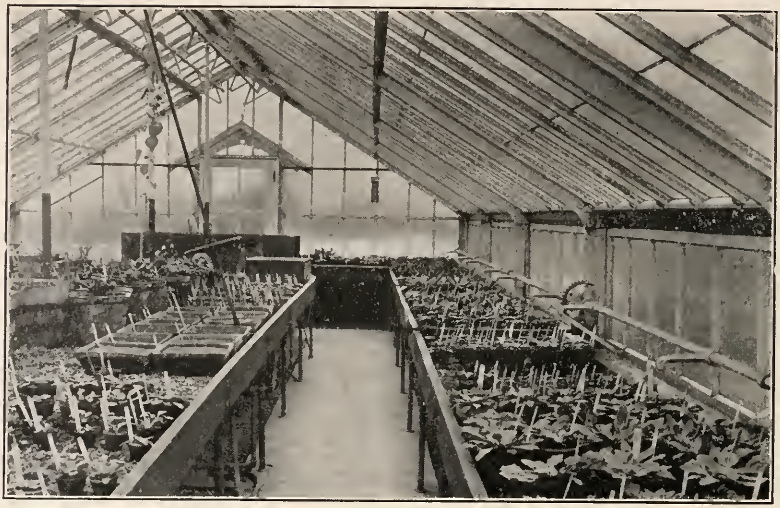


Fig 1. Seed-pans and Seedlings in Pedigreed Cultures in Propagating House, New York Botanical Garden.

water from a hose, the contact of the nozzle or of the spout of a sprinkler, the careless brushing of the sleeve or the hand against the dirt, may result in the transference of seeds and the vitiation of the results of years of labor and care, especially if a complicated series of tests is under way.

Having observed all the above precautions, the seeds finally sprout in due time, unfold the seed-leaves and begin development. The remote possibility is to be taken into account that the parental individuals may have been hybrids and that this, the second, generation may illustrate the resolutions, combinations, dominancy and recessivity of the ancestral characters. If, however, no such differentiation be encountered the observer has before him a progeny which by the multiplication of his seed-pans may be made to include as many individuals as might be found in a great geographical area during any season. The accurate examination of this material, and of that offered by succeeding generations may reveal evidence of the highest interest, bearing upon various problems in heredity. It is to be noted that if guarded seeds were not obtained for the beginning of the test, the more important work must await the second generation under culture for its beginning.

In even the first examination of the progeny of any physiologically unified strain it will be evident that some diversity of form and appearance is present. This variability is generally of the fluctuating type, that is, the entire number of individuals present may be arranged in a series with respect to any quality. Thus, if the one with the narrowest leaf-blades and the one with the widest are placed at opposite poles, the others may be arranged in a continuous series which shades by the smallest increments from one to the other. In making such an arrangement, it will be found that the greater number resemble, or lie near, an individual or group of individuals at some place along the line between the two extremes. Statistical measurements of this or any other quality may be made, and the position of the norm of the type determined accurately, as well as the range of variability. In like manner, the correlation between any two characters or qualities may be worked out. Now, having in hand such a mass of data from the initial culture, a fair basis is had for taking up various questions. Thus a succeeding generation might be grown in soil deficient of any nutritive factor, or poor in all of them, in rich soil, or in a substratum highly charged with any element, or at unusual temperatures for the purpose of determining the extent and manner in which the range of variability may be altered by such special treatment, and this may be accentuated by the continuance of the test in successive progenies.

Then again the pedigree-culture offers a fair opportunity for a demonstration of the influence of the effects of selection upon the range of variability and the mean value of any quality. Comparative cultures of seeds taken from the widest-leaved individuals with those of the narrower leaves will tend to show the result of such selection, especially if the selection is continued through several generations. In all these tests it is assumed that the seeds for the following generations should in all instances be guarded as directed above and sown in sterilized soil. With such close series of cultures the question of self- and close-fertilization might arise, and here again the culture affords invaluably exact material for a test of a subject upon which but little definite information exists.

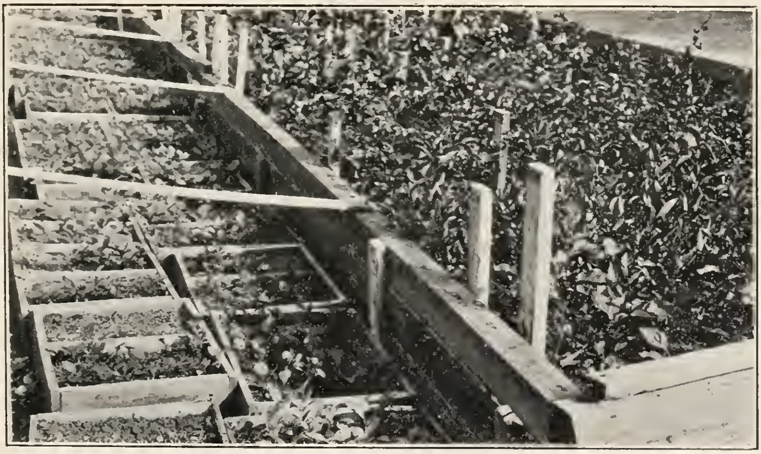


FIG. 2. SEED-PANS AND PLANTLETS OF *Cratægus*, IN PURE CULTURES, ARNOLD ARBORETUM

The possibilities of the pedigree-culture are by no means exhausted with covering the above points, especially if the plants chosen for the objects happen to be capable of vegetative reproduction, that is by cuttings or slips, in which case comparisons should be made of progenies grown from seeds of the plant with wide leaves with individuals grown from slips of the same, and also compared with others from the extreme end of the series.

It is by undiscriminating discussions of horticultural operations involving seed-selection and hybridization, followed by vegetative propagation, that the public mind has become confused as to the nature and

Fig. 3. Experimental Grounds showing Pedigreed Cultures of *Œnothera* in New York Botanical Garden.

possibilities of selection, notably by the 'popular' and pseudoscientific descriptions of the thorough work of Mr. Burbank. By successive selections certain features, such as size or quality, of a fruit are forced to a maximum development perhaps in a single plant, or may be in hundreds, after which the desirable quality is carried along, or propagated, in quantity by cuttings of the original plant, thus excluding a possibility of a return to the average habit of the species.

Or, in hybrid combinations and resolutions, the desirable constitution of some horticultural form may be secured only after the most highly complicated and repeated crossing, with a result too complex to be easily analyzed. With one desirable individual at hand which produces nuts, berries, cherries, apples, potatoes or plums, or timber, it may be made to produce hundreds and thousands exactly like it merely by using its buds and branches for grafting and budding or propagating.

The use of seeds of the desirable form, either from the original or from any of its derivatives, might give play to all the complex activity of the splitting of hybrids and of the free play of fluctuating variability.

During the course of study of the fluctuating variability of a species, by means of successive generations grown from purely fertilized seeds, the observer may be so fortunate as to encounter individuals which do not fit into his series by reason of the possession of some quality not visible in the greater number of the progeny, or by the lack of such qualities. Thus in a progeny of red-flowered plants one may be encountered which has white bloom, or an individual with laciniate leaves may come in a pure progeny the remainder of which has entire leaves,

FIG 4 *Opuntia fulgida* AND *O. mammillata* GROWING IN CONTIGUITY IN THE VICINITY OF DESERT LABORATORY, TUCSON, ARIZONA.

or a wholly glabrous specimen may be in a hairy progeny, or an individual may depart from the progeny in all these particulars. In either case it is apparent that the variability here is not one of the modification of a character, but by the total accession or loss of a character, and the variability is therefore a discontinuous one. In the progenies in which such variants have been seen hitherto, they form a proportion never larger than six or eight per cent. of the whole number and generally in much less quantity. If such mutants or saltatory variants be found, they should be closely followed, as they may furnish the observer with facts of the greatest value. Care should be taken to secure purely fertilized seeds and a minute anatomical examination should be made of the entire plant, with special respect to the characters in which it appears to differ from the mass of the fluctuating variants around the type.

The seeds secured from such mutants should be sown as directed above, and as many individuals as possible secured for comparison with the parental type, which should be continued as before. If now the individuals of the mutant progeny are placed in a series with respect to any given quality, statistical observations may show whether it is included within the range of fluctuating variability of the parental type. The question therefore as to whether a plant is a continuous or discontinuous variant is one of simple measurement and estimation of qualities, not a matter of opinion. With that simple question easily disposed of, the investigator may next turn his attention to the correlations, a phase of the question of vivid interest, since the sparse data at hand seem to point to a wider range of variation and less degree of correlation in such mutants than in the parental type. That is to say, instead of mutants being derived from forms showing widely fluctuating variability, we have them appearing in the progenies of species in which the range of variation is small and the correlations close, while the derivatives themselves swing through a wide range of fluctuations.

Fig. 5. Rosette of *Raimannia odorata*.

As a complementary means of investigation of the constancy and independence of the various characters dealt with in selection and mutation as observed above, hybridization forms an invaluable means of analysis, and is to be constantly resorted to in all stages of the various phases of the work, since in the observation of the behavior of unit-characters in combination and after resolution the clearest appreciation of their character may be reached.

Fig. 6. Rosetta of Mutant of *Raimannia odorata* induced by Chemical Treatment of Ovules.

Still another phase of variability is that in which the greater portion of an individual will be found to comply with the requirements of a continuous series, but which bears a branch or portion of a branch which differs notably by the loss or acquisition of characters. Although all discontinuous variations or mutations are essentially vegetative, yet these are generally termed so exclusively. It is to be noted that such sports, or mutants, when closely fertilized, come true to their aberrant characters. Among other numerous conclusions sustaining this point, one which has recently come to notice again is that of Kerner,[3] who said:

> The fact is that all plants may at some time sooner or later produce an aberrant branch, which differs from the parental type in many characters, and numerous phenomena force us to the conclusion that under the conjunction of favorable circumstances such aberrants would become the starting points of new species.

In the context the author is careful to point out the limitations of such a method of origin of forms.

FIG. 7. *Œnothera biennis*, THE COMMON EVENING PRIMROSE.

Actual pedigrees from such sports or aberrant branches have been tested, in one instance by de Vries and in two others by myself, with the unanimous result that they were found to be constant to their aberrant characters.

Having carried on such pedigree-cultures with a large number of species for several years and having encountered some which did and others which did not give rise to aberrant individuals, attention was directed to the possibility of inducing changes in the hereditary elements in such a manner that the qualities transmitted would be altered or destroyed. A theoretical consideration of the subject seemed to indicate that the changes constituting the essential operation of mutation ensued in a stage previous to the reduction divisions in the embryo-sac, or the pollen mother cells. It was planned therefore to subject

[3] 'Die Abhängigkeit der Pflanzengestalt von Klima und Boden,' Innsbruck, 1869.

these structures to the action of chemical agents, not ordinarily encountered by the elements in question, at a time before fertilization occurred. The tests were planned to include the use of a solution of high osmotic value, and mineral compounds, some of which are toxic in concentrated solutions and stimulating in the proportions used. The probability of success would be heightened with the number of ovules contained in any ovary operated upon, and therefore the common evening-primrose, *Œnothera biennis, Raimannia odorata* a relative of it and a member of the same family, *Begonia, Cleome, Abutilon, Sphæralcea* and *Mentzelia* and others were experimented upon. Without recourse to the detail of the work, it may be stated that the use of radium preparations, sugar solutions (10 per cent.), and solutions of calcium nitrate one part in one to two thousand of distilled water with capsules of *Raimannia odorata,* and zinc sulphate in a stronger solution used with *Œnothera biennis,* was followed by very striking results. In the first-named plant, there appeared in the progeny obtained from a few capsules of one individual, several individuals which were seen to differ notably from the type with the appearance of the cotyledons, and, as development proceeded, it was evident that a mutant had appeared following the injections and nowhere else, and thus to have some direct relation to the operation. The characters of the newly arisen form were so strikingly aberrant as to need no skill in their detection. The parent was villous-hairy, the mutant entirely and absolutely glabrous, the leaves of the parent have an excessive linear growth of the marginal portions of the leaf-blades and hence become fluted; the excess of growth in the mutant lies along the midrib and the margins become revolute. The leaves are widely different in width, those of the mutant being much narrower. The parental type is of a marked biennial habit and near the close of the season the internodes formed are extremely short, which has the result of forming a dense rosette; the mutant forms no rosette by reason of the fact that the stem does not cease, or diminish its rate of elongation, and hence presents an elongated leafy stem, which continues to enlarge as if perennial. The first generation

Fig. 8. Mutant of the Common Evening Primrose, induced by Treatment of Ovules with Solutions of Zinc Sulphate.

of the derivative came to bloom at the beginning of the present year, and bare mention of the existence of the derivative was given in a lecture before the Barnard Botanical Club at that time. The real value of the changes induced however lay in the transmissibility of the newly exhibited qualities. The flowers of the mutant were closely guarded and as soon as seeds were obtained these were planted to obtain a second generation. A few plants were obtained, which in every particular conformed to the new type and exhibited no return to the parental type.

Injections of the ovaries of *Œnothera biennis* were followed by the production of one individual, which was recognizably different from the parental type in many qualities, some of which were plainly apparent even in the earliest leaves of the seedlings. These differences have become accentuated with the adult plant and are graphically illustrated by figures 7 and 8. The succeeding generations of this mutant are yet to be tested. The parental form has been under observation for five years in cultures and in a wild condition. An aberrant form, which appears to be ever-sporting, has been previously figured, and while this form appeared in the injected or treated seeds in a normal proportion, yet the newest aberrant has not been seen elsewhere. The probability must be taken into account that it may be a mutant of rare occurrence, the cycle of which came within the experiments, but in either case it is plainly a mutant, and it only remains to be seen whether or not it was induced by the action of the zinc solution. The presumption seems to favor such an affirmative conclusion.

In finding our way about in the voluminous literature of evolution it must be borne in mind that the subject embraces the origin and development of the universe, and that it has engaged the serious attention of workers in all branches of knowledge. The multiplicity of viewpoints has resulted in the greatest diversity of conclusions as a necessary concomitant of widely differing methods of approach to the subject. Much that has been written concerning the subject is of a purely literary or polemical character, embodying prejudices, general opinions and beliefs, putting forward conclusions drawn at long range from attempted interpretations of the results of investigations not properly considered, and brought out for the sole purpose of swaying opinion or influencing sentiment. All work of this character as well as narrow and insecurely founded investigations are futile and ineffectual except to befog the subject and hinder progress.

The problems included in a study of organic evolution are essentially physiological, and the elucidation of the mechanism and action of heredity by which qualities, characters and capacities are transmitted from generation to generation may be accomplished only by accurate observations and experimental tests with active or living material. The examination of preserved material not in hereditary series, or the wide generalizations derived from geographic studies, may not contribute to the progress of exact knowledge of genetics, or methods and manner of inheritance.

The combined and organized efforts of all the botanists in the world concentrated upon all the herbaria in existence would add but little to existing conclusions upon this subject, if we may judge by past achievements or immediate promise, while the most precise information upon geographical distribution can be of interest only in deciphering what has been accomplished, what forms exist and where, the factors influencing their movements, and where these have probably originated.

To appreciate the mechanism of heredity an exact knowledge of the nature and behavior of the bodies which form its physical basis must be gained. To ascertain the action of heredity, statistical and accurate observations must be made upon long series of pedigreed progenies, and these must be carried out in such manner that environmental conditions may be either controlled or their influence measured. Pedigree-culture, first extensively applied by de Vries to clovers, teasels, poppies, snap-dragon and evening-primroses with such marked success, and now used by many workers with animals as well as plants, has proved to be one of the most efficient forms of research yet used by the biologist, and its usefulness is hardly beginning to be realized. The various phases of selection, the accurate measurement of fluctuating variability, correlations, the amount and character of the influence of environmental factors, the effects of close and cross breeding, and the detection of saltatory variations may be accomplished under circumstances which allow a thorough and exact appraisement of the general physiological value of such phenomena by the use of cultures in continuous series.

While the phases of evolution are generally estimated in terms of origin or formation of species, the basal problems of heredity are not especially concerned with the taxonomic estimation given by this author or that author as to the taxonomic standing of any form, nor does it matter whether it is a subspecies, elementary species or 'real' species. The questions of evolution are to be answered by the acquisition of more accurate knowledge concerning the accession, modification or loss of functions, capacities and characters of physiologically unified groups of organisms, regardless of the necessarily more or less artificial appraisements of taxonomy. The briefest review of recent literature will reveal the widest diversity of opinion between botanists and zoologists, and also unexplainable differences among botanists and zoologists as to the species-conception. The value of discussions into which such possible differences may enter is not enhanced by this fact.

The carrying out of pedigree-cultures in New York has revealed the occurrence of discontinuous variants or mutants in *Œnothera biennis,* and *O. grandiflora* among the species tested, in addition to furnishing ample exemplification of the derivatives of *O. Lamarckiana,* as described by de Vries. Mutations in other genera await further test and observation, a matter which may occupy some time, before final announcement.

Despite general assertions to the contrary, no evidence has yet been obtained to prove that the influence of tillage, 'cultivation,' or the mere pressure of environmental factors has produced any permanent changes in hereditary characters of unified strains of plants.

The above is not meant as a sweeping assertion that inherited characters may not be affected by agents external to the protoplasts that bear them. On the contrary, the experiments now well advanced and conclusively verified, first announced in December, 1905[4], and here described for the first time, show that saltatory inheritance has been induced by the action of external agents upon the ovules of two species of seed-plants.[5] The alterations in question consist in the total suppression of some qualities of the parental form and the substitution therefor of new characters or of a total gain of new qualities in some instances, and the differentiating points between the parental form and the derivative are both anatomical and physiological.

The atypic form which has been tested to the second generation in one species is found to constitute a mutant in the sense in which that term is used by de Vries, and is a real and actual departure from the course of the hereditary strain. The capacity of the mutants induced in this manner for survival would depend entirely upon the environment into which they might be thrown.

If we seek a similar possible intervention of external forces which might act upon the plant unaided by man, we might find such influence coming from radio-active substances, such as spring- and rain-water, or from the effects of sulphurous and other gases which are being set free in numberless localities, or the protoplasts most nearly in contact with the egg-apparatus may well excrete substances which would produce the same effect, without regard to the forces which originally caused the disturbances in the extra-ovular tissue. Lastly it is to be said that the actual technique of injection might be imitated in a measure by the action of foreign pollen which might find lodgment on the stigmatic surfaces, and sending down tubes through the style introduce unusual substances to the vicinity of the egg-cell without participating in normal fertilization, which would ensue in the customary manner. Lastly it is to be said that it would appear that a most prolific source of such disturbances might be expected to result from the stings and lacerations of insects, or the action of parasitic fungi, both sources of the most profound morphogenic alterations in somatic tissues, profusely exemplified by the well-known gall formations of plants.

[4] MacDougal, 'Heredity and Origin of Species.' Reprinted in advance from the *Monist* for January, 1906, and distributed December 18, 1905.

[5] The possibility is not excluded that the reagents may have affected the elongating pollen-tubes.

AMERICA AND SEISMOLOGICAL RESEARCH

BY DR. WILLIAM HERBERT HOBBS

BUT a few years ago, the American naval officer serving his tour of duty upon the European station found in the antiquated vessels in which he was compelled to appear a constant source of mortification. This condition has now passed, and it is the geologist who in his turn is humiliated as the modern European earthquake station is opened for his inspection. A great earthquake upon American home territory has been registered by all first-class seismographs throughout the world, and the records have been collected for comparison and study at central stations. It is the kindest thing to say of the American records that they are a negligible quantity—for measured by modern standards they are—but unfortunately their inclusion in the autograph albums of the California earthquake of 1906 does not allow them to be overlooked. Thus the backwardness of our country in a most important branch of the great science, in which we had perhaps thought ourselves entitled to some respect, is patent to all.

It will hardly be claimed for us that the United States offers no opportunity for earthquake investigation. In 1811 a devastating quake affected a large area in the central Mississippi Valley, in 1872 occurred the great Owens Valley earthquake in Nevada, and in 1887 the Sonora earthquake of even greater violence; not to mention the Charleston and the recent California seistus. Lighter shocks have been frequent, and the greatest of earthquake authorities, the Count de Montessus de Ballore, showed some years ago that New England, the St. Lawrence Valley, the central Atlantic coast generally, the central Mississippi Valley, and above all the Pacific coast of the United States, must be regarded as notable earthquake provinces.

The better to understand our true position, let us consider what has been accomplished in earthquake investigation within the last ten years. First, and most important, the laws of earthquake distribution have been determined, and the relation of earthquakes to topography and geology has given us a new branch of science—seismic geography. This is almost exclusively the work of one man, the Count de Montessus de Ballore, major of artillery in the French army, who has given the better part of his life to this arduous labor.

From a wholly different direction the problems of earthquakes have been approached through the perfecting of seismometrographs, until they register all great seisms of our planet, however distant. This point reached, an entirely new field has been opened before our eyes. The new autographs of earthquakes have characters dependent upon the distances the waves have traveled to produce the record; so that the observer at a station can unaided give the distance of the disturbance within 50 miles, an error negligible in view of the extended area disturbed.

By combining records made at several widely-separated stations, not only the distance from a given station, but a sufficiently exact location for each quake, is easily obtained. To have developed a great system of some forty such stations, scattered throughout the length and breadth of the globe, is the great service rendered to science by the British Association for the Advancement of Science, especially, and by the leader of its Seismological Committee, Professor John Milne. Thus it has been revealed that the great earthquakes of the planet are twentyfold more numerous than those reported by observation *in situ,* and that most of them occur upon the floor of the ocean, where other methods of observation would have failed to reveal their presence.

The analysis of the complex of waves registered in the seismogram is extending our knowledge of the nature of the earth's interior, and affording the solution of problems which, in importance and in difficulty of approach from other directions, can only be compared with those now being solved by the study of radioactivity.

No attempt to sum up the achievements of seismological research during the past ten years should fail to note the fact that the Japanese have for systematic and thorough study of the general problems, but even more for the practical applications of these investigations to the amelioration of the conditions in an earthquake-tormented country, taken the first place among the nations. Italy, also, with almost a score of stations of the first rank and with two hundred correspondents scattered throughout its small territory (to telegraph the first news of a quaking to the main office at Rome), has played no mean part in the advance of the science.

The center of earthquake investigation upon the continent is now, however, the Imperial German Chief Station for Earthquake Investigation at Strasburg. Professor Gerland, its director, now issues the annual catalogue of earthquakes, and he has the credit of having organized, in 1903, the International Seismological Association, and of having founded its journal, the *Beiträge zur Geophysik.* The work of the station now devolves largely upon his highly-trained assistants, Professor Rudolph and Dr. Sieberg.

The writer assumes that a beneficent result may be expected to follow from the frightful calamity at San Francisco in the stimulation of seismological research in America; so that we may later take our proper share of both the labor and its rewards. The start can easily be made too hastily. There is much contention over the merits of the different types of seismometrograph, which differ as widely as possible. It is perhaps not strange, in view of the new vistas opened for discovery, that the analysis of the records from some instruments not provided with compensating devices has brought out waves supposed to originate in the earth, which exist only in the vibration periods of the instruments themselves.

America's broad extent and her outlying territories and protectorates (Alaska, Cuba, Porto Rico, Panama, the Philippines, Hawaii, Guam and Tutuila), offer her special advantages for a correlated system of earthquake stations; but she will do well to wait until her principal station has been well established, her type of seismograph determined, and a corps of trained expert observers found. This will require some time, and can be greatly hastened if pride be put aside and some one of the thoroughly trained men available in Europe be invited to superintend the erection of the first earthquake station.

Some sacrifice the pioneer must always make, and so it happens that the English stations are fitted out with a type of instruments already obsolescent. On the point of establishing her outlying stations (German East Africa, Shan Tung, Samoa), Germany will be more fortunate. The maker of scientific instruments for almost the entire world, she has steadily perfected her types before launching upon the larger undertaking. America will have at least the consolation of profiting by the experience of the other nations during the past ten years, and there is need for much study of it.

The recent investigation of earthquakes has thus developed along two somewhat different lines: (1) the macroscopic study upon the ground of felt quakes, undertaken by men trained as geologists; and (2) the microscopic investigation of the distant, large, locally 'unfelt' quakes, undertaken at special earthquake stations by men trained as physicists. There is much need that these different lines of endeavor should be brought into as close a union as possible, for only through mutual support can the best results be achieved. As Dr. Sieberg, the secretary of the Strasburg station, told the writer, the more difficult of the seismograms afford equivocal data if not checked by the reports 'from the field.'

The American Association for the Advancement of Science brings the geologist into association with his brother the physicist, as well as with many other scientists who take an interest in investigations of such general interest as those upon earthquakes. The writer takes this opportunity to urge that the association follow the glorious example of its British cousin and select from its membership a committee to watch over the interests of seismological research in America and to direct the course of legislation in accordance with its teachings.

IMMUNITY IN TUBERCULOSIS[1]

BY SIMON FLEXNER, M. D.

ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH, NEW YORK CITY

I CAN not begin this address without delaying a moment to testify to my sense of the great honor which has been conferred upon me by your invitation. Neither can I proceed with it until I have expressed to you my conviction that there are persons present in this audience whose scientific work on tuberculosis makes them far abler than I to discuss the complex problem of immunity in tuberculosis. My work in bacteriology in the past has not led me to an especial consideration of the highly important problem of the prevention and cure of tuberculosis, and I can therefore account in no other way for my selection to address you this evening than that you desired this topic presented to you from the point of view of one who has done some work in the general field of bacteriology.

The modern study of tuberculosis, as you know, begins with the generation which immediately preceded the epoch-making discoveries of Koch. It may, I think, be said with justice that this study was inaugurated by the first purposeful transmission by inoculation of the disease from animal to animal. For whatever may have been the speculations upon the infectious and transmissible character of the disease before this demonstration, yet the demonstration was necessary before further steps in the elucidation of the cause and prevention of the disease could be taken. Koch in his masterful monograph gives the credit of successful inoculation to Klencke, who in the year 1843 succeeded in inducing an extensive tuberculosis of the lungs and liver in rabbits by inoculation with portions of miliary and infiltrating tubercles from man. Klencke, after accomplishing this result, did not continue his investigations and they were consequently soon forgotten. In the meantime Villemin's experimental investigations were begun and pursued to a successful termination. He inoculated not only with tubercular material from human beings, but also from cases of bovine tuberculosis, and he seemed to have proved experimentally the identity of the latter disease with human tuberculosis. Villemin's researches, from the number of his experiments, the careful manner in which they were carried out and the employment of suitable control experiments,

[1] Address delivered at the joint meeting of the Association of American Physicians and the National Association for the Study and Prevention of Tuberculosis, held at Washington, D. C., May 16, 1906.

appeared to decide the question in favor of the infective theory of tuberculosis. The numerous workers who repeated Villemin's experiments, after the same or modified methods, arrived at very contradictory results. The opponents of the infective theory strove to prove that true tuberculosis could be induced by inoculation with *non-tubercular* material. To the decision of this question Cohnheim and Salomonsen contributed largely by selecting for inoculation the anterior chamber of a rabbit's eye. The great advantage which this method possesses over all others arises from the fact that the course of a successful tubercular inoculation can be watched throughout by the experimenter until the pathological process has advanced so far that the whole organism—the neighboring lymphatic glands, the lungs, spleen, liver and kidneys—becomes tuberculous. A further point in favor of this method of inoculation is that spontaneous tuberculosis of the eye has never been observed in rabbits. It was reserved for the genius of Robert Koch to discover nearly twenty years later, in 1882, by the employment first of an original staining method, the tubercle bacillus in sections of tuberculous organs, and next by the use of a special method of artificial cultivation, to secure growths of the bacillus free from all admixture with extraneous matter. With these pure cultivations he succeeded, as you well know, in reproducing in certain domestic animals all the characteristic appearances of tuberculosis in man. Furthermore, Koch's studies of this period convinced him of the unity of causation of the various tubercular affections met with in man and also of those met with in the common domestic animals. Refusing to be daunted by the fact that tuberculosis tends to appear under different aspects in each species, and directing his attention not upon the gross appearances of the disease, but focusing it upon the microscopical appearances of the primary tubercle, which as he said recurs with typical regularity in all the different processes in man, Koch recognized the essential identity of the apparently widely different forms of tuberculosis in the various species of animals. It does not detract from the immense value of his work that Koch failed to distinguish between the tubercle bacilli isolated from the tubercular tissue in fowls, cattle and man. This failure was by no means accidental, for the possibility of the existence of differences in nature of the cultures depending upon their origins was clearly in his mind. Many of you will recall the long list of cultures which is given in the paper on tuberculosis published in 1884. In regard to this list Koch says: "It may cause some surprise that so relatively large a number of cultures was set on foot when a few would have sufficed for observing the behavior of bacilli in cultures. It seemed to me, however, not improbable that though bacilli from varying forms of tuberculosis—perlsucht, lupus, phthisis, etc., presented no differences microscopically, yet, that in cultures differences might become apparent between bacilli from different sources. But although I devoted the greatest attention to this point, I could find nothing of the kind. In all the cultures, whether taken from miliary tubercles, lupus or perlsucht, the tubercle bacilli behaved exactly the same."

Our knowledge of the nature of the tubercle bacillus has been increased until at this time several distinct kinds are recognized. These may conveniently be classified according to their chief sources into human, bovine and avian tubercle bacilli, and into so-called tubercle bacilli of cold-blooded animals. This last group of bacilli, which will detain us only a short time, differs greatly from the other varieties, as can readily be seen when the fact is recalled that the high temperatures —temperatures approaching blood heat—which are required for the growth of the mammalian and avian bacilli, quite preclude their multiplication under conditions of ordinary external nature. Hence they are not adapted to a life outside the living body except as cultivated artificially at this relatively high temperature. In man's conflict with tuberculosis this fact is of the greatest service, since by reason of it he is enabled to disregard the danger of any increase in tubercle bacilli outside the animal body. The relatively low temperatures at which the tubercle bacilli of cold-blooded animals develop adapt them, indeed, to an independent existence; but, as they are wholly devoid of power to cause disease in warm-blooded animals and as they would appear to have a restricted dissemination even among cold-blooded species, they are of comparatively small importance.

Of far greater consequence is the question whether the disparity which exists between the several kinds of tubercle bacilli derived from warm-blooded animals is a wide one. This question, which at first sight may appear to be chiefly of academic interest, has, in reality, far-reaching practical significance. The close relationship which man bears to domestic animals makes every fact of animal disease of high value to him. And in the case of no animal disease are facts of greater moment than in tuberculosis. Not only is the human race, by reason of its dependence upon the animal kingdom for food, work, etc., exposed to the diseases of animals which are transmissible to man, but domestic animals are also exposed to diseases of human beings. This correlative susceptibility may, therefore, cooperate to produce a vicious circle of events by which infection or the dangers of infection are kept alive and threatening. Hence it is that an effective solution of the problem of limitation of tuberculosis, whether by suppression outright or by suppression through the induction of immunity, must take into account the degree to which tuberculous animals of different species, through direct or more remote association, are a source of danger to one another.

There is no longer any doubt that the avian tubercle bacillus departs considerably from the human and from the bovine types of bacilli.

The early observations of the Italian investigators, Rivolta and Mafucci, have been confirmed and so extended as to give us a fairly comprehensive knowledge of the capacities for pathogenic action, upon different animal species, of the avian bacilli. At the same time painstaking studies of the degree to which birds are subject to inoculation with pure cultures of tubercle bacilli of human origin support the view of diversity in type of bacilli and susceptibility of species. And yet, while fowl react only with slight local lesions, as a rule, to inoculations of tubercle bacilli of human origin, certain mammals have proved themselves fairly subject to experimental inoculation with avian bacilli. While the guinea-pig, otherwise so sensitive to inoculation tuberculosis with the mammalian bacilli, is relatively resistant to the avian variety, the rabbit, which exhibits a marked degree of refractoriness to the human bacilli, succumbs quite readily to the avian bacilli. It is, however, worth noting that the reactions in the rabbit which avian tubercle bacilli call forth do not conform to those observed in tuberculosis in general; there is absence of typical tubercles and caseation, and the chief pathological alterations observed are found in connection with the enlarged spleen.

The literature on tuberculosis contains a small number of references to the cultivation from human subjects of the avian tubercle bacillus. From our present knowledge it may be postulated that avian tubercle bacilli occur rarely in man. Rabinowitsch has, indeed, recently emphasized the occasional occurrence of the avian bacilli in cattle, swine, horses and monkeys; but they constitute a small source of danger in the spread of tuberculous disease among mammals. The parrot, because of its use as a pet and of its susceptibility to the avian bacillus, on the one hand, and of the human bacillus, on the other, is a greater menace to public welfare.

The subject of bovine tuberculosis and of bovine tubercle bacilli is among the most important of all the questions relating to the suppression of tuberculosis. The admirable studies of Theobald Smith established the distinction in type subsisting between certain bacilli of human and of bovine origin. We have come now to regard these types as separate and not to be transmuted, at least not readily under artificial conditions of cultivation, into each other. Into the disputed questions of variation due to environment I can not afford to enter. But I would have you believe that transformations of avian, bovine and human bacilli into each other have probably not been accomplished by experimentation. The cultivation of one variety of bacilli in the body of an alien species has been said to alter profoundly the properties of the bacilli; but the observations upon this point are in my opinion far from convincing. The mere fact that avian and bovine varieties of bacilli preserve their peculiar properties when occurring naturally in the diseased body of an alien species—man, for example—tends to discredit the experimental transmutations referred to.

Bovine tubercle bacilli are characterized, as ascertained by Smith, by a greater degree of pathogenic power for mammals in general than human bacilli, with which fact is correlated certain peculiarities of cultural and physiological properties serving further to separate the bovine from the human bacilli. The bacilli of mammalian origin are, perhaps, closely related and less removed from each other by the sum of their properties than they are from the avian bacillus. With the few exceptions mentioned all forms of mammalian tuberculosis are caused by either the human or the bovine bacillus.

In view of the general fact that the bovine bacilli show a greater degree of pathogenic action for the lower mammals than the human bacilli, it was natural to assume that bovine bacilli would be powerfully pathogenic for man also. To test this probability directly by experiment is, of course, not permissible. But the belief that tuberculosis in cattle is a menace to man is expressed in the many regulations by which it is aimed to control and prevent the use as food of products derived from tuberculous animals. It was not until Koch's address was delivered in 1901 that any serious doubt existed in the minds of sanitarians and pathologists that tuberculous cattle offered a source of danger to man. The specific knowledge which has accumulated since that date has served to establish the transmissibility in some degree of bovine tuberculosis to the human subject. The inherent difficulty and tediousness of the investigation of the specific types of tubercle bacilli existing in human cases of tuberculosis necessarily limit the total number of instances in which it has been established, beyond peradventure, that the bovine type of bacillus does occur in tuberculous processes in man. In this country the responsibility of refuting the too general statement of Koch has fallen chiefly upon Ravenel and Theobald Smith, whose admirable studies in this direction are of a convincing nature.

If we pause for a moment to consider upon what data Koch based his statement of the independence and non-communicability of tuberculosis in cattle and man, we shall appreciate that, in so far as he dealt with established fact and not hypothesis, he had long been anticipated. That cattle are highly resistant to infection with tuberculous material and tubercle cultures obtained from human subjects can be concluded from the early experiments of Baumgarten, Sidney Martin, Frothingham and Dinwiddie. The most conclusive evidence upon this subject is contained in Theobald Smith's paper of 1898, in which he summarizes his experiments by stating that "putting all the facts obtained by experiments on cattle together, it would seem as though the sputum bacillus can not gain lodgment in cattle through the ordinary channels."

In view of these facts, it is not surprising to find that Koch and Schütz later failed to produce marked or general tubercular infection of cattle by feeding or inoculating directly into the circulation tuberculous materials and cultures of tubercle bacilli of human origin. That this result does not dispose of the entire question at issue, but leaves open the important consideration of the implantation of the more virulent bovine bacilli upon man was, of course, present in Koch's mind, and was met by him by emphasizing the infrequency with which primary intestinal tuberculosis, which is the form of tuberculosis presumably arising from ingested virulent tubercle bacilli, is encountered in human beings. The reports which have appeared since have tended to show that primary tuberculosis of the abdominal viscera, especially in children, is not so infrequent as Koch believed it, and the researches inspired by Koch's address have brought out the important fact, now based upon actual observation under the microscope, that tubercle bacilli may pass through the intact intestinal wall and reach, by means of the lymph current, the mesenteric glands; and have made it seem probable, also, that by entering or being carried into the blood vessels in the intestine the bacilli may be carried to the lungs. When all the known facts of food infection in tuberculosis are assembled, they make quite an imposing array, for they indicate, quite in opposition to the exclusive view expressed by Koch, that tubercle bacilli entering the body with food may be implanted upon the mucous membrane of the mouth, from which, probably, chiefly in the region of the tonsils, they may be carried to the lymphatic glands of the neck and adjacent parts where they develop and produce tubercular disease; or they become implanted upon the intestinal mucosa and pass the epithelial barrier without first causing disease there, and set up lesions in the mesenteric lymph nodes or even be transported by the blood or lymph to the distant lungs; or they may first multiply in the intestine, cause tubercular disease there, and then migrate further, involving the abdominal and thoracic organs.

If I have seemed to tarry too long over this aspect of my subject, I will ask you to consider for a moment in how far the endeavor to limit the spread of tuberculosis among the human race must be influenced by the avenues of infection to which the race is exposed. If we side with Koch in the view expressed in 1901, and reiterated just the other day in his Nobel-prize address, that, as he says, human tuberculosis and tuberculosis in cattle are so distinct from each other that the latter is not to be feared as transmissible to man, at least, as his last utterance puts it, not in a form which comes in consideration in regard to tuberculosis as a '*Volkskrankheit*,' or race disease, then it is only necessary to direct efforts to the suppression of tubercle bacilli of human origin. For, if the danger of infection of surroundings and healthy individuals is limited to the expectoration of persons suffering from tuberculosis of the lungs and upper air passages, the problem before us, while still very large, is less by a considerable amount than if there must also be taken into account the widely prevalent disease among cattle, swine and other domestic animals. While I do not pretend to speak in terms of great authority, yet it would seem to me that the time is not yet ripe to disregard, in attempting to suppress tuberculosis, the disease in domestic animals. Greatly as I sympathize with the active propaganda which is being made by instruction and material help to protect tuberculous human beings from injuring themselves and others, and greatly as I hope to see promoted the means of caring for the tuberculous in sanatoria, etc., yet I hope that there may occur, at this time, no relaxation in the efforts being made to control the spread of tuberculosis among cattle and to prevent the consumption of infected milk and flesh by man and other animals. That, on the other hand, the suppression completely of tuberculosis among cattle would not be followed by a great reduction in the morbidity due to tuberculosis in man is shown by Kitasato's statistics from Japan. In that country the human disease prevailed with its usual activity at a time when the cattle disease, owing to the absence of cattle, was unknown, and milk formed no appreciable element in the food of children.

In dealing with the complex problem of tuberculosis—a problem whose difficulties enlarge with the continued growth in size of cities—we are materially assisted by the knowledge of the manner in which the virus of tubercle is separated from the diseased body, the conditions of its contamination of our environment, and the avenues through which it endeavors to enter the healthy body. Though it is, perhaps, scarcely to be hoped that a time will arrive when tuberculosis will have become, through precautions against infection, as rare as are to-day smallpox and typhus fever, yet it is a most hopeful result of the crusade against tuberculosis that a marked reduction in the mortality, and probably in the incidence of the disease, has been going on in some countries—as, for instance, in England—for forty years. In New York, the system organized by Biggs has brought about a reduction since 1886 of 35 per cent. in the mortality of the disease; and while in Prussia the mortality was stationary in the decade from 1876 to 1886, since that time a reduction of more than 30 per cent. has been noted. These figures show what may be accomplished in reducing the dangers of infection with tuberculosis by a régime of education, improved conditions of living for the poorer classes, and the segregation in hospitals and sanitoria of any considerable number of the infective tuberculous during the most dangerous period of the disease.

The discovery of the microbic agent of tuberculosis naturally awakened the hope that a specific means of treating and, possibly, of preventing tuberculosis might now be found. The early years following the cultivation of the tubercle bacillus saw no realization of this hope, and to-day we are still far from the desired goal. However, the prodigious labor which has been expended in the search for a means of protection against infection with the tubercle poison has not been wholly devoid of results.

In an address of this kind it is not practicable to deal with the separate contributions, in detail, which the many workers have made to the subject of immunity in tuberculosis. The most that can be accomplished is to bring together the more important results of all the workers and, after having assembled them, to judge of their value and to consider, possibly, in what important respects they are still imperfect. I can not do better, at the beginning, than to remind you that the successful point of departure has been the discovery that variations in type and in virulence exist among tubercle bacilli. The earlier view which taught that the tubercle bacillus is a micro-organism of uniform and fixed virulence has been shown to be erroneous, first by the discovery of variations according to certain origins, and second by a gradual decline in pathogenic power suffered by certain strains through long cultivation outside the animal body.

The animals which have been of special use for tests of immunity are rabbits, cattle and goats. The guinea-pig, which furnishes an almost ideal animal for the detection of tuberculosis, because of the sensitiveness of its reaction to inoculations with tubercular material, fails, for the same reason, to be a highly suitable animal in which to carry out tests of immunity; and yet it has been employed with some success.

The first important contribution to the subject of experimental immunity in tuberculosis was made by Koch in connection with his researches on tuberculin—a product of the growth in broth of tubercle bacilli, freed from the bacilli and concentrated. In spite of the failure of tuberculin to bring about a favorable issue in all cases of human tuberculosis in which it is administered, it still remains a useful, perhaps the most useful, strictly medicinal agent employed for the treatment of tuberculosis. But the sum of its useful properties is not embraced in its employment as a therapeutic substance: it is also a diagnostic agent of high value, and its action upon the tuberculous organism is so specific and remarkable that it has proved itself of the greatest importance and aid in the effort to unravel the complicated series of biological phenomena which constitute the tubercular state.

It is possible to increase somewhat the resistance of animals to tubercular infection by previous treatment of tuberculin; but this increase is not remarkable. It is possible to bring about arrest of the tubercular process in the infected organism by means of tuberculin;

and in some instances this arrest leads, through the changes induced in the tuberculous tissue by means of the tuberculin injections, directly to cure, or indirectly, through an increased power of resistance and attack on the part of the forces of the organism, to eventual cure. But a high and lasting degree of immunity has never been secured by the use of tuberculin. This fact, disappointing as it was at first, is now easily explicable. Tuberculin does not represent the entire series of forces contained in the bacilli which the body has to resist in preserving itself from infection with tubercular poison. The peculiar principles contained in tuberculin are, indeed, not highly toxic for the normal individual; and our experience in securing immunity to micro-parasites and their products has taught us that where no reaction or response to the introduction of the foreign poison is called forth, no degree of protection to larger doses or more virulent poisons of the same nature is to be expected. Toxic as is tuberculin to the tuberculous organism, it is almost innocuous to the tubercle-free body. It has been found, in keeping with this distinction, that the normal animal shows after tuberculin treatment evidence of the minimal production of the neutralizing or antibody for the tuberculin, which, were tuberculin a direct poison for the tissues, would probably be produced in larger amounts. On account of this absence of action on the normal organism it has been thought that the active principle in tuberculin does not exist in a free state, but occurs in some combination, from which the tuberculous, but not the non-tuberculous, organism can free it, and that the separation takes place in the tubercular foci upon which the specific action of the poison is directly exerted. If this view is correct then the failure of tuberculin to exercise any profound action on the healthy organism is easily grasped.

Increased knowledge of bacterial infection and immunity has taught us that in case of bacteria which invade the depth of the body and produce their peculiar effects by reason of their immediate presence, we can not expect to achieve marked immunity through the use of the soluble gross-products of the parasites. The reaction of the body to the invasion depends not upon the presence in the invader of one set of toxic principles, but of many, some of which are contained in the solid substance of the micro-parasite and do not go over into the fluids in which they multiply. Thus it has been found, in case of certain bacteria, that a degree of immunity or protection which it is impossible to obtain even after very prolonged treatment with the fluid portions of cultures, can be secured quickly when small quantities of the living or even dead microorganism are injected into the body. A high degree of bacterial immunity has been secured up to now for a small number of micro-organisms by vaccination—by the method introduced by Pasteur—for several animal diseases, notably anthrax or splenic fever, fowl-cholera and black-leg. In these cases the living attenuated micro-organisms are employed.

Neither lasting nor marked immunity in tuberculosis can be obtained by the inoculation of cultures of tubercle bacilli killed by heat, sunlight or other agency. Dead tubercle bacilli are poisonous and bring out a striking reaction of the organism, but this reaction does not confer immunity to subsequent inoculations of the living germ. It may well be that the dead bacilli, especially if reduced to impalpable powder so as to facilitate absorption, may after injection raise the powers of resistance in the organic forces, although the height of the sustained forces is not sufficient to enable the body to throw off completely the living infecting organism. It is easy to prove that the animal organism is modified by the development within it of the tubercle bacilli; and merely disposing of dead bacilli increases its power of reaction against a second injection of dead tubercle bacilli; the second action being much more vigorous than the first. (Theobald Smith.) The experiments of Koch which immediately preceded the discovery of tuberculin clearly demonstrated that tuberculous guinea-pigs into which tubercle bacilli are reintroduced subcutaneously react in a very especial manner. An active inflammatory process develops about the site of second inoculation which eventually brings about the expulsion of bacilli with the exudations; a voluminous slough forms, which, when shed, carries with it a large number of bacilli; and this shedding is followed neither by the formation of a permanent ulcer nor hypertrophy of the neighboring glands, a regular result of the primary inoculation. The tubercular organism reacts in the same manner to dead as to living bacilli; the tuberculous animal has acquired immunity against reinfection or reintoxication by the tuberculous virus, which, however, in no way prevents the first inoculation from becoming generalized and setting up a tuberculosis of almost all the organs.

If we attempt an interpretation of these phenomena we can conclude that the organism, once it is poisoned with tubercle virus, becomes supersensitive to the tubercle poison. This supersensitiveness is displayed in the manner of reaction upon re-inoculation of the tuberculous organism to tuberculin and to dead and living tubercle bacilli. But the organism poisoned with dead tubercle bacilli is not in reality tuberculous; it is, however, sensitized. In keeping with this distinction, it can be said that while the tuberculous organism has acquired a degree of immunity to reinfection, the organism merely poisoned with tubercle bacilli has failed to develop this state of resistance.

The experimental results, which I shall relate to you, upon which are based our belief in the artificial production of immunity to tuberculosis, were all obtained by the use of living bacilli. It would, therefore, seem as if in the course of their residence and development within the body the immunizing organisms behave differently from those in artificial cultivations. This difference in behavior could be accounted for on the supposition that under conditions of parasitic life, surrounded as the bacilli are with complex fluids and more complex cells, they form, in their growth, products which either are distinct from those which are formed by them in cultures, or these products, in *statu nascendi,* are acted upon and modified by the active and labile ferments in the fluid and protoplasm of cells, with which the growth-products must come into immediate contact. Professor Welch, to whom this variation in behavior of bacteria under parasitic and saprophytic states of existence was fully apparent, endeavored a few years ago in his Huxley lecture to explain the difference in activity of bacteria growing within and outside the body by supposing that in the body they are induced to secrete substances the stimulus to the production of which is absent in the culture tube. However this may be, it is evident that the only form of immunity in tuberculosis which deserves the name has been obtained by the employment for inoculation of living cultures of the tubercle bacillus.

Although the earliest experiments which had for their object the production of immunity in small animals by means of previous inoculation of products of the growth and of attenuated cultures of the tubercle bacillus were published in 1890 (Martin and Grancher, Courmont and Dor), yet, I think, the first really promising, because successful, achievements of this end were made by Trudeau in 1902 and 1903 and by de Schweinitz in 1904.

Trudeau protected rabbits from virulent tubercle bacilli by first injecting them with a culture of bird tubercle bacilli, the subsequent injection of virulent mammalian bacilli being made into the anterior chamber of the eye. The rabbits to be protected were twice injected subcutaneously at intervals of 21 days with cultures of the avian bacilli. About one in four of the rabbits died within three months, profoundly emaciated, but without tubercular lesions. The remaining animals recovered and were apparently in good health, when, with an equal number of controls, they were inoculated in the eye with a culture of mammalian tubercle bacilli. The results are instructive: In the controls little or no irritation following the operation is observed and the eye remains quiescent or nearly so for about two weeks, when the changes described in the early parts of this address manifest themselves. After a few weeks general inflammation of the structures of the eye develops, the inoculation wound becomes cheesy and the eye is more or less completely destroyed. The disease, however, remains usually localized in the eye for many months, and may remain there permanently, depending upon the virulence and number of bacilli injected.

In the vaccinated animals, on the contrary, the introduction of the mammalian bacilli at once gives rise to a marked degree of irritation. From the second to the fifth day the vessels of the conjunctiva become engorged, and evidences of marked inflammation appear in the anterior chamber and on the iris (reaction of immunity). However, at the end of the second to the third week, when the eyes of the controls begin to show progressive and steadily increasing evidence of inflammatory reaction, the irritation in those of the vaccinated animals begins slowly to subside and the eyes to mend. In from six to twelve weeks, in the successful cases, all irritation has disappeared and the eyes present only the evidences of traumatism and inflammation. This experiment leaves no doubt of the protective influence exerted by the first inoculations of the avian bacilli and clearly establishes that related cultures of tubercle bacilli of moderate virulence for an animal species, can afford protection to subsequent inoculation with special and more pathogenic strains of the bacillus. Notwithstanding the fact that, as Dr. Trudeau records, some of the protected animals slowly relapse and the disease resumes its progress, although by almost imperceptible stages, the experiment still shows that protection, not absolute immunity, from tuberculosis may be obtained in rabbits by a species of vaccination.

De Schweinitz in 1894 reported certain experiments which he made on guinea-pigs and cattle. He inoculated the former with a culture of tubercle bacilli of human origin cultivated for about twenty generations in broth. This culture was of a low grade of virulence for these animals, but it served to protect them to such an extent that when they were afterwards inoculated with tuberculous material from a cow they remained healthy, while control pigs injected with the same material became tuberculous and succumbed in about seven weeks. De Schweinitz injected large quantities of human tubercle bacilli into cattle—beneath the skin, into the peritoneal cavity and into the circulation—without injury.

I may, at this time, digress for a moment and leave the more strictly chronological method of presentation, to allude to the set of experiments on the protection of guinea-pigs from tuberculosis, which Trudeau reported to the National Tuberculosis Association at its last meeting. The special merit of this experiment is that it shows the existence of a connection between virulence and infectivity in the germ and its capacity to confer immunity. Unless the bacillus has the power to gain some foothold in the body it affords no protection; if on account of high pathogenic power or virulence, it easily gains a foothold, then it brings about infection. To choose a culture of tubercle bacilli of just the right grade of virulence is one of the conditions, apparently, of successful experiment, as it must also be, in view of this fact, one of the difficulties of the method. The same difficulty has been encountered in the practical carrying out of this method of immunization in cattle. Several series of guinea-pigs were inoculated with tubercle bacilli as follows: (*a*) with dead bacilli, (*b*) with living bacilli from cold-blooded animals, (*c*) with a culture of human bacilli cultivated artificially for more than twenty years which produces on inoculation no appreciable local lesions and never tends to generalize, and (*d*) another human culture cultivated artificially for more than fourteen years, which still causes in all the pigs slightly enlarged inguinal glands near the site of inoculation, and occasionally brings about slight caseation of the nodes with a tendency to partial generalization of the virus. The dead bacilli and the bacilli from cold-blooded species gave no protection; the second human culture, by reason of its greater invasive properties, protects better than the first, which is almost devoid of power to grow in the animal body. In no case, however, was the growth of the virulent bacilli wholly suppressed.

In man the question of acquired immunity has been answered by many authorities, as far as the main considerations go, in the negative. A large number of well-observed facts demonstrates that a person who has suffered from localized tuberculosis of the lymph glands—scrofula so-called—or other form of local tuberculosis, can not count upon an immunity from pulmonary tuberculosis. And yet it can, I think, be shown by reference to statistics that in man there exists a refractory condition which becomes increased after infection, since the number of persons who have been the victims, at some period of their life, of a tuberculous infection, is very large in comparison with the number who die of this disease, or the even larger number who develop severe forms of it. Hirsch gives the mortality of tuberculosis as compared with deaths from all other causes as 3:22, in other words, tuberculosis claims as victims of death 1 in every 7 persons. This proportion does not, however, express the morbidity from tuberculosis, which is, in reality, far greater than these figures indicate. It is difficult to secure by vital statistics reliable data of the incidence of tuberculosis; but trustworthy observations made at autopsies upon human beings indicate that as many as 90 per cent. of persons, dying from all causes, have at some period of their life been the victims of a tubercular infection. In far the greater number of instances the disease remains fixed in the bronchial or other lymphatic glands or the apex of the lungs and exerts no injurious effect upon the organism as a whole. We may, therefore, fairly conclude that the human organism possesses a strong inherent tendency to overcome infection with the tubercle bacillus. So much can be safely predicated. But whether the suppression of a local infection, such as I have described, gives an increased capacity for overcoming subsequently invading tubercle bacilli remains for the present an open question. It is certainly not disproved by the facts cited; and some authorities hold fast by the belief that a degree of immunity to tuberculosis may be acquired by man.

In the year 1901, on December 12, on the occasion of his acceptance of one of the Nobel prizes, Behring announced that he was engaged upon the study of artificial immunization of cattle to tuberculosis. In this address the claim was made that a method had been perfected whereby it was possible to vaccinate cattle successfully against tuberculosis. These experiments consisted in the endeavor to immunize cattle by means of tuberculin, other toxins, so-called, from the tubercle bacillus, dead tubercle bacilli, bacilli weakened with chemicals and living, active cultures of the tubercle bacillus. In the four years which have elapsed since this announcement was made a series of monographic papers bearing on this subject has appeared from Behring's laboratory in Marburg. The plan of immunization has, in this time, undergone a number of modifications until now it consists in the inoculation intravenously of young cattle—calves twelve weeks old preferably—with a standard human culture, which is now furnished commercially. A second inoculation of an increased quantity of this culture is injected three months later. Cattle treated in this way are regarded as highly immune and are denominated by Behring as 'Jennerized.' If to them a dose of a virulent bovine culture of tubercle bacilli is given, no permanently bad results follow, although an equal dose of the virulent culture will cause, in an unvaccinated animal, the development of generalized tuberculosis leading, in a few weeks, to death.

In his endeavor to find a culture of the tubercle bacillus which would fulfill the requirement of producing a transient illness and leave protection behind, Behring discovered that not all tubercle bacilli of human origin were without danger to cattle inoculated with them. We were, indeed, not unprepared for this announcement, since, in the first place, we had learned that in some instances tubercle bacilli of the bovine type have been cultivated from examples of human tuberculosis, and, on the other, that not all the bacilli, of any type, exhibit equal degrees of virulence. The culture employed by Behring, although it has now been employed to inoculate several thousand cattle, is said never to have produced severe disturbances of health; even when animals already tuberculous are inoculated the results are not serious: fever lasting several days sets in, the animals may cough, and they may eat less and lose somewhat in weight, but even they return to what is for them the normal.

It would appear that McFadyean is entitled to the credit of the discovery equally with Behring of the immunization of cattle from tuberculosis; and, indeed, there is reason to believe that his results even anticipated those of Behring. By using for injection first tuberculin and then in succession tuberculin and tuberculous material containing bovine and possibly human tubercle bacilli, McFadyean succeeded in increasing the resistance of several cattle to artificial tubercular infection.

Pearson and Gilliland, 1902, in this country early published accounts of some experiments which they carried out upon the immunization of cattle from tuberculosis. They employed a culture of human tubercle bacilli for producing immunity and found that subsequently the protected animals, as compared with the controls, which all succumbed to the virulent inoculation, either developed no lesions or very inconsiderable ones upon being given large quantities of highly pathogenic bovine cultures. As far as I know these experimenters are the only investigators who have endeavored to carry the principles of the method a step farther, so as to bring about arrest of the disease in cattle already tuberculous. While it is unlikely that such a therapeutic use of 'Vaccination' will ever be made in veterinary practise, the facts are of considerable theoretical interest, especially in view of the somewhat similar means employed to arrest tuberculosis in man.

The immense importance to scientific agriculture of the matter of immunization of cattle from tuberculosis and the even greater collateral interest which the subject has for man, as enlarging the possibilities of immunity even for him, have led to a discussion on the priority of the discovery between Neufeld, a pupil of Koch, and Behring. It would appear from Neufeld's writings that, while working under Koch's direction, he ascertained as early as 1900–1901 that large animals—donkeys chiefly, but cattle also—could be protected from artificial infection with virulent tubercle bacilli, always fatal to control animals, by previous treatment with tubercle vaccine, of which several different preparations were studied. It is not within the scope of this address to apportion the credit of priority; but in any case, assuming the facts to be as stated by the contestants, McFadyean should receive as great credit as either of the others, if not the chief credit. The principle which all the investigators employed is not new in experimental medicine, but has come to us from the genius of Pasteur. It may, however, be said that our knowledge of the tubercle bacillus and its varying activities had by the year 1900 become so much enlarged that the possibility of putting the facts of the newly discovered properties to a practical test of immunity occurred to the several independent workers in bacteriology. There can, I think, be no doubt that Behring deserves the credit of making the protection of cattle from tuberculosis a feasible, practical object of study. This alone is a merit of no small order.

From the mere fact that cattle have been successfully protected from infection by the tubercle bacillus, even under the severest conditions of laboratory experiment, it can not be concluded that they will be equally refractory when exposed to the natural sources and modes of infection. In the laboratory the virulent infectious agent is brought into the animal by injection, under the skin, into the serous cavities or into the circulation, which are avenues through which in the natural disease infection rarely if ever takes place. And while this mode of introduction of the virulent bacilli into the body may, theoretically, be more severe than their introduction into the lungs with inhaled air, or into the stomach through infected stalls and food, yet the profound differences in the defenses of the body with which the bacilli come into conflict, under these different circumstances, may, after all, determine the issue in a manner quite contrary to our expectations. It is, therefore, of the highest interest to learn that in their later tests Behring and his co-workers exposed vaccinated cattle to stalls and herds which were known to be badly infected, with the result that at the time of the report, they had apparently escaped infection. I am enabled through the courtesy of a private communication from Dr. Pearson to state that cattle vaccinated by himself and Gilliland which were kept for two years under natural conditions of infection have not contracted tuberculosis, while the control animals, exposed to the same conditions, have all developed the disease, some dying spontaneously by reason of the severity of the infection. Dr. Pearson also informs me that their experiments indicate that the degree of resistance bears a rather definite relation to the number of vaccinations given the cattle. No cattle vaccinated three times with their standard vaccine—a living culture of tubercle bacilli of human origin—have developed tuberculous lesions even after two years' severe exposure. In their experience, two injections of Behring's vaccine do not always suffice for such heavy exposure as they employed.

As regards the question of duration of the protection, it may be said that Behring, basing his views on results of vaccination made three years before, expressed the belief in 1904 that it would endure during the life of the animal. As young healthy cattle are vaccinated before they fall victims to infected stalls and herds, it would seem as if infected herds might therefore gradually be replaced by healthy ones. The gain, this being true, would be almost incalculable to agriculture.

I am in the fortunate position of being able to bring before you a critical summary of the subjects just presented by one wholly conversant with its practical as well as its theoretical aspects. Through the courtesy of Dr. Leonard Pearson I have been enabled to read the advance sheets of a review on immunization in tuberculosis which will soon be issued from the Phipp's Institute. Dr. Pearson concludes that there appears to be no doubt that different cultures of human bacilli have different immunizing values. Some can not be used at all because they are of too high and others, possibly, because they are of too low, virulence for cattle. There is also need for comparison in immunizing value of fresh cultures and cultures that have been dried in vacuum and reduced to powder. Some observations appear clearly and strongly to indicate that the fresh cultures are preferable. Although it has been shown that vaccination can be practised so as to be entirely harmless to the animals, yet, on the other hand, it is not always unattended with danger. What is the shortest and most economical procedure for the protection of cattle on a large scale is still to be established. Only prolonged observation of carefully recorded results of vaccinations practised on a large scale can settle this point. The question of duration of immunity is still an open one. It has been shown that the immunity endures a year. To say, at the present stage of the studies, that it will last during the entire life of an animal is to make a statement for which there is no experimental proof. Modes of vaccination, as illustrated by the intervals between the successive injections, differ greatly. Behring recommends an interval of three months, while others have obtained a high degree of immunity by repeated injection at short intervals. As artificial immunity is relative and not absolute it need not excite surprise that the immunity to the tubercle bacilli can be overcome by the injection of large quantities of active bacilli. What is desired in practise is a degree of immunity that will suffice to protect animals from acquiring the disease under natural, and consequently highly variable, conditions. In some herds, where the natural disease prevails in a mild form, a lower degree of immunity may suffice than in other herds in which the disease is more severe and wide-spread. We are, therefore, at the beginning of this complex and highly important subject. These are Dr. Pearson's conclusions.

There is another aspect of this subject which demands attention. When it is recalled that immunity in cattle is obtained by the injection of living human tubercle bacilli the question arises whether this procedure is wholly free from danger to the consumers later of the flesh and milk of these cattle. It would appear that the human bacilli do not excite in cattle the tubercular lesions, in which doubtless the bacilli are so enclosed as to be, to a considerable degree, protected from perishing. It is equally true that as the living micro-organism can not be replaced by dead ones in bringing about immunity, the immunizing process is in some way bound up with their survival and even, possibly, with a restricted multiplication. Hence it is necessary that we ascertain, first, how long the human bacilli survive in the organs of the vaccinated animals, and second, whether they are ever eliminated with the milk of cows. The observations already made upon these points are so few as at present not to be useful for any scientific deductions. But before the method is too implicitly relied upon these questions should be answered.

It is an interesting subject of speculation as to what the result will be when cattle in general, and possibly, man later, shall have been immunized to tuberculosis. Will the race of tubercle bacilli disappear in large measure from the world? This would indeed be a beneficent result. But Dr. Smith has pointed out in a recently delivered address that doubtless host and parasite eventually come to hold a kind of equilibrium to each other, and hence an increased degree of resistance in the former might tend to bring about that selection among the parasites through which races of greatly augmented power for invasion would be produced. If this were true, and he even suggests that the natural process of weeding out the weaker among the human race tends to this result, the parasite would try to keep up with the host as his resistance increased until a point was reached beyond which further enhancement of power was impossible. Would the higher animal or the lower vegetable organism finally claim the victory? We need perhaps at this moment not to relax our efforts to achieve a practical immunity for man as well as for animals because of this future danger. I am not aware that the smallpox germ has increased measurably in virulence since vaccination became general, but I would also add that a century is a small period of time in the life history of any living organism.

Before closing this address I should like to refer briefly to the new interest which has been excited in the use of tuberculin in the treatment of human tuberculosis by reason of the application to the study of tuberculosis of a method introduced by E. A. Wright, of London, whereby it is held that the exact effect of the tuberculin injection can be measured and controlled. The method consists in the determination of the capacity of the blood leucocytes to take up tubercle bacilli when the blood and the bacilli are brought together outside the body in a test tube. Wright and his pupils have worked out the normal power of the blood to cause the englobing of the bacilli; and they have noted a diminution of this capacity in the blood of many persons suffering from tuberculosis. They speak of this englobing capacity of the blood as 'opsonic index,' from the word meaning to prepare—to cater for; since the bacilli must first be prepared by substances in the blood serum before they can be ingested by leucocytes. The injection of tuberculin, when cautiously done, tends to bring about a rise in the tuberculous, of the 'opsonic index,' which Wright believes is a measure of the good done, as an increase in immunizing substances in the blood is the cause of the rise. He also discovered that time is required for the occurrence of the rise and that the immediate result of the injection is a fall of the index—so-called negative phase. This latter must be permitted to pass away and be succeeded by the positive phase before another injection is given. Gradually the 'opsonic index' is driven up in the cases that are favorable to the treatment.

I do not intend to discuss the value to the clinician of this interesting method and Wright's observations based upon it. The subject appears to me to be one of great intricacy and therefore to be approached in a spirit of proper criticism despite its evident allurements. My purpose in mentioning it at all is to bring again to your attention a method of exciting the tuberculous body to put forth an effort at self immunization which is sometimes efficient to a marked degree. It is not the injected tuberculin that accomplishes directly the changes in the condition of the patient, for there already exists, doubtless, an excess of similar poisons in the tuberculous foci in the body. The healthy body, indeed, does not react in this manner and is not to be protected, enduringly, from tuberculous infection by a previous treatment with tuberculin. As Koch's phenomenon shows the tuberculous organism to have developed defenses against subsequent tuberculous infection which the normal body does not possess in equal degree, the employment of tuberculin indicates that the diseased body can be aroused artificially to put forth a stronger effort than its unaided natural forces enable it to make, in order that the disease may be overcome. Herein resides a great principle, an immense power for good, and, consequently, a great hope for future progress in the rational and specific treatment of tuberculosis in man. Efficient efforts at suppression of the causes of tuberculosis, deeper knowledge of the principles of bacterial immunity, are the two forces which in time may stay the ravages of the 'White Death.'

THE PROTECTION OF THE ALLUVIAL BASIN OF THE MISSISSIPPI[1]

By ROBERT MARSHALL BROWN
WORCESTER STATE NORMAL SCHOOL

THE hydrographic or drainage basin of the Mississippi River (Fig. 1) is equivalent in area to one third of the United States. Thirty-two states and territories contribute water to the volume of the river; eight of these divisions send water to no other system. The discharge of rivers is not in any proportionate way related to the size of their drainage basins. The potent factors which determine the volume of discharge are the precipitation of rain over the basin and the character of the soil. The upper Ganges has a basin less than one seventh that of the Mississippi. It equals the latter river, however, in the volume of its discharge. The Hoang Ho, with a basin area fifty per cent. only of that of the Mississippi, discharges more than twice as much water into the sea. If the discharge of the Mississippi proportionally to the size of its basin equaled that of the Po, the volume of the discharge of the former would be multiplied by six. The Danube more nearly equals the Mississippi in the ratio of discharge to size of

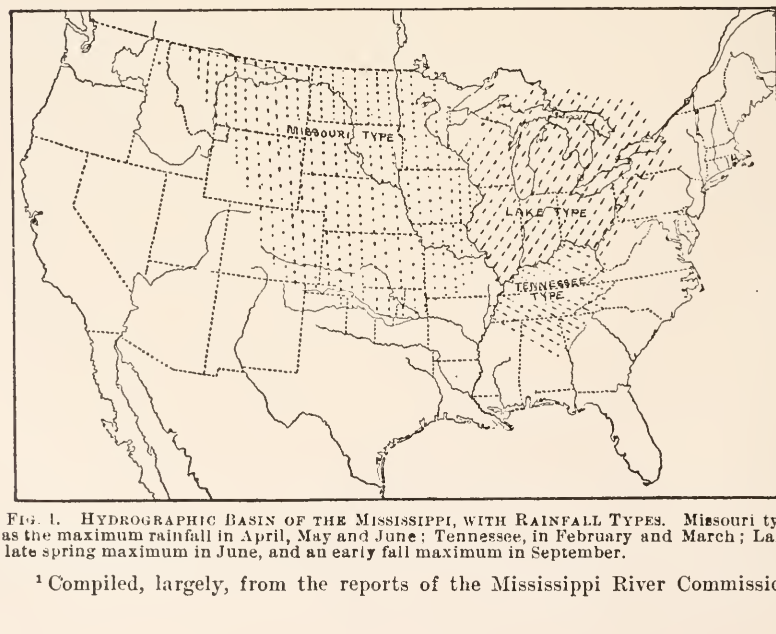


FIG. 1. HYDROGRAPHIC BASIN OF THE MISSISSIPPI, WITH RAINFALL TYPES. Missouri type has the maximum rainfall in April, May and June; Tennessee, in February and March; Lake, a late spring maximum in June, and an early fall maximum in September.

[1] Compiled, largely, from the reports of the Mississippi River Commission.

basin. It, too, has a portion of its drainage area protected from the prevailing winds and approximates to the rainfall condition of the Mississippi. The Ganges and the Hoang Ho are in regions of copious seasonal rainfalls.

The portion of the Mississippi Basin subject to inundation includes land on both sides of the river south of Cape Girardeau, Mo., and aggregates 29,700 square miles, or an area equivalent to the state of South Carolina. Through this alluvial basin (Fig. 2) the river winds in a rather tortuous path, the distance by river (1,700 miles) being nearly three times as long as a straight line drawn from Cape Girardeau to the Gulf of Mexico (600 miles).

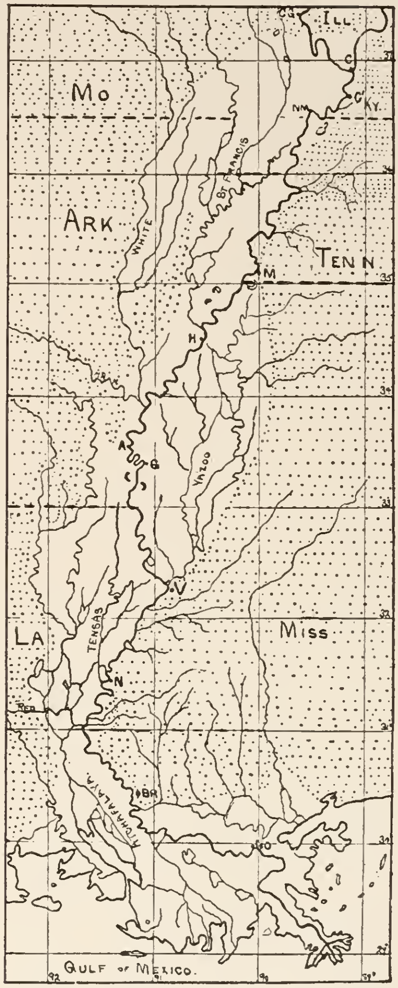


Fig. 2 The Lower Mississippi, with Tributaries, Alluvial Basin, and Higher Lands (dotted). *CG*, Cape Girardeau; *C*, Cairo; *C'*, Columbus; *M*, Memphis; *H*, Helena; *A*, Arkansas City; *G*, Greenville; *V*, Vicksburg; *N*, Natchez; *BR*, Baton Rouge; *NO*, New Orleans.

It is characteristic of many flood plains that the land immediately adjacent to the river is higher than the more distant parts of the plain (Fig. 3). These higher parts are called natural levees by some writers. If, therefore, the river rises beyond the limit of its banks, there is likely to be a general inundation of the alluvial basin. This feature of flood-plain form is further illustrated by the tributaries of the lower river. From an ordinary valley slope a river flows fairly direct into the trunk stream. If a tributary crosses a flood plain of some size, the stream must force an entrance against the rise of the back slope of this plain and finally must breach the banks of the river.

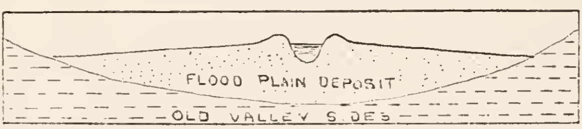


Fig. 3. Cross Section of a River Valley.

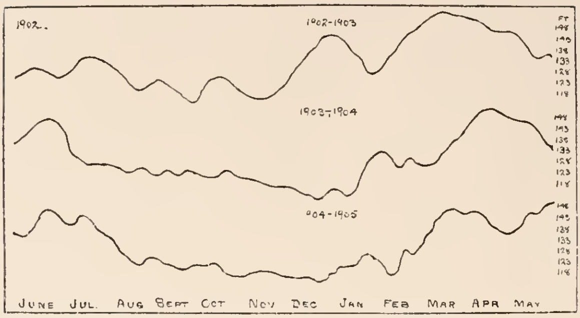


FIG. 4. HYDROGRAPH OF THE MISSISSIPPI RIVER FROM JUNE, 1902, TO MAY, 1905.

It is apparently easier for the tributaries of the lower Mississippi not to overcome this rise, but to flow down stream in the back swamp lands or bottoms until some more formidable barrier forces them to empty their waters into the main channel. In this manner the St. Francis (Fig. 2) flows for 100 miles and is forced into the Mississippi just above Helena, where the main river, after crossing the alluvial basin, touches the higher land on the west. The Yazoo River is forced in at Vicksburg, after flowing along the flood plain for 200 miles. Other tributaries show this same characteristic. The Atchafalaya pursues its course to the Gulf as an independent stream. The height of the river bank over the back swamp districts varies from 10 to 25 feet.

The large area of the drainage basin of the Mississippi would yield an unmanageable amount of water to the lower river during the stage of flood if the excess of discharge at that time resulted from a uniform rainfall condition. The very size of the basin with the tributaries of the main stream reaching far into rainfall areas of different types and seasons is beneficial to the control of floods. Reference to the map (Fig. 1) and the appended explanation may aid one in understanding the condition of rainfall over the basin. In addition, it is well to bear in mind that the condition of the ground affecting the amount of run-off of water is an important factor in the amount of discharge. The Ohio basin has its heaviest rains in January, February and March. Its largest tributaries, the Cumberland and the Tennessee, rise in regions of copious winter rainfall and add enormous volumes of water to the Ohio. The basin of the Ohio is less than one half that of the Missouri, yet it furnishes over twice as much water to the Mississippi. The melting of the snow and the frozen condition of the ground which increases the percentage of run-off at the time of the early spring rains swell the volume of the Ohio, while the late spring rains over the Missouri basin fall on an absorptive soil. Only 15 per cent. of the rainfall is drained from the latter basin and 24 per cent. from the former. The percentage over the Ohio during the flood months because of the conditions stated above is probably much higher.

The sequence of floods from the tributaries is first, the Ohio, then the upper Mississippi, followed by the Missouri and the western streams. Great floods are not annual occurrences. Recently the years marked by excessive floods have been the years of 1893, 1897 and 1903. The floods of the years 1898 and 1904 did not fall much short of the records of the seasons previous.

The hydrographs of floods (Fig. 4) and the profiles of rivers at different seasons (Fig. 5) show that the floods proceed down stream somewhat as a wave. The highest point or crest marks the extreme danger limit due to height of flood. The hydrograph also indicates that the crest flattens somewhat in its down-river progress. The reason for this may be seen in the river profiles which are drawn from three stages of the river's annual fluctuations. The down-stream slope (*AC*) of the flood wave has increased over the normal slope of the river (*GE*). This results in an increase of speed of the waters on this slope. On the other hand, the up-stream slope (*BA*) is a less one than the normal slope (*DG*), and a decrease of speed over normal flow results. At certain times the slope (*BA*) may be against gravity, and a further retardation will be experienced. This would occur when a tributary added a large volume of water to the Mississippi, as in the case of the Ohio during the spring freshets. The tendency of this increase of speed on the down-river side of a flood wave and the decrease of velocity on the up-river side is to reduce the size of the flood wave by draining off the excess of water faster than it can accumulate. If the river is long enough to allow this proc-

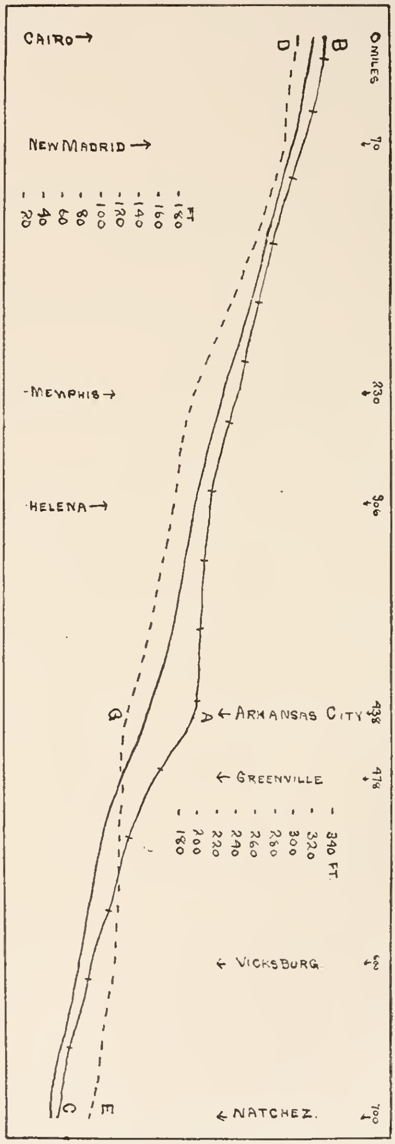


FIG. 5. PROFILES OF THE MISSISSIPPI RIVER AT FLOOD STAGE (*BC*), at low water stage (*DE*), and at an intermediate stage.

ess to so act, a flood would finally reduce itself to an insignificant rise of water.

The following table may yield additional information concerning flood conditions:

Station.	Miles below Cairo.	High Water 1903.	Low Water 1895.	Difference.	Standard High Water.	Highest 1903.	Above Danger Line 1903.	
							At or Above.	5 Feet Above.
Cairo	0	328 ft.	278 ft.	50 ft.	50.17 ft.	50.60 ft.	24 days	8 days
New Madrid	70	302	263	39	42.90	39.50	53	8
Memphis	230	210	169	41	41.60	40.60	54	13
Helena	306	199	145	54	54.10	51.00	67	22
Arkansas City	438	156	99	57	56.30	53.00	82	42
Greenville	478	144	92	52	50.50	49.10	42	12
Vicksburg	599	104	46	58	55.00	51.80	45	21
Natchez	700	74	22	52	54.00	50.40		
Red River Landing	765	60	10	50	52.50	50.00		
Baton Rouge	833	47	7	40	43.20	40.00		
Carrollton	957	27	7	20	20.35	19.40		

Levees were constructed by the early settlers of Louisiana about the year 1717 near New Orleans. In 1844 the right bank of the river in this state was embanked and many other isolated levees were in existence, especially along the Yazoo basin front. The average height of the Louisiana levees was four feet. Through the years up to 1882, there seems to have been a constant agitation of the question of levee building. Riparian states, districts and owners formed committees and boards, taxed the protected lands or the products of these lands and in general managed to put their constituents under a heavy burden of debt. In 1858 a tax of 10 cents per acre was demanded on all lands which were freed from inundation, and such lands as bordered the river were often subjected to a tax of 25 cents per acre. In 1865 a board was constituted with a revenue derived from a tax on cotton of 1 cent per pound. In the year 1882, a flood greater than any previously experienced overflowed the entire basin and destroyed most of the levees then existing. The Mississippi River Commission, created a few years before, now entered upon its work at an opportune time. With the landowners disheartened, their labors resulting in little gain, their money invested in levees swept away, the allotments of the commission were enough to revive the courage of the riparian proprietors. There has been a steady gain in the protection of the alluvial basin since the creation of this commission. At present the levee system comprises about 1,500 miles of structure and is 71 per cent. completed.

The height of the levees is a varying one. If the levee is built on the immediate banks of the stream (Fig. 3), which is the highest part

[2] High water, 1903, and low water, 1895, are reckoned from the Memphis datum. The numbers are, roughly, seven feet too high for Gulf levels as the base.

of the flood plain, a levee less high might suffice as well as one built on the back-slope. Where the original four-foot levee stood in Louisiana is a levee about 16 feet high. The top is not much higher than the top of the original levee, but, being situated at some distance from the banks, its base is 10 feet lower. The immediate banks of the river are so subject to caving that they do not make, in all cases, a safe foundation for a levee. It has been considered safer and wiser in many instances to build a more stable foundation. Furthermore, if the levees are built upon the higher and immediate parts of the flood plain, the levees would be nearer each other. The nearer they approach one another, the higher they must be. Any detraction from the horizontal expansion of the waters must be evidenced in a vertical expansion. With the levees placed further apart, a less height is possible. The thing that determines the height to which they shall be built is the flood. There is an endeavor to place the grade of the levees at from 2 to 4 feet above the gauge measurements of the highest floods. Thus for a while the provisional grade was 2 to 3 feet above the 1897 flood. Later the 1903 flood set a new mark and a grade 2 to 2.5 feet above the 1903 high-water line is suggested. At Lake Providence, the 1903 water mark was 2 feet above the 1897; at Greenville, it was 2.4 feet above. Accordingly, the high water of 1903 reached approximately to the tops of the levees suggested after the 1897 flood, and a new height, 2 to 3 feet above the former, is now demanded. This latter height is about 5 feet above the provisional grade of five years ago. As the levees approach completion, higher and higher grades must inevitably result. As long as there is relief of the waters by incompleted levees and crevasses, the necessary height can not be determined. When the remaining 29 per cent. of levee is constructed and the system withstands one of the greater floods without a crevasse, the excessive flood will determine the height of the levees. Even then certain factors of flood conditions may so unite as to cause a flood which will overtop the system. The statement of the character and condition of the system under the strain of the 1903 flood indicates that there is much left to be done. In the upper part of the basin, the levees were reported too low and of insufficient dimensions. The high water reached to the top for one half of the entire length of the lower St. Francis district and for many miles was above the tops, being restrained from spreading over the basin by capping the levees with planks, dirt and sand-bags. In the district below, it is reported that because of the settlement of the embankment and because about 20 miles of the line in the lower end of the district had not been raised to the provisional grade, considerable work was required to prevent the water from overflowing the levees. Again in Louisiana, the topping of the levees by planks and sand-filled bags was necessary over a distance of 71 miles of the line in order to prevent a wash-over. An engineer reports that the most vulnerable feature is the instability of the foundation along much of the levee line.

The efficiency of the levee system is the test of all the labor and the justification of the large expenditures. The increasing efficiency may be measured in many ways. A comparison of the number of crevasses and the total number of miles of destroyed levees with the records of previous floods is the one in general use. In 1882, 284 crevasses were recorded and 59 miles of levee were destroyed. This record has been gradually improved. In 1890, but 23 crevasses were reported and 4.25 miles of levee destroyed. In 1897, the number of crevasses increased to 49, with a loss of 8.3 miles of levee. In the 1903 flood, 9 crevasses of importance were recorded and 5 of these caused a loss of 2.1 miles of the levees. The loss of levees by caving banks was a little less than 1 per cent. of the entire contents. Last year (1904) in a period of quieter flood the percentage was over 2.5. The number of square miles of overflowed area in 1903 was .5 the mileage for 1897. There is no doubt but that the levee system as it approaches completion is being made stronger and safer. Yet each crevasse or natural break spreading the confined waters over larger areas releases the tension on the banks and to some extent prevents others from occurring. To enclose the water which has spread naturally over 29,700 square miles between two walls less than 5 miles apart and covering about one tenth of its former area is no easy task. Till the present system is completed, the possibility of a flood will be uncertain that always grave dangers may be incurred to life and property within the limits of the alluvial basin of the river.

The commissioners in their general report furnish the best proof of the increased confidence in the levees by citing the progress and growth of the Yazoo Basin since their board was created. The population of this district was 94,672 in 1880, and 195,346 in 1900. The present valuation of the basin is $42,000,000. The number of banks have increased from 2 in 1893 to 51; the mileage of railroads from 225 in 1884 to 816. The cotton production in 1879 was 185,868 bales; in 1903, 426,414. The increase in corn, peas, clover and alfalfa is reported to be even greater than that of cotton. The original timber of the basin is being cleared, and there are now large shipments of lumber, as logs, boards, staves, headings and the like. Flourishing crops are seen to-day where in former years the floods measured from 20 to 25 feet in depth. 'The Yazoo Basin is sprinkled with towns whose sites were the home of the bear and the wild cat ten years ago.'

The engineers of this district in their reports of the flood of 1903 state that about one fourth of the Yazoo Basin was under water during this flood. Two crevasses occurred, letting water into the basin. One of these, three miles below Greenville, Mississippi, was at its greatest width a breach of 3,900 feet. The water flowed back upon Greenville and more than one half of that city was under water. Forces were set to work building a protection levee in the city and moving goods to a place of safety. As far as can be ascertained no lives were lost. Little or no loss in crops was sustained, as the flood came before the planting and the area was largely drained before that season arrived. The losses were mainly in live stock, fences and buildings. In contrast with the security so easily shown, in the reports of the commission, to be the experience of the inhabitants of the Yazoo Basin, the engineers in charge of the levees seem to congratulate themselves that no other crevasses occurred, for many weak places developed that required the utmost care and attention to hold intact. They state as their expectation that crevasses are liable to occur at any high-water season and at any point in the system or 'until all levees are brought up to a sufficient section to withstand the long-continued strain due to the water remaining for weeks near the top.' Although the statement quoted, on the face of it, rather begs the question, we are at liberty to infer therefrom that too much confidence had better not be held in the protective value of much of the present line. So near to disaster do the floods approach oftentimes, that every element which the engineers can control is considered a necessary ally in cooperation for the protection of the levees. During the 1903 flood, the river boats were required to run at a reduced speed along a portion of this basin front. So full was the river and the waters stood so near the top of the levee that it was not considered wise to subject the embankment to the wash of passing steamers. It may be stated in this connection that a storm, arising as the water is nearing the crest of the levees, can not be so summarily dealt with; and it often causes a day or two of apprehension, if indeed the beating waves do not tear their way through the structure.

A harsher note is sounded by an observer of the Weather Bureau[3] than is struck in the reports of the commission. There were favorable and mitigating circumstances which decreased the volume of the 1903 flood. Two factors materially modified the destructive feature of the flood; one of these was an early occurrence and the other a shorter duration. Both these factors spring from the same cause. Notwithstanding these compensating qualities this writer reports that the water during a period of two weeks was higher at Greenville and Arkansas City than it had ever before been known to be. The destructive work of the flood is summed up as follows: Some loss of stock in the basin; 115 houses evacuated in Greenville; 200 acres of fine farming land badly washed and left covered with sand; suspension of traffic on the Yazoo and Mississippi Valley Railroad from March 27 to April 17, and on the Riverside Division from March 27 to May 7; 1,460

[3] Bull. M, 'The Floods of the Spring of 1903 in the Mississippi Watershed,' H. C. Frankenfield.

square miles of land overflowed in this basin, one half of which was farm land; 60,000 people lived in the overflowed district and were, therefore, inconvenienced; this number of people represents about one third of the inhabitants of the basin. For some time previous to the coming of the flood, the dwellers in the basin were preparing for the flood season. Mounds were built for temporary refuge. Stationary platforms were constructed to the same end. Rafts were also made. The mules, horses and the feed were in many instances transferred to places of safety, often to the lofts of the barns. Farm implements and machinery were put beyond the reach of the water. That the warning of the Weather Bureau was so extensively heeded explains why there was no loss of life and little loss of stock.

These three reports of the same thing are not so contradictory as they sound. Each observer is looking for the things that sustain him and his point of view, and is not directly interested in the things that are foreign. One writer tries to establish the security of the basin of the Yazoo against danger to life and property, because that is what the board was created to do; a second writer tries to show how weak the levee is, in order to press home the need of funds—and he makes imminent danger to the basin area a means; the third shows that without the services of the branch he represents, the loss of property and life would be multiplied. The first man is right to some extent, and he is sustained by the second, who sees how near to each other danger and safety sometimes approach—and they are aided by the third. I doubt not but that the Weather Bureau may make as just a claim for the credit of the progress in the Yazoo Basin as the River Commission.

If the increasingly better reports influence a larger population and larger expenditures in holdings within the alluvial basin of the Mississippi, and the hopes of the engineers become realized to the extent of normal safety, then, perhaps, the levee system can be called efficient. Twenty years may be too short a time to consider the effect of the system upon population, and at the same time we must remember that but two thirds of the levee lines are completed, yet in this time the commissioners report an increase of population over the Yazoo Basin of over 100 per cent. It seems as if the people were becoming confident that there is 'security and permanence of protection' in the work that is in progress. Yet just so far as this confidence is expressed in settlement within the area liable to overflow, so much further must the levees protect beyond peradventure of disaster. In an increase of 100 per cent. in population and a decrease of 50 per cent. in mileage of overflow, if the terms are commensurate, there is no gain; if the terms are incommensurate, there is as good a chance for a loss as a gain. Just meeting the limit of strain, or preventing a break only by excessive vigilance and energy, or saving from disaster by some mitigating circumstance is not the end to be aimed at; but to be as reasonably sure as it is given man to be that an overflow can not occur must be the plan.

THE JEWS: A STUDY OF RACE AND ENVIRONMENT

BY MAURICE FISHBERG

IN the search for the causes of various social phenomena characteristic of the Jews, most writers have been content to give 'race influence' a prominent place. The effects of the physical and social environment on the individual, or group of individuals, have been neglected. Once that remarkable cloak for our ignorance, 'race,' had served the purpose of explaining easily the causation of a given social fact, it was an easy matter to rest content with this explanation. It was repeatedly alleged that the Jews, though scattered in all the regions of the habitable globe, subjected to all varieties of climatic, social and economic conditions, nevertheless present everywhere the same characteristics with a remarkable uniformity. Demographic and social phenomena, such as fertility, mortality, marriage rates, illegitimacy, intermarriage, divorce, criminality, etc., were all attributed to ethnic origins, to Semitic influences.

Anthropological research has, however, revealed that there is no such thing as Jewish race, that ethnically Jews differ according to the country and even the province of the country in which they happen to live, just as catholics or protestants in various countries differ from each other. It was shown that there are various types of Jews, tall and short, blond and brunette, brachycephalic and dolichocephalic, etc.; and that all these types appear to correspond to the types encountered among the non-Jewish population among which they live. 'Race' can, consequently, not be the only cause of the demographic and social peculiarities said to be characteristic of the Jews. Other causes are to be sought for.

In the following studies statistical data of recent censuses in various European countries have been utilized in an attempt to find primarily whether the Jews do actually present uniformly, as has been alleged, similar social and demographic phenomena in every country, irrespective of difference of the physical and social environment. While the ethnic factor has not been neglected, still, in cases in which race influence is not sufficient to explain satisfactorily a social or demographic fact, or is in direct contradiction with actual conditions, the effects of the physical environment and of social conditions have been looked into. The author assumes that if an ethnic cause exclusively underlies a given social fact observed among the Jews, then we should expect that the Jews in every country would present the same peculiarity.

I will begin with the question of the fertility of the Jews.

I. *Natality*

From the enormous mass of vital statistics collected during the past century, nothing definite could be established as to the influence of race on the birth rate. On the one hand, one would be led to believe that the Teutons have a high birth rate, when judged by the proportion of births in the German empire, but, on the other hand, in Scandinavia, where the Teutons have preserved themselves in a much greater purity, the rates are comparatively low, and the same is true of England. The Slavonic races in eastern Europe have a very high degree of fertility, but the differences in the various provinces of Russia, Poland and Austria are so great as to disprove directly the contention that race is necessarily the cause. In the same manner, the differences in the rates in Italy and southern France are striking. The racial elements are about the same in both countries, yet the birth rate of Italy is much higher than that of France. The Jews in Europe, owing to their isolation and alleged abstinence from intermarriage with other peoples, should offer good material for the solution of the question on the influence of race on fertilty.

Country.	Year.	Birth Rate per 1,000 Population.[1]	
		Jews.	Christians.
Algeria	1903	44.67	32.57
Galicia	1900	38.01	45.86
Warsaw (Poland)	1897	35.79	37.92
European Russia	1897	35.43	53.36
Austria	1901	33.89	38.01
Hungary	1900	33.81	39.34
Roumania	1902	32.36	42.86
Bukowina	1900	29.54	42.81
Amsterdam	1900	24.82	31.53
Lower Austria	1901	20.51	32.10
Prussia	1903	18.40	36.03
Bohemia	1900	17.85	34.88
Bavaria	1903	17.80	37.80
Berlin	1904	17.02	27.36
Prague	1901	15.85	31.31

It has been observed all over Europe that the birth rates of the Jews are low. When compared with the non-Jewish population of some countries, like Prussia, Bavaria, Bohemia, etc., they are only one half as fertile as the christians. Some authors have asserted that

[1] The term 'christian' includes: In Algeria, the Europeans living in that colony, the Mohammedan inhabitants being polygamous can not be compared with the monogamous Jews; in Warsaw Roman Catholics are referred to, and in European Russia, Greek Orthodox; in all the rest it includes the total non-Jewish population of the country.

this has a definite physiologic or ethnic basis as its cause, which is common to all Jews living in different countries. The social environment, such as economic prosperity, occupation, city life, etc., and also climatic conditions, were not considered. 'Race' was a satisfactory explanation. But the figures in the appended table giving birth rates of the Jews and christians in various European countries disprove the ethnic theory of the low birth rate of the Jews. If it was a physiological characteristic of the Jews, we should expect that the rates in every country would be about the same. As a matter of fact, however, the figures show wide limits of variation. In Algeria the rate is 44.67 per 1,000 population; in Galicia, 38.01; in Russia, 35.79, etc., while in Bavaria and Bohemia it is only 17.8, and in the city of Prague only 15.85. Ethnic conditions are never known to display such wide limits of variation.

It appears from these figures that wherever the rates are higher among the christian population, the Jews also show higher rates and the reverse. This is particularly striking when separate provinces of a country are considered. The following figures, taken from Ruppin's work on the Jews, illustrate this fact in Austria:

Province.	Year.	Birth Rate per 1,000 Population.	
		Jews.	Christians.
Bohemia	1900	17.85	34.88
Lower Austria	"	20.51	32.10
Bukowina	"	29.54	42.81
Galicia	"	38.01	45.09
Total Austria	"	33.89	38.01

It is thus seen that in Galicia and Bukowina, where the birth rates of the christians are high, the Jews also have a high rate, while in Bohemia and Lower Austria the rates for both Jews and christians are low. In the fifteen provinces of Russia which constitute the so-called 'Pale of Settlement,' the same phenomenon was revealed in the statistics collected during the census of 1897, the birth rate of the christian population being 51.71, and that of the Jews, 32.13 per 1,000 population. From the figures presented below, it appears that in the provinces where the higher birth rates are observed among the christians the Jews also are more fertile, and the reverse. Thus in Ekaterinoslav and Kieff the highest rates are recorded among both Jews and christians, while in Kovno, Bessarabia and Wilna the lowest rates are recorded both among the Jews and the christians. With two exceptions (Minsk and Poltava) the rule appears to hold good.

	Christians.	Jews.
Ekaterinoslav	61.82	42.28
Kieff	55.44	39.92
Minsk	54.90	29.76

	Christians.	Jews
Cherson	54.30	34.15
Volkynia	53.31	35.67
Taurida	52.51	34.48
Mohileff	52.30	28.16
Podolia	52.06	34.66
Grodno	50.96	29.84
Chernigoff	50.67	28.13
Poltava	49.00	36.51
Witebsk	46.85	31.00
Wilna	45.10	21.72
Bessarabia	44.46	27.91
Kovno	40.98	27.34

An attempt has been made by several statisticians to find some geographical differences in the birth rates of Europe. Sundbärg points out that some striking differences are to be noted in the rates when eastern Europe is compared with western Europe. He calculated an annual rate per 1,000 population for eastern Europe, 46.1; for western Europe, 33.6; southwest, only 32.3, and northwest, 34.7. On the whole, his calculations are well-founded, although there are some exceptions which are attributed to social conditions of a local nature. A glance at the table of the birth rates of the Jews in various European countries shows that while their fertility is everywhere lower than that of the christians, still in general they follow the rule laid down by Sundbärg. Taking Russia, Poland and Galicia as typical of eastern Europe, we find that the rates for the Jews are highest, reaching 38.01 in Galicia. Considering Bavaria as typical of the west, we find here the lowest rate, only 17. Amsterdam is intermediate between these two, only 24.82, corresponding roughly to the northwest of Europe. For the south there are no available data, except some collected in the middle of the last century (1861) showing that in Tuscany the birth rate was 27.2 among the Jews as against 39.0 among the christians.

It thus appears that the Jews follow quite closely the rates observed in Europe. The highest rates are observed in the east, the lowest in the west, etc. It is also known that in Denmark the birth rate of the Jews is very low, corresponding to the north, and in France conditions are similar to those observed among the French. In general it can be stated that with some local exceptions Sundbärg's rule holds as good for Jews as for non-Jews in Europe.

It would be misleading to explain the lower birth rates of the Jews when compared with christians as due to a physiological characteristic having as its cause a peculiar ethnic trait. The facts that the rates are not everywhere the same, but show wide variations, and that these variations correspond more or less closely to those observed among the non-Jewish population, are against any such theory. A close study of certain social conditions of the Jews offers a more reliable explanation.

It is noteworthy that the birth rates of the Jews are decreasing much more decidedly than those of the Christians in the countries in which they live. In Poland, for instance, the birth rate of the Jews was in 1891, 36.98, sinking in 1901 to 30.85, while among the Catholic population of that city it remained stationary, 41.58 and 41.59, respectively. In Roumania it decreased among the Jews from 40.14 in 1896 to 32.36 in 1902, as against an increase of from 41.19 to 42.86 among the Christians. In Hungary also the rate sank from 36.86 in 1891–95, to 32.19 in 1903. In western Europe this decrease of the birth rates among the Jews is actually appalling. In Bavaria the difference between 1876 and 1903 is nearly one half:

	Annual Birth Rate.	
	Jews.	Christians.
1876	34.4	45.9
1903	17.8	37.8

The birth rate of the Jews has thus decreased nearly one half within twenty-seven years, while among the christians the decrease is only slight. That this is not due to any special cause operating in 1903 is shown by the fact that it has been steadily going down. The average annual rates were in Bavaria as follows:

1876–1880	33.5
1888–1890	26.3
1890–1900	19.9
1903	17.8

In Prussia the same phenomenon is to be observed. The rates have decreased since 1875 among the Jews and increased among the Christians. As Arthur Ruppin shows, if we consider the absolute number of births during 1875 as 100, then it is found that during 1903 only 61.56 per cent. were recorded among the Jews, while among the christians it increased to 118.47 per cent. In the following table, giving the rates in Prussia for eighty consecutive years, is shown the steady fall of the fertility of the Jews; it shows that the christians bear nearly double the number of children as the Jews:

	1822–40	1841-66	1878–82	1888–92	1893–97	1898–02
Christians	40.01	39.55	37.92	37.03	36.89	36.19
Jews	35.46	34.75	29.96	23.75	21.61	19.71

The same is found to be the case in the United States: from statistics collected by the eleventh census (Census Bulletin No. 19, 1890), it is seen that the birth rate of the Jews is only 20.81 per 1,000 population, which is at least ten per 1,000 lower than the average birth rate among the general population. A fairer means of comparison, however, is the ratio of births with reference to the number of women of child-bearing age present, *viz.*, those between 15 and 49 years of age, inclusive. This rate was found to be 72.87 per 1,000, as against 82.9 in Massachusetts, and 86 in Rhode Island. During the six years in which this investigation was made by the census officials, the rates among the Jews were decreasing perceptibly, showing the same tendency as is observed among the Jews in Europe. Physicians who practise their profession among the immigrant Jewish population of New York City all agree that its fertility is decidedly decreasing. Those who have been a longer time in the United States are always inquiring about the best means of limiting the size of the family, while the native Jews are hardly to be distinguished in this respect from the average American city population.

In no country in the civilized world is there to be seen such formidable decline in fertility as among the Jews in western Europe. In Germany the rates among the christians have remained about the same since the beginning of the nineteenth century; since 1840 the rates have been about 36, and remained so at the beginning of the twentieth century. In some provinces it has only slightly decreased, as in Prussia from 37.8 in 1841 to 36.5 in 1900. The most striking decline in procreative capacity is said to be observed in France. But even there it was 27.3 in 1841–50, and it sank to 22 in 1900. This is considered the most appalling decline. But among the Jews racial self-effacement is much more pronounced; in Prussia the birth rate sank from 35.46 in 1822–40 to 18.71 in 1904; in Bohemia, Bavaria, etc., the rates are lower, only 17 per 1,000 population; in large cities like Berlin, it is even lower, almost on the verge of reaching a vanishing point. The effects of this violent race suicide are evident to every one: the number of native Jews in those countries is decreasing in rapid strides to an extent unknown in the history of any civilized people.

It has been stated that, notwithstanding the low birth rate, the Jews have a higher marriage fecundity or fruitfulness than the christians. This is not borne out by facts. In the following table it is seen that in Russia, Prussia and Bavaria the average number of children to a marriage is smaller than among their non-Jewish neighbors.

Country.	Year.	Jews.	Christians.
Russian	1897	4.33	5.63
Poland (Warsaw)	1901	4.59	2.95
Austria	1901	5.37	4.59
Roumania	1902	3.22	2.15
Prussia	1875–99	3.77	4.40
Bavaria	1903	2.31	4.30

That this low fecundity has not always been observed among the Jews is shown from figures about the Bavarian Jews collected by J. Thon.

The average number of children per marriage was during 1876–80, 4.75; it decreased during the next five-year period to 4.15; during 1886–90 to 3.49; then a further fall was observed to 3.01; during 1896–1900 it was only 2.50, and during 1902 and 1903 it again decreased, falling to an average of 2.20 and 2.31 children, respectively, per marriage.

In western Europe, where the birth rates of the Jews are lower, their fecundity is also lower. As will be seen later, this goes hand-in-hand with late marriges, celibacy, etc., among the Jews. In Russia, Poland, Galicia, Algiers, etc., where they are isolated from their christian neighbors and remain unaffected by what is generally known as modern civilized life, they marry earlier, have few celibates and raise large families. The birth rates are as a result quite high, though not as high as among the christians, who are largely engaged in agriculture and marry even earlier, as is the case in Russia and Galicia. Yet it must be remembered that in small cities in eastern Europe it is considered a sin for a Jew to remain unmarried, and an old maid in the family is a disgrace. In western Europe, on the other hand, the Jews are on a high social, economic and intellectual plane. Such people can not afford to marry early, and, after marriage, are not anxious to raise large families, for reasons known to-day in every large city. As a result they bear fewer children. Striking illustrations of this condition are presented in Austria. In Galicia and the Bukowina the rates are high, which goes hand-in-hand with poverty and strict adherence to their religious belief; in Bohemia and Lower Austria the rates are low, corresponding to the social and economic prosperity of the Jews in these provinces with the concomitant late marriages, celibacy, voluntary restriction, etc. In the United States also the newly arriving immigrants have a high fecundity, while the native Jews rarely raise large families.

Sex at Birth

The number of boys at birth exceeds the number of girls among most European nations. In some countries, like Greece and Roumania, the ratio is 112 to 100 girls, but the average appears to be about 103 and 105 for European countries. It was alleged that among the Jews this excess of male births is more pronounced than among the non-Jewish population. Ignorant, as we are, of the cause of the preponderance of males at birth, this excess, not being influenced by the social and physical environment, was considered a race trait of the Jews.

From statistics of the Russian Pale of Jewish settlement it is seen that there is actually a very large excess of male births among the

Jews: During 1897 there were recorded 115,344 Jewish births, of which 66,036 were males and 49,308 were females, or 133.91 boys to 100 girls. But a careful study of these figures brings forth strong suspicion as to their accuracy. Thus, when we examine the various provinces we find great variations. In Taurida the ratio was only 106.15 boys to 100 girls; in Cherson, 112.15; in Poltava, 112.87; while in Wilna it reached 177.47; in Grodno, 170.62; in Minsk, 165.45. In general, it can be stated that in the southern provinces the excess of males is not much larger among the Jewish population than among the christians, while in the northwestern provinces the excess is very high. That climatic conditions are not the cause is shown by the fact that among the christians the excess of males is not much more pronounced in the northwest than in the south. In Wilna it was only 110 and in Grodno 112. Two provinces not far distant from each other, like Wilna and Curland, show great differences in the proportion of male births among the Jews—172.8 in the former and only 115.4 in the latter. Climatic conditions can not therefore be considered.

If the excess of males were really as large as the above figures would seem to indicate, we should expect that the number of male infants below one year of age would also be excessive among the Jews. But from the census statistics of 1897 it is shown that it was only 104.21 boys to 100 girls below one year of age. The higher mortality of male infants is not sufficient to account for the loss of so many boys during the first year of their life.

The only plausible explanation for this apparent excess is that a large number of female births are not reported to the authorities by the midwives and rabbis, who are expected to register each birth. The birth of a boy in a Jewish family is accompanied by important festivals and ceremonials, while the birth of a girl, particularly among the poorer classes, is not considered of any special importance and is not attended by any ceremonials. It is very dangerous in later life for a boy who has not been registered at birth: he can not obtain a passport, and may be drawn into military service unjustly. All this brings it about that practically all the boys are registered, while a large number of female births is missing from the registry books. That this is the true explanation is seen from the fact that in 1893 the proportion of male births was 145.9 to 100 females, while in 1899 it was only 130.6, indicating a more complete registration of female births in recent years.

If the excessive proportion of male births was a racial trait of the Jews it would be expected that the same phenomenon should be observed among Jews in other countries. But this is not the case. In Warsaw, Poland, the ratio was in 1897 only 106 boys to 100 girls.

Ethnically there are hardly any differences between the Polish and Lithuanian Jews, still the latter show a ratio of 173 in Wilna, which again confirms the opinion that the excess in Wilna is due to neglect in reporting female births. In Prussia also the proportion was in 1893–1902, 106.24 (105.94 among the christians); in Austria in 1901 it was 107.85 (106.04 among the christians). In Prague the number of male births among the Jews in 1901 was equal to that of the female births, although among the christians there was an excess of males amounting to 104.1 per cent. In the United States the excess of male births is not large among the Jews, only 103.16 (Census Bulletin No. 19, 1890), while among the general population of Massachusetts and Rhode Island it is much higher.

Proportion of Stillbirths

Older statistics of stillbirths quoted by Bergman, Lagneau, Jacobs, etc., indicate that stillbirths occur less frequently among Jews than among Gentiles. More recent data on the subject show that this is not the case with the Jews in every country. Thus in Amsterdam the proportion of stillbirths in 1900 was among the Jews 3.48 per cent. of the total number of births, and much larger among the non-Jewish population, 4.81 per cent.; but in Warsaw it was in 1901 5.68 per cent. among the Jews, and only 4.13 among the christians. On the other hand, in Bavaria, it was in 1902–03 about the same among both, Jews (2.6 per cent.) and christians (2.9 per cent.). In Austria there are also no important differences. In 1901 the percentage of stillbirths was among Jews 2.61 and among christians 2.79. The most reliable statistics are collected in Prussia. The following are the percentages

	Jews.	Christians.
1875–1899	3.20	3.58
1901	3.07	3.03
1902	2.93	3.00
1903	2.83	2.99

There is practically no material difference in this respect among Jews and christians in Prussia. It should be mentioned in this connection that the smaller number of illegitimate births among the Jews would lead one to expect a smaller percentage of stillbirths, because the proportion of stillbirths is very large among illegitimates. The suggestion made by some that the large proportion of boys born among Jews is due to the fact that the percentage of stillbirths is small is also not to be seriously considered, simply because the proportion of stillbirths is not smaller among them. It must, however, not be overlooked that the percentage of stillbirths among the Jews varies with conditions observed among non-Jews in a given country. It is high among the Jews in Warsaw, and low among the Jews in Prussia, just as it is among the christians in these countries. In other words, in eastern Europe, where childbirth is attended to by ignorant midwives, the proportion of stillbirths is larger than in western Europe, where either physicians or trained midwives are in attendance. Stillbirths are, after all, greatly dependent on economic conditions. They are very frequently met with among people in the lowest social and economic strata, and rare among the prosperous.

Illegitimate Births

Illegitimacy has often been taken as an index of the morality of a community. While it may be a true index in many countries, yet in some countries, owing to special marriage laws, an excessive proportion of illegitimate births is not necessarily an indication of vice. A good illustration is presented in Austria. There a child is considered illegitimate in case the parents have not registered their marriage with the civil authorities. It appears that the Jews in Galicia and Bukowina very often neglect to register their marriages and consider their religious ceremony as sufficient. As a result of this special law, it is found that while nowhere else is the proportion of illegitimate births among the Jews over four per cent., it reaches in Austria 61.37 per cent. In Storozynee the records even show 99.61 per cent. of illegitimate births among the Jews, which is manifestly absurd.

Country.	Year.	Per Cent. of Illegitimacy.	
		Jews.	Christians.
Bavaria	1876–1900	1.80	13.60
"	1902	2.39	12.40
"	1903	2.55	12.50
Amsterdam	1900	1.71	5.88
Austria	1901	1.37	11.38
Warsaw	1901	2.14	14.33
Prussia	1875–1899	2.83	8.81
"	1901	2.89	7.88
"	1902	3.90	7.13
"	1903	3.46	7.06
Berlin	1904	5.55	16.70
Budapest	1898–1902	12.50	30.50
Russia	1897	0.36	2.61
"	1898	0.37	2.67
"	1899	0.54	2.65

It is seen from the above figures that about seven illegitimate children are born to christians in Bavaria to one to Jews; in Amsterdam it is about three to one, in Warsaw seven to one, in Prussia and Budapest two to one, and in Russia five to one. The high percentage in Austria and in part of Budapest has already been explained above as being of no significance.

It is noteworthy that the percentage of illegitimacy among the Jews increases as we proceed from east to west of Europe. It is very low in Russia, about one-half of one per cent. higher in Bavaria, 2.5 per cent., and reaches over three per cent. in Prussia, while in Berlin it is even 5.55 per cent. This indicates that where the Jews are not affected by modern civilized conditions, the chastity of the women is much superior, the family ties are much stronger, and the girls only rarely go wrong. In the small towns of Russia, Poland and Galicia, one only rarely hears of a Jewish child born out of wedlock. Unmarried women seldom associate, even socially, with men before marriage. The absence of alcoholism, particularly among Jewesses who never drink, is another factor in keeping the sexes apart. But in the large cities in eastern Europe, where the separation of the sexes is not so strict, illegitimacy is encountered. In western Europe it is more frequent for the same reason. It was shown by Ruppin that in Germany illegitimacy is rarer among the Jews in eastern Prussia (Posen, Pomerania, East and West Prussia) where they adhere strictly to their orthodox religion, while in the large cities, where they have adopted many of the habits and customs of their christian neighbors, the percentage of illegitimacy is much higher, though still smaller than among non-Jews. In Russia also it is rare in Lithuania, only 0.02 per cent. in the province of Wilna, 0.24 per cent. in Minsk, 0.19 in Kovno, etc., while in the southern provinces it occurs more often, reaching 1.57 per cent. in Bessarabia and 1.19 per cent. in Ekaterinoslav.

It is well known that illegitimate births are very rare among women living with their parents, while agricultural servants, domestics, factory hands, etc., show the highest percentage of births out of wedlock. The Jewish women in eastern Europe only rarely live away from their parents or relatives, comparatively few are engaged in domestic service, and practically none are agricultural servants. In the small town a Jewish girl rarely works outside of her home. In western Europe social conditions of the Jews are nearer those of the christians among whom they live, and illegitimacy is more frequent than in the east. But inasmuch as the economic condition of the Jews in western Europe is superior to the average non-Jewish, the women being taken better care of, illegitimacy is rarer than among Gentiles.

THE DEVELOPMENT OF MECHANICS

By S. E. SLOCUM, Ph. D.
UNIVERSITY OF CINCINNATI

THE history of mechanics affords a notable instance of what may be called the relativity of science. In the course of its development three distinct sets of mechanical principles have been formulated, each having served in its turn as the foundation of a complete system of mechanics. The first set of principles may be regarded as the first mental image which man formed of the causes underlying the natural motion of material bodies, and, although admirable in many respects, was necessarily somewhat crude and naïve. With increased mentality came the formation of a new image, showing a greater maturity of thought than the first and offering a more powerful method of analysis. Finally, in recent times, a third image has been formed, which, although not essentially different in content from the others, exhibits a greater refinement of thought and expression. It is the purpose in what follows to outline briefly these three stages of development, and sketch the chief characteristics of each.

The first scientific development of mechanics arose from investigations concerning the equilibrium and motion of the simple machines in common use, such as the lever, inclined plane and pulley. This order of development was inevitable for the twofold reason that these implements had become familiar by centuries of use, and that they made a direct appeal to the understanding through the grosser and more elementary sensations of weight and pressure. In the second century, B.C., these investigations culminated in Archimedes's famous statement of the principle of the lever, but for seventeen centuries thereafter this statement remained the only instance of correct reasoning on natural phenomena. Apparently human experience did not yet suffice to extend the interpretation of natural law, as witnessed by the Ptolemaic system of astronomy, and Aristotle's division of motions into *natural* and *violent;* a classification which served rather to obscure than elucidate the subject.

In the latter part of the fifteenth century a fresh start was made, and the principle of the lever, handed down from Archimedes, was further investigated and generalized by Guido Ubaldi and Leonardo da Vinci. In 1586 these results were extended by Simon Stevin, who, by hanging a string of fourteen balls over a triangular support, established the properties of the inclined plane, and generalized his results by stating the triangle of forces. These pioneers were followed by a host of lesser investigators, and by the middle of the sixteenth century this activity had resulted in the establishment of that branch of mechanics which is now called statics.

The next step was the introduction of the fundamental elements of time and mass in an attempt to investigate the laws of motion. At first little progress was made, as the misconception prevailed that a constant supply of force was necessary to keep a body in motion. Prolonged experiment and investigation, however, gradually resulted in a clearer understanding of these phenomena, and finally led to a correct statement of the first law of motion by the great Italian philosopher Galileo Galilei. Subsequent investigation of the motion of projectiles and falling bodies led Galileo to the two great ideas of inertia and the accelerating action of force, and enabled him to also state the second and third laws of motion. In addition to these great discoveries, Galileo generalized the law of equilibrium by stating the principle of virtual velocities, thus giving the first general solution of all problems in statics.

For the next century the development of mechanics consisted chiefly in an application of the principles of statics to liquids and gases. The only notable advance in mechanical principles during this period was made by Huyghens, who, in connection with his invention of the pendulum clock, investigated the center of oscillation and was thus led to a more general statement of the third law of motion.

The four fundamental ideas of space, time, force and mass were now firmly established, but until the time of Newton found expression only in an inorganic mass of facts and principles. Newton's discovery of gravitation, however, led to such a broad generalization of these ideas as to make possible a systematic treatment of the subject, and mechanics as a science may be said to date from the publication of his famous Principia in 1686. Newton's claim to preeminence, therefore, rests not on the discovery of new mechanical principles, but on the immeasurably greater service of bringing all natural phenomena under the reign of universal law.

Only one element was now lacking to complete the series of independent fundamental statements necessary to constitute the foundation of a complete system of mechanics. There still remained the establishment of a general relation between these fundamental concepts, and after eighty years of experiment and investigation along the lines indicated by Newton, this relation was furnished by d'Alembert in the statement of his famous principle.

This closed the first stage of development. The image was now complete, and henceforth a system of mechanics based on this foundation must be a purely deductive science. The subsequent history of mechanics verifies this statement, for since the time of d'Alembert no essentially new principle has been discovered, and Gauss may be quoted as authority for saying that none ever can be.

The second stage of development was characterized by the elaboration of the system of mechanics formulated by Archimedes, Galileo, Newton and d'Alembert. In the course of this process a new view of the fundamental ideas underlying the subject was attained, which resulted in establishing mechanics upon an entirely different basis. The first step in this direction was made by Euler, and consisted in replacing the geometrical methods of Newton and his predecessors by those of analysis. Euler thus laid the foundation for a system of analytical mechanics which was brought to its perfection by Lagrange in his generalized equations of motion.

This new representation of mechanics was followed by the establishment in the early part of the last century of two great principles; the principle of least action and the principle of the conservation of energy. It is important to note in this connection, however, that each of these principles is deducible from that of d'Alembert, and, consequently, that their establishment did not increase the number of independent fundamental postulates.

The first of these principles dates back to the attempt of Maupertuis to establish on theological grounds a principle of similar nature but of much more limited scope. This attempt, although fruitless in itself, served to direct thought in a new channel, and finally led Gauss to the statement of his 'Principle of Least Constraint.' This in turn led investigators to the idea that all natural phenomena present a maximum or a minimum, and induced Euler and Jacobi to seek expressions whose conditions for a minimum would give the equations of motion. From this it was but a step to the establishment of Hamilton's principle, which consists in the analytical statement that the variations of work and energy vanish for the initial and final configurations. As Hamilton's principle includes both conservative and non-conservative systems, it constitutes a generalization of the principle of least action.

This second principle, like the first, was the product of evolution, as the ideas underlying it had been the subject of investigation from the time of Leibnitz and Descartes. The principle did not assume definite form, however, until the middle of the nineteenth century, when it was stated by several investigators almost simultaneously as the law of the conservation of energy. The names most closely associated with this principle are those of Mayer, Joule and Helmholtz, and it is curious to note that each of these scientists arrived at his results by a different process; Mayer by philosophical reasoning, Joule by experimentation and Helmholtz by mathematical analysis.

The establishment of this law marked the close of the second stage of development. Energy replaced force as a fundamental idea, and a new system of mechanics resulted, founded on the relations between space, time, mass and energy, as embodied in Hamilton's principle.

Although a comparatively short time has elapsed since the establishment of energetics as the basis of mechanics, a third stage of development is already clearly marked. To characterize each stage by a single word, the first may be called *constructive,* the second *deductive* and the third, or present stage, *critical.* To the founders of the first two systems the concepts of force and mass, although more artificial than the intuitive ideas of space and time, were probably no less axiomatic. With the growth of modern scientific criticism, however, came the desire to go back of intuition, if that be possible, and subject the foundations of science to the last analysis. As the result of this tendency the foundations of the first two systems were found open to certain objections, which have been admirably expressed by the late Heinrich Hertz. The chief objection to the first system is in relation to the idea of force, any definition of which seems to involve its author in certain logical difficulties somewhat similar to those encountered in attempting to define a straight line. In the second system, criticism is aimed not at the fundamental concepts, but at the relation between them as expressed in Hamilton's principle, the objections to which are twofold: namely, that it has no simple, natural interpretation, and that it seems to endow matter with the attributes of thought and volition. A further objection is made to both systems on the ground of a certain redundancy in the fundamental ideas, three fundamental concepts being both necessary and sufficient, according to Kirchhoff, for the development of a complete system of mechanics.

In view of these and other objections, Hertz and his followers have outlined an ideal system of mechanics based upon three elements only: namely, space, time and mass. To supplement the deficiency caused by the lack of a fourth element without increasing the number of fundamental concepts, Hertz has introduced the idea of concealed motions acting in connection with those visible to the senses. This idea was originated by Lord Kelvin in his theory of vortex atoms, and was further developed by Maxwell in his attempt to explain electromagnetic action. The first complete treatment of concealed motions, however, was given by Helmholtz, and in the hands of his pupil Hertz it has proved a powerful instrument in establishing mechanics upon a more satisfactory basis.

What the future of mechanics may be it is of course impossible to predict. However, the brief review of its development that has just been given suggests that the foundations have reached bed rock, and that future effort must be directed toward the enlargement of the superstructure and its adaptation to the growing needs of humanity.

DIAMONDS AND CARBONS IN BRAZIL

By H. W. FURNISS,
FORMERLY AMERICAN CONSUL AT BAHIA

THE best diamonds of the world, those of finest color and fire, come from Brazil, though most of the stones mined are small in size as compared with those from other sections. They occur in various places, more particularly Goyaz, Matto Grosso, Minas Geraes and Bahia. It is only in the latter two states in which they have been found in sufficient quantities to warrant mining.

The exact date of the discovery of diamonds in Brazil is unknown. At the end of the seventeenth century miners were taking out gold in Minas Geraes at Serro Frio and failed to recognize the diamonds which were occurring therewith. It is said that some stones were collected more because of their regularity and beauty of crystallization than from knowledge of their value. These in 1729 found their way to Portugal, where they were recognized as diamonds of unusual purity. The discovery caused quite a sensation at the court of King John V. There was a rush to the newly-discovered region, but the king so restricted mining that little was done until subsequent to 1832, when the present laws became effective.

Diamonds were first discovered in Bahia in 1840 at Santo Ignacio at the extreme northwest of the present region, but not until 1844, when discovery was made by a slave on the banks of the Mocojé river, the present location of São João do Paraguassú, was any great impetus given to mining. The mining area has gradually extended, but no new section was discovered until 1881 when by accident a find was made at Salobro, the diamonds of which usually take the name of Cannavieiras, the port to which they pay tribute.

The quality of Brazilian diamonds varies greatly with the locality in which found, while there is always a considerable difference between those of the same mine. In general, those from Minas Geraes are fairly assorted in quality, about Salobro (Cannavieiras) the beautiful whites and priceless blue whites predominate, while the other Bahia stones are inclined to be more off-colored and frequently contain black specks, thereby lowering their value.

The greater part of the Bahia diamonds differ from those found elsewhere in the world in that they frequently have a thin coat of surface color which gives the whole stone a bad appearance. This color will not yield to acids. To one particularly skilled, the underlying true color can be determined, but to make the stones marketable abroad where this is unknown, recourse is had to heating them red hot and pouring on a chemical when the crust is consumed and the real color appears. I have seen apparently dirty red, green, brown, blackish and yellow stones after burning turn out to be pure whites and blue whites. Stones so treated lose in weight about one per cent., and those with cracks or defects frequently break to pieces.

The largest authentic Brazilian diamond ever found is the famous 'Estrella do Sul' (Star of the South). It weighed 254.5 carats in the rough, and cut and polished weighs 124½ carats, with a value of $450,000. The greater number of diamonds found are less than one carat; the average weight is about two carats, while a stone of 10 carats is a great exception.

Carbons which occur along with diamonds are very ordinary looking stones and would be refused as a present by any one not well acquainted with them. Their history is very obscure. Other than a few small ones found in Minas Geraes, and those are of poor quality, Bahia is the only known place where they occur. They seem to have been known in 1848, when a Frenchman traveling through Bahia bought them for twenty-seven cents a carat under the name of 'ferragens' (iron stones). In March, 1856, Mr. Domingos Gomez, of Roncador, took to London 6,475 carats, which he had bought for sixty cents a carat, and was more than pleased to sell them at $1.25 a carat. At that time their sole use was to be pounded to dust for use in diamond polishing.

The later history of the carbon is the history of the so-called diamond drill which now constitutes their principal use. For this purpose stones weighing from 1½ to 4 carats are desired and larger stones have to be broken to these sizes. The drill consists of 6 or 8 carbons set in a crown or cylinder of steel forming the bit. They are set in such a way that they alternately slightly project beyond the inner and outer edge, thereby cutting as they are rotated a core, which is brought to the surface from time to time as desired. Being the hardest known material they will cut the most refractory ores or stones.

As the drill goes around the carbons wear off and have to be from time to time reset, until finally they become so small as to be useless. For this reason, unlike the diamond whose chief use is for adornment, the number of carbons is constantly growing less while the demand is exceeding production. With the perfection of the drill and its great use in cutting tunnels, mines, canals, etc., the price of carbon has steadily gone up from $17 a carat in 1892 to $60 to-day in New York for the best quality of proper size, and the price obtained at the mines has been a fair equivalent.

The average weight of carbons encountered is much larger than that of the largest diamonds. The frequency of occurrence where the two occur together is in the proportion of three of the carbon to one of the diamond, by weight, while good quality carbon is worth more per carat than gem rough diamonds. The miners know this, and it is their constant desire to encounter carbons. Many of them go to the sacred tree near Lençõ̃es, where the spirit of Santa Barbara, the patron saint of the miner, is said to have appeared, and place on a limb thereof a stone of the size of the carbon they wish to find. They then pray and it is said that the saint has blessed many of the faithful.

The largest carbon ever encountered was found near Lençõ̃es in 1895, on the ledge of a mountain which had been worked some time before. It weighed, when found, 3,165 carats, was purchased from the miner for $16,000 and was finally exported to London, where it sold for $31,145, having lost about 50 carats meantime in drying out. In London it was broken into pieces suitable for drills and these pieces sold for about $40,000, while at the present price of carbon they would be worth about $158,000.

The next largest carbon was found this year, and is still in the hands of the miner. It weighed when found 869 carats, but has lost

Fig. 1. The Largest Carbon ever found.

several carats in drying. It is of finest quality and almost rectangular shape. The equivalent of $26,400 was offered for it six months ago, but the owner has set a price which to-day is the equivalent of $45,625, an impossible figure, as in breaking there is always considerable loss. When carbon advances beyond a certain figure the sale of necessity decreases, as then there are other products which are used even though lacking in durability and other desirable qualities.

The genesis of the diamond and carbon has not been worked out for this section. Whatever it proves to be, it is certain that at one time they were all confined in a conglomerate which shows evidence of being of more recent geological date. The conglomerate differs in character in the different sections. In the neighborhood of Lavras Diamantinas it consists of many colored water-washed pebbles and boulders, chiefly sandstone of the same nature as the strata found immediately below it; in the Salobro region it consists chiefly of granite pebbles. In both instances the matrix is sand of different degrees of hardness, fineness and color.

With the ages a great part of the conglomerate has disintegrated and the rains and rivers have washed the diamonds and carbons to the places where they are now being found. There are large masses of conglomerate in many places which have resisted this action, and unless mechanical means are brought to bear will continue to yield diamonds and carbons for the ages during their disintegration.

The region about Salobro is comparatively flat, in fact the greatest deposit occurs in an area practically level, doubtless the old river bed. In the other sections of Bahia the country is rocky and mountainous. There is so much of rock and so little of soil that only small plants grow, and then only during the rain time. In some cases the rivers pass through gorges cut into the solid rock and most precipitous and awe-inspiring. On all sides there is much of interest. The rock formation is a very hard reddish sandstone which completely underlies the conglomerate and like it shows the disintegrating effects of water and climate. In places it has deep cracks which have become natural canals, accumulating with the ages a concentrated diamond- and carbon-bearing gravel. In other places immense pieces of sandstone and conglomerate are piled up heterogeneously as if they had been dumped there. The canal Simplicio Braga is a combination of these two varieties and was one of the richest finds of the region.

The diamond section of Bahia is much more accessible than that of Minas Geraes. One can arrive at Andarahy, the heart of the region, in four days from Bahia City, five hours of one day being spent in journey by boat, twelve hours of the next by train and two days by mule. The trip is without hardships to one accustomed to travel. It is along attractive scenery, across rivers and mountains, passing through a section with beautiful calcareous caves, but with an entire

FIG. 2. HOME OF A WEALTHY MINE OWNER.

lack of water, except that of the river miles away and that caught during the few rainfalls.

Most of the mining is done by individuals called 'garimpeiros,' who either work for themselves or on shares with the owner of the claim. In Bahia, the number of owners hiring laborers to work their claims is not more than half a dozen.

The miners are almost entirely blacks or of mixed race. The greater part of them live in near-by towns, but many have quarters built beneath an overhanging ledge, from which they have removed the diamond-bearing material.

Their food consists chiefly of native beans with jerked beef and an abundance of mandioca meal, which takes the place of bread, with now and then fresh meat, a much prized boiled dinner or a piece of salt fish. Drinking water is in abundance everywhere. Native rum can be had very cheap, yet the number addicted to intemperance is very small, wonderfully so for a mining region.

Many times provisions are advanced by the grocer until a find is made, when all is paid up, and if there is a balance such high-priced articles as beer, American canned oysters, lobsters, etc., are indulged in as long as the money lasts.

The health of the region leaves much to be desired. Because of the great quantities of semi-stagnant water on every hand, every facility is given to create anopheles mosquitoes, with the result that malaria in its worst types is always in abundance.

By far the greater part of the successful mining is still done by antiquated methods which have the advantage that they require little capital for an outfit. A miner's tools consist of a short-handled hoe with which to stir up diamond-bearing gravel in a sluice; a crowbar to pry up stone to lay bare deeper layers or to break down banks of clay or gravel; an iron hook on a pole with which to take diamond-bearing gravel from beneath large stones or from cracks otherwise inaccessible; a small wooden basin, called 'carimbé,' for carrying the gravel on the head; a large wooden basin, called 'bateia,' for final washing and concentrating the gravel; some kind of a sieve, from a tin can with nail holes to a more pretentious wire sieve, for sorting gravel and sand during the washing or concentrating process; a hammer and drill for making holes in rock for blasting, but quite often instead fire is built upon a rock desired to be removed, and after the

Fig. 3. Home of Miner, built under Ledge from which Diamonds and Carbons have been removed.

rock has become very hot cold water is poured thereon, effectively cracking it and permitting its removal.

In the home of the carbon there are no carbon or other mechanical drills. At present one man can make from two to three holes a day, which with proper methods could be made in a few minutes.

The method of mining differs in various sections. In the richest areas the work is of two kinds: removing the subsoil surface disintegration and gravel and that in the gullies, cracks and beneath the more accessible stones, or mining by tunnels or following cracks into the pockets of the mountains, taking out the diamond- and carbon-bearing material consisting of soil, sand, gravel, boulders, broken and disintegrated stone, etc., called 'cascalho.'

The other method is in diving to the bottom of rivers and taking out the cascalho from there. This method is confined to a small section of the district where the river runs through a natural canal

FIG. 4. METHOD OF MINING; REMOVING SUBSOIL TO EXPOSE CASCALHO.

cut into the rock. The diving can only be done when the river is low and is chiefly done naked, though there are a few diving suits in use. The naked divers descend a pole planted in the river and fill a sack with the cascalho, which is taken on shore for washing. The ability of some of these men to go to great depths and stay under for long intervals is extraordinary. In some places attempt is made to work the old river bed, but this is done with great difficulty, as water will seep in almost as fast it can be bailed out, leaving little time for the collection of cascalho.

Whatever the method of taking the cascalho out, the great desideratum is an abundant supply of water for washing. Where it is possible water from mountain streams is conducted down by ditches and flumes, and into these the cascalho is thrown. It is worked with a hoe, by which method the lighter particles are washed away, thus leaving a greater concentration which includes the diamonds and carbons. The concentration is taken out of the ditches and accumulated until the week's end, when it is laboriously further concentrated in bateias.

This final concentration and wash-up requires considerable dexterity as well as strength. It consists in revolving and shaking the bowl that the portions of heavier specific gravity accumulate in the point in the bottom, while the lighter particles and the large stones are thrown on the edge of the bowl and are from time to time scraped away with the hand, being examined meantime. While the vision of

Fig. 5. Six Miners bailing Water that One may collect Cascalho.

Fig. 6. Final Concentration and Wash-up in Bateias.

those engaged in this process is very sharp and they will frequently yet from investigation I know that by this method many large diatake out from sand and pebbles diamonds smaller than a pin's head, monds and carbons escape them. This in part accounts for the reason why large diamonds and carbons are frequently found in gravel already washed and picked over. I have heard of places which have been washed for the fourth time and paid, though doubtless in some of these instances the later finds were due to disintegration of conglomerate which yielded up stones heretofore inaccessible.

The limit of a good man is to concentrate and pick over a cubic yard of cascalho per day, but this presupposes that the cascalho is easy of access and that the water is near at hand. If the cascalho has to be taken from the cracks, crevices, caves, etc., and with the present methods of mining those are the only places with virgin material which are accessible, it is accumulated very slowly. When it is remembered that at the South Africa mines there is worked over 192,000 cubic feet per day, it can readily be seen why the output of Brazil with its few thousand of hand-workers sinks into insignificance, if indeed the diamonds are in Brazil to extract.

The mines of Minas Geraes have been worked regularly since their discovery, chiefly by hand methods until during the last ten years when some machinery has been installed to aid in the separation of the diamond-producing gravel from the clay and sand and later on in partly sorting the gravel prior to the final clean-up which is always by hand process. In Bahia a little machinery consisting of a few pumps, a gravel sorter and a so-called automatic separator, which does not separate, has been installed at Salobro, but it is being allowed to rust out, work at present being done by hand entirely ignoring the machinery. The only other machinery in the great Bahia district consists of a few pumps mounted by an English company on the São José river. This company has machinery in transit for mounting a small electro-hydraulic plant, but will still leave the clean-up to hand process instead of adopting the automatic table in use at South Africa.

The diamondiferous lands of Bahia are owned by the state and leased either as small claims or large parcels to parties or companies desiring to work them. About all of the known areas capable of work with groups without machinery have been preempted. The nature of the work already done has been such that many productive areas have been covered with tailings. The river beds and other productive sections which will necessitate machinery are still awaiting exploitation.

SHORTER ARTICLES

SIGMA XI

Conspicuous among the events that attended the recent Ithaca meeting of the American Association for the Advancement of Science, was the twentieth celebration of the founding of the Sigma Xi, and, as so little is known about this organization, I venture to give a brief description of its history.

The career of the Phi Beta Kappa Society has been a long and honorable one, having been founded at William and Mary College in Williamsburg, Va., on December 5, 1776, and it is, therefore, the oldest of the so-called Greek letter societies. This organization, as is well known, admits to membership honor students in the humanities who are about to graduate, and the lack of any organization that should similarly recognize distinction in the study of the scientific branches led in 1886 to the organization in Ithaca of the Society of the Sigma Xi, which has as its objects to encourage original investigation in science, pure and applied, and by meeting for the discussion of scientific subjects, as well as for the publication of such scientific material as might be deemed desirable; and also to establish fraternal relations among investigators in scientific centers. Its name is derived from its motto Σπουδῶν Ξυνῶνες, signifying Companions in Zealous Research.

The success of the organization led to the establishment of a chapter in the Rensselaer Polytechnic in Troy and in Union University in Schenectady a year later. A chapter in the University of Kansas in 1890 and one at Yale University in 1895 followed. In 1896 a chapter was established at the University of Minnesota and one at the University of Nebraska in 1897. The Ohio State University came next in 1898, and the University of Pennsylvania in 1899. With the opening of the new century came chapters at Brown and the University of Iowa, and then Stanford University and the University of California, and Columbia University in 1901 and 1902. Three chapters were established in 1903, namely, at the University of Michigan, the University of Illinois, and the University of Chicago, and a year later organizations were effected at the Case School and in the University of Indiana.

Application for a chapter is now before the council for the University of Wisconsin. Thus it will be seen that this organization has already secured a good foothold and has been established at nearly all of the larger universities.

The first president was Henry S. Williams, of Cornell, one of the founders, who was succeeded by S. W. Williston, of the University of Chicago, who two years ago gave place to E. L. Nichols, of Cornell. Biennial conventions are usually held in connection with the meeting of the American Association for the Advancement of Science, the next of which will be held in December, 1906, many of its members being prominently connected with that organization. The membership is already a large one, numbering more than a thousand persons, most of whom are either teachers of or advanced students of science.

The different chapters hold public meetings at which speakers of eminence are invited to address the organization.

The badge or insignia is a watch charm or pendant consisting of the monogram formed in gold of the Greek letter Sigma superimposed on the greek letter Xi, the former being somwhat smaller than the latter. On the reverse side of the badge is engraved on the upper bar the name of the college in which the owner was initiated, together with the date of such initiation; while on the lower bar is the name of the owner with the numerals of the class in which he was graduated. The society has adopted as its colors electric blue and white. Its seal consists of a wreath of laurel, typifying the honorary character of membership in the society, arranged as an oval enclosing the words 'the Society of the Sigma Xi' at the top and the Greek motto at the bottom. These words form an inner oval concentric with the first, punctuated with ten stars enclosing a field illuminated by the lamp of research. Above the lamp in the field of illumination is placed the monogram composed of the two Greek letters Sigma and Xi, and below it the date of the foundation, 1886.

At the celebration of the twentieth anniversary, representatives of nearly every chapter were present, and under the auspices of the local chapter a public address, commemorative of the occasion, on 'The Recent California Earthquake' was delivered by Professor John C. Branner, of Stanford University. Subsequently a dinner was tendered to the visiting members which was presided over by Professor E. L. Nichols, when addresses were made by Dr. L. O. Howard, who spoke of the affiliation of the Sigma Xi with the American Association, and by Professor Henry S. Williams who described its founding, and by other members of the society.

M. B.

THE PROGRESS OF SCIENCE

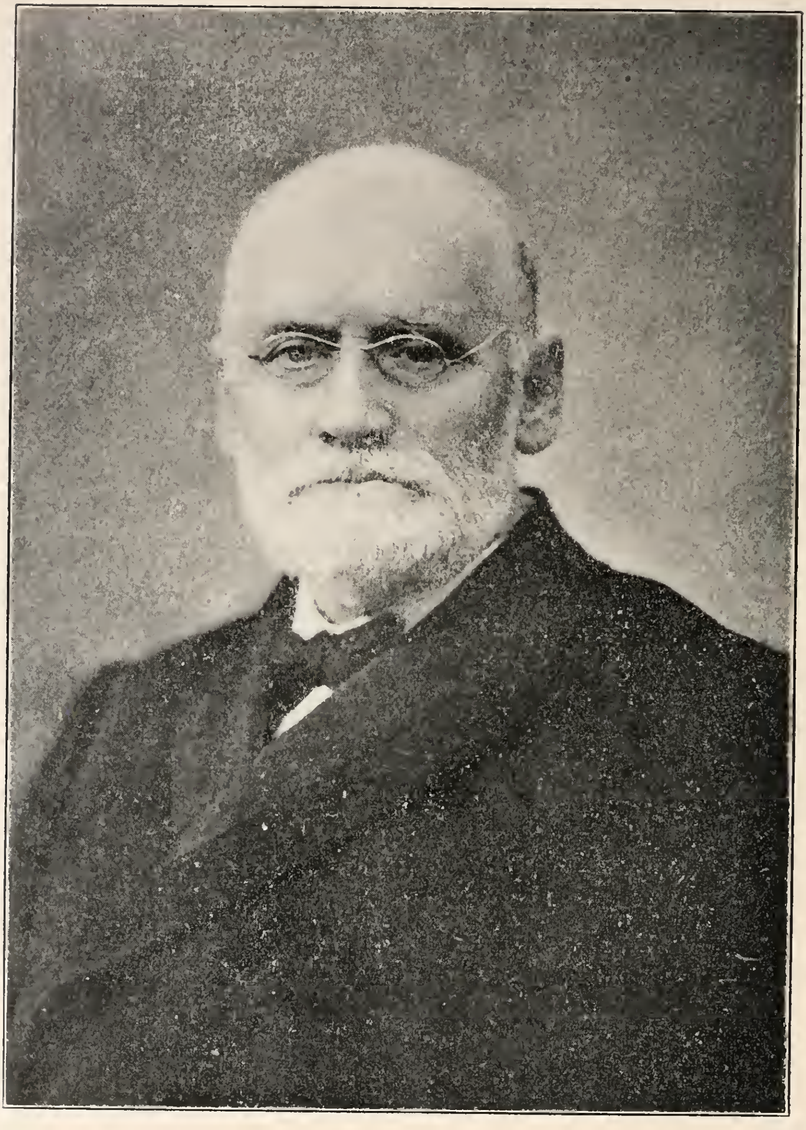
THE BUREAU OF EDUCATION.

The retiring commissioner of education has been so completely identified with the Bureau of Education that it is difficult to imagine the institution without the man. Dr. Eliot, at Harvard, and Dr. Harris, at Washington, have been our two great educational leaders, and when we turn to other lines of service—to the church, to medicine, to law, to journalism, to business, to politics—it is doubtful whether we can find elsewhere two men equally great. This is not a time to attempt an analysis of the work and limitations of a complex personality. It is better to quote the appreciation of a personal friend, Dr. Canfield: "He is indeed whole in himself, a common good —a man of amplest influence yet clearest of ambitious crime, our greatest yet with least pretense; rich in a saving common sense, and, as the greatest only are, in his simplicity sublime. His is the good gray head which all men know, and his the voice from which their omens all men draw. In the great battle of the public schools for sound and effective citizenship he is a tower of strength which stands foursquare to all the winds that blow."

The commissionership of education has been filled by the appointment of Dr. Elmer E. Brown, professor of education in the University of California. We may again quote, this time from the editorial pages of the *Outlook:* "He has shown himself to be safe and sane, philosophic in temper, practical in choice of ends and means, with unusual administrative ability, ready to take the initiative, not carried away by undue enthusiasm for novelties, yet always alert for all that marks true advancement, energetic and active and industrious, an able writer and speaker, and of a personality which makes him very acceptable in the educational world. In many ways and because of many characteristics and qualities he promises to be a worthy successor of one of the most widely revered educators this country has ever had the good fortune to enlist in its service."

No one can fill the vacancy left by Dr. Harris, but the new commissioner has a great opportunity for useful service. It is safe to say that there is no other country where public education is such an important factor in national life and at the same time no other country in which it is so completely neglected by the national government. This paradox is of course due to the fact that public education is left to state and local authorities, as was doubtless intended by the federal constitution. But wisely or otherwise, the national government has continually extended its functions. If it can examine banks, it can examine schools; if it can cooperate with states in their geological surveys, it can cooperate with them in their educational systems. As a matter of fact the constitution gives the congress power to 'provide for the common defense and general welfare of the United States.' Under the changed conditions of modern civilization, education, science, health and well-being are far more important for the common defense and general welfare of the nation than are the army and the navy.

But apart from cooperation with the states, such as now in fact exists in the case of the Department of Agriculture and the land grant colleges of agriculture and the mechanic arts, there is ample room to strengthen the Bureau of Education. After a secretary of



Dr. WILLIAM T. HARRIS

COMMISSIONER OF EDUCATION, 1889–1906

commerce and labor has been added to the cabinet, it would be only decent to provide for a secretary of education. There is probably no other nation without a department of education. The salary of the commissioner of education is $3,500, and the powers of his office are very limited. The bureau has charge of education in Alaska and prepares an annual report containing statistics and papers on education; but this is all. It may be wise to let the work of the government for education, science and art be distributed among different departments on the financial side. But there should be cooperation and a great extension of what is now being done. The Bureau of Education is the natural center, and we may look to a great enlargement of its powers and influence in the near future.

PROFESSOR MENDELÉEFF.

Dimitri Mendeléeff, the greatest of Russian chemists, was born in Siberia seventy-two years ago. From 1856 till 1859 he was an instructor at the University of St. Petersburg. After two years of study at Heidelberg, he returned to Russia in 1861. Two years later he was made professor of chemistry at the Technological Institute in St. Petersburg and was transferred to the university in 1866.

From the beginning Mendeléeff has been interested in theoretical problems. His first paper was on isomorphism. For years he worked on the relations between specific volumes and other properties. While others, notably Kopp, have worked along similar lines without making any great generalization in consequence, it must be admitted that Mendeléeff's great discovery of the periodic law seems a natural development from the earlier work.

In 1869 Mendeléeff announced that if the elements be arranged in the order of their atomic weights, it will be found that similar variations in their chemical properties repeat themselves periodically, and that the order of the faculty of the elements to combine with other elements also corresponds with the order of their atomic weights.

Like many another important generalization, the real significance of this one is not self-evident. Before the periodic law was formulated, the atomic weights of the elements were purely empirical numbers, and it was not always easy to tell what multiple of a given value should be taken as the true atomic weight. This was changed by Mendeléeff's discovery. The periodic law made it possible to determine the atomic weights of yttrium, indium and beryllium, for instance. Mendeléeff went further than this. He pointed out that there were gaps in the table; that these must correspond to unknown elements; and that the properties of these unknown elements could be predicted from those of the known elements surrounding the gaps in the table. Gallium, scandium and germanium have since been discovered and have the properties assigned to them in advance by Mendeléeff.

A more striking, though less dramatic, proof of the soundness of Mendeléeff's generalization is to be found in the fact that the inert gases of the atmosphere, argon, helium, neon, etc., find places in the classification, though the possibility of there being such substances was not suspected in 1869. It is not too much to say that the periodic law of Mendeléeff is recognized to-day as the only basis for the classification of the elements. Only two contradictions have been found in nearly forty years. The atomic weights of the elements, iodine and tellurium, should be transposed to make these substances fit into the table, and there is no place for most of the so-called rare elements. The first difficulty will disappear if any one can show that either tellurium or iodine contains an unknown impurity. It must be admitted that the chances of this are not good at present.

We can avoid the difficulty as to the rare earths by considering a group of

PROFESSOR DIMITRI MENDELÉEFF

them as equivalent to one element. Doing this puts the rare earth elements on a somewhat different footing from the other elements. While this is justified to a certain extent by the chemical properties, it can not, in the nature of things, be a final solution. If we are not to throw over the periodic law, we must either split other so-called elements into groups of elements or we must show that certain groups of elements alone are possible. To succeed in the first would be to revolutionize chemistry. To succeed in the second would be to explain the reason for the periodic law—which would also revolutionize chemistry. Whatever the outcome, Mendeléeff's law will be for many years one of the dominant factors in chemical progress.

APPROPRIATIONS FOR THE DEPARTMENT OF AGRICULTURE.

THE agricultural appropriation bill for the fiscal year ending June 30, 1907, as finally passed by the recent session of congress, carries an appropriation of $9,932,940. Of this amount the sums appropriated for what may be termed work in applied science are distributed as follows: The Bureau of Animal Industry receives $4,029,460, but of this amount $3,000,000 are to be devoted to the meat inspection, the discussion of which has occupied so much of the time of congress and of the public press during the past few weeks; Weather Bureau, $1,439,240; Bureau of Plant Industry, $1,024,740; Forest Service, $1,017,500; Agricultural Experiment Stations, including the Department Office of Experiment Stations, $974,860; Bureau of Entomology, $262,100; Division of Publications, $248,520; Bureau of Soils, $221,460; Bureau of Statistics, $210,560; Bureau of Chemistry, $174,180; Office of Public Roads, $70,000; Bureau of Biological Survey, $52,000; Library, $25,880.

Large as is the annual appropriation

for the Department of Agriculture, it is only one six-hundredth of the value of the agricultural products of the country, and there is every reason to suppose that it is a good investment. The figures of our agricultural wealth as given in the last report of the Secretary of Agriculture are so vast as to be difficult to grasp. Thus the corn crop alone is valued at $1,216,000,000. Hay, cotton, wheat, butter and milk and poultry and eggs each produced products worth over $500,000,000. Farm products of the value of $827,000,000 were exported. Thanks to such exports the balance of trade in favor of this country in the course of the past sixteen years amounts to over $5,000,000,000. The farms of the United States are said to have increased in value to the amount of $6,131,000,000 in the course of the past five years.

The Secretary of Agriculture awards mainly to the department credit for the great advances in the prosperity of the farmer in recent years. In concluding his report he says: "The gratifying evidences of well-being in our farming community, the extraordinary progress made in the past few years, and the rapidly enlarging recognition of the true position of the farming industry in the economic life of this country are mainly the result of this continued and combined effort on the part of these agencies to add to the sum of the farmer's knowledge, and must be regarded as the triumph of intelligence in the application of scientific knowledge to the tillage of the soil. This is so obviously true that it would seem superfluous to urge the generous maintenance of the department in its grand work. Great as has been the work undertaken and accomplished, gratifying as have been the results, as shown in the first few pages of this report, be it remembered that we are still at the threshold of agricultural development, and that the educational work which has led to such grand results has only been extended as yet to a portion of our agricultural population. There is not an intelligent, patriotic citizen in the Union who will not say with his whole heart, 'Let the good work go on.'"

THE WILL OF ALFRED BEIT.

Mr. Alfred Beit, who accumulated a vast fortune in South Africa and died on July 16, has by his will given large sums for public purposes. The most notable bequest is $6,000,000 to his partners to constitute a fund, the income of which is to be devoted to the construction, equipment or furtherance of any such methods of communication or transportation in Rhodesia, Portuguese Southeast Africa or the German possessions, and any parts of Africa that may be traversed by the Cape-to-Cairo Railway. The trustees are to have absolute discretion, and if two thirds decide that the fund is no longer required for furthering the work of communication or transportation, they can apply the proceeds to educational, charitable or other public purposes in Rhodesia.

One million dollars is left to the University of Johannesburg to build and equip buildings on the land previously given by Mr. Beit; one million dollars for educational or charitable purposes in Rhodesia and other territories within the field of the British South Africa Company; $125,000 to the Rhodes University, Grahamstown, Cape Colony; $100,000 for educational or charitable purposes in the Transvaal, and $75,000 for similar purposes in Kimberley and in Cape Colony. To the College of Technology, London University, the sum of $250,000 and 1,000 shares in the DeBeers Company are bequeathed, and to the research fund of London University $125,000. Two hundred thousand dollars is to be distributed equally in London and Hamburg for educational or charitable purposes. To King's Hospital and Guy's Hospital, London, the sum of $100,000 each is given. Mr. Beit's property near Hamburg, which was his birthplace, is left to that city, and his art collections are left to the galleries in London, Berlin and Hamburg.

SCIENTIFIC ITEMS.

We regret to record the death of Dr. Samuel Lewis Penfield, professor of mineralogy at the Sheffield Scientific School of Yale University, and of Dr. Paul Drude, professor of physics in the University of Berlin.

Sir David Gill, H. M. astronomer at the Cape of Good Hope, has been elected to succeed Dr. E. Ray Lankester, director of the British Museum of Natural History, as president of the British Association for the Advancement of Science. The association will meet next year at Leicester, beginning on July 31. The meeting the following year will be in Dublin, and in 1909 the association will for the third time visit Canada and meet in Winnipeg.

A knighthood has been conferred on Dr. W. H. Perkin, F.R.S., the jubilee of whose discovery of the aniline dye mauve has recently been celebrated.—Professor Seubert, hitherto the German member of the international committee on atomic weights, has resigned, and Professor Ostwald has been appointed his successor. The committee now consists of F. W. Clarke, United States, chairman; T. E. Thorpe, Great Britain; H. Moissan, France, and W. Ostwald, Germany.—Mrs. W. P. Fleming, curator of astronomical photographs in the Harvard College Observatory, has been elected an honorary member of the Royal Astronomical Society. Mrs. Fleming has also been appointed honorary fellow in the department of astronomy in Wellesley College.

The General Education Board, endowed by Mr. John D. Rockefeller with $10,000,000, has made appropriations to nine institutions on condition that three times the sum be appropriated from other sources. The appropriations, which amount to $312,500, are as follows: Coe College, Cedar Rapids, Ia., $50,000; Washburn College, Topeka, Kan., $25,000; Tulane University, New Orleans, $75,000; Wofford College, Spartanburg, S. C., $25,000; Furman University, Greenville, S. C., $25,000; Wake Forest College, Wake Forest, N. C., $37,500; Howard College, Birmingham, Ala., $25,000; Southwestern University, Jackson, Tenn., $25,000, and Mississippi College, Clinton, Miss., $25,000.

THE POPULAR SCIENCE MONTHLY

OCTOBER, 1906

THE EARTHQUAKE RIFT OF 1906

By President DAVID STARR JORDAN

LELAND STANFORD JUNIOR UNIVERSITY

THERE are two sets of disturbances which shake the crust of the earth and therefore go by the name of earthquakes. Eruptive earthquakes are explosions, usually of steam, about a volcano. Tectonic earthquakes are breaks in the overloaded or overstrained crust of the earth, and, for the most part, have nothing to do with the steam vents we call volcanoes. To the last class most earthquakes belong, certainly almost all that have been felt within the United States.

Again, under the name of earthquake we include two very different sets of phenomena, the one the rock-rift or fault, which is the disturbance itself, the other the spreading or interfering waves set in motion by the parting, shearing and grinding of the sundered walls of rocks in the earthquake fault. It is the jarring waves extending in widening and interfering circles which do the mischief to man and his affairs. It is the rift of rock which sends these waves forth on their blind mission of confusion or destruction.

In every tectonic earthquake there is somewhere a fault or rift of rock, with some sort of displacement, permanent or temporary, of the relations of the two sides. In extreme cases, this break extends for miles in a straight line, breaking the surface soil and passing downward to a depth which can be only guessed at, five or ten miles perhaps, perhaps as far down as the crust is rigid. There are undoubtedly destructive earthquakes in which the soil is not broken over the rift of rock, but as a rule, in violent disturbances, the crack comes to the surface, breaking through the overlying soil. In all severe earthquakes there are, moreover, breaks or fissures in the earth having no connection with the fault itself. These are slumps or landslides, and geologically they signify but little. They mean simply that loose soil has been shaken down. They do not go down into the underlying rock. From the true earthquake crack they may usually be known at once, because their course is determined by the topography. They are not straight. The true earthquake rift moves on in straight lines, broadly speaking, careless of topography. But topography is not careless of the earthquake rift. On either side of it, for perhaps hundreds of feet, the

Relief Map of California.

rocks are crushed to flinders by the impact and grinding of the opposed walls. An old fault is therefore marked by an excess of erosion. A valley or saddle marks its general course. Streams choose it for their basins, and when it crosses a mountain the softened rock yields to form a saddle or other form of depression. For these reasons, an earthquake fault is often marked in California by successions of dairies and of reservoirs. The valleys thus formed are fertile and well watered. For the most part, in much-faulted regions, such as form the rim of

EARTHQUAKE RIFT, as it comes up from the sea at Point Arena. Mendocino County.

the Pacific, each earthquake rift follows the line of an old fault, and the original break goes back to the mountain-making periods of Tertiary times. The California earthquake of 1906 follows the axis of an ancient break, the 'Portolá-Tomales fault,' or 'San Andreas fault,' first studied, so far as I know, by Dr. John C. Branner in 1892. In this fault hundreds of thousands of earthquakes, large and small, have

FISSURE AND LANDSLIP, SAN JACINTO VALLEY, 1897.

preceded the recent one. In it the aggregate displacement horizontally has been very great, and the aggregate vertical displacement as shown by the rock strata on either side of it exceeds half a mile.

It is the purpose of this article to trace the earthquake rift of April 18, 1906, across the map of California. The accompanying photograph of a relief map by Dr. Noah Fields Drake will show the topography of the state. In California there are multitudes of valleys of various kinds. Those formed by water and ice surface erosion are variously curved and ramified. Such are the mountain cañons of the west flanks of the Sierras. Those valleys formed or marked by earthquake cracks have almost invariably straight axes. These extend in general toward the north-northwest, more or less distinctly parallel with each other, and often intersected by cross-faults.

Examples of faulted valleys are the great valley of the Sacramento and San Joaquin, the Santa Clara Valley, San Francisco Bay, with the Valley of Santa Rosa, Eel River Valley, the Santa Catalina Channel, Owens River, the San Jacinto Valley and many others. A cross-fault extends from Monterey Bay up the valley of the Pájaro River. In some of these faults earthquakes have taken place in historic times, in others no break has been noted save that recorded in the rocks. Dr. Branner has compared a fault to a break in a bone. It represents a weak place which will give in a time of strain. On the other hand, if not freshly broken, it will tend with time to heal. A broken bone

Alder Creek Bridge, Mendocino County. The earthquake rift is near the middle of the picture.

Tomales, Marin County. The North Shore Railroad and the earthquake rift.

will be naturally renewed. A faulted rock bed will be cemented in the course of ages of pressure and of cementation.

The most interesting of these breaks in California is that recorded as the Portolá-Tomales fault. Its course can be plainly traced on the relief map. It enters the shore near the mouth of Alder Creek and near the low headland called Point Arena, in Mendocino County on the north, and runs to Chittenden, on the Pájaro River, in Monterey County, on the south. The line is almost perfectly straight, and its course and direction can be determined by placing a ruler on the map, using the line of Tomales Bay as an axis. This long, narrow, straight inlet is a resultant of past earthquakes, probably beginning in Tertiary times. It is bounded on the west by mountains which have their origin in some ancient upward thrust of the walls on the west side of the ancient fault.

On the eighteenth of April the trouble began in the sea. Just where, we may find out later. We know that the center is in the sea, because where the rift enters the land it was broader and its effects more violent than at any other point along its extent. As the rift can be traced for 192 miles across the land to the southward from Point Arena, it is safe to say that it goes as far to the northward under the sea. A steamer crossing it the moment of the earthquake, off Mendocino, ninety miles to the northward of Point Arena, bears witness to this fact. The captain thought that he had struck a raft of logs, so fierce and hard were the shocks of the waves in the water.. The movements were short, quick and violent, not forming a tidal wave, but a strange choppy sea. For the time being all rollers and surf were broken up. Off the bold headland of Cape Mendocino is a deep submarine valley, to the west of which is a high mountain which does not rise to the surface of the ocean. In the channel between the cape and the submerged mountain the earthquake rift may be supposed to run. In this channel numerous earthquake shocks have been recorded by different passing vessels. If not itself a center of disturbance, it records the line along which great disturbances have frequently passed.

The rift struck the land at the mouth of Alder Creek, above Point Arena. It crept over the hill as a deep furrow in the black, sticky adobe, veering a little to left or right according to the resistance of the soil, but always keeping in a straight line in its general direction. It may be imagined as a sort of devouring dragon, leaving its trail on the hills and destroying the works of man wherever it passes. It is hard in following its course, not to think of it as endowed with a sort of wicked life. Its movement is properly from north to south, but the opening of the great fault seems to have been really instantaneous. It took place at 5:13 A.M. and the waves lasted forty-seven seconds. It may be noted in passing that the complication of the waves at any one point was mainly due to the great length of the rift. A point immediately near the crack felt mainly the first great shock, its wave and the return wave. A point farther away felt the wave and its return jolt, followed at once by waves from farther to the north and farther to the south, these waves becoming more and more opposed to one another. The waves would then augment, neutralize, override and otherwise modify one another, the final result being the violent twisting motion, the most remarkable trait of the latter portion of the earthquake in question.

Coming over the first ridge, from the sea, the rift passed under the long bridge over Alder Creek. The land on the west side of the bridge

POINT ARENA. Picket fence was in one continuous line. Photograph shows short section put in to fill up offset by land on the west side.

Land Slip at Sobrante.

was jerked sixteen feet to the north; or that on the east sixteen feet to the south—only a careful re-survey of the region can tell us which. Or it may be that both sides went to the northward, but the west side pulled away, distancing the other by sixteen feet. In any case, the bridge was torn to splinters, and the crack went on, always the west side some sixteen and a half feet to the northward, though the sticky soil tends to lag back, and not every place shows the maximum of sheering or horizontal displacement. Passing under a barn, the rift tore it to splinters. The spreading wave displaced or destroyed most of the

Land Slip at Sobrante

houses in the villages of Manchester and Point Arena, wrecking the magnificent lighthouse of solid masonry on the Point itself. In low ground the rift formed successions of little ponds. On hillsides the lower side of the crack fell away like a drivelling lower lip, leaving an open chasm, ten to twenty feet in apparent depth. On level hard ground the soil like the rock below closed with a snap a little tighter than it was before. Line fences were broken and sheered from sixteen to twenty feet. Lines of trees met with similar readjustments. In Mendocino County the horizontal displacement is about sixteen feet. In Marin County, where exactly measured, it is sixteen feet and seven inches. Southward it becomes less. In San Mateo County it is six to eight feet, and at the Pájaro Bridge at Chittenden, where the open fault ceases, the western pier was moved northward about eighteen inches. This shifting of position, evident along the line of the crack, seems to have included the whole region, mountains and valleys, through which the crack passes. Either the region to the westward with the Santa Cruz Mountains and the mountains called Sobrante de la Punta de los Reyes have been stretched out toward the northward or else the region on the east side, including most of California, has been correspondingly humped up. It is impossible at present to say which is the fact, perhaps both. The vertical displacement is small. To the north of San Francisco the west side has been raised two or three feet. To the southward the slight relative change in elevation—two or three feet—is in favor of the east side.

Marshall Hotel, thrown into Tomales Bay.

Earthquake Crack in Country Road from Olema to Point Reyes.

The rift left the pastures of Point Arena, passing up Gualala River, always in a straight line, making havoc among the redwood

Earthquake Rift, Freeman's Ranch, near Tomales Bay.

trees, thence into the sea, where it runs close along the coast, past Fort Ross, throwing down everything movable in this and other towns. It then crosses Bodega Head and again falls into the sea, where it passes up the axis of Tomales Bay. At the head of the bay its course through the tules or bulrushes looks like a swath through a grain

Earthquake Rift, Freeman's Ranch.

field. Through this region (Marin County) the shock was very violent, and numerous cracks parallel with the main crack in the bay extended along the shores. In the town of Tomales, much and varied mischief was done. The parallel cracks toyed with miles of the North Shore Railroad between Tomales and Point Reyes. At Marshall the hotel was thrown bodily—and upright—into the bay, the boarders unharmed; and at aristocratic Inverness, on Tomales Bay, three summer cottages suffered the same fate. A fisherman in the bay reports that the waters receded, leaving his boat in the mud. Afterwards they came back in a 'great wave, which looked a hundred feet high, but which was probably not more than ten.'

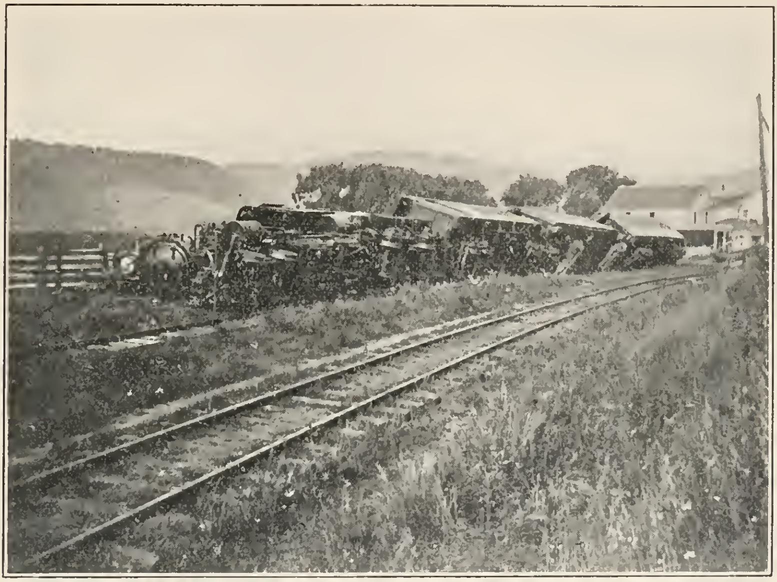
At Point Reyes Station at the head of Tomales Bay the 5:15 train for San Francisco was just ready. The conductor had just swung on when the train gave a great lurch to the east, followed by another to the west, which threw the whole train on its side. The astonished conductor dropped off as it went over, and at sight of the falling chimneys and breaking windows of the station, he understood that it was the *Temblor*. The fireman turned to jump from the engine to the west when the return shock came. He then leaped to the east and borrowing a kodak he took the picture of the train here presented.

Paper Mill Creek runs past the same village, a considerable stream, noteworthy lately from the successful stocking with king salmon. The two banks of the stream were forced toward each other so that the length of the bridge was shortened by about six feet and the bridge was correspondingly humped at its north end, an arch about six feet high being forced up.

From Point Reyes Station (at the base of the peninsula also called Point Reyes) the earthquake rift passed along the Inverness Road to Olema, where all the houses not standing on rock foundation were thrown from three to six feet to the westward, toward the crack itself.

Skinner's Ranch is a large dairy near Olema. The house stands near the road, a dairy house some thirty feet to the south of it, and a large barn with cowyard just behind that. A row of large cypress trees stood just before the house on the roadside, between them and the house a little rose garden, to the south of these, opposite and partly behind the dairy, a group or row of large eucalyptus trees.

The earthquake rift passed directly in front of the house, between the buildings and the road. All that stood to the westward of the crack was violently jerked to the north a distance of sixteen feet seven inches, or it may be that the east side moved an equal distance to the south. If Mr. Skinner had chanced to look at the right instant he would have seen the whole row of cypress trees file past his window to take their station in front of the dairy, taking the rose garden with them. A few raspberry bushes came from farther north to take, partly, the place of the roses. The eucalyptus trees in front of the dairy moved on to a position opposite the barn, and one detached from the others and to the westward of the crack was left near the head of the line instead of at its foot. The crack passes obliquely under the barn, entering it at the northwest corner and leaving it at the middle of its posterior or southwest side. The barn remained intact, thanks to its weak foundation, for the east side pulled loose from the ground, and the barn went northward with the west side. Sixteen and one half feet of its former foundation at the southeastern corner is exposed. A driveway under the barn is divided in the middle. You pass in on the east side, the western half is sixteen and one half feet to the north



TRAIN OVERTURNED BY EARTHQUAKE, POINT REYES STATION, MARIN CO., CAL.

of the entrance and completely blocked in the middle. Under each of the east windows of the barn stood a pile of manure. Each pile is intact, sixteen and one half feet south of the window to which it belongs. The people at the ranch were milking at the time of the shock. Each man and cow was thrown to the ground and it took two hours to get the frightened cattle back into the 'corral.' The stone steps to the dairy, on the east side of the crack, now stand sixteen feet seven inches to the southward of the door to which they led. About Skinner's, line fences and water pipes crossing the fault were broken, a break of sixteen and one half feet being left in each case, the west side of the fault in all cases more or less overriding the other. In the matter of line fences interesting legal problems are raised. Were the

EARTHQUAKE RIFT, OLEMA.

farms on the west stretched sixteen and one half feet or those on the east side crowded together to the same amount? If either, who stands the loss and what store can be set on ancient landmarks?

Next to the Skinner Ranch is the Shafter Ranch. Here the houses and barns are on the east side of the crack, but the transposition of

SKINNER'S RANCH, OLEMA.

roads, trees and fences was the same in kind. The rift passed through the corral, and one of the astonished cows dropped into it, soon falling deeply till only rump and tail were visible. The hysterical dogs barked at her, the water came into the rift, and the dairymen, doubtless with a sense of the impotence to struggle against fate, buried her in the grave from which they could not rescue her.

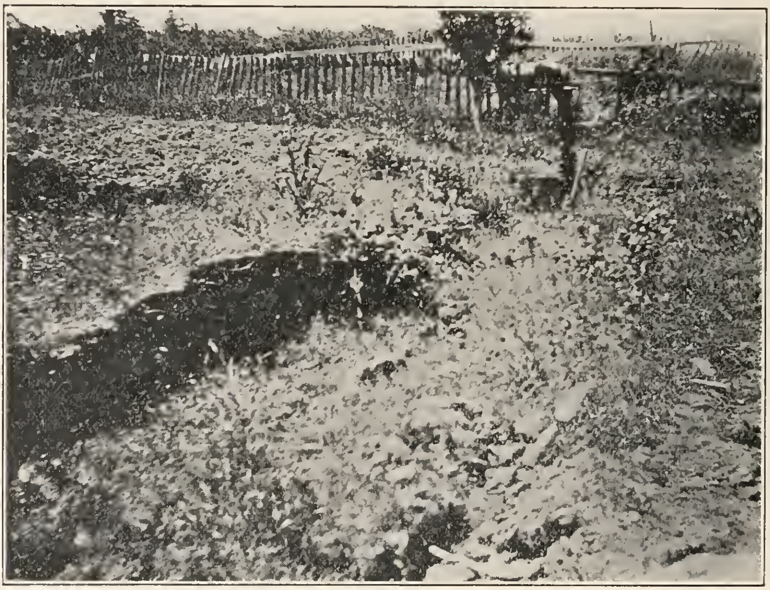
Crossing the valley the rift split a small hill, throwing down four large spruce trees, all of which fell at right angles to the crack. A very large oak tree standing on level ground was shoved violently, still standing, sixteen and one half feet to the southward into the base of the riven hill, or perhaps the western half of the hill was shoved violently about the tree. Dr. G. K. Gilbert seems to think it probable that both sides partook in the motion.

On through the valley of Olema went the rift, past more dairies, but leaving their buildings altogether to the east. Crossing the road above Bolinas, the two sides of the highway are rudely separated. Reaching Bolinas Bay, the rift is visible in the mud at low tide, and good authority reports the sea-bottom to the westward, along Duxbury Reef, to be raised two or three feet. The gatherers of abalone shells venture out into regions of sea-bottom formerly inaccessible at the lowest tides. On the east side of Bolinas Bay the clams are hopelessly buried. At Bolinas the pretty Flagstaff Inn was thrown into the sea and completely wrecked. The crack again enters the sea, passing across the entrance to the Golden Gate five or six miles west of the center of San Francisco, and giving to that breezy and joyous town a jolt which will live in history, but from which the fine-spirited people will recover long before the world at large will clearly understand what their experiences have really been. The rift reached the shore again at Mussel Rock to the southwest of San Francisco. Here the cliff was hurled down, a gradual incline was made a steep one and four thousand feet of newly graded railroad was thrown into the sea. It passed up the narrow valley of San Andreas, not harming the reservoir, but wrecking all the water mains entering San Francisco from the great reservoirs, Crystal Springs, San Andreas and Pilarcitos. The dam of the Crystal Springs reservoir, across the fault line, was so well built that the visible crack passed around it along the bank by its side, returning afterwards to its former direction. The bleak and boulder-strewn saddle called Cañada del Raymundo, scarred by previous earthquakes, was then passed, and beyond it the narrow, fertile valley, Portolá, named for the first governor of California, the discoverer of San Francisco Bay. The crack runs along the base of the Sierra Morena, four to five miles west of Stanford University, to the head of Portolá reservoir; then ascends in a cañon to a saddle on the summit, connecting two parallel ranges, Monte Bello to the east and

Castle Rock to the west. Down from the saddle between these runs Stevens Creek (Arroyo de San José de Cupertino) and down this creek went the earthquake crack, tearing up the road behind it, and throwing down landslides from every steep slope. Stevens Creek is made up from the union of two streams which meet from opposite directions. The crack descends the one and remounts impartially in the valley of the other. Both streams follow old earthquake tracks. Over another saddle the crack goes to Saratoga Creek. Across it and over another saddle it follows Campbell Creek, draining its reservoir. Thence it crosses obliquely the valley of Los Gatos Creek, over the hills of which Bret Harte wrote—

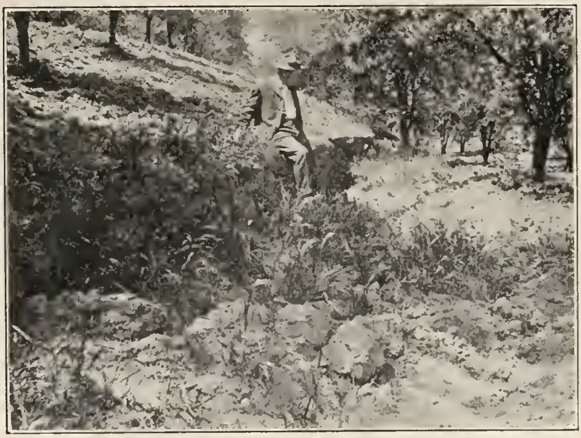
> The ridges 'round Los Gatos Creek
> Arched their spines in a feline fashion,

in the earthquake of 1818. Into this creek, from the Feely ranch,



Earthquake Rift, Morrill's Ranch, Skylands, Santa Cruz County.

some ten acres of land was thrown in a great landslide. At the head of the creek is the long tunnel which cuts under the saddle, from Wright's to Laurel. This tunnel has been the source of endless trouble since it was made, and for the reason that the rock in the mountain through which it passes is made up of minute chips of rock. No wonder, for the earthquake crack follows the mountain ridge, which is here narrow and low. It cuts tunnel and railroad track at right angles, and every earthquake disturbance is sure to make matters worse. Already forty feet of crushed rock has fallen from above what was the



Earthquake Rift, Morrill's Orchard, Santa Cruz County.

roof of the tunnel. On the hill above the tunnel is Morrill's fruit ranch. The earthquake ripped its way through the orchards, shifting the rows of trees six to eight feet and treating roads and fences in the same reckless fashion. The large hospitable Morrill farmhouse stood partly over the track and was split in two and utterly ruined. Farther on at Skylands, on the ridge of the mountains, Fern Gulch was filled with wreckage; redwood trees four and five feet through, two or three hundred years old, were snapped off like whip-lashes. The rift crossed Hinckley's Gulch at right angles. This is a narrow gorge about a hundred feet deep, in which stood the large Loma Prieta sawmill. The gorge was filled by landslips thrown from both sides. The mill was completely buried, with nine mill hands, and a redwood tree over a hundred feet high was set erect and unhurt over the place where the mill stood. The bodies of six men were recovered. One of these, the foreman, was found erect, smothered in mud, but standing with extended arms and limbs in the act of running from the mill. With him, equally erect and in the act of running, was the body of a Siberian mastiff. Their position marked the meeting point of the two walls of

Inverness Road, near Olema, Marin County.

Wreck of Loma Prieta Saw Mill, Hinckley's Gulch, Santa Cruz County.

the cañon. The crack went on across the hills, always in the same direction, southeast by south, till it came to the Chittenden Ranch in the Pájaro Valley. Here it tore off the hillside, destroying the highway at its base; then descended to the Pájaro River, shifting a pier of its railroad bridge about eighteen inches to the northwest. Here it met the Pájaro cross-fault. But here the straight line from Point Arena came to an end. A series of short breaks creeps off to the southeast, ending two miles southwest of San Juan, the last act being the final, almost complete, wreck of the beautiful and venerable Mission of San Juan Bautista.

That the oblique crack from Chittenden, famous as an 'earthquake ranch' of earlier times, to San Juan, is part of the original rift, is not

Site of Loma Prieta Saw Mill, covered to the depth of 125 feet.

clear. It may be that this is part of the Pájaro cross-fault. The original Portolá-Tomales fault, if continued in a straight line from Chittenden, would pass along the flanks or the foot of the Gavilan mountains to Priest Valley, fifty miles to the south-southeast. Beyond Priest Valley is a well-marked earthquake crack, which opened in the earthquake of 1868, and in earlier times. This extends through desert land in the same direction, its course being the axis of the Cholame Valley and the uninhabited desert sink known as Carisa Plain. This old rift extends at least one hundred and forty miles beyond Chittenden to Monte Pinos in the north edge of Ventura County. This whole fault from Point Arena to Monte Pinos is clearly a single break, but only 192 miles of a possible 330 were opened in the earthquake of 1906.

But while the surface break seemed to end at Chittenden, it seems probable that the rift in the rocks below extended much farther. At Priest Valley, fifty miles along this line, the shock was violent, while at localities ten miles or more to the east and west of the line, as at Lone Oak or the Pinnacles, it was very little felt. In Priest Valley chimneys and shelving were thrown down, buildings badly shaken and the contents of a country store impartially scattered over the floor, the shock being apparently about as severe as in San Francisco.

With the opening of the great rift it is conceivable that faults in the neighborhood should also be affected. There is evidence (most of which the writer has not examined) of the opening of a parallel fault behind Cape Mendocino. This seems to have passed across the base of the cape, cutting across the smaller headland called Point Delgada, losing itself in the Sonoma Valley to the southwest of Santa Rosa. There are distinct traces of it across Burbank's famous orchard at Sebastopol. Here on a slope lines of fruit trees were shifted, a well was moved bodily three or four feet, and a crack about one fourth mile long extended across a neighboring field, its direction parallel with that of the Tomales rift. Other similar cracks open at intervals on the road toward Point Delgada. The extreme violence of the shock in Santa Rosa perhaps indicates its nearness to this second rift, as the Tomales rift caused little damage in other towns equally far away. Dr. G. K. Gilbert takes a somewhat different view of this case. He seems to regard this Point Delgada crack as part of the great Tomales-Portolá rift. In his map (Popular Science Monthly, August) Dr. Gilbert marks the great rift as swinging eastward in a curve across Point Delgada and to the eastward of Cape Mendocino between that headland and Humboldt Bay. It seems to the present writer far more probable that the Point Delgada fault is a separate rift, parallel with the main rift, and similar to it, except that it is a little less violent. There is some evidence that a fault line at the foot of San Francisco

Bay opened for a short distance to the southward of Milpitas. But the soft soil in that region was filled with slumps and cracks due to the shaking down of loose deposits, and one could not be sure that the actual fault in the rocks was really disturbed. The same remark applies to the breaks at San Bruno about ten miles south of San Francisco in marsh deposits over the recognized San Bruno-Lake Merced fault. It is readily conceivable that a great disturbance like the one in the main fault might be accompanied by similar breaks in parallel or associated faults.

The chief center of disturbance in the earthquake of 1906 would seem to be in the sea. The evidence for this lies in the fact that at the point where the fault enters the land near Point Arena the displacement is greater than anywhere else. As the land fault is traceable for nearly two hundred miles to the southward, it is reasonable to suppose that the sea-bottom is broken for at least an equal distance to the northward. The point of earthquake disturbance off Cape Mendocino has been frequently noticed in the past, and this is in a right line with the rest of the fault. It is possible that the center of trouble is located in the valley between Cape Mendocino and the off-lying submarine mountain.

There is also another possibility, very remote perhaps, but still worth considering, that is, the connection between this rift and the disturbances about the islands of St. John Bogoslof, in Bering Sea. In the year 1768, to the north of the island of Umnak and about seventy miles northwest of Unalaska, a large island arose from the sea, in an earthquake disturbance. This island, Old Bogoslof, recorded by Captain Cook, was, as the present writer recalls it, about two miles long and fifteen hundred feet in height. In 1796, in another seismic disturbance, an addition was made to this island. In 1883 (October 28) a second island, of about the same size, arose, also from the sea, to the northward of the first. A photograph in my possession, taken in 1892 by Mr. N. B. Miller, of the steamer *Albatross*, shows this island, still hot and steaming. In 1896, when I visited it, it was apparently cold. These islands are supposably parts of the sea-bottom with a backing of melted rock, forced to the surface by pressure. On October 30, 1883, a severe earthquake was reported off Cape Mendocino. If on a globe one extends the axis of the Portolá-Tomales-San Andreas fault as far as Alaska, it would not fall far from these Bogoslof Islands. This fact suggested to the writer that possibly the earthquake of 1906 meant the birth of another Bogoslof. And it appears that this indeed was the case. The scanty reports which have reached us from the visit of the *Albatross* in May tell us that a third Bogoslof 'five times as large as either of the others' and between them, has arisen, and according to Captain Dirks, who reported the matter at Unalaska, the water was so hot that a vessel could not approach within five miles of the island. It is conceivable that the great earthquake rift has its center in the area of weakened sea-bottom occupied by the Bogoslofs. It is possible, even probable, that the coincidence of time does not show any real connection between Bogoslof and the earthquake of 1906. Against the connection may be urged the great distance, the great depth of most of the intervening sea, and the alleged facts that the seismograph at Sitka showed a shock from the south, while that at Tokyo indicated waves from the east. It is also stated that no shock was felt at St. Paul, St. George or Unalaska, but that a great shock was felt at Unimak. Unimak, like Unalaska, lies to the east of the supposed line of the fault. In any case, the birth of the third Bogoslof is a matter worthy of thorough investigation, and its approximate coincidence in time with the earthquake in California is very suggestive.

The earthquake of 1906 is receiving the most thorough study possible, and in such a way as to give promise of important practical results. The many previous earthquakes have been recorded, but their most essential feature, the location and extent of the causing fissure has rarely been indicated. In the records we read again and again that 'fissures opened in the ground,' but whether these were rifts in the crust or mere slumps of soft ground as a rule has escaped attention. The great earthquake of 1868 opened rifts at intervals from Tomales Bay to Carisa Plain, and also a fissure on the east side of San Francisco Bay, where a straight crack about ten miles long extended from Haywards toward the south. One side of this rift showed a lateral displacement of about four feet. To this short rift, rather than to the Portolá-Tomales fissure, the shock in San Francisco in 1868 may have been due. The shock in that year was more violent in Oakland than in San Francisco and most violent about San Leandro and Haywards, to the south of Oakland. It is conceivable that the shock of 1865, having its center in the Portolá fault, not far from San Francisco, gave that city a degree of immunity in 1868. Other destructive earthquakes, as recorded by Holden ('Catalogue of Earthquakes on the Pacific Coast, 1769 to 1897') are as follows:

1800. This earthquake was severe about San Juan Bautista, but whether in the Portolá fault or the Pájaro fault is not clear.

1812. This earthquake wrecked the mission of San Juan Capistrano in southern California, and was felt along the line of the southern missions. It had its center possibly in the Santa Catalina Channel.

1818. This earthquake injured the mission of Santa Clara; hence it may have been along the Portolá fault.

1836. This was said to be similar to the shock of 1868, its center along the Portolá line; 'great fissures were made in the earth.'

1839. This was severe from Redwood to San Francisco, 'a great fissure opened to Mission San José.' It was probably also in the Pájaro fault.

1857. Sacramento to Fort Tejón, San Bernardino and Fort Yuma. At Fort Tejón 'a fissure 20 feet wide and 40 miles long: the sides came together with such violence as to make a ridge ten feet wide and several feet high.' Fissures at San Bernardino.

1865. This was a smart shock from San Francisco to San José, apparently along the line of the Portolá fault. The severity of this earthquake, as suggested above, may have mitigated the local severity of the earthquake of 1868, which was in the same rift, but not so severe in this part of it.

1867. This was violent disturbance about Klamath Lake. A great crack said to have opened in Siskiyou County, but the locality is not recorded.

1868. A very severe earthquake, there being a rift on the east side of the bay, as also at Olema, in the Santa Cruz Mountains and for over a hundred miles from Cholame through the Carisa Plain.

1872. Owens River, Inyo County. Fissure at Big Pine 50 to 200 feet wide, 20 feet deep, extending 50 miles or more. Numerous shocks, very violent, these preceded by weaker shocks for a year or more.

1890. Mono Lake, similar disturbances.

1892. Vacaville, Winters, etc., extensive local disturbances, the fissures not traced, but said to have been along Rio de los Putos on the west side of the valley of Solano and Yolo.

1897. San Jacinto Valley, with a notable fissure, the details not at hand.

To these might be added the vigorous single jolt of 1893 in the San Fernando Mountains, which did little harm because occurring in an uninhabited region. The writer was at Saugus at the time, and noted the fall of trees and the flinging of rocks down the mountain-side. There seems to have been but a single wave, which would have done great injury in a populous district. It came from a short fissure in Pico Cañon.

Since the earthquake of 1906 many small earthquake waves have followed, evidently harmless details in the process of adjustment. Looking over Holden's record, we see that many small disturbances have taken place along the line of the great fault in question, besides the great earthquakes of 1868 and 1906 and the lesser ones of 1800, 1818, 1836, 1839, 1865 and 1868.

In 1808 there were twenty-one shocks at the Presidio of San Francisco. In 1812 the shocks caused a tidal wave in the bay extending up to the plaza. In 1813 or 1815 'all the buildings' in Santa Clara Valley were shaken down. There were not many and all these were of adobe or sun-dried brick. In 1851, a sharp shock in San Francisco. In 1852, a shock at San Francisco, with a fissure on the San Bruno fault, through which Lake Merced drained into the sea.

1853. Heavy shocks near Humboldt Bay.

1856. Severe shocks at San Francisco, the water in the bay sank two feet.

1863, 1864. A sharp shock at San Juan Bautista.

1890. Sharp shock along Portolá fault. The Pájaro bridge had a pier shifted 18 inches, as in 1906. The same crack opened at Chittenden, and the main arch in the Mission Church at San Juan Bautista was injured. A rift opened in the soil from Chittenden to San Juan as in 1906.

It is evident that minor disturbances occur along all the fault lines in California and that but one break comparable to that of 1906 has taken place within historic times in California. This was the earthquake of 1868. This was far less violent than that of 1906, along the San Francisco peninsula, although extending farther to the south than the other. It may be remembered that the population of the region is now much greater than in 1868, and in like manner, the possibilities of mischief on the part of earthquakes has been correspondingly increased. The danger from earthquakes itself is relatively a small matter, but it should be considered in the building arrangements of regions where such disturbances are likely to recur. It is as easy to make buildings virtually earthquake-proof as water-proof, unless standing directly over the fault itself. In this connection we may close with the pertinent words of the engineer, William H. Hall, of San Francisco: "The earthquake has put a definition on the word *sham,* which seems positively cruel. It has established a value on the solid foundation and genuine superstructure which is indeed ennobling."

It would redound to the moral and spiritual elevation of any community to be assured of a smart shock at intervals and of a real *temblor* once in each generation.

THE VALUE OF SCIENCE

By M. H. POINCARE

MEMBER OF THE INSTITUTE OF FRANCE

Chapter II. The Measure of Time

I.

So long as we do not go outside the domain of consciousness, the notion of time is relatively clear. Not only do we distinguish without difficulty present sensation from the remembrance of past sensations or the anticipation of future sensations, but we know perfectly well what we mean when we say that, of two conscious phenomena which we remember, one was anterior to the other; or that, of two foreseen conscious phenomena, one will be anterior to the other.

When we say that two conscious facts are simultaneous, we mean that they profoundly interpenetrate, so that analysis can not separate them without mutilating them.

The order in which we arrange conscious phenomena does not admit of any arbitrariness. It is imposed upon us and of it we can change nothing.

I have only a single observation to add. For an aggregate of sensations to have become a remembrance capable of classification in time, it must have ceased to be actual, we must have lost the sense of its infinite complexity, otherwise it would have remained present. It must, so to speak, have crystallized around a center of associations of ideas which will be a sort of label. It is only when they thus have lost all life that we can classify our memories in time as a botanist arranges dried flowers in his herbarium.

But these labels can only be finite in number. On that score, psychologic time should be discontinuous. Whence comes the feeling that between any two instants there are others? We arrange our recollections in time, but we know that there remain empty compartments. How could that be, if time were not a form preexistent in our mind? How could we know there were empty compartments, if these compartments were revealed to us only by their content?

II.

But that is not all; into this form we wish to put not only the phenomena of our own consciousness, but those of which other consciousnesses are the theater. But more, we wish to put there physical facts, these *I know not what* with which we people space and which no consciousness sees directly. This is necessary because without it science could not exist. In a word, psychologic time is given to us and must needs create scientific and physical time. There the difficulty begins, or rather the difficulties, for there are two.

Think of two consciousnesses, which are like two worlds impenetrable one to the other. By what do we strive to put them into the same mold, to measure them by the same standard? Is it not as if one strove to measure length with a gram or weight with a meter? And besides, why do we speak of measuring? We know perhaps that some fact is anterior to some other, but not *by how much* it is anterior.

Therefore two difficulties: (1) Can we transform psychologic time, which is qualitative, into a quantitative time? (2) Can we reduce to one and the same measure facts which transpire in different worlds?

III.

The first difficulty has long been noticed; it has been the subject of long discussions and one may say the question is settled. *We have not a direct intuition of the equality of two intervals of time.* The persons who believe they possess this intuition are dupes of an illusion. When I say, from noon to one the same time passes as from two to three, what meaning has this affirmation?

The least reflection shows that by itself it has none at all. It will only have that which I choose to give it, by a definition which will certainly possess a certain degree of arbitrariness. Psychologists could have done without this definition; physicists and astronomers could not; let us see how they have managed.

To measure time they use the pendulum and they suppose by definition that all the beats of this pendulum are of equal duration. But this is only a first approximation; the temperature, the resistance of the air, the barometric pressure, make the pace of the pendulum vary. If we could escape these sources of error, we should obtain a much closer approximation, but it would still be only an approximation. New causes, hitherto neglected, electric, magnetic or others, would introduce minute perturbations.

In fact, the best chronometers must be corrected from time to time, and the corrections are made by the aid of astronomic observations; arrangements are made so that the sidereal clock marks the same hour when the same star passes the meridian. In other words, it is the sidereal day, that is, the duration of the rotation of the earth, which is the constant unit of time. It is supposed, by a new definition substituted for that based on the beats of the pendulum, that two complete rotations of the earth about its axis have the same duration.

However, the astronomers are still not content with this definition. Many of them think that the tides act as a check on our globe, and that the rotation of the earth is becoming slower and slower. Thus would be explained the apparent acceleration of the motion of the moon, which would seem to be going more rapidly than theory permits because our watch, which is the earth, is going slow.

IV.

All this is unimportant, one will say; doubtless our instruments of measurement are imperfect, but it suffices that we can conceive a perfect instrument. This ideal can not be reached, but it is enough to have conceived it and so to have put rigor into the definition of the unit of time.

The trouble is that there is no rigor in the definition. When we use the pendulum to measure time, what postulate do we implicitly admit? *It is that the duration of two identical phenomena is the same;* or, if you prefer, that the same causes take the same time to produce the same effects.

And at first blush, this is a good definition of the equality of two durations. But take care. Is it impossible that experiment may some day contradict our postulate?

Let me explain myself. I suppose that at a certain place in the world the phenomenon α happens, causing as consequence at the end of a certain time the effect α'. At another place in the world very far away from the first, happens the phenomenon β, which causes as consequence the effect β'. The phenomena α and β are simultaneous, as are also the effects α' and β'.

Later, the phenomenon α is reproduced under approximately the same conditions as before, and *simultaneously* the phenomenon β is also reproduced at a very distant place in the world and almost under the same circumstances. The effects α' and β' also take place. Let us suppose that the effect α' happens perceptibly before the effect β'.

If experience made us witness such a sight, our postulate would be contradicted. For experience would tell us that the first duration $\alpha\alpha'$ is equal to the first duration $\beta\beta'$ and that the second duration $\alpha\alpha'$ is less than the second duration $\beta\beta'$. On the other hand, our postulate would require that the two durations $\alpha\alpha'$ should be equal to each other, as likewise the two durations $\beta\beta'$. The equality and the inequality deduced from experience would be incompatible with the two equalities deduced from the postulate.

Now can we affirm that the hypotheses I have just made are absurd? They are in no wise contrary to the principle of contradiction. Doubtless they could not happen without the principle of sufficient reason seeming violated. But to justify a definition so fundamental I should prefer some other guarantee.

V.

But that is not all. In physical reality one cause does not produce a given effect, but a multitude of distinct causes contribute to produce it, without our having any means of discriminating the part of each of them.

Physicists seek to make this distinction; but they make it only approximately, and, however they progress, they never will make it except approximately. It is approximately true that the motion of the pendulum is due solely to the earth's attraction; but in all rigor every attraction, even of Sirius, acts on the pendulum.

Under these conditions, it is clear that the causes which have produced a certain effect will never be reproduced except approximately. Then we should modify our postulate and our definition. Instead of saying: 'The same causes take the same time to produce the same effects,' we should say: 'Causes almost identical take almost the same time to produce almost the same effects.'

Our definition therefore is no longer anything but approximate. Besides, as M. Calinon very justly remarks in a recent memoir:[2]

> One of the circumstances of any phenomenon is the velocity of the earth's rotation; if this velocity of rotation varies, it constitutes in the reproduction of this phenomenon a circumstance which no longer remains the same. But to suppose this velocity of rotation constant is to suppose that we know how to measure time.

Our definition is therefore not yet satisfactory; it is certainly not that which the astronomers of whom I spoke above implicitly adopt, when they affirm that the terrestrial rotation is slowing down.

What meaning according to them has this affirmation? We can only understand it by analyzing the proofs they give of their proposition. They say first that the friction of the tides producing heat must destroy *vis viva.* They invoke therefore the principle of *vis viva,* or of the conservation of energy.

They say next that the secular acceleration of the moon, calculated according to Newton's law, would be less than that deduced from observations unless the correction relative to the slowing down of the terrestrial rotation were made. They invoke therefore Newton's law. In other words, they define duration in the following way: time should be so defined that Newton's law and that of *vis viva* may be verified. Newton's law is an experimental truth; as such it is only approximate, which shows that we still have only a definition by approximation.

If now it be supposed that another way of measuring time is adopted, the experiments on which Newton's law is founded would none the less have the same meaning. Only the enunciation of the law would be different, because it would be translated into another

[2] 'Etude sur les diverses grandeurs,' Paris, Gauthier-Villars, 1897.

language; it would evidently be much less simple. So that the definition implicitly adopted by the astronomers may be summed up thus: Time should be so defined that the equations of mechanics may be as simple as possible. In other words, there is not one way of measuring time more true than another; that which is generally adopted is only more *convenient*. Of two watches, we have no right to say that the one goes true, the other wrong; we can only say that it is advantageous to conform to the indications of the first.

The difficulty which has just occupied us has been, as I have said, often pointed out; among the most recent works in which it is considered, I may mention, besides M. Calinon's little book, the treatise on mechanics of M. Andrade.

VI.

The second difficulty has up to the present attracted much less attention; yet it is altogether analogous to the preceding; and even, logically, I should have spoken of it first.

Two psychological phenomena happen in two different consciousnesses; when I say they are simultaneous, what do I mean? When I say that a physical phenomenon, which happens outside of every consciousness, is before or after a psychological phenomenon. what do I mean?

In 1572, Tycho Brahe noticed in the heavens a new star. An immense conflagration had happened in some far distant heavenly body; but it had happened long before; at least two hundred years were necessary for the light from that star to reach our earth. This conflagration therefore happened before the discovery of America. Well, when considering this gigantic phenomenon, which perhaps had no witness, since the satellites of that star were perhaps uninhabited, I say this phenomenon is anterior to the formation of the visual image of the isle of Española in the consciousness of Christopher Columbus, what do I mean?

A little reflection is sufficient to understand that all these affirmations have by themselves no meaning. They can have one only as the outcome of a convention.

VII.

We should first ask ourselves how one could have had the idea of putting into the same frame so many worlds impenetrable to each other. We should like to represent to ourselves the external universe, and only by so doing could we feel that we understood it. We know we never can attain this representation: our weakness is too great. But at least we desire the ability to conceive an infinite intelligence for which this representation would be possible, a sort of great consciousness which should see all, and which should classify all *in its time,* as we classify, *in our time,* the little we see.

This hypothesis is indeed crude and incomplete, because this supreme intelligence would be only a demigod; infinite in one sense, it would be limited in another, since it would have only an imperfect recollection of the past; and it could have no other, since otherwise all recollections would be equally present to it and for it there would be no time. And yet when we speak of time, for all which happens outside of us, do we not unconsciously adopt this hypothesis; do we not put ourselves in the place of this imperfect god; and do not even the atheists put themselves in the place where god would be if he existed?

What I have just said shows us, perhaps, why we have tried to put all physical phenomena into the same frame. But that can not pass for a definition of simultaneity, since this hypothetical intelligence, even if it existed, would be for us impenetrable. It is therefore necessary to seek something else.

VIII.

The ordinary definitions which are proper for psychologic time would suffice us no better. Two simultaneous psychologic facts are so closely bound together that analysis can not separate without mutilating them. Is it the same with two physical facts? Is not my present nearer my past of yesterday than the present of Sirius?

It has also been said that two facts should be regarded as simultaneous when the order of their succession may be inverted at will. It is evident that this definition would not suit two physical facts which happen far from one another, and that, in what concerns them, we no longer even understand what this reversibility would be; besides, succession itself must first be defined.

IX.

Let us then seek to give an account of what is understood by simultaneity or antecedence, and for this let us analyze some examples.

I write a letter; it is afterward read by the friend to whom I have addressed it. There are two facts which have had for their theater two different consciousnesses. In writing this letter I have had the visual image of it, and my friend has had in his turn this same visual image in reading the letter. Though these two facts happen in impenetrable worlds, I do not hesitate to regard the first as anterior to the second, because I believe it is its cause.

I hear thunder, and I conclude there has been an electric discharge; I do not hesitate to consider the physical phenomenon as anterior to the auditory image perceived in my consciousness, because I believe it is its cause.

Behold then the rule we follow, and the only one we can follow:

when a phenomenon appears to us as the cause of another, we regard it as anterior. It is therefore by cause that we define time; but most often, when two facts appear to us bound by a constant relation, how do we recognize which is the cause and which the effect? We assume that the anterior fact, the antecedent, is the cause of the other, of the consequent. It is then by time that we define cause. How save ourselves from this *petitio principii?*

We say now *post hoc, ergo propter hoc;* now *propter hoc, ergo post hoc;* shall we escape from this vicious circle?

X.

Let us see, not how we succeed in escaping, for we do not completely succeed, but how we try to escape.

I execute a voluntary act A and I feel afterward a sensation D, which I regard as a consequence of the act A; on the other hand, for whatever reason, I infer that this consequence is not immediate, but that outside my consciousness two facts B and C, which I have not witnessed, have happened, and in such a way that B is the effect of A, that C is the effect of B, and D of C.

But why? If I think I have reason to regard the four facts A, B, C, D, as bound to one another by a causal connection, why range them in the causal order $A\ B\ C\ D$, and at the same time in the chronologic order $A\ B\ C\ D$, rather than in any other order?

I clearly see that in the act A I have the feeling of having been active, while in undergoing the sensation D, I have that of having been passive. This is why I regard A as the initial cause and D as the ultimate effect; this is why I put A at the beginning of the chain and D at the end; but why put B before C rather than C before B?

If this question is put, the reply ordinarily is: we know that it is B which is the cause of C because we *always* see B happen before C. These two phenomena, when witnessed, happen in a certain order; when analogous phenomena happen without witness, there is no reason to invert this order.

Doubtless, but take care; we never know directly the physical phenomena B and C. What we know are sensations B' and C' produced respectively by B and C. Our consciousness tells us immediately that B' precedes C' and we *suppose* that B and C succeed one another in the same order.

This rule appears in fact very natural, and yet we are often led to depart from it. We hear the sound of the thunder only some seconds after the electric discharge of the cloud. Of two flashes of lightning, the one distant, the other near, can not the first be anterior to the second, even though the sound of the second comes to us before that of the first?

XI.

Another difficulty; have we really the right to speak of the cause of a phenomenon? If all the parts of the universe are interchained in a certain measure, any one phenomenon will not be the effect of a single cause, but the resultant of causes infinitely numerous; it is, one often says, the consequence of the state of the universe a moment before. How enunciate rules applicable to circumstances so complex? And yet it is only thus that these rules can be general and rigorous.

Not to lose ourselves in this infinite complexity let us make a simpler hypothesis. Consider three stars, for example, the sun, Jupiter and Saturn; but, for greater simplicity, regard them as reduced to material points and isolated from the rest of the world. The positions and the velocities of three bodies at a given instant suffice to determine their positions and velocities at the following instant, and consequently at any instant. Their positions at the instant t determine their positions at the instant $t + h$ as well as their positions at the instant $t — h$.

Even more; the position of Jupiter at the instant t, together with that of Saturn at the instant $t + a$, determines the position of Jupiter at any instant and that of Saturn at any instant.

The aggregate of positions occupied by Jupiter at the instant $t + e$ and Saturn at the instant $t + a + e$ is bound to the aggregate of positions occupied by Jupiter at the instant t and Saturn at the instant $t + a$, by laws as precise as that of Newton, though more complicated. Then why not regard one of these aggregates as the cause of the other, which would lead to considering as simultaneous the instant t of Jupiter and the instant $t + a$ of Saturn?

In answer there can only be reasons, very strong, it is true, of convenience and simplicity.

XII.

But let us pass to examples less artificial; to understand the definition implicitly supposed by the savants, let us watch them at work and look for the rules by which they investigate simultaneity.

I will take two simple examples, the measurement of the velocity of light and the determination of longitude.

When an astronomer tells me that some stellar phenomenon, which his telescope reveals to him at this moment, happened nevertheless fifty years ago, I seek his meaning, and to that end I shall ask him first how he knows it, that is, how he has measured the velocity of light.

He has begun by *supposing* that light has a constant velocity, and in particular that its velocity is the same in all directions. That is a postulate without which no measurement of this velocity could be attempted. This postulate could never be verified directly by experiment; it might be contradicted by it if the results of different measurements were not concordant. We should think ourselves fortunate that this contradiction has not happened and that the slight discordances which may happen can be readily explained.

The postulate, at all events, resembling the principle of sufficient reason, has been accepted by everybody; what I wish to emphasize is that it furnishes us with a new rule for the investigation of simultaneity, entirely different from that which we have enunciated above.

This postulate assumed, let us see how the velocity of light has been measured. You know that Roemer used eclipses of the satellites of Jupiter, and sought how much the event fell behind its prediction. But how is this prediction made? It is by the aid of astronomic laws, for instance Newton's law.

Could not the observed facts be just as well explained if we attributed to the velocity of light a little different value from that adopted, and supposed Newton's law only approximate? Only this would lead to replacing Newton's law by another more complicated. So for the velocity of light a value is adopted, such that the astronomic laws compatible with this value may be as simple as possible. When navigators or geographers determine a longitude, they have to solve just the problem we are discussing; they must, without being at Paris, calculate Paris time. How do they accomplish it? They carry a chronometer set for Paris. The qualitative problem of simultaneity is made to depend upon the quantitative problem of the measurement of time. I need not take up the difficulties relative to this latter problem, since above I have emphasized them at length.

Or else they observe an astronomic phenomenon, such as an eclipse of the moon, and they suppose that this phenomenon is perceived simultaneously from all points of the earth. That is not altogether true, since the propagation of light is not instantaneous; if absolute exactitude were desired, there would be a correction to make according to a complicated rule.

Or else finally they use the telegraph. It is clear first that the reception of the signal at Berlin, for instance, is after the sending of this same signal from Paris. This is the rule of cause and effect analyzed above. But how much after? In general, the duration of the transmission is neglected and the two events are regarded as simultaneous. But, to be rigorous, a little correction would still have to be made by a complicated calculation; in practise it is not made, because it would be well within the errors of observation; its theoretic necessity is none the less from our point of view, which is that of a rigorous definition. From this discussion, I wish to emphasize two things: (1) The rules applied are exceedingly various. (2) It is difficult to separate the qualitative problem of simultaneity from the quantitative problem of the measurement of time; no matter whether a chronometer is used, or whether account must be taken of a velocity of transmission, as that of light, because such a velocity could not be measured without *measuring* a time.

XIII

To conclude: We have not a direct intuition of simultaneity, nor of the equality of two durations. If we think we have this intuition, this is an illusion. We replace it by the aid of certain rules which we apply almost always without taking count of them.

But what is the nature of these rules? No general rule, no rigorous rule; a multitude of little rules applicable to each particular case.

These rules are not imposed upon us and we might amuse ourselves in inventing others; but they could not be cast aside without greatly complicating the enunciation of the laws of physics, mechanics and astronomy.

We therefore choose these rules, not because they are true, but because they are the most convenient, and we may recapitulate them as follows: "The simultaneity of two events, or the order of their succession, the equality of two durations, are to be so defined that the enunciation of the natural laws may be as simple as possible. In other words, all these rules, all these definitions are only the fruit of an unconscious opportunism."

(*To be continued.*)

FRANCIS BACON AND THE MODERN UNIVERSITY

By Professor EDWARD O. SISSON

UNIVERSITY OF WASHINGTON, SEATTLE

THE student of the history of education marvels at the tenacity with which the aims and methods of scholasticism and the middle ages maintained their hold upon the universities. The use of Latin as exclusive means of communication and the worship of Aristotle as source of final authority upon all questions marked the universities as medieval long after the world at large had moulted its chrysalis and become modern. At the opening of the nineteenth century only traces of advancement were perceptible; fortunately that century has seen changes nothing short of revolutionary in the ideals and methods of the institutions of higher learning.

It is a commonplace that Francis Bacon was the herald, if not the originator, of that scientific method which more than anything else has wrought the reform of the university; but the work went on for two centuries outside the sacred limits of the universities, and, as we have seen, gained access to them only in the third century after Bacon's time. And yet we hope to show that he foresaw and described accurately the essential changes needed to fit these institutions for their true work.

One naturally thinks first in this connection of the New Atlantis, and the 'Salomon's House' described therein, which was 'the lantern of the kingdom—and dedicated to the study of the works and creatures of God'; and no one can fail to perceive in this fantasy of Bacon's imagination striking hints of modern scientific investigation and of that organization and cooperation which are so essential a means of progress in knowledge. But for our present purpose better material is found in a serious discussion of the needs of higher education and scientific research in the first chapter of Book II. of the 'Advancement of Learning.' Bacon proposes here to set forth the 'direction, or the pointing out and delineation of the direct way to the completion of the object in view,' that is, of the advancement of knowledge. The lines of progress he advocates are six in number; let us examine them briefly in order.

First, "among so many illustrious colleges in Europe, all the foundations are engrossed by the professions, none being left for the free cultivation of the arts and sciences. . . . There is no collegiate course so free as to allow those who are inclined to devote themselves to history, modern languages, civil policy and general literature; . . ." With this compare President Eliot's 'What is a Liberal Education,' written in 1884; President Eliot names as subjects entitled to full admission into higher culture these five: English literature, French and German, history, political economy and natural science. The first four are practically identical with Bacon's list; and the last, natural science, is abundantly championed by Bacon in discussing what he names as the third and sixth defects of the existing system. What Bacon, as intellectual seer, prescribed, Eliot, as foremost actor in university reform in America at least, confirms and urges. Moreover, the fulfilment of Bacon's word is the more wonderful in that for two centuries after he wrote, almost no movement occurred in the dry bones of the traditional system, and that within the limits the third hundred years the five subjects in question have conquered their rightful place in higher education.

The second defect which Bacon saw in the institutions of his own day is one which will appeal at once to those who even in this better age earn their bread by service in the armies of science and learning—'the mean salaries apportioned to public lectureships, whether in the sciences or the arts.' Even in this matter all must admit that progress has been made since Bacon's time; and all will agree that Bacon was right in pointing to higher compensation of the scientific laborer as one of the indispensable conditions of large and general progress in the work. It is safe to say that the economic condition of the individual worker in these fields is far better to-day than it was in the sixteenth century; and the total sum applied to the advancement of learning, and especially to those very branches that Bacon so much advocated, is immeasurably vaster than ever before in the history of the world, and yearly increasing.

"The next deficiency we shall notice," says Bacon, "is the want of philosophical instruments. . . . To study natural philosophy, physic and many other sciences to advantage, books are not the only essentials—other instruments are required." Bacon goes on to mention what has already been done in this direction—the use of spheres, globes, astrolabes, maps; and the provision of 'gardens for the growth of simples' and dead bodies for dissection, for schools of medicine. But what has been done is entirely inadequate; 'there will be no inroad made into the secrets of nature unless experiments, be they of Vulcan or Daedalus (air ships?), furnace, engine or any other kind, are allowed for; . . . you must allow the spies and intelligencers of nature to bring in their bills, or else you will be ignorant of many things worthy to be known.' With what joy would the writer of this have beheld the laboratories of a modern university; how he would have glowed with enthusiasm over experiment stations and a Carnegie Institution!

The fourth point is more distinctly educational: 'neglect of proper supervision or diligent inquiry into the course of studies, with a view to a thorough reformation of such parts as are ill-suited to the age, or of unwise institution.' Bacon gives two specimens of faults in the existing course of study: first, that scholars are inducted too early into logic and rhetoric; and second, that invention and memory are not exercised together. These are evidently mentioned rather to indicate the kind of reforms which Bacon here intends, than for any peculiar importance attaching to them. Perhaps nothing is more characteristic of modern education than just this 'diligent inquiry into the course of study' which Bacon recommends; not indeed that we may yet count ourselves to have attained to perfection in the matter of actual reformation, nor to have yet cast off all that was fit only for the 'obscure times' in which our curricula were first formed. But throughout the civilized world those responsible for the training of the young—teachers, parents and statesmen—are giving themselves with resolute purpose to the discovery and ordering of the best means of education, for all ages from infancy to maturity.

The next defect is 'the little sympathy and correspondence which exists between colleges and universities, as well throughout Europe as in the same state and kingdom.' Evidently the possession of a common academic language did not insure complete academic harmony and cooperation! One can not doubt that Bacon would have seen many fulfilments of this desire of his in modern university life: learned societies, associations of colleges and schools, conferences, philosophical and scientific journals, scientific congresses at international expositions, and the like. Perhaps the ceremonies of inducting a president of a university into his office would have seemed to him particularly to show forth 'a fraternity of learning and illumination, relating to that paternity which is attributed to God, who is called the Father of lights.'

"Lastly," says Bacon, "I may lament that no fit men have been engaged to forward those sciences which yet remain in an unfinished state." Lastly, indeed, but not least; rather may we believe that this forwarding of the unfinished sciences lay nearest of all to the heart of the author of the Instauration Magna, a work undertaken, he tells us, to 'perform, as it were, a lustrum of the sciences, and to take account of what have been prosecuted and what omitted.' None of Bacon's admonitions have been more fully heeded by the universities most worthy of the name; indeed it has come to be a mark of a genuine university that its teachers should be all 'fit men to forward the unfinished sciences'—in other words, investigators. How vast has been the actual progress in the lustrum of the sciences may perhaps best be felt by comparing the strange and antiquated terms in the outline of Books III. and IV. of the Advancement, where Bacon catalogues the sciences, with the list of departments of study in some great university of to-day. The sciences of which Bacon knew have been advanced to a place far beyond the highest imaginings of even his great mind in that time; and new regions of knowledge have been opened of which he could not dream. Even more significant is the fact that we have given up believing in even the possibility of a *finished* science; all are unfinished, and therefore it is the duty of every devotee to labor for advance. The universities, after centuries of inertia, have at last waked, or rather been somewhat vigorously aroused, to their duty to be creators as well as conservators of the store of knowledge.

Thus when the sixteenth century had barely closed, Bacon pointed out these six ways in which higher education and research needed to advance: a more liberal curriculum, more generous financial support, better and more abundant apparatus for experiment and investigation, a rational organization of the course of study, sympathy and cooperation between all colleges and universities, and the prosecution of the 'unfinished sciences.' History has wonderfully justified his verdict, and we may well pronounce him the prophet of the modern university.

BRITICISMS VERSUS AMERICANISMS

By Professor EDWIN W. BOWEN
RANDOLPH-MACON COLLEGE

IT is a recognized fact that there is a considerable variation in the English language as spoken by the two great branches of the Anglo-Saxon race. The English people differ from the American people in the use of our common speech not only in their characteristic mode of pronunciation and orthography, but they also differ from us in no less striking a manner in the use of certain idioms and household phrases, which constitute the small change of our every-day speech. This difference is the natural outgrowth of the separation of the two peoples by the estranging ocean, which is of necessity a great barrier to complete intercourse. To be sure, the fact that the English people and the American people have distinct national entities with the resulting difference, during the last hundred years, of national ideals and pursuits, has had the natural and inevitable effect of widening the breach between the speech of the two countries. No doubt the present variation will be accentuated more and more as the years go by, and the language of Great Britain and of America, far from becoming absolutely identical in pronunciation and idiom with the flight of centuries, will go on developing with an ever-increasing divergence from the common standard. If this be true—and certainly the facts as to the present tendency seem to warrant such a conclusion—the final result may be the unique linguistic phenomenon of two separate and distinct English tongues, if such a thing be not an impossibility.

Before pointing out the variations of our American English from British English, it may be interesting to note the source of our American vernacular, and the contributing causes of the chief variations from the authoritative standard of the mother country.

When our Saxon forefathers found their way to the shores of this western continent and here established their permanent abode, the settlers naturally brought with them the language of their native country. This was, of course, the noble tongue of Shakespeare and Milton. Our British cousins who criticize our English so freely and cast reproach upon it as if it were a mere jargon, a barbarous *patois,* evidently lose sight of the fact that it boasts the same high pedigree as their own much-vaunted Elizabethan speech. When the English language was first transplanted in American soil, it was identical in orthography, orthoepy and idiom with the speech of the mother country. But the transplanted tongue, having a new and different habitat, began at once to adapt itself, however imperceptibly, to its changed environ and new conditions. Nor was the connection with the parent stock a sufficiently close and vital bond of union to prevent the English speech on American lips from undergoing at least some slight modification in the course of time, as a natural consequence of the altered conditions in the new world.

It is a well-established linguistic principle that a language inevitably undergoes a slight change, determined by the varying conditions, as long as it is spoken. When a tongue ceases to be spoken, then and only then does it cease to change and become a dead language, as, for instance, Latin and Greek. This fact of the gradual change in a living language is demonstrated through the difficulty one experiences in understanding the English of Chaucer, or even of Shakespeare, for the matter of that, although he is not so far removed from the present age. If a living tongue underwent no alteration with the lapse of years, then why should not Anglo-Saxon be as readily intelligible to us as modern English?

Furthermore, a language is affected in its development by contact with a foreign tongue and by outside influences, such as the climate. The first of these reasons is so apparent to all that it hardly deserves comment. But not so the second. Yet the influence of climate on a living language is very fruitful of change. Ready proof of this is furnished in our own country in the soft, musical utterance of the south in contrast with the rather shrill and forceful habits of enunciation characteristic of the north. In Europe, for example, the vast preponderance of the harsh, guttural character of the German tongue offers a glaring contrast to the smooth, liquid notes of the pure Tuscan speech. This is the reason why Italian appeals so strongly to music lovers and to all who have an ear trained to be especially sensitive to sound. Now, this difference between German and Italian, as respects the musical character of the two languages, is doubtless to be explained in large measure as the result of climate conditions extending through many long centuries. If by some violent political upheaval the Italians were transported to the extreme northern part of Europe, it is altogether probable that their speech in the course of centuries would lose much of its native vocalic development, much of its melody, and become harsh and strident, somewhat like the Russian language. It follows, therefore, that the English speech on American soil has undergone some slight modification, in consequence of climatic influence. Perhaps this explains the variation of the American pronunciation of the long *o*-sound as in 'stone' and 'bone' from the British norm. But the difference in climate between the two countries is not sufficiently marked to produce any very radical departure.

A striking feature of the English speech on American lips is the leveling of the long *a*-sound heard in such words as 'past,' 'fast,' 'plant,' 'command,' 'dance,' 'path,' etc. This could hardly be the result of climatic influence, however, for it does not appear that the climate has had the effect of producing any modification in the pronunciation of such terms in any part of America. The prevailing pronunciation of these terms is the same, at the south and at the north alike. Such a variation must, therefore, be inherent in the natural growth of the English language on American soil. For it must be borne in mind that just as the English speech, as any other living organism, has been growing and developing during the centuries in England, so, likewise, in America it has been growing and developing during the last three centuries, but not necessarily in the same manner. Those employing the language in Great Britain and in the United States are no longer a homogeneous people with the same national ideals and destiny. On the contrary, they are two separate and distinct nations with different forms of government and with different aims and aspirations. Add to this the fact that the nations have been estranged by political differences which resulted in wars and that they are separated by the physical barrier of a vast ocean. In the face of these obstacles it is not at all surprising that the English speech has not gone on developing *pari passu* on both sides of the Atlantic. The wonder is that the present variations are not really greater and more striking than they are.

Another contributing cause of variation of American English from the British norm must not be overlooked, the more especially as it has proved a prolific factor. In our new country some conditions of life arose which were totally unlike those existing in the old country. Such strange conditions called imperatively for the invention of new names and thus gave rise to the employment of new phrases and new locutions. These had to be coined immediately for the emergency. Since the most distinctive traits of the American are initiative and wealth of resource, no time was lost in making such additions to the English speech as seemed to supply a felt need, and that, too, without any special reference to British models and precedents. Hence a large class of terms distinctively American and bearing upon their face the trade-mark 'made in America' found their way into the English vocabulary on this side of the Atlantic, much to the disgust of the British precisians and purists, who proceeded forthwith to put these new coinages under the ban and to brand them with the bend sinister of 'Americanism.' Of this class are many terms indicating mechanical inventions and appliances, such as 'elevator' instead of the British 'lift,' to mention only a single example of a long catalogue of useful things which American genius has given to the world. Here also belong numerous words expressing things associated with modern transportation and rapid transit, such as 'street-car,' 'railroad,' etc.

Perhaps it may be well just here to call attention to some of the ordinary terms and expressions heard in England which strike an American as being quite odd and peculiar. It is to be presumed that the good Britons will not be offended if we, using the same license as themselves, venture to call such expressions 'Briticisms.' Let it be distinctly understood, however, that this is not intended as an opprobrious epithet, but only to signify a word or an idiom which is peculiar to Great Britain and not familiar in America. For surely the English people have the right to employ whatever terms they may choose both in their colloquial and in their written speech.

If an American in London wishes to use a language that is readily understood, when he goes to the ticket office he must call it the booking office of the railway station. There he must ask the clerk, or rather the 'clark,' for a first single or a second return, instead of a single fare (first class) and a round trip (second class). He must then have his luggage labeled, not his baggage checked, and, having secured his brasses or labels, not his checks, he sees his box, not his trunk, put in the proper van and then takes his seat in the carriage, not in the car. Before the train starts off, the guards slam the doors of the carriages, turning the handles, and at the conductor's whistle the engine-driver starts his locomotive-engine. The points all being set for a clear track ahead, the train speeds along the metals, passing perhaps a shunting-engine about the station and a train of goods-vans.

The variation of British from American usage is not more noteworthy in railway parlance than in other circles. If an American goes shopping in London, he must call for a packet, not a paper, of pins; a reel, not a spool, of cotton. If he desires to buy a pair of shoes, he must call for boots, unless he wishes low quarters or Oxford ties; if a pair of overshoes, he must ask for footholds or galoshes; if a soft felt hat, he must ask for a squash hat, or if he prefers a Derby, he must ask for a billy-cock hat or a bowler; if he wishes a pad of paper, he should request a block of paper. If he goes to a restaurant, he indicates whether he desires his meat underdone, not rare; if he wishes corned beef, he calls for silversides of beef; if beets, he calls for beetroot; if chicken, he calls for fowl; if a cereal of any sort, he calls for corn; if cold bread, he must order cut bread; and if he desires pudding, pie, jam, preserves or candy, he must order sweets, short for sweetmeats. If the waiter should fail for any reason to give him a napkin, an American should ask for a serviette; and when he has finished his repast, he is handed a bill which he may pay with his cheque, or, if he prefers, with the cash from his purse, not his pocket-book.

If in England you find no bowl and pitcher in your room, you are expected to call for a jug and basin, since there a pitcher means only a little jug and a bowl is used exclusively for serving food in. On the street, instead of a letter box near a lamp post, you see a pillar box near a lamp pillar, and you perhaps meet a person pushing a perambulator, called 'pram' for short, instead of a baby-carriage. For dry-goods you go to a mercer's, where you will find white calico sold for muslin. For cloth you go to a draper's, for wooden ware to a turnery, for hardware to an ironmonger's, for milk, butter and eggs to a cow-keeper's or a dairy, and for fish, game and poultry to a fish shop. If you desire any of your purchases sent to your address, you order them sent by express-carrier, carriage paid.

If at any time you desire the services of a scrub-woman to clean your apartments, you send for a charwoman. If you wish to have some furniture upholstered, you request the upholder to undertake the work for you. If you need the services of a doctor, you call in a medical man. You must be careful to address surgeons and dentists by the common democratic title 'mister,' since the English custom does not warrant you to address them as 'doctor.' If you are well, to your inquiring friends you are reported 'fit,' if unwell, 'seedy,' if sick, invariably 'ill.'

To an American ear British orthoepy offers quite as noteworthy surprises as the idiomatic diction does. Of course it is to be presumed that there should be more or less marked variations in the matter of habitual utterance of certain sounds, especially the long *o*- and the long *a*-vowel, as in 'fast,' 'dance,' 'sha'n't,' etc., which are at striking variance with American usage. Indeed, these sounds are so characteristic that, like the English custom of ending almost every sentence with a question, when clearly natural and not an affectation, they serve as a shibboleth of British nativity. But notable eccentricities are to be observed in the English mode of pronouncing many proper names such as Derby, pronounced 'darby'; Berkeley, pronounced 'barclay'; Magdalen, pronounced 'maudlin'; Cadogan, pronounced 'kerduggan'; Marylebone, pronounced 'merrybone'; Cholmondeley, pronounced 'chumly'; Marlborough, pronounced 'mobrer'; Albany, pronounced so that the first syllable rhymes with Al- in Alfred, etc. It is unnecessary to multiply examples. Suffice it to say that there is a large class of these words the spelling and pronunciation of which seem to an American rather curiously divorced. Certainly American usage offers no parallel where there is so complete a divorce of orthoepy from orthography. American usage makes for phonetic spelling and tends to make the conventional pronunciation and spelling conform somewhat, at least.

Having drawn attention to a few Briticisms, we are now prepared to discuss some of our Americanisms which seem to excite in the pure minds of the English precisians alternate feelings of disgust and indignation. Let it be premised, however, that it is not proposed to include ordinary slang in the present discussion. It must be admitted that too much slang is employed even in polite circles, not to mention the speech of those who make no pretense to refinement and culture. But one should not confuse vulgarisms with so-called Americanisms, just as one should not confuse vulgarisms with legitimate slang. The discriminating student distinguishes between ordinary slang and legitimate slang. The vulgar slang of the street is, of course, to be universally condemned and tabooed. Legitimate slang, on the contrary, performs an important function in the development of a living language. It is not to be inconsiderately ostracized, therefore, and put under the ban as the chief source of corruption of our vernacular, as certain of our purists, in their zeal without knowledge, tell us and attempt to maintain. It is idle for them in their self-appointed rôle of guardian of the pristine purity of the English tongue to endeavor to defend so unsound and so indefensible a thesis. For legitimate slang, far from being an unmitigated evil and a constant menace to the purity and propriety of our noble tongue, is standard English in the making, is idiom in the nascent state before it has attained to the dignity of correctness of usage. To change the figure, legitimate slang is the recruiting ground whence come the new and untried words which are to take the place in the vernacular, of the archaic and obsolete words, dropping out of the ranks. But it is aside from the main purpose of this paper to discuss the relation of slang to standard usage (cf. 'What is slang?' POPULAR SCIENCE MONTHLY, February, 1906), and hence this only in passing.

By an Americanism, as here used, is meant a word, phrase or idiom of the English tongue, in good standing, which has originated in America or is in use only on this side of the Atlantic. It will be seen, therefore, that all mere slang expressions, even though they be of American origin, are barred from the present consideration. In his dictionary of 'Americanisms,' Bartlett gives a large collection, many of which the above limitation, of course, excludes.

Of reputed Americanisms, as one might surmise, there are several classes to be distinguished, without any very clearly defined line of demarcation separating them. One class includes a large number of phrases which had their origin in England and were transported thence to our shores by the first settlers who came from the mother country and established themselves in Virginia and Massachusetts. In the last analysis these locutions appear to be transplanted British provincialisms, not a few of which came over in the *Mayflower*. Some of our British critics who are not as familiar with the history of the English language as they might be do not hesitate to deliver an offhand opinion, pronouncing an apparent neologism an Americanism, when as a matter of fact the expression shows a good English pedigree extending back many generations. A more intimate acquaintance with the history of our common speech would save them the embarrassment from such a glaring blunder. But it is so easy to fall into the careless habit of branding as an Americanism an unfamiliar idiom or a phrase that is rarely heard in England. This convenient term has thus become in England a reproach, inasmuch as a certain stigma, somehow, attaches to it in the British mind. But for all that, like charity, it covers a multitude of sins, sins of keen prejudice, no less than of crass ignorance.

Many of the so-called Americanisms are really survivals of Elizabethan English and boast a Shakespearean pedigree, although they are no longer heard in the country of that consummate master of our speech. Somehow, they seem to have drifted out of the main current of British English. Perhaps they have been caught up by an eddy and carried into one of the provinces where they are still preserved, as they are in America, fresh and vigorous. A moment's reflection will show that we Americans come rightly by our Elizabethan English. For surely New England, Maryland and Virginia were settled by those who spoke the tongue of Shakespeare, even though they did not all hold the faith and morals of Milton. Many of these settlers—both Puritan and Cavalier—were college-bred men, graduates of Oxford and Cambridge. Therefore they inherited the best traditions of the English speech and transmitted it uncorrupted to their children. Nor were their children wilful traducers and corruptors of the King's English, but contrariwise they conserved it and safeguarded its purity quite as sedulously as the inhabitants of the mother country. Thus the English speech was handed down, undefiled, from one generation to another, in America. Hence some words and phrases of good Elizabethan usage have been preserved in America, which long ago became obsolete and dropped out of the living speech in England, where the growth of the language was, of course, not arrested by the rude shock incident to its being transplanted in a foreign country.

Let us now point out a few examples of reputed Americanisms, social pariahs which have lost caste and no longer move in polite circles in England. An interesting example is found in the word 'fall' used in the sense of autumn. Both these terms are in favor in America, although the pedants, following the lead of British critics, proscribe the use of 'fall.' We are told it is not employed in standard English, and hence must be censured as provincial. Yet 'fall,' which enjoys a certain poetic association with the fall of the leaf, can offer in its support the high authority of Dryden, who employed it in his translation of Juvenal's satires:

What crowds of patients the town doctor kills,
Or how last fall he raised the weekly bills.

In his 'Northern Farmer,' Tennyson used the offending word, but of course under the cloak of a provincialism. Still Freeman did not deign to employ it. Commenting on it, he remarks: "If fall as a season of the year has gone out of use in Britain, it has gone out very lately. At least I remember perfectly well the phrase of 'spring and fall' in my childhood."

Another good illustration of a word still surviving in American usage, but long ago discarded in England, is 'sick' in the sense of ill. British usage restricts the meaning to nausea, employing ill to describe a man suffering with a disease of whatever sort. Yet 'sick' is supported by the very best literary authority. The term occurs again and again in Elizabethan literature. Reference to Bartlett's concordance will convince even the most skeptical that the word abounds in Shakespeare, and that, too, in passages where the correct interpretation leaves no doubt that 'ill' is meant. Suffice it to cite only an example or two: In 'Midsummer Night's Dream' (act 1, scene 1), Shakespeare makes Helena say, 'Sickness is catching'; again in 'Cymbeline' (act 5, scene 4), we read, 'Yet am I better than one that's sick of the gout'; and in 'Romeo and Juliet' (act 5, scene 2), we read, 'Here in this city visiting the sick.' Not only so. 'Sick,' in the American acceptation, has an unbroken line of the best literary authority from Chaucer, 'that well of English undefiled,' down to Doctor Johnson, whose dictionary defines the word in reference to a person afflicted with disease. American usage, furthermore, is supported by the King James version, in which 'ill' is nowhere found, and also by the Anglican Church ritual. It is needless to multiply citations. If Americans sin in the improper use of 'sick,' it may be urged in extenuation that they can at least plead a long array of illustrious and unimpeachable authority and are in good company.

The use of 'well' as an interjection is mentioned by Bartlett in his dictionary as one of 'the most marked peculiarities of American speech.' Moreover, he adds, 'Englishmen have told me that they could always detect an American by the use of this word.' If this is an infallible hall-mark of American speech, then American English is nearer the tongue of Shakespeare than British English of the present day. For the word 'well' in the sense of an interjection occurs again and again in Shakespeare. In 'Hamlet' (act 1, scene 1), Bernardo asks, 'Have you had a quiet guard?' Francisco replies, 'Not a mouse stirring.' Whereupon Bernardo adds, 'Well, good-night.' Again, in 'Midsummer Night's Dream' (act 3, scene 1):

Bottom. And then indeed let him name his name, and tell them plainly he is Snug the joiner.

Quince. Well, it shall be so.

In Beaumont and Fletcher's 'Captain' (act 3, scene 3), we find an excellent example in the line, 'Well, I shall live to see your husbands beat you.' No one, of course, would think of charging Tennyson with using unidiomatic English. Yet, in 'Locksley Hall,' you read:

'Well—'t is well that I should bluster.'

Surely it is superfluous to cite further examples from English authors showing that American usage in the case of 'well' as an interjection is perfectly good English, even if the locution is censured by British pedantry and never heard on British lips.

The trite and hard-worked 'guess,' as characteristic of American speech as the much-abused 'fancy' is of British speech, furnishes another conspicuous example of a reputable word in Elizabethan English which has become obsolete in England, but is still preserved on this side of the Atlantic. There is no doubt that our constant employment of this good old Saxon word to do service on every occasion and to express every shade of thought from mild conjecture to positive assertion is somewhat inelegant; and this circumstance has perhaps contributed to bring the overtaxed phrase into disrepute with our kin across the sea. Yet there is abundant warrant in Elizabethan usage for the familiar notation we give 'guess' in our every-day speech, although it is generally confined to its strict meaning of conjecture in that period of the language. We find it used in the familiar sense of 'think' in several passages in Shakespeare, notably in 'I. Henry VI.' (act 2, scene 1):

Not altogether; better far, I guess,
That we do make our entrance several ways.

Likewise, in 'Measure for Measure' (act 4, scene 4):

Angelo. And why meet him at the gates and redeliver our authorities there?

Escalus. I guess not.

So, again, in the 'Winter's Tale' (act 4, scene 3):

Camillo. Which, I do guess, you do not purpose to him.

But this meaning of 'guess' is common throughout the entire history of English literature, for the word has always borne the sense of think, cheek by jowl with its specific meaning of conjecture. It is so employed by Chaucer and Gower in early times and in the last century by Sheridan and Wordsworth, certainly good literary authority enough. However, this meaning of the term appears to have died out in the present-day British speech, and the word is there employed strictly in the sense of conjecture, its lost sense being supplied by 'fancy.' Now, as between the Briton's 'fancy' and the American's 'guess,' there may not be much choice. But certainly the employment of 'guess' which our British cousins claim to be a shibboleth of American nationality does not indicate any misuse of our mother tongue, as they contend.

Only one more case shall be adduced in illustration, to wit, our word 'baggage,' which the other half of the Anglo-Saxon race has discarded for 'luggage.' Here again, as elsewhere in the exercise of our prerogative, we have demonstrated our independence of the mother country in the matter of our speech and have chosen one term while the English people have adopted another, to designate the same thing. Both words have a good literary pedigree extending several centuries back. Shakespearean usage seems about equally divided, perhaps, with the odds in favor of 'baggage.' The Shakespearean coinage 'bag and baggage and scrip and scrippage,' which falls from the lips of Touchstone in 'As You Like It,' and which enjoys the familiarity of a household word, ought to have given 'baggage' a wider currency, especially in the author's own country. But language, like the heathen Chinee, has ways that are dark, if not tricks that are vain, and does not develop according to logic or our *a priori* conceptions. Between the Briticism 'luggage' and the Americanism 'baggage' it appears, therefore, to be a drawn battle. So the British have nothing to reproach us with on this score, since convention has adopted 'baggage' on one side of the Atlantic and 'luggage' on the other.

So much for this interesting class of Americanisms which repose on standard Elizabethan usage, but are social outcasts in the land of their birth. There is another class of Americanisms which are not bolstered up by a long literary pedigree, inasmuch as they originated on American soil and were not imported from the old world. As compared with the class just considered, these latter are mere *parvenus,* without any illustrious ancestral history to commend them. This class of Americanisms is composed of phrases which have found their way into our speech from various foreign sources. They have been introduced into our tongue from our contact with diverse peoples from remote parts of the globe. They constitute a small residuum of terms and phrases, the presence of which in our vocabulary attests the fact of our relations with different nations of the earth. For instance, in the early history of our country, we had to do with the Indians, and so borrowed from them certain terms especially pertaining to natural objects. We also had relations with the French, and consequently borrowed from them sundry phrases employed in official parlance, such as 'bureau of information,' for which British usage prefers 'office'; 'exposition' for the British 'exhibition,' and the like. Let these few examples represent the class. It is apparent here that we have made a slight departure from British usage. But it does not follow that our speech, for this reason, is less pure or less idiomatic. Both American usage and British usage show that the respective nations have decided to employ Romance importations in official language, but they have adopted different terms for the same object. This proves, in the first place, the independence of the two great English-speaking nations even in the matter of language, and, in the second place, the wide-reaching influence of French as the recognized official and diplomatic language during the eighteenth and early nineteenth centuries.

In addition to these two distinct classes of Americanisms there is a third class composed of phrases and expressions which have not yet attained to the dignity of universal currency throughout the entire country. These are rather provincialisms which are peculiar to certain localities. This class, therefore, does not command the importance which the first two classes already considered do. In a heterogeneous population like ours, made up of people from every nationality under heaven, it is quite natural that in certain localities there should exist some eccentricities of speech, some departures from the received standard—in a word, some provincialisms. It need hardly be recalled that parts of our vast country were settled by other nations than the English, as, for instance, New York by the Dutch and Louisiana by the French, to mention two specific cases bearing on the point in question. The people of these respective states, when they were incorporated into the union, of course, did not immediately forsake their native modes of speech and inherited vocabulary for pure, unadulterated Saxon. When the vast southwest territory was made a part of the United States, the people in that quarter of the land spoke a lingo which had a decided foreign complexion. What more natural, then, than that in the speech of that portion of our land there should exist traces of this old foreign element? Assuredly it would have been the height of artificiality and an unprecedented proceeding for the French element of New Orleans, when they became citizens of the United States, to have renounced their native French names for such natural objects as 'bayou,' 'levee' and the like, in order to adopt pure Saxon terms. Likewise, it was not to be expected that the Spanish settlers in the western section of our country, specifically California, should abandon such native terms as 'cañon' and 'ranch' and so on, for the corresponding names of genuine English origin. Thus it happens that there is a pronounced foreign flavor, or at least a slight tang, in the eccentricities of speech heard in certain localities of the United States. But these are mere provincialisms and do not impair the quality of our standard speech, which is English to the very core.

However, it was inevitable that the English language in America should have received an influx of foreign words on American soil. But our speech possesses a marvelous capacity for assimilating non-Saxon elements from whatever source. Hence the various foreign elements, such as Indian, Dutch, French and Spanish, to mention only the chief importations, have all been absorbed without any appreciable alteration in the constitution of our English speech, and only traces here and there are seen of non-Saxon elements surviving in a word or an idiom as an enduring monument to the influence of other tongues upon our own on American soil. Some of these foreign loans, it is true, are confined to certain localities, and consequently are to be viewed in the light of solecisms, or at best provincialisms. They circulate freely in a limited area, but are not recognized as legal tender throughout the length and breadth of the country. Such expressions are confined chiefly to the western portion of the United States and very rarely find their way east. It is questionable whether they are entitled to be termed Americanisms except in the most liberal interpretation of that phrase, because they are not everywhere current and are not readily intelligible, not 'understanded of the people.'

It seems appropriate at this juncture to say a word concerning dialects in America. The assertion is sometimes made that there are no dialects in America, that the railroad and printing press, the two potent and indispensable agencies in our modern civilization, have leveled out all eccentricities and peculiarities of speech and reduced our language to a uniform standard throughout our entire country. This statement is, in the main, true. Yet it requires only a little reflection to see that the assertion is not absolutely accurate and in accord with the facts. Certainly a brief residence in the several principal sections of the United States would bring convincing refutation. There is the western dialect, as implied in the comments in the preceding paragraph. There is also the Yankee dialect of New England, the salient features of which Lowell described very fully in his famous 'Biglow Papers.' There is no less truly the southern dialect with its definite peculiarities of idiom and utterance. These dialects are quite sharply defined by their respective characteristics of colloquial speech. Each dialect has its own phrases and locutions familiar enough within its own geographical divisions, but not readily understood, perhaps unknown, elsewhere. For instance, the native southerner 'reckons' and 'don't guess,' whereas the Yankee to the manner born does not 'reckon,' but 'guesses' *à tort et à travers*. As for the western dialect, it is said that three elements enter into its constitution, *viz.*, the mining, the gambling and the cowboy element, a rich vein of billingsgate running through each. An effort has been made by our writers of fiction to register and record the salient features of these respective dialects incidentally in their stories, but the shades and gradations of speech are not easy to reflect and preserve on the printed page with the corresponding local color. Hence the work has been but partially done, and nowhere with complete success.

We Americans are far less trammeled by dialectal inconveniences and perplexities, however, than are the English people. For in Great

Britain there is much less uniformity of speech than with us, and the difference between the language of a Scotchman and that of a Devonshire man is almost infinitely greater than the difference between any two American dialects. But the dissimilarity of the British dialects is historic and dates back from time immemorial. The story of Caxton, the first English printer, is well known, how the good merchant from a southern shire, when he inquired for eggs of a goodwife in a northern shire, could not make himself understood, his southern dialect being mistaken for French. To be sure, the dialectal differences are not so great to-day as they were in those remote times, largely as the result of the printing-press Caxton set up in Westminster. But even yet the differences between the dialects of the extreme parts of the British Isles is so pronounced as to be a barrier to complete interchange of thought.

It appears from the foregoing that the indictment of corrupting the English language which certain British critics have brought in against the American people is not a true bill, since no count has been established. Our British critics seem loath to acknowledge any American rights in our common language. Americans have as much right to enrich the English vocabulary with useful words as the English people themselves. We also have as just a claim as they to revive and preserve an obsolescent phrase or idiom. Because a given English word is no longer in use and esteem in England, but is recognized as standard usage in the United States, it does not follow that it is not good English. The number of those using the English language in America far exceeds the population of England, and the English speech is just as vigorous and virile in America as it is in the parent country. Indeed, it has given indubitable proof of its vitality and vigor on American lips by adapting itself to the infinite variety of new conditions in this new country and by the added flexibility, strength and richness as exhibited in its augmented vocabulary. English now is the language of the American people as well as of the English people. It is, therefore, no longer proper or scientific to speak of the queen's or of the king's English. Such a phrase is really an anachronism in the twentieth century, when the English-speaking subjects of King Edward are numerically inferior to those not owning allegiance to Britain's sovereign, who speak the same tongue. Moreover, it is manifestly not in keeping with the eternal fitness of things, as well as unscientific, for our British kith and kin to stigmatize an idiom or a phrase in good American usage as a provincialism simply because it is not current in Great Britain. The Britons have no more right to attempt to prescribe and limit the growth of the English tongue than we have. Nor do they enjoy an exclusive prerogative of determining whether a given expression, be it a new coinage or a survival from a former period, shall live and flourish or decline and perish in the English tongue. No sovereign, no nation can determine this, either by decree or by statute. The most that the British can say in derogation of an alleged Americanism is that it is current only in America and is not authorized by British usage. But this does not make it un-English, if it bears the American sign manual.

It is perfectly absurd for the British critics to condemn Americanisms offhand and to attempt to read them out of the language, simply because they are not in accord with British usage. In so doing they give proof of their insularity and fail to exhibit a spirit of liberality and sweet reasonableness. Indeed, they seem disposed, at all events, to take themselves too seriously as guardians of the English language. It is well enough for a critic to throw his influence on the side of the preservation of the purity and propriety of speech. But it is sheer folly to allow one's pedantry to go to such a length as Malherbe, that 'tyrant of words and syllables,' who on his death-bed angrily rebuked his nurse for the solecisms of her language, exclaiming in extenuation of his act, 'Sir, I will defend to my very last gasp the purity of the French language.' It is related of him that he was so fatal a precisian in the choice of words that he spent three years in composing an ode on the death of a friend's wife, and when at last the ode was completed, his friend had married again, and the purist had only his labor for his pains. Now your true British pedant seems to think it his bounden duty to reject summarily every word or expression which does not bear the pure English hall-mark, and that as for Americanisms they are an abomination which must inevitably work the speedy corruption and ultimate decadence of the noble English tongue. Such an one, whether from his precisianism or his prejudice, fails utterly to recognize in Americanisms conclusive evidence of the inherent potency, vigor and vitality of the English language on American lips.

DIFFICULT BOYS

By J. MADISON TAYLOR, A.B., M.D.
PHILADELPHIA

A LONG and somewhat intimate acquaintance with boys and teachers of boys, many of whom are my close personal friends, has given me opportunity to formulate certain conclusions which may help others. I have always been fond of the society of boys, being endowed with youthful tastes and aptitudes, and find it profitable to study boyhood hopes, pleasures and ambitions. I have also taught boys and traveled with them in various capacities, and have a grown son whose friends I have tried, and with some success, to make my own. My personal work has brought me in intimate contact with many phases of the human mind other than normal and particularly with problems of psychologic imperfections. This attention to abnormalities of the mind and character has not had the effect of making me over-suspicious of finding defects of the mental processes, because it is obvious to the student that few brains are free from obliquities and regrettable limitations. The tendency is for me rather to view with tolerance inevitable vagaries which surprise and shock those who assume that the mind of most folk is sound and dependable. Teachers and parents are overready to become amazed at sudden variations and deviations in the thoughts and actions of those entrusted to their keeping. Kindergarteners seem to assume as a fundamental principle that any child subjected to what they define as suitable conditions of environment and education can develop into a perfect being. Lawyers divide people into two sharp-cut classes; those who are altogether sane and responsible, in season and out of season, and those who are insane, fit only to be held in check by restraint. Clergymen are over-tolerant of peculiar action and speech, often to a degree that they are not so helpful as they should be in enforcing authority where capability for responsibility is questionable. They frequently urge the objection that a stigma falls upon those who are at any period admitted to be in need of special training or restraint. Among medical men there is too little knowledge and much unwarranted fear of mental problems. They know something, but not enough, as a rule, and occasionally err on the side of condemnation.

Physicians and teachers should clearly appreciate that the mind of man in his earlier years varies widely in degrees and qualities of development, even more than in differences of bodily growth. Again, varying conditions of home influence, early schooling or accidental training may, and does often, bring forward rapidly one part of the mind while another remains distinctly infantile. Conditions of bodily health, not always obvious or even readily estimable, produce profound changes in cerebral energizing, so that one day certain beliefs, capacities and limitations may exist and to-morrow the balance of power be far otherwise. Under certain conditions, not readily determinable by common criteria, we may note and encourage in some the most bubbling spontaneity, and in others similar circumstances may check all this, inducing introspection, discontent with self or surroundings, even a brooding melancholy. Tastes and inclinations differ enormously, especially in boys; also standards of excellence. Conceptions of objects worthy of pursuit in sport, or study, or plans for life, are often widely at variance, not only in different individuals, but in the same boys at different times and under dissimilar conditions.

Many boys are possessed of greater fixity of purpose than others. This is usually assumed to be an altogether desirable quality. Not always so, because one boy may possess a nature large with possibilities and varied capacities, some of which are bad, revision being most desirable. It would be a deplorable unfairness to compel such a one to become molded into a definite form before time and circumstances have permitted a symmetric shaping of the best several parts of a complex organism. It may be that such a boy will require many years of opportunity and training to furnish scope to vast inherent powers for good. Put him into a narrow line, and only warping and possibly embitterment and deterioration follow. However, fixity of purpose is to be welcomed in the main, because direction can be given to strong impulses; but it is a hard task to steer a drifting ship.

The subject is so wide and capable of being treated under such a variety of headings that my purpose here is only to offer from my experience remarks upon two of the chief influences which either make for corrective development, or emphasize the original bent and impair usefulness and citizenship. The one is home training and early environment, the other is the school and the teacher.

A long experience in the specialty of diseases of children has brought me in contact with many children in their homes. A large and important book could, indeed should, be written on the subject of parenthood. In a paper I wrote some years ago ('The Nervous Mother,' *Univ. Med. Mag.*, N. Y., 1895) I said:

> We all love to contemplate our eidos, or highest concept of the mother, the unspeakable beauty of which has alternately lured and baffled thinkers and poets throughout recorded history. Nothing is too good or can be claimed as too lovely in description or praise for the ideal type of maternity. It is then with regret we must admit that the average mother is often disappointing. It has been permitted me to meet many superbly beautiful mothers. Yet this

crowning embellishment and glory of womanhood comes too often as a surprise, nor is it always welcomed, and only rarely does it bring unalloyed joy.

There are obviously some faults here which must be local or due to remediable conditions. It is our duty to search out the defects and correct them. This in my judgment is chiefly in negligences in teaching mothers their duties. When it is realized that the most valuable influential impressions upon the infantile organism (whereby standards are acquired, moral impulses initiated), are made during the first year of life, it is plain that no omission of maternal care can be otherwise than hurtful. However much the mother may lack of perfect fitness for her sphere, however blameworthy in her attitude toward her trust, still she is a trustee for whom there can be no substitute comparable to herself. The child who has failed to enjoy the tender all-enfolding care and love of a mother, acting up to her best endeavors, is bereft of the greatest gift obtainable. She may leave in her personality, in her conduct, much to be desired. She may be a mass of minor faults, not wise or strong of mind, yet if she be sincerely desirous of fulfilling her instinctive obligations, no other being can replace her.

The difficult boy stands clearly differentiated in my mind from the backward-minded or irresponsible boy, although there are grounds on which they may become merged. The difficult boy, as I conceive him, is one endowed with normal impulses, usually overstrong, which, because of defects of early guidance, have become diffusive, unsymmetrical, lacking inhibition, one who is commingled of more bad than good, yet often capable of great things under favorable conditions. There are those in whom the ingredients vary in other directions, among the worst of which are apathy, laziness, secretiveness, moral shortcomings. These, however, will soon or late become classifiable differently.

The difficult boy may appear to be a liar, a bully, selfish, unwilling to exert himself in worthy directions, of even other and perhaps worse characteristics. All this may be due to pressure of circumstances obtunding a none too vigorous sense of right and wrong, distorting conceptions, inducing acts and speech which belie inherent normal instincts which are undeveloped or chronically impaired. In short the seeds of wholesome manhood are present, in fair measure, capable at times of splendid development, often to admirable citizenship, but not strong enough unaided to nullify the blanketing effects of circumstance. How are we to estimate what these counteracting forces are, or were, in the instance? How should we have conducted ourselves under the same baffling influences? What would have been the effect of the same plainly indicated disheartenments, evil influences, examples on one nature as compared with another? If we examine our own personalities, we can see evidences of effects springing from apparently trivial causes out of all proportion to that which should have followed. A critical, candid self-survey will often astonish and alarm us at the close escapes we have made from impulsions which swayed us forcefully. What consequences have we escaped by sheer accident? In short, how can we wisely make allowances for forces potent in others, the nature of which we may only dimly know and are practically unable to appreciate in all their temporary despotism? The question is how far will the normal impulses carry any one? We plume ourselves on our own individual solidarity, poise, achievements, our importance in the community; yet we have survived endless perils by means of some judgment and more luck.

G. Stanley Hall, the master mind in childhood psychology, tells us in this connection that:

> Many of the morbid mental phenomena are merely those of overaccentuation of processes normal at puberty. The germs of many of these disturbances lie in the common faults of childhood, which are now studied under the name of pedagogic pathology. We must seek the key to these perversions by addressing ourselves to the larger underlying and preliminary problems of determining the natural forms of psychic and somatic transitions from childhood to maturity, and study what puberty and adolescence really mean as developmental stages of human life.
>
> Adolescence begins with the new wave of vitality seen in growth; in the modifications of nearly every organ; new interests, instincts, and tendencies arise, increased appetites and curiosity, so that it is the physiologic second birth. Passions and desires spring into vigorous life, but with them normally comes, or should come, the evolution of higher powers of control and inhibition. The momentum of inheritance may be sufficient and Binschwanger conceived the psychic morbidities of this age as due to exhaustion or lack of capital.

In the earliest education of all boys, whether in the family, the kindergarten or the school, one definite principle, it seems to me, should be held in mind as of paramount importance. This is motor training. The potentiality of this postulate is readily demonstrable, yet the history of education exhibits here a neglect, seeming to argue that if the principle were so vital it would have been enforced long ago. There is some modification in these later years. Froebel makes partial use of motor training in his beautiful idealizations, but it is subordinated to an optimistic expression of the good, the beautiful, the divine, needful but lacking in robust practicability. Man is put into the world to do something, to be something, and the obvious way to accomplish this is by primitive forms of labor. He may, and should, think, worship and aim for high ideals. In all this he should achieve concrete things. It is by no means proved that he can do this, except through the gradual process of fitting himself to become a practical part of the divine scheme. In this there is no place for drones. In due time he may devote himself, after earning the right, to physical quiescence, to thought, to contemplation. Man may, if he so elects, try to achieve a serene mental attitude (nirvana or kaaf) until he shall become released from all bonds as the teachings of Brahma make possible. This is what the Froebelian conception leads to.

Action is the key-note to habit and character. Good habits make for progress. Habits are definite actions resulting from sensations, motor modifications in nervous matter which have become stable through repetitions of actions. They are thus more easily performed. At first there is friction between sensory and motor nerve cells and this must be decreased by work. Memory is thus the same as habit; the nerve cells continuing to act in the way they have been induced to act before. We remember most easily things or acts which have been most often performed; new paths are thus ploughed out in nervous matter. When actions have been repeated often enough there are then almost no new paths to be formed. Hence habits acquired become fundamental courses of action, they constitute organic memory, which may or may not be accompanied with consciousness. To form these there must be *accurate repetitions of dynamic associations between nerve cells in early life,* during the plastic period. After plasticity of these cells has passed away guiding habits can only be acquired imperfectly, and if at all at enormous expense of energy. Hence the imperative need to form correct early habits, which are bundles of memories or tendencies enabling us to act again in the way we acted before. They become parts of our essential nature. A man does in middle life what he began to do in childhood—it may be good or bad—it is imperative. The boy unconsciously molds and trains his nervous mechanisms in such fashion that they will continue to act and react in the same way. At the start he is master, after a time habits master him. When these facts are more clearly appreciated there will be broader acceptance of the truth of the principle that dogmatic authoritative training in early life is best, provided always parents and teachers can be trusted to initiate action judiciously. Many a man is a failure in some direction, because he omitted to acquire the habit of courtesy, self-restraint, correct diction, punctuality, dexterity, accuracy in fundamental motions, even truth-telling. What evil may follow from the acquirement of vicious habits, however heroically resented, can readily be imagined. Habit is the process of associating a definite muscular action with a sense impression or with an idea. A child properly trained gives the right motor response with unerring accuracy. Sensation must be associated so often with action that one shall flow automatically into the other. An image is a revised sensation leading to mental conceptions, impulses, etc. No image can be formed without causing a more or less intense motor outflow. Movements can be checked by the introduction of cause, and counter cause. Thus the will is invited to oppose a movement through the function of inhibition, whereby it is modified in accordance with judgment.[1]

[1] See Reuben Post Halleck, 'The Education of the Central Nervous System.'

The child of rapid growth usually fails of symmetric development in several directions. Disease processes, infections, accidents of nutrition, environment, emotional influences, etc., all tend to initiate and emphasize minor deformities. Overgrowth usually leads, for instance, to poor thoracic capacity. If the thorax is for any reason disproportionately small and narrow a variety of special predispositions are encountered.

If as physicians we fail to devote sufficient attention to morbid phenomena of the mind and morals, we perform less than half our duty. Disorders of the mind are dependent upon one of two factors: either defects of development in the brain, or diseased processes of the brain, or retroactively. The purpose and aim of diagnosis rest upon the concept that by the early recognition of manifestations of morbid physiology, we shall find means to check the changes which would otherwise pass on to destructive alterations.

If this proposition obtains for the disorders of the physical functions, how much more must it fulfill a valuable service for those of the brain, which is a far more sensitive structure and especially liable to permanent damage from relatively slight irritations. It is a great privilege to mitigate bodily suffering, to limit the progress of structural degenerations, to prevent disablement and save life, but how vastly higher is the prerogative to turn aside those perils which jeopardize the budding intellect and rescue a tottering moral nature. Yet how little of this subject is the medical student taught, or again how much interest does the average practitioner display in this incomparably higher phase of his duties?

It should be the aim of the clinical teacher to emphasize unceasingly the urgency of obtaining the earliest possible indications, omens or prefigurements of departure from normal functionation; especially in children. When this is accomplished the greatest economy is effected; first in the limiting of suffering and the progress of disease, and second, in forefending the organism from developmental defects. All life is a process of development, but the effects of interferences are vastly more forceful and significant in the young. M. W. Barr points out a fact, especially obvious in children of impaired mentality, which, however, obtains to a certain extent in all. There are at any one period, three ages which must be estimated: (1) the actual age in years, etc.; (2) the psychologic age, the degree of mental development or retardation; (3) the physiologic age, the status of conformation and function.

Diagnosis of the morbid conditions of childhood involves something more than a mere search for evidences of disease. During the period of plasticity numerous influences prevail in all ranks of life to alter normal growth and organic development by which the foundations of constitutional weakness are often laid. These are in a great measure preventable, at least in part. It is the duty of the physician to recognize and promptly rectify the evil effects of environment and training, and in so far as possible of inheritance. Hence it is a most important department of differentiation to possess clearly defined standards of growth, proportion, activities, sensitiveness, functional competence, intelligence and capacity for endurance. These standards should be the product of wide observation, reading and experience, among normal as well as abnormal conditions, but unless tempered by judgment, right conclusions are not assured.

The prototype for each teacher and physician is the ideal child, a composite picture of normal children, and can not be formed too carefully by a thorough interpretation of all data at command. Next to the ideal child the teacher should erect for himself standards with permissible variants. In America we must not limit our attention to children of pure Anglo-Saxon stock, but hold in view the many other racial characteristics with which we are likely to come in contact. There are crosses of the Latin, Celt, Slav, German, Hebrew and other white races; also the hybrids of red, yellow and black races. These modifications exhibit laws of their own, as yet by no means clear, but deeply significant. Inheritance of tendencies is recognized as a potential factor. Predisposition to physical and moral derangement is an obvious factor, admittedly forceful for harm.

Difficulties of differentiation are many enough among children normal in structure, in neural balance and in mind, but these grow greater where constitutional variations or deviations are present. Hence it is desirable to weigh variants in type, such as peculiar and exceptional children. The normal processes are profoundly modified by peculiarities of temperament due to inheritance or acquired. E. W. Bohannon in a statistical study of over 1,000 children (*Pediatric Seminary,* Oct., 1894) covers the ground sufficiently to warrant using his classification. The psychic factor demands deeper attention in pedagogics than ordinarily obtains.

Bohannon formulates certain types of mental and physical conformation:

> These types are the heavy, the tall, the stout, the small, the strong, the weak, the deft, the agile, the clumsy, the beautiful, the ugly, the deformed, those with birth marks, the keen and the mentally precocious, those with defects of sense organs or mind, the nervous, the clean, the dainty, the dirty, the disorderly, the teasing, the buoyant, buffoons, the cruel, the selfish, the generous, the sympathetic, those with imagination, the liar, the ill-tempered, the silent, the dignified, the frank, the loquacious, the inquisitive, the courageous, the timid, the whining, the spoiled, the gluttonous, and 'the only child in a family.'

Many of these types cross; several are liable to include similar features, constituting composites of the types, making the study somewhat complicated if carried to legitimate conclusions.

A review of Bohannon's findings and conclusions from the observation of this large group reveals much of practical interest. As to general health and mental ability there appears good reason for believing that the larger children, except the extremes, are superior to others. But it must not be forgotten that there are pathologic cases in this group, especially in those showing marked departures from the average. Those who suggest too early maturity are generally even-tempered.

Small children evidence delayed development. The less vigorous show degenerative phenomena, many are delicate, ugly, deformed, or vicious, dull, mean or spiteful, and tend toward morbidity. The strong children, while exhibiting many admirable qualities, are likely to be aggressive, harsh, coarse, rough. More is expected of them, hence they are often early exhausted by compulsory work; their offenses are the result of excessive, often explosive, energy. The weakly children are likely to show pronounced evidence of degeneracy, often they are ugly and deformed, cruel and mentally deficient. Inheritance was not so frequently recognized a cause as parental follies, especially during the embryonal period. Temperamentally they are usually unfortunate.

The deft and agile show better health, yet are undersized. Clumsiness is found due to two causes; first, want of development of the mechanisms which function the accessory movements; and second, excessive inhibition of the same, along with lack of emotional balance. Ugliness is usually accompanied by many evidences of degeneration, physical and mental; in the deformed these deviations are even more decidedly present. Deformities are largely (*ipso facto*) manifestations of deviation, defects of central development exhibiting anatomic and physiologic faults, some of which are remediable. Under good care many of these improve greatly, some becoming distinguished adults. They are found to be treated by parents and associates with amazing lack of consideration, hence they suffer temperamentally.

Among those showing defects in mind, sense organs and speech, there is much to indicate a general decline. They are morbidly retiring, dependent, and lacking in symmetrical development, due in part to original defects and deficiencies in normal stimulus.

Those children grouped as 'nervous' exhibit delicacy and instability of constitution, are deficient in size and vigor, are timid, sensitive and changeful, disposed to be irritable and meddlesome, defective in control, hence untruthful. The extremely over dainty and the distinctly dirty, each excite suspicion of mental abnormalities. Buoyancy and teasing both indicate excess of energy; so also of cruelty, but here ancestral traits seem manifested.

Lying and imaginativeness are allied, and point to lack of self-control or to selfish imitation; the associated traits are disobedience, ill-temper, thieving and bad health. Those who are peevish, untruthful, discontented, are usually of delicate make and evince instability and poor vitality. Those who are loquacious, voluble and inquisitive lack inhibition or control.

Courageous children are usually healthy and strong in mind and body. Timidity has a physical basis, but may be acquired from bad environment, habitual discouragement. The 'only child in the family' in 66 per cent. shows disadvantageous traits; they are usually of poor health, lacking much of normality, both mental and physical. The 'youngest child,' the 'only boy' or the 'only girl' often displays many striking resemblances to the 'only child.'[2]

Classification of grades of mental deviation is only important for purposes of teaching. Types of mind there are, and they must be fully appreciated that individuals may secure the right kind of influence and training. Degrees and qualities of mentality are even more important, because by this means we may know where to place the individual, how much control to insist upon, how much compulsion to exert on the parents. Types of all the adolescent insanities merge into each other. Those who have the charge of young children may have no need of psychiatric training, but they do need to employ a common-sense recognition of abnormalities, deviations, obliquities, patent enough to the intelligent observer. Children of pronounced dominant impulses may exhibit at times self-will, naughtiness, ill temper, even exuberant imagination to the point of mendacity, buoyancy or apathy in changing moods, and yet become wholesome admirable citizens. Distinct and continued nervousness, fretfulness, timidity, brooding, causeless variations in moods, cruelty, vengefulness, should put us on our guard and warrant suspicion of deep-seated perturbations foreshadowing psychoses.

Educational methods are still defective in many particulars. Tradition holds us in a powerful grasp. In the public, and in most private schools, the course of study is analogous and aims to meet the supposed needs of the child of average intelligence. This would be well enough if certain fundamental truths were recognized by both school boards and parents. Custom has created a public opinion from which it is difficult to appeal. For instance, it is a fact that all children develop on some lines more rapidly than on others, in differing degrees of rapidity. In one there may be exhibited early motor aptitudes with late intellectual capacities. In another the reverse, yet at a certain age they may be to all intents equal. One child may acquire language, grammar, and the elements of literature early, with a late grasp of numbers, arithmetic, the natural sciences. Another may reverse this, and yet at a given time these two may be on a par. It will be plain that to get the best results due allowances should be made

[2] See article by the author, *Brit. Jour. Childrens' Diseases*, January, 1905.

for these variants. In some schools full cognizance is taken of these normal peculiarities. Economy, however, demands that all children of about a certain 'grade' shall pursue a 'systematic course.' The product is not what it should be.

The personal influence of the teacher is recognized increasingly. In some of our colleges a plan of subdividing the classes into small groups and placing them under tutors has been found of largest value. (At Princeton University a modified Oxford tutorial system was first initiated with excellent effect by Woodrow Wilson. This is now adopted by several other colleges.)

Indeed valuable horses and dogs get more careful personal teaching than most children. Boys whom we characterize as 'difficult' have become so largely by neglect or postponement of some important item of education. They have become warped, unsymmetric, psychically and physically. The prevention is right education, so also is the cure. The first thing is to correct faults of misdirected impulse, the next is to teach the elemental principles of self-restraint, disentangling errors, illuminating doubts, always encouraging and leading to wholesome customary lines of action and thought. Endless difficulties would be prevented, boundless good would be afforded, if from the earliest teachers to the highest university professors there should be pursued some uniform plan of notes or records on individual aptitudes, tastes, tendencies, capacities. Some teachers are endowed with instinctive capabilities for meeting unusual problems. Some also, the majority, are astonished and distressed, even annoyed and resentful in the face of individual peculiarities, good or bad. No one should judge too soon whether the peculiarity be altogether good or bad.

Errant impulses are by no means understood. Geniuses have exhibited strange individualisms. They are rare (geniuses), it is true, but how many times do well-meant efforts to suppress spots on the leopard, or to paint out the stripes on the tiger, fail to make of a well-bred wild cub a respectable tabby cat. The power of a nation resides in men of individual dominant personality.

We want our boys and young men to have ample opportunity to evolve their own individualism. University curricula are now made increasingly liberal. Why should not the primary schools adopt similar principles? It is quite true a 'system' is desirable for the average boy, but a sliding scale ought to be within the reach of any one who is recognizably unfit to pursue the customary methods.

Our 'difficult boys' may be divided roughly into those who are provided with overmuch impulse or too little, the robust exuberant doer, or the torpid dreamer. It is obvious that each needs motor education, partly similar and partly diverse. The chief defect of our school system is the lack of opportunity for motor education. In country districts where boys acquire of necessity more of handicraftsmanship they need manual training less. In cities it is essential.

The boy of mental peculiarities will not settle down to efficient work till he finds his own place, his level, range of action, and by his own initiative. This discovery is always the outcome of a gradual evolution; it should not be forced. From this secure position, once attained, he can fare forth satisfactorily and finally achieve his adult sphere of usefulness. That boy is fortunate who is content with his own province when discovered and does not invade that of another. Many a boy fitted to make an admirable and happy ranchman, soldier, sailor or farmer becomes a misfit self-detesting clergyman, physician or lawyer. His early advisers are generally to blame in compelling him to masquerade as a scholar, who was formed to be an excellent capable every-day man. Intelligence, capacity, is not to be measured by degrees so much as by qualities, aptitudes, characteristics. A first-class foreman in a factory may possess a far more symmetric intelligence, a clearer judgment, than the lopsided genius who invented the objects which he manufactures.

One of the surest criteria of capacity, at any level, is, according to M. W. Barr, the grasp, the quality of the grip of the hand. The grasp shows many things indicating the comprehensiveness of the mind. Certain minds can readily learn by doing, yet they grope feebly in pure intellectual effort. They reach an equally worthy goal if only they know their limits, stop at their own station, go to their own home. Over-stimulation in scholasticism is as hurtful as overtraining in track athletics; the staleness may pass into permanent mental impairment.

Shyness, inertia, resentment of interference, timidity, gloom, indolence or stubbornness may indicate no essential defect, but may be due to awkwardness, defective coordination. The cure is kindly encouragement, guidance in activities, development of unrecognized aptitudes, praise, wholesome incentives. Many have little tactile sense; this should be encouraged in all ways; it may finally come in fair measure and form the ground for conspicuous abilities by cooperation of other faculties long overshadowed. Barr quotes Buffon 'how wonderfully the senses are alike at bottom, how they supplement each other!'

The limit of receptivity is often reached early. It is then wise to be content with careful training on a lower level in which excellent capacities can be attained. One man can become a thoroughly good soldier, to obey orders, to die at his post, to follow to the death, who never can lead a company, much less plan a campaign or sail a ship. The best captain is by no means necessarily a good oarsman or a gunner. *Always it is essential to achieve even qualified success to begin special training as early as possible during the plastic stage.* No good sailor can be made from an old farmer or an old professor of mathematics.

The most promising agency in eliminating the difficulties which impair manhood in boys, future men, citizens, is the kindergarten, the principles of education outlined by Froebel. This aims at the highest idealization of life, largely through the play instinct. Whatever criticisms are made on the kindergarten teaching can only hold against methods of application. So far these objections have to do chiefly with its lack of adjustability to established educational methods, and will cease when the exponents of Froebel acquire greater breadth of knowledge, a clear appreciation of the practical needs of society. Our established methods of education leave much to be desired, but it will take time and thought to bring about a perfect system. Meanwhile it seems plain that the one means of both prevention and cure of difficult boys is to be found in a perfect home.

The ideal home, where two parents live with and for their children, where mutual helpfulness is fully afforded, where the fundamental impressions are given and received, is the greatest agency in primary education. Unfortunately this ideal home is made difficult of attainment because of a multitude of factors, especially in large cities by altered, artificial, perverted methods of living. The instinctive natural helpfulness, so necessary to arouse the sense of individual responsibility, finds little opportunity for growth. Unless the boy is encouraged to bear his part of the burden, to contribute his share to the body domestic, as in the primitive home, he can not grow symmetrically, or become certain, exact, in his more robust impulses. Instincts of responsibility find small encouragement.

To be sure, we can not check the inevitable trend of modern industries which aim by over-specialization to reduce the individual to the rating of an intelligent machine, whether in the lower or in the higher industries. If, however, we can succeed in fostering the spirit of the home, in implanting early, in the plastic childish brain cells, the idealities, the desirabilities of home, much will then be accomplished. Admitting that the conception of the home, once implanted, is forceful for so wide an influence, let us waste less time in other directions and concentrate our efforts on erecting and preserving the ideal of the home. This the teaching of Froebel is capable of accomplishing. The concept of divided responsibility is constantly presented. Pictures of domesticity, object lessons in practical helpfulness, are parts of the course of instruction. Children taught on this principle will carry through their lives clear ideals of home. When they become parents these instinctive promptings, these deeply suggestive pictures, early implanted, will act as unerring guides to parenthood.

The first thing a troublesome boy must learn is unquestioning obedience. In this way he may become a perfect intelligent machine.

Not, however, if he run wild and lawless till manhood, or not then without endless pain and punishment can he learn his life lesson. All good in human beings comes from seizing and utilizing the period of receptivity to all manner of impressions, the formation of habits of obedience, of accurate response to orders, to the facilities of craftsmanship. This is highest at birth and diminishes in a parabolic curve.

The boy physically strong, but intellectually weak, should not be judged by the same standards as the one physically weak but intelligently keen. Each will tire in his line of defects before the complemental capability has time to assert its potency. Neither should he hastily be judged inferior, because within his sphere he may be worthy of confidence and equally useful. Barr thinks that the stimulus of music is equal to that from books. The ever-present sensitiveness to disharmonies is developed by rhythmic sounds as well as by military drill. As energies are developed they can be specialized, directed into suitable channels, to varying applications, if not falsely forced, only wisely encouraged.

The problem of educating the tumultuous, effervescent or exasperating boy is usually solved by the military school. The enforcement of implicit obedience, the sharing of responsibilities by boys acting as petty officers and many other features constitute satisfactory methods, in the main sufficient. They often lack something essential. Soldierly qualities in the teachers may be absent, they being not themselves adequately trained for their accidental rôle. Again the routine of an ordinary school, constructed on military lines, even those of governmental foundation, often fails, because the industrial feature is absent, the only relaxations being leisure, or the ordinary athletic games.

Probably the best means of making clear the ideal methods, so far as we can adduce them, is to cite the course of training at a school where the best results are attained; where the boys, all difficult problems, yet become developed into, not only useful citizens, with rarest exceptions, but some of them achieve high qualities although their early status was desperately bad. The one in my mind is the Glen Mills School, Pa., originally the House of Refuge for Philadelphia. The boys are only admitted when committed by the law after perpetrating overt acts. Every one is of the most difficult kind. The special features of the Glen Mills School are the paternal, intellectual, agricultural, industrial and military. Other schools there may be conceived on a similar system, but I am safe in claiming that nowhere are these features in all branches so consistently and thoroughly carried to a legitimate issue. None achieve such uniformly satisfactory results. One item of equipment is superior, a magnificent gymnasium, the gift of Mr. Alfred Harrison. Here the boys enjoy every opportunity of a gymnasium, a drill hall, indoor games, basketball, preliminary baseball and football, etc., and a splendid swimming tank; all under expert instruction. The one element of industrial training impresses me as the most important of all. The boys make all their shoes, clothes and many other essential articles for home consumption; all furniture needed or desired. They decorate in highly artistic fashion all walls for esthetic and sanitary reasons and add to buildings, as the two new wings of the schoolhouse built last year show. Agricultural instruction is not only the best form of physical training, but a constant source of object lessons, the wholesome means of correction and moral stimulation. The week's work is divided judiciously between these various industries, and always, daily, certain portions of scholastic instruction, military routine, drills, etc. The scholars live and sleep and eat in houses presided over by the teachers and their families, securing the paternal influences.

In conclusion let me urge all those who are charged with the care of a difficult boy to be openminded at all times; to be prepared to modify the original concept, the earlier estimate; to read him in the lights revealed along the way. Above all things exercise toward him companionability; encourage confidences, especially as to hopes, ambitions, views on life. Be quick to see the good, the forceful, qualities and help the spontaneous exercise of these. Above all never be betrayed into forcing on such a boy plans of action contrary to his bent, his tendencies. *Let him evolve a course of action,* help him to perfect it, be it large or small. The small may become elaborated, the large may need modification. When the course is chosen, emphasize, praise, encourage spontaneity. Always leave the door open to a return to you for renewals of stimulus; encourage the appeal to you for judgment, for wisdom.

GENIUS

By Dr. ROBERT MORRIS OGDEN
UNIVERSITY OF TENNESSEE

CONSIDERED genetically, intellectual and physical functionings oppose one another. The business of organisms is to act. And action means primarily a direct response to stimuli. It is only as the organism grows complex and there is opportunity for more than one reaction to a given stimulus that there comes about a retardation involving an inhibition of action. And this retardation is filled out with weak reflections of the nerve paths which are being stimulated, *i. e.*, with thought. Thought, then, comes at the expense of the organism's natural functionings. Thought brings bodily inertia. Were there no thought, we should be mere reflex organisms. Health is conditioned by physical acts and the healthful rest of the organism is accompanied by sleep.

Now all this means that we who think are in a sense artificial folk. We are transcending nature in a way—at least in comparison with the great mass of animal life which has a more or less reflex existence. Still we have our compensations. We are *knowing* beings, having two sides to our natures, a physical and an intellectual. We can react on a given presentation in two ways, either intuitively in accordance with our natural physical bias, or logically in accordance with our more artificially developed reason.

It is no mere matter of hyper-intellectualism which leads us in our genesis from the first to the second of these modes of reaction, but a matter of increasing complexity of the organism making simple intuitive reaction more and more impossible. Therefore, as a general proposition, this development is nothing we can or wish to strive for or against. We simply have to accept it as it is.

Yet, turning to the study of individual man, we find great diversity of mental bias and disposition. The happiest and healthiest of men is doubtless he who lives an active life out in the fresh air and amid pleasant natural surroundings. His physical bias is strongly developed and affords a ready and never-failing intuitive force for good and health. His mental outlook is clear if not profound. He takes things as they are and, unless accident befall him to disturb his habitual methods of functioning, he is able to meet the various situations of life with positive equanimity.

On the other hand, he who by reason of physical inefficiency or environmental conditions spends his life in inhibiting physical action, finds a substitute for action in thought.

The intellectual life has two main attitudes: active reasoning and esthetic contemplation. Though these two complement one another, we find them variously accented in different individuals. The scholar leads, in the main, an intellectual life, yet the esthetic complement to his nature may be very slightly developed. His reasoning processes have the aim of elucidating and, therefore, of bringing peace of mind with respect to some phase of life. But his pleasure is more largely in the business of thinking, in the solving of his concrete problem, than it is in the contemplation of a complete result such as characterizes the esthetic attitude. The true artist has his esthetic ideal always before him. His function is to express this ideal as a complete and conformable whole. Whether his work be of head or of hand, it is always informed by such an ideal and the artist's genius rests all in his ability to give adequate expression to this ideal.

Each of us has these esthetic ideals in some degree, but only a few of us attempt to express them. We seek, rather, an expression for them in the work of another and, finding it, we obtain esthetic pleasure in the contemplation. But he whose ideas become crystallized to such an extent that he can objectify them and give them expression in a picture, a drama or a symphony, he has a peculiar talent which the rest of us, however esthetically appreciative we may be, do not possess.

The genius must possess a certain technical skill to enable him to express himself objectively, and he must also be so imbued with the force of his ideas that he is, in a sense, impelled to give them such expression. This *necessity* of artistic expression is one of the factors of his esthetic nature. A demand is felt to realize a certain ideal, to give it a clear objective expression such as must always be lacking so long as it remains clothed only in the vague imagery of the mind. The hack author, painter or musician may make a mere business of his talent. In so far as he does this his work must be of low merit as art. It is forced rather than spontaneous. It caters to an audience instead of being a natural expression of his own ideas.

There is something very intimate about true art. It always expresses the man behind it and, in the last analysis, its merit is a token of the character and mental bigness of the artist. Those who have no clear and definite ideas, but busy themselves with vague intentions, only reflect in their works the unrest of hyper-sensitive natures. We, their audience, may recognize and sympathize with their unhappy states and, indeed, derive a certain esthetic enjoyment from their expression, but it remains for the man able to give a positive impetus to his work to be termed a *genius;* one possessing a certain sublimity of purpose and accent which brings his work into touch with the eternal.

As to the artist's technique or means of expression, it must always take a place subordinate to the idea. The idea is always the motive power, the dominant force. So soon as the artist's interest lapses into an active pleasure in his work as such, he loses sight of his aim. And this shows quite conclusively that no amount of skill and special training in expression ever makes an artist and, indeed, explains the artistic puerility of so much work produced by men of splendid technical equipment. The pernicious influence of the 'academic' training is due to just this, that the artist is led to see the value of his work in such beautiful lines and relationships of form and color as all can imitate, rather than in an individual idea clamoring for artistic expression. Thus it is that much of our greatest work is expressed in crude, unfinished form, at times by men who apparently knew not how to express themselves completely or, at least, *would* not; for example, Rodin, Michelangelo, Manet, Whitman and Richard Strauss. Yet the force and dominance of their esthetic ideas justify and lend a value to their work higher than that of any faultless academician faithful to his classic traditions.

The true genius never learns his art. It is intuitive with him. There is but one way of expressing a great thought adequately and that is naturally, therefore intuitively. So soon as the artist begins to reason as to how he shall express himself, he loses sight of that which he has to express. His process is no longer an esthetic one, but becomes a practical one. Except to introduce a person into the realm of art, to teach him some of its manifold possibilities, art training is a thing of doubtful value. In so far as it attempts to substitute rules and methods for one's natural intuitive ways it is positively detrimental. The artist must be first and foremost an individual; without individuality he certainly can never be termed a genius. Still, if he can not be taught as one is taught a trade, he can be taught clearness of thought and perception, and this should be the true function of his training.

When we interpret things too much in the light of our knowledge concerning their objective natures, we usually interpret them wrongly. It is only when we understand them subjectively as 'experience' as well as objectively that we know how to represent them adequately. The best means of expression is the intuitive and natural, but a true psychological knowledge may aid one's expression when too much experience and reasoning has interfered with one's natural intuitive bias.

We find, then, that our true artist is a person of esthetic ideas plus an ability to express them adequately. Now, what are the consequences of this mode of mentality on the life of the man?

We have noted the artificiality of intellectualism as compared with naturalism. There is something even more absorbingly intellectual in the artistic mode of life than in the scholarly, for in the latter one is occupied with a *process* more or less limited in scope, whereas the artist is always striving to represent a complete ideal. The effect of this on the artist's active life depends largely on his natural propensities. Esthetic contemplation has evolved genetically out of sensationalism, and it is doubtless the sensual factor in his nature which leads to most, if not all, his pathological tendencies.

Moral and utilitarian ideas can scarce be other than healthy when viewed either from an esthetic or a practical view-point. Sensualism, however, is decadence. The effort of the individual to give free expression to his senses is always met with failure. Having risen above our mere sense natures into the realm of intellect, it is now impossible to revert to them. The clear, happy, unaffected, hedonistic lives of the Greeks are no more possible in these days of modern artifice. Therefore, he who turns to the senses for a true expression of his nature finds his effort clogged with all manner of false and related concepts which his experience with society has thrust upon him. There is no way to live 'naturally' in our day.

What, then, is the fate of him who attempts it? Taking all things in life to be natural expressions and being interested more in the states of body and mind than in any particular results to body or mind, this person is led to seek peculiar complexes of sensation, peculiar situations, bizarre effects, all which it may be a pretty esthetic problem to justify and sublimate. But the taste thus cultivated always craves something new and the attitude ceases soon to be one of esthetic intent and becomes instead one of low sensual desire. Contemplation is supplanted by desire, which now becomes the dominant note. Habits of passion develop and grow until both body and mind are ravaged by their deleterious effects. This is the real significance of decadence and it demonstrates, too, how works of art produced under such influences must fail in their universal import.

There is no denying that the genius in art is peculiarly exposed to these affections. The very inertia of his type of mind produces a species of hyperesthesia which, unless properly subordinated by a strong and forceful physical and mental nature, often leads to his downfall. We have so many instances where drugs, alcohol and other harmful habits have destroyed both body and mind of a bright genius, the factor is certainly one which can not be overlooked. Yet a positive element in his character may suffice to save him from this fate. So long as the individual asserts himself sufficiently to justify all his acts in the light of all his knowledge, bringing everything into connection with everything else without losing himself in a mesh of particulars, there is not much danger of degeneracy. Even an occasional lapse may find adequate place and absolution in such a character.

But so soon as such lapses from the physical and social order become dominant notes in one's life, either as things which one constantly deplores, or as things which one vaunts and praises, then the individual is shocked and begins to lose his positive force in society.

The genius may be as healthy and normal as another man; yet many indeed there are who have produced true works of genius only to succumb afterwards before their ruling passions. As a mode of life that of the artist is hardly to be commended. It is an artifice, an excrescence. It leads to too much objectifying and too little practise. Few individuals can stand this.

On the other hand, the true artist in an ideal sense is at the same time the true man. For he should be strong both of body and of mind, with a wide experience and a deep insight, with an understanding so broad that nothing is foreign to him, yet in whom nothing dominates so as to protrude beyond its proper setting. Such a man is, indeed, inspired with intuitive insight, but he is rare, even impossible. Yet there have been those who possessed this attitude in all its completeness for a time, and while under its influence they have produced undying works. These are the men to whom we commonly attribute genius. The usual critical mistake in dealing with such personages is either to attempt to make their complete lives perfectly consistent with these higher moments, or else by pointing out their weaknesses to decry even their greatest works. Needless to say there is neither sense nor use in either method.

The strength of the genius is only the strength of the ordinary man slightly intensified; the weakness of the genius is just the weakness of the ordinary man, but more conspicuous by contrast. Psychologically it is not at all incomprehensible to conceive a man of alternate high and low moments, alternate strength and weakness. It would be well-nigh inconceivable that a man should be always the one or the other.

If consistency of character is less marked in the genius than in the ordinary man, it is this which constitutes his uniqueness among men and may even at times determine his genius. The genius is more than apt to be a poor citizen, yet we can tolerate him for his work and because his kind is exceptional and few in number. If we would understand his nature and his art we must study his life in detail, unbiased and with broad understanding, for we are dealing with one who runs the gamut of emotions in order that he may sublimate them all.

THE BIRTH OF THE IDEA OF SPIRIT IN GREEK THOUGHT

By President MARY MILLS PATRICK, Ph.D.

THE AMERICAN COLLEGE FOR GIRLS, CONSTANTINOPLE

THE ideal side of life came into Greek consciousness on the eastern shore of the blue Mediterranean, under the shadow of Mt. Ida, in sunny Ionia with its fertile plains and luxuriant verdure and its rich and brilliant cities.

The poets were its forerunners, Homer, Alkaeus, Sappho and Anakreon. First there were the wandering poets, and then a school of poetry arose in the many tinted isle of Mitylene with Sappho at its head at the end of the seventh century, a school which was compared in antiquity to the circle of Sokrates. Schools of philosophy followed in Miletos, that hot-house of intellect, and later on in Ephesos, where Sappho and Herakleitos were born. We do not know whether these schools were organized *Thiasai,* dedicated to the goddesses like the school of Pythagoras in southern Italy and the Greek schools of philosophy of a later age, but it may well have been so, for in Ionia as in Greece there was a 'shrine at every turn of the mountain path, and a religious ceremony for every act of daily life.'

On the southern shore of the Gulf of Smyrna, opposite the river Hermus, with Mitylene in the distance across the sea, was the city of Klazomenai, the modern Voorla. There Anaxagoras was born, who was the first among the Greeks to evolve the idea of spirit as a philosophical principle. Yet like all great ideas, this one, perhaps the greatest, was vague and uncertain in its first appearance. Anaxagoras belonged to the school of Anaximenes of Miletos. Miletos lay only a few miles south of Klazomenai on the shore of another picturesque gulf of the eastern Mediterranean, and from the time of Thales it had been a center of philosophic thought. Theophrastos states that Anaxagoras was an 'associate of the philosophy of Anaximenes,' but these two great thinkers were not contemporary, as Anaximenes died in 520 B.C., two decades before the birth of Anaxagoras. The connection between them lay especially in a love of scientific research, and in similar methods of explanation of astronomical and cosmological facts. Anaxagoras lived in Ionia until he was about forty years of age, and he attained great fame in his own country during the last ten years of his residence there, gaining a reputation for depth of thought and integrity of life, and slowly evolving his theory of the universe.

The Ionian philosophers were monists and materialists. They sought a fundamental substance. water, air or fire, or some other form of matter, as the reality of life. In the language of the early Greeks we find the words soul and spirit synonymous with breath, and while the Greeks had the practical idea of the soul as the active power in being, they conceived of it as a thinner, finer form of matter. For example, Anaximenes speaks of air as being the breath of life. These old Ionian thinkers were not materialists, however, in quite the modern sense, which explains spirit as a function of matter, but they held rather the childlike idea that spirit is a purer, higher form of matter, for matter with them was the eternal existing something. It was not created, neither did the gods of Grecian mythology give it its form, for the gods had very little to do with the inner life of the Ionian thinkers in their efforts to find a natural cause for all phenomena.

Anaxagoras did not have very much difficulty in formulating a cosmic theory which suited him, that is, in making 'cosmos out of chaos.' His method of working was reasonably scientific, but the results of his theory in regard to the origin of things around him were ludicrously childish and impossible, and were not of especial service to Greek thought except as they led up to his one great idea. We will give in a few words the substance of his world theory. Herakleitos, the philosopher of the flux, had founded his cosmos upon constant change, or becoming. Anaxagoras repudiated the idea of change; absolute change was impossible. "The Hellenes," he said, "are wrong in using the expressions 'coming into being' and 'perishing,' for nothing comes into being or perishes, but there is mixture and separation of things that are." Chemical change he had never thought about; therefore, things must always have been what they are now. All objects, organic or inorganic, in which respect he made no distinction, as bone, flesh or gold, for example, had existed from eternity in the same form in small particles. The apparently simple substances, like air, fire, earth and water, are really the most complex, because they contain the greatest number of these particles. In the beginning this infinite number of small particles was in the form of chaos. In chaos a wonderfully rapid whirling motion started, and like particles joined with like until objects as we know them, including all forms of animal life, came into existence. Aristophanes, in his 'clouds,' ridicules Anaxagoras's idea of the whirl with pungent wit, for he represents one of his characters as saying that Zeus is no longer the leading god, but 'whirl' has taken his place.

Anaxagoras, however, was not as illogical in regard to the origin of motion as he had been regarding the construction of matter. He knew that motion could not start of itself. The origin of motion was the problem which his contemporaries were solving in different ways, according to their trend of thought, Empedokles with his love and hate, or primitive form of chemical affinity, and Leukippos with atoms in a vacuum, the heaviest falling faster and uniting. Neither of these theories, however, seemed satisfactory to Anaxagoras. How then could he start the whirl in chaos? Long years of meditation were doubtless necessary before he evolved his great idea, which revealed a dim understanding of the power of reason in the origin of being. To start a whirl he must have an outside something, and if reason is the strongest element of human power, why should there not be some form of reason which is independent of matter and able to originate the whirl in chaos, and then to retire from the scene of action and return to the separate and lonely existence of its unknown past? Thus was born the idea of the Nous. The Nous is half spirit and half matter, as yet a vague force, the beginning of a conception of the thinking element in the universe. There is only one fragment preserved from the sayings of Anaxagoras which would imply a kind of personality in the Nous, in which he speaks of its having knowledge of the past, present and future. In general, however, we find that Anaxagoras's understanding of the Nous was rather that of a kind of matter, a thinking essence, the lightest of all things, a semi-material force.

When Anaxagoras was forty years old, having partially at least formulated his world theory, he went to Athens, the first philosopher to live there. Athens was then in the dawn of its brightest day. Perikles was coming into power, and his mind was seething with all the possibilities which the development of the Athenian democracy provided, and he was ripe for the strongest idealistic teaching of his age. Anaxagoras's migration to Athens has sometimes been attributed to Aspasia, who, herself from Miletos, would be desirous of bringing to Athens as much as possible of the brilliancy and culture of Ionia. There are chronological difficulties, however, against this supposition, as Aspasia must have been too young at that time to have gained influence over Athenian society; in fact, it may be quite possible, on the contrary, that Anaxagoras was himself the cause of Aspasia's going to Athens. Perikles, in his desire for the best for his beautiful Athens, very probably himself invited Anaxagoras from Ionia to Greece.

Anaxagoras's influence over Perikles was strong, and from the congenial counsels of these two great men was brought forth a wonderful atmosphere of love of freedom and reign of reason in Athens. We can picture Athens as she was in the beginning of Perikles's power from the excavations of Dr. Dörpfeld, president of the German School of Archeology in Athens, begun in 1891, on the northwest side of the Akropolis—a primitive town with small, insignificant houses and narrow streets—and it was during the three decades of Anaxagoras's life in Athens that the marvelous changes there were produced by Perikles. Eager pursuit of knowledge and art arose. Astronomy was influencing the reckoning of time. A new Athens was building with straight, broad streets and graceful columns. Music and gymnastics were being made prominent, and on the Akropolis was beginning to blossom the highest expression of beauty ever made by the human race. In the latter part of this period books, also, were in common use, although not as yet very numerous. Peisistratos had founded a library for those who applied themselves to letters, which had passed through various vicissitudes, and the Athenians had increased it with a great deal of care. To the stock of books in existence at that time Anaxagoras made an important addition. His book did not have an original title, being called *πέρι φύσεμς*, or 'On Nature,' like many other productions of Ionian philosophers, but his ideas were original, and it was the first book to be illustrated by diagrams, with the exception of geometrical writings. In Plato's time the book was on sale for a drachma, although it is said to have consisted of several volumes. Probably the volumes may have been rather what we should call chapters. This book, alas, is no longer in existence, although we possess important fragments of it, mostly found in Simplicius's commentary on Aristotle's physics, which was written in the sixth century, at which time a copy of the book was to be had.

Let us now consider some strange phenomena in connection with the first appearance of the idea of spirit in Greek philosophy. Anaxagoras himself had the characteristics of the idealist, but his world theory and the general trend of his studies were closely allied with the teachings of his materialistic predecessors in Ionia. He could not wholly escape from his age. When Sokrates heard of Anaxagoras's book he was delighted that some one had attributed the universe to an all-pervading spirit, and immediately sent for the book; but he was greatly disappointed on reading it, as he did not find there the idealism for which he had sought. Anaxagoras belonged not to the age of Sokrates, although he was partly contemporary with him, but he belonged wholly to the Ionian school of mathematical astronomy. The thought of Anaxagoras was scientific rather than philosophic, and his book was devoted to scientific, mathematical explanations of cosmography and astronomy. The Nous was not to him the all-important part, but only a necessary cause for the beginning of motion—a secondary first cause, so to speak. Yet the idea of the Nous was sufficient to introduce rationalism into Greece, for it was the first presentation of an existing rational force wholly distinct from matter. Anaxagoras was bent upon scientific discovery, and the important things in his mind were his method and his original theory of matter. How often it happens that what seems secondary to a great man proves after all his most far-reaching service to the world. As, for example, with Plato his philosophy was secondary in his own mind to his ideas of political reform, and, while it is true that the latter have been much regarded, yet the former have revolutionized all philosophic thought. Anaxagoras's rationalism did not enable him to produce a rational theory of matter, yet it rationalized all his thought and was a steppingstone between the earlier study of nature and the later study of man. Indeed, his rationalism affected Greece through his followers, who were Perikles, Euripides and Thukydides. It is probable, also, that Themistokles studied with him at some period, perhaps when Anaxagoras was still in Asia Minor during the time of Themistokles's ostracism.

The introduction of the Nous into Greek thought changed the basis upon which rested the accepted opinions of the multitude. We see this first of all in the necessary metamorphosis of religious beliefs which began in the age of Perikles.

The first strong point of influence on the part of Anaxagoras in revolutionizing thought was in his astronomy, which was sufficiently developed to enable him to give a comparatively correct explanation of eclipses and other astronomical phenomena. It was a part of the creed of the age that the heavenly bodies were gods, and even in the time of Plato it was considered a crime not to believe in the godhead of the sun and moon. Anaxagoras asserted that the sun was not Helios, the god, but a mass of ignited stone as large as, or larger than, the Peloponnesus. He even tried to explain how it became ignited. He attempted to reduce all meteorological and elemental phenomena to law, and although some of the laws were wrong, yet the idea of law as a force in nature controlling phenomena was a rationalizing power that we can hardly compute, for according to the belief of the multitude, the gods interfered to produce these phenomena. Anaxagoras has left no writings, to our knowledge, directly on religion. The Nous even does not seem to have been a god, but rather a force; yet by introducing laws to control the outward phenomena of the universe, by one fell stroke he destroyed the deepest-seated religious ideas of those around him. The lightning blast that Zeus produced from Mt. Olympus by shaking his ægis, was accounted for sacrilegiously by Anaxagoras. The rain, the storm and the seasons the people regarded as the work of Zeus; and Anaxagoras in explaining them according to natural laws seemed to threaten the foundation of their religion. The world had been the plaything of the gods. It was now the work of a rational principle. Anaxagoras separated the gods from the procession of natural phenomena; but that he did not wish to destroy the reverence with which they were regarded is shown by the spirit in which the restoration and the enlargement of the Akropolis was undertaken, while his influence was still strong over Perikles.

Science, too, was changed by Anaxagoras, not only because he did much toward reducing to order and formulating the astronomical and cosmological theories of the time, but because he made law the basis of scientific research, and sought to find the uniformity of law in the phenomena of nature. He received a strong incentive to rational study of science in his young manhood when he had the opportunity of visiting a large meteoric stone which fell near the Aegospotamos River, on the northern shore of the Hellespont. Who can tell what his thoughts were then, as he perceived with his own eyes the material character of this messenger from the heavenly bodies, the so-called gods?

The influence of the idea of the Nous on the political life of Athens can not be estimated. Perikles was a political idealist, bent on making the most of the intellectual ability of every Athenian citizen, and the close intimacy with a man like Anaxagoras probably accounts for much of the fineness of his work and his freedom from the superstitions of his age. The ruins of the Akropolis of Athens at the present time show us something of what his idealism did for art. Anaxagoras taught that the Nous exists in all things in a greater or less degree, and the art of his age, the highest that the world has known, expresses to a degree never before attained the psychical basis of beauty.

Anaxagoras's service to philosophy was, however, the greatest, although it has not been fully appreciated. For the first time the psychical element entered into philosophic research. The Nous had to be reckoned with, as well as matter so-called, and since then we have had different grades of world theories, some of which attribute to the psychical the whole of reality, like that of Plato, some the part, as with Aristotle, and some none at all, as with the materialists. With Anaxagoras was born the idea of spirit, yet in the vague and glimmering way in which all ideas come into existence, and the gratitude of the world for this idea has been given to Sokrates and Plato, who presented it in its fulness. Anaxagoras, therefore, does not rank as great among philosophers in popular opinion, because he was so soon overshadowed by those who completed his conception of the spiritual.

When Perikles's power began to wane and he could no longer protect his friend and teacher, the vengeance of the multitude whose gods had been attacked fell upon Anaxagoras. He was cast into prison, and saved with difficulty by his pupil, and exiled to Lampsacus, on the southern shore of the Hellespont. There he organized a school of philosophy, and the Anaxagoreans are referred to occasionally by later Greek philosophers, but the school was soon overshadowed by the results of the age immediately succeeding. When Anaxagoras was ill and likely to die, his friends in Lampsacus asked him what they could do in his memory, and he replied that he would be pleased to have the anniversary of his death kept as a holiday, and this custom was long observed. The people of Lampsacus also honored Anaxagoras after his death by erecting an altar to him, bearing on one side the word 'Nous' and on the other the word 'Truth.'

The greatest tribute, however, to Anaxagoras was paid in the time of imperial Rome, a tribute of which he was not unworthy. An imperial Roman coin was issued at Klazomenai, on the reverse of which was the philosopher Anaxagoras with the globe in his hand.

SCIENTIFIC ASPECTS OF LUTHER BURBANK'S WORK

BY VERNON L. KELLOGG
PROFESSOR OF ENTOMOLOGY, STANFORD UNIVERSITY

MR. BURBANK has so far not formulated any new or additional laws of species-change, nor do his observations and results justify any such formulation, and we may rest in the belief that he has no new fundamental laws to reveal. He has indeed the right to formulate, if he cares to, some valuable and significant special conclusions touching certain already recognized evolution factors, in particular, the influence on variability of the two long-known variation-producing factors of hybridization and modification of environment. His reliance on the marked increase in variability to be got after a crossing in the second and third generations over that obvious in the first will come as a surprise to most men first getting acquainted with his work. He has got more starts for his new things from these generations than in any other way. He is wholly clear and convinced in his own mind as to the inheritance of acquired characters; 'acquired characters are inherited or I know nothing of plant life,' he says; and also convinced that the only unit in organic nature is the individual, not the species; that the so-called species are wholly mutable and dependent for their apparent fixity solely on the length of time through which their so-called phyletic characters have been ontogenetically repeated. He does not agree at all with de Vries that mutations in plants occur only at certain periodic times in the history of the species, but rather that, if they occur at all, they do so whenever the special stimulus derived from unusual nutrition or general environment can be brought to bear on them. He finds in his breeding work no prepotency of either sex as such in inheritance, though any character or group of characters may be prepotent in either sex. He believes that no sharp line can be drawn between the fluctuating or so-called Darwinian variations and those less usual, large, discontinuous ones called sports. Ordinary fluctuating variation goes on under ordinary conditions of nutrition, but with extraordinary environmental conditions come about extraordinary variation results, namely, discontinuous, sport or mutational variation. These variations are the effects of past environment also, having remained latent until opportunity for their development occurs. Starvation causes reversions, but reversions can also be produced by unusually rich nutrition. New variations are developed most often, as far as environmental influences go, by rich soil and generally favorable conditions. So-called *new* qualities are usually, if not always (the fact may sometimes not be obvious), simply new combinations of old qualities, both latent and obvious. To get a new and pleasing odor it may often be sufficient simply to lose one bad element in an old odor. So one might go on for some pages with specific conclusions or deductions reached by Burbank on a basis of experience. But it is true that he has at his command the knowledge of no new fundamental scientific principles to give him advantage over us. And yet none of us has done what Burbank has been able to do, although many of us have tried. What then is it that Burbank brings to his work of modifying organisms swiftly and extremely and definitely that others do not?

To answer this it will be advisable to analyze, in general terms, at least, the various processes which either singly, or in combinations of two or three, or all together, are used by Mr. Burbank in his work. We may roughly classify these processes and means. First, there is the importation from foreign countries, through many correspondents, of a host of various kinds of plants, some of economic value in their native land and some not, any of which grown under different conditions here may prove specially vigorous or prolific or hardy, or show other desirable changes or new qualities. Among these importations are often special kinds particularly sought for by Burbank to use in his multiple hybridizations; kinds closely related to our native or to already cultivated races which, despite many worthless characteristics, may possess one or more particular, valuable ones needed to be added to a race already useful to make it more useful. Such an addition makes a new race.

Second, the production of variations, abundant and extreme, by various methods, as (*a*) the growing under new and, usually, more favorable environment (food supply, water, temperature, light, space, etc.) of various wild or cultivated forms, and (*b*) by hybridizations between forms closely related, less closely related and, finally, as dissimilar as may be (not producing sterility), this hybridizing being often immensely complicated by multiplying crosses, *i. e.*, the offspring from one cross being immediately crossed with a third form, and the offspring of this with still another form, and so on. These hybridizations are made sometimes with very little reference to the actual useful or non-useful characteristics of the crossed parents, with the primary intention of producing an unsettling or instability in the heredity, of causing, as Burbank sometimes says, 'perturbations' in the plants, so as to get just as wide and as large variation as possible. Other crosses are made, of course, in the deliberate attempt to blend, to mix, to add together, two desirable characteristics, each possessed by only one of the crossed forms. Some crosses are made in the attempt to extinguish an undesirable characteristic.

Third, there is always immediately following the unusual production of variations, the recognition of desirable modifications and the intelligent and effective selection of them, *i. e.*, the saving of those plants to produce seed or cuttings which show the desirable variations and the discarding of all the others. In Burbank's gardens the few tenderly cared for little potted plants or carefully grafted seedlings represent the surviving fittest, and the great bonfires of scores of thousands of uprooted others, the unfit, in this close mimicry of Darwin and Spencer's struggle and survival in nature.

It is precisely in this double process of the recognition and selection of desirable variations that Burbank's genius comes into particular play. Right here he brings something to bear on his work that few other men have been able to do. It is the extraordinary keenness of perception, the delicacy of recognition of desirable variations in their (usually) small and to most men imperceptible beginnings. Is it a fragrance that is sought? To Burbank in a bed of hundreds of seedling walnuts scores of the odors of the plant kingdom are arising and mingling from the fresh green leaves, but each, mind you, from a certain single seedling or perhaps from a similar pair or trio. But to me or to you, until the master prover points out two or three of the more dominant single odors, the impression on the olfactories is simply (or confusedly) that of one soft elusive fragrance of fresh green leaves. Similarly Burbank is a master at seeing, and a master at feeling. And besides he has his own unique knowledge of correlations. Does this plum seedling with its score of leaves on its thin stem have those leaves infinitesimally plumper, smoother or stronger, or with more even margins and stronger petiole or what not else, than any other among a thousand similar childish trees? Then it is saved, for it will bear a larger, or a sweeter, or a firmer sort of plum, or more plums than the others. So to the bonfires with the others and to the company of the elect with this 'fittest' one. Now this recognition, this knowledge of correlations in plant structure, born of the exercise of a genius for perceiving through thirty years of opportunity for testing and perfecting it, is perhaps the most important single thing which Burbank brings to his work that other men do not (at least in such unusual degree of reliability). Enormous industry, utter concentration and single-mindedness, deftness in manipulation, fertility in practical resource, has Burbank—and so have numerous other breeders and experimenters. But in his perception of variability in its forming, his recognition of its possibilities of outcome, and in his scientific knowledge of correlations, a knowledge that is real, for it is one that is relied on and built on, and is at the very foundation of his success, Burbank has an advantage of true scientific character over his fellow workers, and in it he makes a genuine contribution to scientific knowledge of plant biology, albeit this knowledge is so far only proved to be attainable and to exist. It is not yet exposed in its details and may never be, however unselfish be the owner of it. For the going to oblivion of scientific data of an extent and value equivalent, I may estimate roughly, to those now issuing from any half dozen experimental laboratories of variation and heredity, is the crying regret of all evolution students acquainted with the situation. The recently assumed relations of Mr. Burbank to the Carnegie Institution are our present chief hope for at least a lessening of this loss.

But let us follow our saved plum seedling. Have we now to wait the six or seven years before a plum tree comes into bearing to know by actual seeing and testing what new sort of plum we have? No; and here again is one of Burbank's contributions (not wholly original to be sure, but original in the extent and perfection of its development) to the scientific aspects of plant-breeding. This saved seedling and other similar saved ones (for from the examination of 20,000 seedlings, say, Burbank will find a few tens or even scores in which he has faith of reward) will be taken from their plots and grafted on to the sturdy branches of some full-grown vigorous plum tree, so that in the next season or second next our seedling stem will bear its flowers and fruits. Here are years saved. Twenty, forty, sixty, different seedlings grafted on to one strong tree (in a particular instance Burbank had 600 plum grafts on a single tree!); and each seedling-stem certain to bear its own kind of leaf and flower and fruit. For we have long known that the scion is not materially influenced by the stock nor the stock by the scion; that is not modified radically, although grafting sometimes increases or otherwise modifies the vigor of growth and the extent of the root system of the stock.

If now the fruit from our variant seedling is sufficiently desirable; if it produces earlier or later, sweeter or larger, firmer or more abundant, plums, we have a new race of plums, a 'new creation' to go into that thin catalogue of results. For by simply subdividing the wood of the new branch, *i. e.*, making new grafts from it, the new plum can be perpetuated and increased at will. Simple, is it not? No, it is anything but that in the reality of doing it; but in the scientific aspects of it, easily understandable.

Perhaps it may not be amiss to call attention to what must be the familiar knowledge of most of us, and that is the fact that many (probably most) cultivated plants must be reproduced by division, that is by cuttings, buds or grafts, and not by seeds, in order to grow 'true.' For a piece of a cultivated plant will grow out to be very much like the individual it was cut from, but the seeds will not, in most cases, reproduce faithfully the parents, but will produce a very variable lot of individuals, most of them strongly reversionary in character. Grow peach trees from the stones of your favorite peach and see what manner of peaches you get; but if you want to be sure of more peaches like the ones you enjoy, graft scions from your tree on to other trees. Indeed one of the plant-breeder's favorite methods of making a start for new things, of getting the requisite beginning wealth and eccentricity of variation, is to grow seedlings, especially from cross-bred varieties. Burbank will give you a thousand dollars for a pinch of horse-radish seed. Sugar-cane seed is needed. The amelioration of many kinds of fruit and flowers and vegetables is checked, because in our carelessness we have allowed these kinds to get into that condition of seedlessness which almost all cultivated races tend toward when grown from cuttings. In our oranges and grape-fruit and in a score of other fruits, the elimination of seeds is exactly one of the modifications we have bred and selected for, in order to make the fruits less troublesome in their eating. But when we lose the seeds entirely of a whole group of related plant kinds we may find ourselves, as we have found ourselves actually in many cases, at the end of our powers of amelioration of these plant sorts. Burbank believes that the very fact that plants when grown asexually always sooner or later lose their power to produce seeds is almost sufficient proof (if such proof is needed) that acquired characters are transmitted.

Another of Burbank's open secrets of success is the great range of his experimentation—nothing is too bold for him to attempt, the chances of failure are never too great to frighten him. And another secret is the great extent, as regards material used, of each experiment. His beds of seedlings contain hundreds, often thousands, of individuals where other men are content with hundreds. Another element in his work is his prodigality of time. Experiments begun twenty years ago are actually still under way.

In all that I have so far written, I have purposely kept to general statements applicable to Burbank's work as a whole. My readers might be more interested, perhaps, to have some illustrations of the application of various processes of making new sorts of things, some analytical account of the history of various specific 'new creations,' but considerations of space practically forbid this. Just a few briefly described examples must suffice. More than is generally imagined, perhaps, Burbank uses pure selection to get new things. From the famous golden orange colored California poppy (*Escholtzia*) he has produced a fixed new crimson form by selection alone. That is, noticing, somewhere, sometime, an Escholtzia individual varying slightly redder, he promptly took possession of it, raised young poppies from its seeds, selected from among them those varying in a similar direction, raised new generations from them and so on until now he who wishes may have his California poppies of a strange glowing crimson for the price of a little package of seed, where formerly he was perforce content with the golden orange. For me the golden orange suffices, but that does not detract from my eager interest in the flower-painting methods of Mr. Burbank. Even more striking a result is his blue Shirley poppy, produced also solely by repeated selection from the crimson field poppy of Europe. "We have long had various shades of black and crimson and white poppies, but no shade of blue. Out of 200,000 seedlings I found one showing a faintest trace of sky blue and planted the seed from it, and got next year one pretty blue one out of many thousand, and now I have one almost pure blue."

But another brilliant new poppy was made in a different way. The pollen of *Papaver pilosum,* a butter-colored poppy, was put on the pistils of the Bride, a common pure white variety of *Papaver somniferum* (double), and in the progeny of this cross was got a *fire-colored single* form. The character of singleness was common to the ancestors of both parents, the character of fire color in the lineage of *somniferum* only, although the red of the new form is brighter than ever before known in the somnifera series. Both characteristics were absent (or rather latent) in both parents. And yet the perturbing influence of the hybridization brought to the fore again these ancestral characters. The foliage of this fire poppy is intermediate in type between that of the two parents.

The history of the stoneless and seedless plum, now being slowly developed by Burbank, shows an interesting combination of selection, hybridization and reselecting. Mr. Burbank found a plum in a small wild plum species with only a part of a stone. He crossed this wild species with the French prune; in the first generation he got most individuals with whole stones, some with parts of a stone, and even some with no stone. Through three generations he has now carried his line by steadily selecting, and the percentage of no-stone fruits is slowly increasing, while quality, beauty and productiveness are also increasing at the same time.

The plum-cot is the result of crossing the Japanese plum and the apricot. The plum-cot, however, has not yet become a fixed variety and may never be, as it tends to revert to the plum and apricot about equally, although with also a tendency to remain fixed, which tendency may be made permanent.

Most of Burbank's plums and prunes are the result of multiple crossings in which the Japanese plums have played an important part. Hundreds of thousands of seedlings have been grown and carefully worked over in the twenty years of experimenting with plums, and single trees

California Poppy (*Escholtzia*) rendered bright crimson as a result of selection only, without crossing.

have been made to carry as many as 600 varying seedling grafts. The Bartlett plum, cross of the bitter Chinese Simoni and the Delaware, itself a Simoni hybrid, has the exact fragrance and flavor of the Bartlett pear. The Climax, a successful shipping plum, is also a cross of the Simoni and the Japanese triflora. This Chinese Simoni produces almost no pollen, but few grains of it ever having been obtained. But these few grains have enabled Burbank to revolutionize the whole plum shipping industry. The sugar prune, which promises to supplant the French prune in California, is a selected product of a second or third generation variety of the Petite d'Agen, a somewhat variable French plum.

Next in extent probably to Burbank's work with plums and prunes

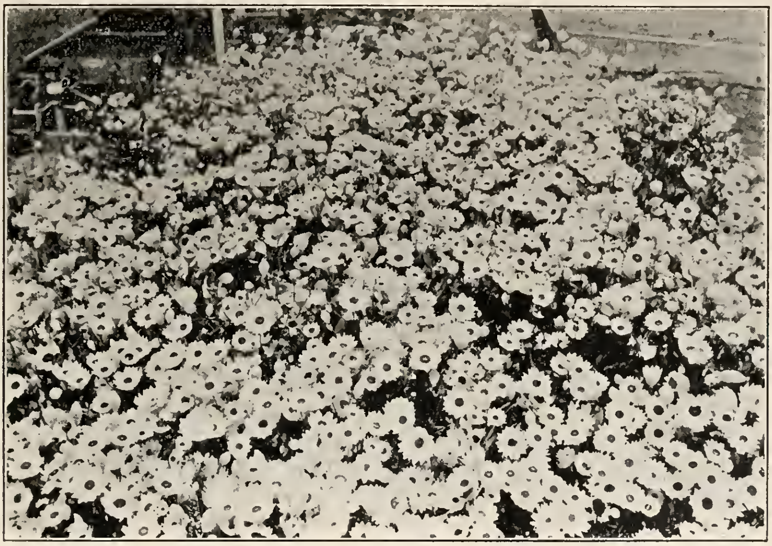
One of the Stoneless Plums (Center) and Two of its Parents. On the right hand the common French prune.

is his long and successful experimentation with berries. This has extended through twenty-five years of constant attention, has involved the use, in hybridizations, of forty different species of *Rubus,* and has resulted in the origination of a score of new commercial varieties, mostly obtained through various hybridizations of dewberries, blackberries and raspberries. Among these may specially be mentioned the Primus, a hybrid of the western dewberry (*R. ursinus*) and the Siberian raspberry (*R. cratægifolius*), fixed in the first generation, which ripens its main crop before most of the standard varieties of raspberries and blackberries commence to bloom. (Mr. Burbank does not recommend this for general cultivation; the 'Phenomenal' and Himalaya are

Early and Abundant Bearing Chestnut Tree. One of hundreds of similar ones, due to crossing and selection. These bear fruit at the age of one and a half years and are never without fruit.

better.) In this Primus berry, we have the exceptional instance of a strong variation, due to hybridization, breeding true from the time of its first appearance. It usually takes about six generations to fix a new variety, but like de Vries's evening primrose mutations, the Primus berry is a fixed new form from the time of its beginning. An interesting feature of Mr. Burbank's brief account, in his 'New Creations' catalogue of 1894, of the berry experimentation, is a reproduction of a photograph showing "a sample pile of brush 12 ft. wide, 14 ft. high, and 22 ft. long, containing 65,000 two- and three-year-old seedling berry bushes (40,000 blackberry × raspberry hybrids and 25,000 Shaffer × Gregg hybrids) all dug up with their crop of ripening berries." The photograph is introduced to give the reader some idea of the work necessary to produce a satisfactory new race of berries. "Of the 40,000 blackberry × raspberry hybrids of this kind 'Phenomenal' is the only one now in existence. From the other 25,000 hybrids, two dozen bushes were reserved for further trial."

An astonishing result of the hybridization between the black walnut, *Juglans nigra,* used as the pistillate parent, and the California walnut, *J. californica,* staminate parent, are walnut trees which grow with such an amazing vigor and rapidity that they increase in size at least twice as fast as the combined growth of both parents. Many tremendous growers are got in the first generation, but in the second there are included some of the most rapidly growing trees, perhaps, in the world. This hybrid has clean-cut, glossy bright green leaves from two to three feet long with a sweet odor like that of apples, but it produces few nuts. Curiously enough the result of hybridization by using the pollen of *nigra* on pistils of *californica* produces in abundance large nuts of a quality superior to that possessed by either parent.



Rhodanthe naglesi, improved in size and color by selection only.

The famous Shasta daisy is the result of a multiple crossing between an American and a European species of field daisy and then between these hybrids and a Japanese form. The fragrant calla, known as 'Fragrance,' is descended from a single individual found by Burbank while critically examining a block of Little Gem calla seedlings. He was surprised in this examination by a fragrance resembling that of violets or water-lilies; as he had long been seeking a fragrant calla,

Heuchera LEAVES MADE CRESTED BY SELECTION OF VARYING SEEDLINGS.

the individual giving this perfume was carefully hunted out. No farther selecting was done; this plant was the single ancestor of the fragrant new race.

And so one might go on for pages, but with slight variations in detail all these pages would tell only the same story: the stimulating or inducing of variability by environmental influences and by hybridizations; the search after, and keen recognition of, promising special variations; the selection of the plants showing these variations; rearing new generations from them, repeated selection, and new hybridizations to eliminate this characteristic or introduce that, and on until a desirable combination is found. Then the careful fixing of this form or type by repeated selection through several generations.

But an end must be made of this. Let us, in a paragraph, simply sum up the essential things in the scientific aspects of Burbank's work. No new revelations to science of an overturning character; but the revelation of the possibilities of accomplishment, based on general principles already known, by an unusual man. No new *laws* of evolution, but new facts, new data, new canons for special cases. No new principle or process to substitute for selection, but a new proof of the possibilities of the effectiveness of the old principle. No new categories of variations, but an illuminating demonstration of the possibilities of stimulating variability and of the reality of this general variability as the fundamental transforming factor. No new evidence either to help the Darwinian factors to their death-bed, or to strengthen their lease on life; for the 'man' factor in all the selecting phenomena in Burbank's gardens excludes all 'natural' factors. Here are some of

Burbank's own words, touching these matters that scientific men are particularly interested in, in his work:

All scientists have found that preconceived notions, dogmas, and all personal prejudice must be set aside, listening patiently, quietly and reverently to the lessons one by one which mother nature has to teach, shedding light on that which before was a mystery, so that all who will may see and know.

Crossing gives the raiser of new plants the only means of uniting the best qualities of each, but just as often the worst qualities of each are combined and transmitted, so that to be of any value it must be followed by rigid and persistent selection, and in crossing, as in budding and grafting, the affinities can only be demonstrated by actual test.

All wild plants of any species are under almost identical environments, having their energies taxed to the utmost in the fierce struggle for existence. Any great variation under such circumstances is not likely to occur, and is much more likely to be stamped out at once by its struggling competitors, unless the variation should be of special use in competition, in which case it will survive, and all others may be supplanted by it. Thus we see how new species are often produced by nature, but this is not her only mode. Crosses and hybrids are very often found growing wild where two somewhat similar species grow contiguous, and if the combination happens to be a useful one,

HYBRID OPUNTIA, WITHOUT THORNS OR BRISTLES, with a parent from which these appendages are to be removed by crossing and selection.

as it often does, the new creation is encouraged by nature; then time and environment fix it, and man comes on the scene, perhaps ages later, and discovers it, and, not knowing all the facts, wonders where the connecting links have gone. It is botanically classified as a new species, which it is most certainly.

In cultivated plants the life struggle is removed, and here we find variation almost the rule rather than the exception.

Varieties are the product of fixed laws, never of chance, and with a knowledge of these laws we can improve the products of nature, by employing nature's forces, in ameliorating old or producing new species and varieties better adapted to our necessities and tastes. Better food, more sunshine, less arduous competition, will of themselves induce variation in individual plants which will be more or less transmitted to their seedlings, which, selected consecutively through a certain number of generations, will become permanent. Environment here exerts an influence as in all chemical cosmical and celestial movements. These small increments from environmental forces may produce a gradual or sudden change according to circumstances. The combustion of food liberates the moving force, environment guides it as it does the planets.

When once the persistent type is broken up, old latent forces may be liberated and types buried in the dim past reappear. This, called atavism, is a concentration of ancestral forces—reverberating echoes—from varieties long since passed away, exhibiting themselves when from some cause, for instance crossing, present forces are in a state of antagonism, division, perturbation or weakness. These echoes, if collected by crossing and selection, produce combinations of superlative importance and value.

Finally, in any summation of the scientific aspects of Burbank's work must be mentioned the hosts of immensely valuable data regarding the inheritance of characteristics, the influence of epigenetic factors in development, the possibilities of plant variability, and what not else important to evolution students, mostly going unrecorded, except as they are added in mass to the already too heavy burden carried by the master of the laboratory, and as they are summed up in those actual results which the world gratefully knows as Burbank's 'new creations.'

DISCUSSION AND CORRESPONDENCE

THE EFFECTS OF IMMIGRATION ON HOMICIDE

Under this heading, Mr. Maynard Shipley has presented extensive statistical material, in the August number of The Popular Science Monthly, to prove the undesirable character of immigrants from eastern and southern Europe, as compared with those from its northern and western countries and, by inference, with native-born Americans. While I do not wish to impugn the good faith of his investigations, he manipulates the figures which he quotes in so remarkable a manner as to justify the belief that he has approached his subject with preconceived notions, which have blinded him to some of the pitfalls in so serious an inquiry. I do not wish to whitewash the immigrants, but merely to point out Mr. Shipley's errors, hoping earnestly that more careful methods may prevail in the study of large masses of our present population, if it is to guide our legislators in the adoption of a policy toward immigration. If the foreigner is to be kept out, let it be done frankly on the score of racial prejudice, but do not seek to blacken his character by the use of ill-digested statistics.

The gravest defect in Mr. Shipley's presentation is the continuous shifting of basis, without attempting to make corresponding corrections. Sometimes figures are given for homicide as a total; at others we have the division into murder and manslaughter. Presently, I shall show why manslaughter must necessarily be on the increase in American court records; hence statistics which do not distinguish it from intentional homicide are valueless as showing a criminal tendency of the population. All Mr. Shipley's comparisons that tell most severely against the recent immigrant fail to make the distinction. Manslaughter is practically the unintentional killing of a human being by another, and it is the charge upon which a man would be held, whose careless handling of machinery caused death; the introduction of cable-cars and trolleys enormously increased the number of fatal street accidents; as have the erection of steel-framed buildings, with the coincident abandonment of fixed scaffolding, the use of power-hoists, the employment of dynamite in excavating, enhanced the dangers of building operations. Did he take these developments into account? Did he realize that the man arrested for such an 'accident' was, nine times out of ten, an 'ignorant foreigner,' whom the greed of a native-born American entrusted with dangerous implements, without providing him with adequate safeguards in their use?

Again, in Table II., homicide is studied according to the nationality of the people *killed* in San Francisco, with the intention of proving the criminality of the Chinese by their relatively large mortality by violence. I admit the truth of the argument that the murderer and his victim are apt to belong to the same race; in fact a sardonic member of the Immigration Restriction League might classify such homicidal tendency among the 'undesirables' as a redeeming virtue. But, for rigid statistical purposes, Mr. Shipley has overshot the mark. His own figures show that 65 out of 100,000 Chinamen in the United States were accused of murder in the year when 75 per 100,000 were being killed; are these extra ten due to suicide or to Caucasian markmanship?

Very serious, too, is the mistake of studying *arrests for homicide*, without taking into account the relative efficiency of the local authorities. Mr. Shipley's failure to realize this is shown, not only by his indiscriminate grouping of cities in all parts of the country, but specifically by his discussion of homicide in Colorado. He compares the small number of *arrests* for this offense in Denver with the large number of *murders* in the state, properly enough emphasizing the lawless condition of the mining-camps; but he fails to ask how many Denver murderers elude a comparatively small police force, and escape into the wilderness on the many railroads radiating from the city. The question of relative police efficiency might have been studied from his Table I., giving the number of arrests for homicide in various cities. The population of New York can not be much worse than that of neighboring cities, with their factories, docks and tramp-infested freight-yards. Yet New York reports 13.23 homicide arrests per 100,000, against 9.16 in Newark and only 4.51 in 'anarchist' Paterson, Jersey City and Hoboken also reporting low figures.

Again, the attitude of the community ought to be considered, if *arrests* for homicide are made the criterion for this form of crime. Is it conceivable that the word 'lynching' should be absent from an inquiry into American murders? Not once does Mr. Shipley allude to the hundreds, nay thousands, who have participated in the lynching of negroes, of horse-thieves and 'rustlers'; were the members of these mobs enumerated, or even the crimes counted singly, how would our statistics look?

Let us now examine his study of the relation of the immigrant to homicide. First comes a diagram, Fig. 1, 'showing changing character of immigration into the United States' by three curves, showing the *percentage* of each year's total immigration, derived from northwestern Europe, southeastern Europe and 'all others.' Below is a homicide curve, which is strikingly parallel to the southeastern Europe curve. But had these curves been drawn to represent the actual numbers of each group of nationalities, instead of their percentage out of a variable whole, the parallelism would have vanished. Furthermore, the southeastern curve represents natives of Italy, Russia, Poland and Austro-Hungary, whereof the Italians form about one third. Another table, Fig. 3, gives the ratio of murderers among 1,000,000 Italians as 50.2; but that of the Poles and Magyars is shown at very much less than the French, and the large masses of Russian and Galician Jews are bunched with unclassified nationalities in a group rated at a trifle over 1 per 100,000. The peoples representing two thirds of that incriminating curve actually produced less than the average number of murderers. Bearing in mind that the criminal tendency of an immigrant could not exhaust itself in the first year after his arrival, it is evident that the homicide curve, if strongly influenced by the immigration of the last fifteen years, the period in which the Russian and Italian immigration has outnumbered the Germanic, ought to have run steeply upward, in a hyperboloid form, instead of remaining almost horizontal, and showing less of an upward tendency than during the preceding decade.

Further to incriminate the Italians, Fig. 5 arranges the states and territories in the order of homicidal statistics, and gives a graphic representation of the number of natives of northern Europe, as contrasted with a combined group of Italians, Mexicans and Chinese, living in each; the southwestern and Rocky Mountain states show the greatest mortality by violence, and by combining these three, otherwise unrelated, nationalities, their numbers are made to appear large as compared with Teutonic immigrants. In all fairness, it ought to be open to question whether the 'Mexicans' enumerated in the census are immigrants, as Mr. Shipley assumes, or Spanish-speaking residents of what was originally Mexican territory, so largely Indian that they could hardly be classified with the Italians, even on account of their 'Latin' blood. But, as for the Italians, the states containing the great bulk of that nationality have the cleanest records, if we except Louisiana and Texas, where the high murder rate is ascribed to the negroes by Mr. Shipley himself. In fact, only one inference could be properly drawn from his Fig. 5—that the racial composition of a state's population has no influence at all, as compared with the general conditions governing the life of its citizens.

As to the influence of these general conditions upon homicide, he merely discusses the relative density of population and the criminal tendency of mining as compared with manufacturing pursuits. Having gone so far, he might naturally be expected to proceed to inquire into the brutalizing effect of certain special trades, such as slaughtering—the periodical debauches of sailors in seaports—reckless disregard for the value of human life engendered by the pursuit of hazardous callings. He should have realized that adult males are far more prone to acts of violence than either women or children, and that ratios of homicide to nationality mean nothing, unless this factor be taken into account. Immigration from northern Europe brings in more females than does that from the south and east; the Chinese, according to the census of 1900, were represented by 81,534 persons, of whom 77,936, or 95.59 per cent. were males over 21. At the same time, the native population contained only 25.55 per cent. of males over 20. I have already shown that Mr. Shipley's figures for homicide are valueless, because they do not differentiate between murder and accident; but, since he has chosen to calculate the ratio per hundred thousand foreign born of each nationality, the following table corrects these ratios so as to apply to foreign-born *adult males*, by dividing them by the corresponding percentage, as calculated from Table 12, of the Twelfth Census, Volume II.

TABLE I.

Showing: I., Number homicides per 100,000 according to Shipley; II., percentage of males over 19 years, according to Census; III., number of homicides per 100,000 adult males.

Nationality.	I.	II.	III.
Irish	1	44.67 %	2.25
German	3	43.83	6.85
English	4	49.62	7.95
Poles	6.5	49.92	13
Magyar	16	50	32
French	27	52.80	51.05
Italian	50	52.29	89.13
Chinese	63.5	95.58	66.68
Mexican	119	43.52	273.5
Entire United States	10	28.78	34.38

The homicides for the entire United States are taken from Mr. Shipley's Fig. 1, for the census year, the percentage of adult males is based on the ages given in that census and the limit of 19 years was chosen, because that was the nearest approach to 18 possible under census conditions. The significance of the correction will be seen by comparing the Italian figures with those of the entire population. Mr. Shipley would make them five times as homicidal as the average, while the ratio in the third column is as three to seven.

Mr. Shipley perhaps realized this disturbing factor, though he does not mention it; for he goes out of his way to impugn the character of the immigrants' children, by quoting statistics according to which more than half the children brought before the New York City Children's Court are of Russian or Italian parentage. As homicide is expressly excluded from the jurisdiction of that court, the use of these statistics might seem irrelevant; but since the prejudice created might have some indirect influence upon the reader's judgment, I present an analysis of the report of that court for 1905, which is fuller than the earlier ones and in many ways might be more disadvantageous to the immigrant. In this analysis, the most serious interpretation has been given in doubtful cases, and yet it will be seen that the great majority of arrests are for trivial offenses, such as selling papers without a badge, breaking flowers in the parks, throwing stones in the street, etc., incidental rather to unguarded childhood than to viciousness. Over one quarter of the arrests have nothing to do with the child's character, as they are for improper guardianship or violation of the child labor law—further comment seems superfluous.

Finally, Mr. Shipley is not happy in his discussion of the conditions in individual cities; to follow him from town to town, however, would be tedious and would frequently result in a mere repetition of strictures already made upon his general inferences. We can take his comparison of Cleveland and Cincinnati, upon which he lays great stress, as an illuminative example. It appears that these two Ohio cities, of nearly equal size, present diverse conditions of nationality, Cleveland containing 46.1 per cent of foreign-born, among whom more than one third come from southern and eastern Europe, while Cincinnati has only 17.8 per cent. foreign-born inhabitants, something more than one tenth of whom come from southeastern Europe. "In Cleveland, the average of arrests for

Table II.

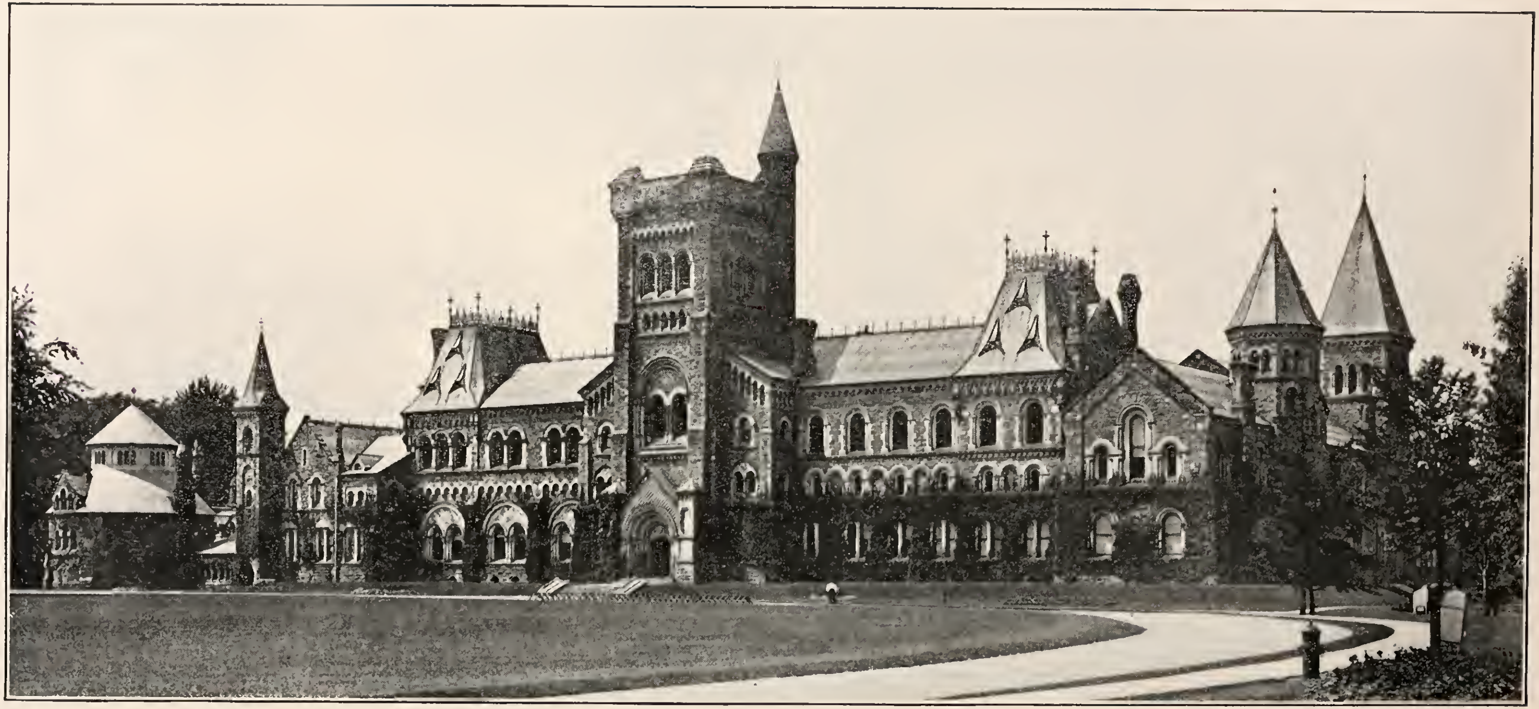
Classification of the arraignments before the Children's Court, New York City, in 1905, according to the gravity of the charges and the nationality of the children. The report furnishes no data of the nativity of the parents of children born here, but it is evident that the parents of American-born children must have been in this country at least ten years where offenses of a heinous character are involved.

	Birthplace.				
	United States.	North-western Europe.	South-eastern Europe.	Other Countries.	Discharged.
Crimes against person or property					
Felonies	809	18	133	4	419
Misdemeanors	1,175	16	211	11	580
Prostitution, etc.	94	4	26	—	25
Petty offenses of all kinds	3,177	38	339	9	767
Truancy from school	48	--	2	—	12
Disorderly and ungovernable child	563	12	104	12	182
Insanity, attempted suicide	1	—	2	—	3
Improper guardianship	1,870	16	212	25	867
Child labor law violation	427	3	54	3	19
	8,164	107	1,083	64	2,874

homicide during the two years 1903–4 was 9.56 per 100,000 of population. In Cincinnati, the average for the six years 1898–1904 was 6.23. In 1890 the disparity was still greater, the ratio being 4.04 in Cincinnati and 13.01 in Cleveland." This higher rate is ascribed at length to the influx of foreigners into Cleveland, in spite of the contrary view of the chief-of-police, from whose report the figures are derived. But is it not true that in a decade this crime has *increased* 50 per cent. in Cincinnati and *decreased* over 25 per cent. in Cleveland, in spite of the latter's growth in 'undesirable' population? Still, the foreigners in Cincinnati are not bad enough for Mr. Shipley's purpose; he therefore records that the 18.61 per cent. of foreigners in its white population allowed by the Twelfth Census furnished 64.04 per cent. of the 7,135 whites arrested in 1904; he then enumerates the arrests for murder and attempted murder and calmly asserts that 'a large proportion of these crimes were undoubtedly committed by foreign-born whites,' although the figures at his command do not seem to even admit of the usual separation of whites and negroes. Anybody ignorant of the fact that 'arrests' are made for misdemeanors as well as felonies might infer a connection between the percentages quoted above and the tendency toward homicide, which Mr. Shipley would doubtless be the first to deny; nevertheless, such careless juxtaposition must seriously inpugn the authority of an investigator.

Murder is the most atrocious crime in our penal code; before we throw the suspicion of homicidal proclivities upon hundreds of thousands of innocent immigrants, let us weigh our evidence seriously and see whether our own statistics are truthful. Is wilful homicide reported and punished uniformly throughout the country? Do our courts deal equally with the foreigner and the native, the Caucasian and the Mongolian, the rich and the poor? Is a classification into nationalities sufficient, or must age and sex also be taken into account? Are certain occupations, leading to violent habits, chosen by the immigrant from inclination or forced upon him by our social and economic conditions? Is the Italian, the Russian or the 'Hun' less amenable to law than were the founders of the Texan Republic, the 'forty-niners,' the 'Filibusters,' the 'cowboys' and the 'rustlers'? Above all, is Mr. Shipley right in asserting, on page 168 of his article, that the second generation of foreigners is always worse than the first; if that be true, what becomes of the boasted strength of our American civilization?

MORRIS LOEB.



The Main Building of the University of Toronto.

THE PROGRESS OF SCIENCE

THE BRITISH MEDICAL ASSOCIATION AND THE UNIVERSITY OF TORONTO

The British Medical Association held its seventy-fourth meeting at Toronto, beginning on August 21. There were in attendance about 1,400 British and Canadian members of the association and about 600 visitors and guests from the United States. In addition to some 300 members from the British Islands, there were delegates from India, South Africa and other widely separated parts of the British Empire, and several scientific men from the European continent. The meeting had thus many of the advantages of an international gathering, without the polyglot confusion. Dr. R. A. Reeve, dean of the Medical Faculty of the University of Toronto, made the presidential address, and there were addresses in medicine, by Sir James Barr; in surgery, by Sir Victor Horsley, and in obstetrics, by Dr. W. S. A. Griffeth. The sections covered dermatology, laryngology and otology, medicine, obstetrics and gynecology, ophthalmology, pediatrics, pathology and bacteriology, physiology, psychology state medicine, surgery and therapeutics. As usual in British scientific meetings, the social features were prominent, and the excursions numerous and well arranged.

Not the least interesting part of the meeting was the opportunity of visiting Toronto and its great university. The movement which ended in the establishment of the University of Toronto was initiated in the eighteenth century, but the institution, which was originally called King's College, was not opened until 1843. Numerous

The New Medical Building.

The Library.

changes in organization took place, and other colleges were established, which have since been affiliated with the university. It is strictly a state institution, but has entered into wide affiliations and has received large private gifts. Several of the buildings of the university are shown in the accompanying illustrations. The main building, which was the headquarters of the association, was completed in 1858, but it was partly destroyed by

The New Science Building.

fire in 1890 and rebuilt with important improvements. The new medical building was opened three years ago. The university library, erected almost wholly by private subscription, was completed in 1892. The chemical building was first occupied in 1895, and a new building for applied science was used for the first time by the section of state medicine.

THE NATIONAL PURE FOOD LAW

THE McCumber-Heyburn-Hepburn federal food and drug law goes into effect on January the first. It includes in its provisions all substances intended for human consumption. It is the most comprehensive measure ever enacted by congress for the control of interstate industries. Its provisions apply to the District of Columbia and to the territories, to exports and imports and to interstate shipments. To receive full benefits from its operation, it will be necessary for each state and city to maintain cooperative inspection over its own commerce, for the law will not apply to adulteration and misbranding practised wholly within the state.

The enforcement of any law depends upon evidence. For this reason it is difficult to enforce laws against industrial combinations and discriminations because the evidence of violation can only be had through a knowledge of the inward transactions of the interest. In enforcing the food law the secret schemes of a board of directors or a manufacturing manager will be at once liable to detection by an examination or analysis of the product put upon the market. The profession of chemistry is well equipped with means for detecting adulteration and misbranding. Methods of analyses have already been established beyond dispute, and a wide and accurate knowledge of the standard qualities of food and drug substances has been compiled. In enforcing this law, however, the effect upon health of the use of minimum quantities of antiseptics, like benzoic acid to preserve such fruit and vegetable condiments as are left open in the bottle or jar by the consumer until used up, will be controverted. Some manufacturers began in an experimental way several years ago to put up these goods without antiseptics, and several of the large firms announce that the antiseptic is no longer necessary if the product is properly sterilized and packed in a small package. Others contend that an immediate prohibition of antiseptics will destroy their business. This is one of the honest problems in the enforcement of food laws, and several of the states have found a good solution for the present in requiring plain labeling to show the name and the amount of the antiseptic used in preserving catsups, sweet pickles and similar foods. This labeling puts a competitive trade influence to work which is more effective than prohibitive statute law.

Some provisions of the law will come in conflict with previous laws enacted by congress relating to special products. Under the general law the term 'butter' would mean the unadulterated fat from milk or cream. A previous federal law, however, permits the unqualified term 'butter' to apply to milk fat which contains 'added harmless color.' Again, under the food law the term 'wine' would be defined to be the product made by the normal alcoholic fermentation of the juice of sound, ripe grapes, and 'sweet wine' would mean wine made sweet by arresting alcoholic fermentation. But a previous statute defines sweet wine to apply without added labeling to a product containing added cane or beet sugar. There should be no objection to the sale of butter containing added harmless coloring matter or wine containing added beet or cane sugar. But the interests in whose behalf these definitions were incorporated in the federal statutes wish to continue these colorations and additions without having to so inform consumers. This is wrong.

It will remain for the courts to adjust the conflict between such statutes and the general food law. The law itself contains an omnibus joker, intended by its friends to exempt certain practises of adulteration or misbranding from the other strict but fair provisions relating to deception by artificial color and to misrepresentative labels. This joker appears in the last paragraph of Section 8, in the definition of the word 'blend.' But it can be safely conjectured that when the act goes before the supreme court this paragraph will not be construed out of harmony with the other strong provisions.

A committee consisting of H. W. Wiley, chairman, from the Department of Agriculture, S. N. D. North, from the Department of Commerce and Labor, and James L. Gerry, from the Treasury Department, has been appointed by the secretaries of these departments to formulate regulations for the enforcement of the law. This committee began hearings in New York City on September 7. The various manufacturing interests will be heard, and the state departments have been asked to cooperate in suggesting regulations. At Hartford, Conn., in July, the association of food-control officials amended its constitution so as to include the food-control officials of the federal government, and an arrangement was adopted whereby there will be coalition of the food standard committee from the state analysts and the food standard committee appointed by the United States Secretary of Agriculture. This action will influence uniformity between the state laws and the national law, and bring about close cooperation between the United States Department of Agriculture and the state officials in the enforcement of food and drug control legislation.

SCIENTIFIC ITEMS.

We regret to record the deaths of Dr. H. Marshall Ward, F.R.S., professor of botany at Cambridge University; of Dr. Alexander Herzen, professor of physiology at Lausanne, and of William Buck Dwight, professor of geology at Vassar College.

Dr. A. A. Michelson, professor of physics at Chicago, has been elected a foreign member of the Accademia dei Lincei, Rome.—Dr. L. A. Bauer, of the Carnegie Institution, and Dr. John M. Clarke, state geologist of New York, have been elected corresponding members of the Göttingen Royal Academy of Sciences.

At a conference of the International Geodetic Association to be held at Budapest on September 20, the principal topics to be considered were the accurate surveying of mountain chains subject to earthquake, with a view to ascertaining whether these chains are stable or whether they rise and sink, and the taking of measures of gravity so as to throw light upon the distribution of masses in the interior of the earth and upon the rigidity of the earth's crust. The drawing up of preliminary reports on these two questions has been entrusted to M. Lallemand, director of the general survey in France, and Sir George Darwin.

THE POPULAR SCIENCE MONTHLY

NOVEMBER, 1906

UNIVERSITY CONTROL[1]

By Professor J. J. STEVENSON
NEW YORK UNIVERSITY

AMPLE justification for further consideration of problems connected with university control exists in the vastness of the interests concerned. The commissioner of education, in his last report, states that in the United States there are 607 colleges and universities, with almost 22,000 instructors and approximately 118,000 students; with property, real and personal, valued at $465,000,000 and an income in 1904 of $40,000,000. All recognize that the management of our colleges and universities as purely business enterprises is almost beyond reproach; but there is no such consensus of opinion respecting the administration of the trust itself, many believing that this is wasteful and inefficient, while some seem ready to assert that good faith has not been kept toward donors, many of whom had no definite conception of the work for which their money was given, but had confidence in the wisdom, integrity and qualifications of those to whom they entrusted the gifts.

The American university is a corporation managed by a board of trustees, often self-perpetuating, which, according to the state law, controls all details of management. The vast material interests have made necessary a separation of business affairs from those of educational work and control over the latter has been concentrated in the hands of a president, who gradually became director of the whole organization, determining not only its educational policy, but also, in

[1] Articles bearing upon this subject have appeared recently by Presidents Draper and Pritchett in the *Atlantic Monthly*, by Mr. J. B. Monroe and Professor Jastrow in *Science*, by Professor Cattell in *Science* and the *Independent*, and by Professor Stevenson in the Popular Science Monthly, all of which have been utilized in preparation of this article.

not a few instances, dominating even the financial affairs. The arguments justifying this evolution are plausible. Experience shows that in every organization, left to itself, some one man, through native force, gains control. University trustees should not permit this matter to adjust itself as the one, thus gaining control, might be swayed by wrong motives or might be ill-balanced—in either case injury would come. Far better for the trustees to select some man of all-around fitness and to recognize him as the responsible head. Acting on this principle, trustees appoint as responsible president one who from their standpoint possesses the necessary qualifications and make him practically attorney in fact for the board, giving him free hand in all departments of the work.

Advantages of the American Plan

That this procedure is good appears at once by comparison of conditions prior to the civil war with those at this time. In the earlier days, when the autocratic system existed only in germ, the resources of colleges and universities were small and increased slowly. Buildings, for the most part, were uninviting and students were few. The faculties, in most cases, were small but made up of strong men, faithful teachers, fruitful investigators. Salaries were modest, but the social conditions were equally modest, and the professor's position made up in honor what it lacked in pecuniary reward. The equipment, even in what are now great universities, was insignificant; a professor desiring to make investigations in physics or chemistry, either purchased or manufactured the necessary apparatus, while another, pursuing special studies in any branch of literature, spent his savings in gathering material. Too often the college provided rooms for teaching, the instructor provided the rest. Yet it must be conceded that the colleges did admirable work. They imparted not a great deal of knowledge, for the courses were very narrow, but there was a system in the training which sharpened many a dull intellect and made the already sharp intellect keener. The purpose confessedly was not to impart knowledge, but to train the intellect, to fit the man for professional study.

All was changed after the civil war. The material needs of the country demanded opportunity for a new type of training, adapted to the needs of men with wholly different aims. This required chiefly the imparting of knowledge with intellectual training as subordinate; not cultural studies, but studies in applied knowledge. Technical or semi-technical schools were established, and wealthy business men, on their own initiative, gave vast sums for such schools. To retain their place, the universities quickly developed along the same lines, placing technical schools alongside of those for law and medicine. This work of expansion was placed in charge of the president, who, under pressure of the new responsibilities, soon ceased to be teacher and became merely administrative officer. The splendid results of this policy are visible everywhere in all departments of our universities. Instead of simple factory-like buildings, imposing fireproof buildings surround the campus, which, in its turn, is no longer a grass plot, mowed two or three times a year, but a beautiful park; the library building is a credit to the architect and the shelves are well filled; the gymnasium is usually a noble building, a proof of anxiety for the physical well-being of students; the laboratories are equipped elegantly and abundantly; the museums are impressive; the mechanical workshops are marvels of completeness; students, in the old as well as in the new courses, formerly counted by scores are counted now by hundreds, and the number of instructors has increased proportionately; in material resources, the unit is no longer tens of thousands, but hundreds of thousands of dollars; the gifts to educational institutions during the last forty years make a sum so vast as to be almost incredible. The history of college growth in material resources during the last four decades is like a leaf from the Arabian Nights and Aladdin's lamp seems no longer a fairy tale. This history tells, too, of devotion and suffering on the part of some college presidents as noble as that of the early martyrs, and deserving a measure of honor which will never be given, as theirs was the day of 'small things.'

Disadvantages of the American Plan

But all this is only one side of the picture, that which presents itself to a merely casual observer; it is the purely material side. There is another side, not so patent in some of its aspects, yet so apparent in others that even the newspaper humorist, that most casual of observers, has not failed to detect and to utilize it. The thoughtful observer, familiar in some degree with matters of education, is led soon to doubt if, in this great development, the interests of education have been regarded as paramount. He asks respecting endowments, and learns, not altogether to his amazement, that in the rush they have been overlooked; and he may learn too that by the acquisition of buildings, the available resources of the college have been lessened, as both giver and receiver failed to provide for maintenance; he may discover also that in carrying out plans of one sort or another, obligations were assumed before means were secured to meet them. It is wise to examine the other side somewhat minutely.

Effect on the Trustees.—On one hand, the growth of financial interests has made compulsory the appointment of successful business men to the boards, while the urgency for additional funds has led to selection of men very prominent in all callings—extremely busy men. On the other hand, the extraordinary growth, internally, of the colleges and universities has made no longer possible for the trustee that familiar acquaintance with professors and with the departmental needs which he ought to have. Ordinarily, one finds in a board two or three members who become so attached to some school or department as to give genuine attention to its affairs and who do gain much information respecting it; but most of the others have no leisure, or think they have none. To make this isolation complete, there is no official communication with the faculties except through the president, for cases are very rare in which the faculties have representatives in or before the board of trustees. Unfamiliar with educational affairs, unacquainted with the needs of the college under their care, too often without personal knowledge of the professors or their qualifications, these trustees select a president. Recognizing their inability to perform the duties devolving upon them under the law, they practically transfer their responsibilities to their appointee, and thereafter their principal function seems to be simple legalization of his acts. Although the average trustee of to-day is a far abler man than his predecessor of a generation ago, circumstances have made him far less efficient as trustee; in too many instances he is director in name only and many men seem to assume the office with as little sense of responsibility as though they were to be directors in a corporation of which one man holds a controlling interest. The creature has become greater than his creator and the board of trustees has lost even its old-time efficiency as 'a pipe-line for shekels.'

Effect on the President.—Formerly, the president was to all intents simply a professor with some additional responsibilities for which he received additional remuneration. But the president of this day is very different. His duties have been summed up recently by Dr. A. S. Draper, and the catalogue as given is sufficiently interesting to deserve at least partial reproduction. The president must

> see that the property is cared for; that the teachers are efficient; that proper men are found to fill the chairs; that the institution's work is organized properly; that the resources are assigned rightly to the several departments
>
> Decide the lines along which the institution should develop; uphold proper ideals and make them attractive to real men—old and young; be forehanded and peer into the future; initiate policies; puncture fallacious logic and much of it; augment the resources of the institution; make the whole efficient for increasing service; manage and guide students, who must be dealt with individually; construct as well as administer; declare the best university opinion concerning popular movements and serious interests of the state; connect the university with the life of the multitude; exert university influence for quickening and guiding public opinion; be able to work harmoniously with others;
>
> but he must work out his official course for himself.

As Dr. Draper is not speaking of the small college with one hundred students, no one will be disposed to dispute his assertion that the position calls for a rare man. It may be added that a board of trustees competent to make intelligent choice of this rare man would be composed of still rarer men. If they should be fortunate enough to find him they could not keep him, for such ability is in demand, and some life insurance company would soon offer him several times the salary for a small fraction of the work. Undoubtedly Dr. Draper has summarized the requirements as they are idealized in some minds and no doubt there are men who feel assured that he has described the work they actually do, the whole work of the trustees as well as the whole internal work of the institution aside from teaching. Beyond all question, there are those who attempt this. But appointments are not made on the basis of this rare, broad qualification. The only question is as to the candidate's ability to meet the requirement which the board thinks most urgent—usually one which in the list seems to be of rather secondary importance. And one may not censure the board for this. As the number of colleges is far beyond the country's needs, financial stringency is ordinarily the only requirement with which the trustee is familiar. The selection, as a rule, is not made because the candidate is qualified to control an educational institution, but rather without any reference to that matter. As a rule the appointee is not a teacher. He is apt to entertain great respect for education and none too much for educating or educators.

The newly-appointed president may or may not have an ideal—that is unimportant. He finds quickly, except in some of the older universities, that the board of trustees has an ideal; that board expects a commercial success, more money, more students. The president's path is marked out for him; he is not to be successor to Hopkins, Witherspoon or Day; he is to be a wandering mendicant, exposed to rebuffs and disappointments of the most galling type; he is to feel that prospective heirs look on him as attempting to rob the widow and orphan. However sharply one may assert that the president's office, as it now exists, is an injury to higher education, he must recognize the heroic sense of duty which prevents so many presidents from abandoning their posts.

The most serious matter in this connection is the complete alienation of the president from the work of teaching. In the smaller institutions, where he is still professor of some branch of philosophy, his work as teacher is wholly subordinate to that as traveling collector of funds. In the larger universities, teaching is impossible, and the president is simply managing officer of a great corporation, with buttons on his desk which keep him in touch with managers of departments. His work is purely administrative, and in the very nature of the case he comes to regard all within the corporation's range as his subordinates. If he have had thorough training as a college professor, it is possible for him to retain some touch with the educational side; but if, as is usually the case, he have had no such training, his interest in educational matters is apt to become purely academic. And such a condition is fully in accord with the popular notion respecting the president's duties; the position needs a man of great executive capacity, great energy, magnetic personality, capable of keeping himself in public view, so as to advertise the university, to attract students and to increase the resources. Recently a new president was chosen for a promising young college. Interviews with trustees and others appeared promptly in the newspapers stating that with this man's magnetism, the institution will have a million dollars and a thousand students within ten years—not a word about education or elevation of grade. A notable illustration is the frequent reference to President Roosevelt as the proper successor to President Eliot of Harvard—though every thoughtful man at all familiar with university needs and objects must recognize that President Roosevelt, with all his remarkable ability, has not the qualifications required for control of a university, large or small.

Yet this officer, becoming every year less and less fitted to preside over educational affairs, becomes each year more firmly fixed as autocrat, for, if at all successful in raising money, he soon develops into the administration. The trustees may be restless, when ignored, but that is unimportant, for they know very little about the institution, while ordinarily the trespass upon their prerogative is so gradual that no new advance is sufficient to justify decisive action. The president, originally a lawyer, clergyman or business man, has sole power over appointments of professors, over the fixing of their salaries and over the curriculum itself, for he may establish a new chair at any time. It is not too much to say that the office of college president, as it exists in most of our colleges and universities, is the great menace to higher education in America.

Effect on the professors.—The all-essential portion of the university is the teaching staff; it does the work for which the college or university was founded; all other portions of the organization, trustees, president and 'what not' were intended for the encouragement and strengthening of this staff. Under the American system, the relations have been reversed.

There seems to be a deliberate attempt to convince the community that college professors are singularly child-like in simplicity and in lack of business capacity. One president has dilated on the unworldliness of college professors, and has left the impression that he thinks low salaries not altogether bad as they tend to encourage high thinking and indifference to worldly affairs. Another describes the ideal trustee in glowing terms, he stands transfixed while contemplating the majesty of the president, but in the professors he finds only material for ridicule. Teachers as a rule are impractical; faculty meetings seem to be but burlesques, and he clinches his description of unfitness by the broad assertion that faculties can not be entrusted with the selection of professors. In reading such statements one can not repress amazement that men so efficient as many college professors are in executive work, in political affairs, in corporations of many types, should lose all as soon as they come into contact with their life's work. At the same time he finds comfort in remembering that some important colleges in this country exist to-day only because professors assumed the business burden when trustees had thrown it down in despair; and he can not forget that the most successful presidents, judged even from the ordinary standpoint of success, were chosen from the faculties of the colleges over which they preside. One is at least safe in asserting that the training of college professors in business matters is quite equal to that of men in the clerical profession, from which so many college presidents have been selected.

Both trustees and presidents act on the principle that professors need guardians. The college faculties, especially, are practically ignored; little by little their authority has been curtailed until now it extends little beyond the class-room. In some of the larger institutions, faculties no longer choose their officers. Faculty meetings in some departments are unimportant affairs, and professors attend them as they perform other unimportant things, because they are on the list of duties. Certainly the meetings are characterized by pointless discussions, but this is due to the presiding officer, the president himself or his representative, who lets go his hold on the tiller and leaves the craft to wander at will. But there is no reason why the discussions should be other than aimless; decisions carry no weight except in matters wholly insignificant. The board of trustees in its innocence is available to correct any erroneous decision. Professor Jastrow refers to a case in which the faculty was informed that its action was a matter of indifference, that the trustees would decide the matter as the president wished. The writer has learned of another case in which the faculty received no such preliminary information, but was permitted to waste its energies in long and careful consideration of a proposition involving an important principle. Not many days after the decision was reached, the faculty was called together to receive information that their action had been overslaughed by the trustees. How much interest or importance should attach to faculty meetings is not difficult to comprehend.

Some newspapers have much to say respecting subordination of professors to millionaires who have given large sums to colleges. The writer has found none of this among professors and he has yet to find the giver who has shown desire to meddle with 'professorial freedom.' But there is a subordination which the writer knows leads young men to despise the professor's calling, leads them, in Mr. Monroe's words, to look with scorn upon a calling in which the individual is annihilated. The president, too often a graduate from one of the narrowest courses in college, too often belonging for much of his later life to a dogmatic profession, has power to make and does make appointments on his own motion to chairs in literature, philosophy as well as in pure and applied science; he controls promotions and salaries; the professors are subordinate to him individually as the faculties are collectively. Young men, knowing the conditions, refuse to enter the calling; others, ignorant of them, spend years in preparation and enter the calling only to find, when too late to escape, that their ideal was as a will o' the wisp.

The assertion is made that, were it not for this control, professors would become perfunctory in their work. Some kind of control is needed even for the best of men that matters be not at loose ends, but it should be intelligent to be efficient. A successful business man, an eminent lawyer, a great clergyman, would not prove efficient as superintendent of a shop for grinding microscope lenses or for manufacturing chronometers. And if occasionally he expressed opinions belittling the skill required for the work or showed preference for quantity rather than for quality of work, his control would be of doubtful value. This is a condition in too many colleges, with the result that the president is on one side, the faculty on the other, with nothing but distrust in common.

It is strange that so few college professors become perfunctory in their work. They receive little personal recognition. If they exert themselves to build up the library, museum or apparatus, if they induce an acquaintance to give an endowment, all these are so many packages thrown into the president's basket of achievements. Their services are not acknowledged even in a material way. Their salaries are petty; the salary of a rowing coach in a great university is larger than that of an assistant professor who has done efficient work for many years; in case of urgent deficit, the first relief suggested is in reduction of the professors' salaries. In other professions, experience and efficiency lead to promotion; in this other matters prevail, and too often a young man, untried, is appointed at higher salary than that received by older men of well-ascertained efficiency. It is surprising that so few men come to share the apparent opinion of president, trustees and many students that their work is of only incidental importance. Yet there is no reason why college professors should be more transcendental than other men.

These statements may seem strange to many persons of wealth. The needs of the 'poor self-denying professor' are exploited so frequently by college presidents in their appeals to generously-inclined people that the 'poor professor' is almost a byword. In truth the professor is often poor enough, but he is not guilty of exploiting his poverty or of seeking praise for self-abnegation. In any event, he has profited little from large gifts, which too often take the shape of buildings, thereby increasing the running expenses and endangering the already too small salaries. There is, indeed, sign of awakening conscience, for one day last summer it was announced that a college had received a considerable sum of money and that the salaries would be increased at once. Harvard has received a great sum, whose income is to be devoted solely to endowment; while the presidents of two other great universities have announced that increase of salaries is the most urgent need. It must be remembered, however, that these universities are in large cities, where the salaries, though counting large in dollars, have comparatively small purchasing power. Five thousand dollars in New York city is actually less than two thousand in many a college town, while two thousand dollars in that city means living in conditions incredibly narrow to dwellers in villages. This matter of salary is, however, relatively unimportant. The all important matter for consideration is the insignificant position of the professor in the organism of which he is the all-important element. These words are written with due deliberation. During his almost forty years of service, the writer has seen the gradual evolution of the president in American colleges and the resulting decadence of the professor.

Effect on Higher Education.—The American university is a great business corporation, conducted on business principles. The sense of ownership is as marked in president and trustees as though the corporation had been formed to make drugs or to build ships and they held all the stock. Within a few months, we have seen the spectacle of two educational corporations endeavoring to unite their properties under one control, though the faculties were opposed to the union. Intervention by the courts was necessary to prevent consummation of the deal. A few years earlier, negotiations of somewhat similar character were conducted between two other institutions, without any reference whatever to the faculties' opinion—properly enough, too, if, as stated by one of the trustees, the professors are merely employees of the corporation. The justification for such procedure is that men outside of boards of instruction see things from a higher plane than do those inside. One must refrain from commenting on this plea.

The anxiety to have the corporation do a big business makes number of matriculants quite as important, to say the least, as the character of instructors or instruction. Summer schools, at first mere incidents, are now recognized parts of several universities, and even modest colleges are not without them. They are important, affording opportunity for instruction in all subjects from Greek up or down to kitchen-gardening and dancing, affording great opportunity for cultivating the social side and adding notably to the list of matriculants. Appendages affording side passages to degrees are as welcome as summer schools, as they benefit a worthy class and add to the matriculant list. The correspondence school has not gained full recognition, but the importance of the others justifies the hope of its founders. This type of expansion has been at the expense of efficiency. New schools, new courses, are added, the catalogue becomes more bulky each year, but the number of instructors is increased in small proportion. The instructors become mere lesson-hearers. In one institution, professors offer twenty to thirty-one hours a week of actual class-room work in various schools. How much energy remains for genuine study is not difficult to determine. One need not wonder that college professors no longer lead in investigation and discovery. This anxiety for bigness has led to the prominence of semi-professional athletics, to the lowering of standards that college champions may 'get through,' to the lowering of ideals and even of morals. A student expressed well the general sentiment of his class when Columbia took its stand against certain forms of athletics—"What is Columbia coming to anyway? It's going to be nothing but an educational factory."

Conceding all that is claimed for the present system, the question still remains, Has the gain equalled the cost?

No candid man, who has examined the subject carefully, who has studied many colleges, will answer the question affirmatively. It matters not how firmly he is attached to the present system, he must acknowledge that the results, from an educator's standpoint, are not commensurate with the expenditures—more, that in some directions it has led to positive waste. If one look over a pile of college catalogues from different parts of the country, he will find whole broods of academies masquerading as colleges, even as universities, with one twentieth to one fourth of their pupils taking college studies, with a long list of teachers, with a president traveling over the country, prating on the advantage of the 'small college,' pleading the cause of the 'poor professor' and working on denominational prejudice to make good the annual deficit of which his salary and traveling expenses form a large part. There is something wrong in a system which creates a public sentiment such as permits a half-million dollar gymnasium or an immense stadium for semi-professional intercollegiate contests to be heralded as a gift to education; that receives gifts for scholarships and fellowships with as much acclaim as gifts for endowments; that points to piles of masonry and to mere lists of matriculants as proofs of success, that places the college on the basis of the shop and proves economy of management by showing as many clerks as possible on a minimum of expenditure. As far as true educational work is concerned, it is not too much too say that a very large part of the gifts might as well as not have been withheld.

Suggestions Looking to Reform

As has been said many times respecting other matters, things have reached such a pass that a change for the better must come soon. But the change will not come of itself, it must be brought about. Some have suggested that there be frequent consultations of college presidents; others, that the presidents and representatives of boards should meet for conference. But there is no promise of relief in such suggestions; whatever of promise there is looks rather toward making matters worse. There is no possibility of change for the better until there is full recognition in practise of the academically undisputed fact that the university in its essence is educational, all else about it being purely incidental. With this will come recognition of another fact, that no one should be entrusted with the executive duties of a university or college unless he have had as thorough training for the post as that required of bank presidents. It will be recognized also that choice of this executive officer should be made by those whose special training has fitted them to judge respecting the qualifications of a candidate. A board of clergymen and college professors, no matter how eminent they might be, would not be thought competent to select a president for one of our great banks. Even before recognition of these facts, men should see that no return to a proper ideal is possible so long as the whole policy of a university is dictated by one man. Recent explosions in the business world have proved this true for commercial corporations; it is equally true for educational corporations, more important than the others, in that their influence is not local and temporary.

The teaching staff must be recognized as the all-important part of the institution, for whose support and encouragement all other parts exist. The presidential wedge, now constantly widening the gap between the business and the educational interests, must be removed and the gap closed. The business man and the teacher must be brought into contact, the inevitable result being, as Mr. Monroe has said, great profit to both.

The organization of many universities is so complex that genuine re-adjustment can not be effected rapidly and a *modus vivendi* is necessary during the interval. This is possible with merely statutory changes. Faculties should elect their own officers. No change in the curriculum or assignments, no subdivision of chairs or creation of new ones should be made except upon recommendation of a committee for each faculty, consisting of trustees appointed by the board and of professors elected by the faculty concerned. Equally, no appointment to the teaching staff should be made except upon recommendation by the same committee. In respect to matters affecting the relations of two or more faculties, the trustees should act only on the recommendation of a senate consisting of the deans and a representative elected by each faculty. All faculty elections should be by ballot without previous open nomination.

But this is merely palliative; organic change is necessary to secure permanency.

Edinburgh university, burdened during a long period by unintelligent control, gained freedom after a severe struggle in which the faculties were led by Sir William Hamilton. The conflicting interests were so many that reorganization was a much more complicated problem than that for our universities. The corporate body of the university is the university court, answering to the American board of trustees, as it has final control over all matters, except appointment to certain chairs, which are under patronage. A general council, consisting of certain officers, the professors and such alumni as have paid, once for all, a stipulated fee, elects a chancellor and four members of the university court. The chancellor is not a member of the court but appoints a representative. All acts of the court involving organic change must receive his sanction, but aside from this he seems to have no serious responsibility. The Senatus Academicus, consisting of the professors, elects four of its own number to the court; the individual faculties elect their own officers and control their internal affairs, subject to appeal to the court. The matriculated students elect a rector, who is presiding officer of the court, to which he appoints a member. The Town Council of Edinburgh elects one member of the court and a principal, whose chief duty is to preside at meetings of the senatus. There are some other regulations due to peculiar conditions surrounding the university, but they are immaterial here. Under this new system, the resources of the university have increased enormously, the courses of instruction have ceased to be medieval, while the number of students, in spite of constantly increasing severity of requirements, has multiplied several fold. This method has the merit of utilizing to the best degree all groups connected with the university, while the faculties are brought into close touch with each other and with all matters affecting university interests.

Many suggestions have been made with especial view to American conditions. Professor Cattell has summarized these, making important additions of his own. The essential features of his presentation are:

The corporation should consist of professors, alumni and members of the community—in the case of a state university, the people should elect part of the corporation; trustees, chosen by this corporation to care for the secular affairs, should elect a chancellor and a treasurer. The professors should elect a president, a man with expert knowledge of education and university administration, whose salary, dignity and powers should be similar to those of individual professors. The university should be divided into schools as units, each of which should elect its dean and executive committee and should nominate men to fill vacancies. These nominations should be subject to approval by a board of advisers, consisting of professors from the school concerned, from related schools and from outside of the university; but final election should be by the university senate, and subject to veto by the trustees. Each school should have educational as well as financial autonomy and its property should be a trust fund for its benefit. Representatives from all schools should be elected to a senate, which should legislate for the university as a whole and should be co-ordinate with the trustees.

Reorganization along the lines of either method would bring about the desired result. The faculty's influence would pervade the whole institution and the work throughout would be directed to educational ends.

Undoubtedly were such changes effected others would come about. In all probability, catalogues, for a time, would show a decreased number of matriculants and possibly the total of benefactions reported to the commissioner of education would be diminished seriously. The writer has long believed with Professor Jastrow that decrease in benefactions and in the number of matriculants would not be serious evils. While the number of matriculants might be less, the proportion of students would be more and while the total of benefactions might be less, the amount devoted to genuine educational work would be more. The number of colleges might be reduced—certainly no calamity. Many so-called colleges, described by the editor of a prominent religious paper as academies with a college annex, might disappear. As academies with a few teachers, they could pay their way—with their petty college annex, they have a deficit. These gone, there would be relief to the generously-inclined; and a great amount of money, no longer wasted on them, would be available for other purposes.

THE VALUE OF SCIENCE

By M. H. POINCARÉ

MEMBER OF THE INSTITUTE OF FRANCE

Chapter III. The Notion of Space

1. *Introduction*

In the articles I have heretofore devoted to space I have above all emphasized the problems raised by non-Euclidean geometry, while leaving almost completely aside other questions more difficult of approach, such as those which pertain to the number of dimensions. All the geometries I considered had thus a common basis, that tridimensional continuum which was the same for all and which differentiated itself only by the figures one drew in it or when one aspired to measure it.

In this continuum, primitively amorphous, we may imagine a network of lines and surfaces, we may then convene to regard the meshes of this net as equal to one another, and it is only after this convention that this continuum, become measurable, becomes Euclidean or non-Euclidean space. From this amorphous continuum can therefore arise indifferently one or the other of the two spaces, just as on a blank sheet of paper may be traced indifferently a straight or a circle.

In space we know rectilinear triangles the sum of whose angles is equal to two right angles; but equally we know curvilinear triangles the sum of whose angles is less than two right angles. The existence of the one sort is not more doubtful than that of the other. To give the name of straights to the sides of the first is to adopt Euclidean geometry; to give the name of straights to the sides of the latter is to adopt the non-Euclidean geometry. So that to ask what geometry it is proper to adopt is to ask, to what line is it proper to give the name straight?

It is evident that experiment can not settle such a question; one would not ask, for instance, experiment to decide whether I should call *AB* or *CD* a straight. On the other hand, neither can I say that I have not the right to give the name of straights to the sides of non-Euclidean triangles because they are not in conformity with the eternal idea of straight which I have by intuition. I grant, indeed, that I have the intuitive idea of the side of the Euclidean triangle, but I have equally the intuitive idea of the side of the non-Euclidean triangle. Why should I have the right to apply the name of straight to the first of these ideas and not to the second? Wherein does this syllable form an integrant part of this intuitive idea? Evidently when we say that the Euclidean straight is a *true* straight and that the non-Euclidean straight is not a true straight, we simply mean that the first intuitive idea corresponds to a *more noteworthy* object than the second. But how do we decide that this object is more noteworthy? This question I have investigated in ‘Science and Hypothesis.’

It is here that we saw experience come in. If the Euclidean straight is more noteworthy than the non-Euclidean straight, it is so chiefly because it differs little from certain noteworthy natural objects from which the non-Euclidean straight differs greatly. But, it will be said, the definition of the non-Euclidean straight is artificial; if we for a moment adopt it, we shall see that two circles of different radius both receive the name of non-Euclidean straights, while of two circles of the same radius one can satisfy the definition without the other being able to satisfy it, and then if we transport one of these so-called straights without deforming it, it will cease to be a straight. But by what right do we consider as equal these two figures which the Euclidean geometers call two circles with the same radius? It is because by transporting one of them without deforming it we can make it coincide with the other. And why do we say this transportation is effected without deformation? It is impossible to give a good reason for it. Among all the motions conceivable, there are some of which the Euclidean geometers say that they are not accompanied by deformation; but there are others of which the non-Euclidean geometers would say that they are not accompanied by deformation. In the first, called Euclidean motions, the Euclidean straights remain Euclidean straights, and the non-Euclidean straights do not remain non-Euclidean straights; in the motions of the second sort, or non-Euclidean motions, the non-Euclidean straights remain non-Euclidean straights and the Euclidean straights do not remain Euclidean straights. It has, therefore, not been demonstrated that it was unreasonable to call straights the sides of non-Euclidean triangles; it has only been shown that that would be unreasonable if one continued to call the Euclidean motions motions without deformation; but it has at the same time been shown that it would be just as unreasonable to call straights the sides of Euclidean triangles if the non-Euclidean motions were called motions without deformation.

Now when we say that the Euclidean motions are the *true* motions without deformation, what do we mean? We simply mean that they are *more noteworthy* than the others. And why are they more noteworthy? It is because certain noteworthy natural bodies, the solid bodies, undergo motions almost similar.

And then when we ask: Can one imagine non-Euclidean space? that means: Can we imagine a world where there would be noteworthy natural objects affecting almost the form of non-Euclidean straights, and noteworthy natural bodies frequently undergoing motions almost similar to the non-Euclidean motions? I have shown in 'Science and Hypothesis' that to this question we must answer yes.

It has often been observed that if all the bodies in the universe were dilated simultaneously and in the same proportion, we should have no means of perceiving it, since all our measuring instruments would grow at the same time as the objects themselves which they serve to measure. The world, after this dilatation, would continue on its course without anything apprising us of so considerable an event. In other words, two worlds similar to one another (understanding the word similitude in the sense of Euclid, Book VI.) would be absolutely indistinguishable. But more; worlds will be indistinguishable not only if they are equal or similar, that is, if we can pass from one to the other by changing the axes of coordinates, or by changing the scale to which lengths are referred; but they will still be indistinguishable if we can pass from one to the other by any 'point-transformation' whatever. I will explain my meaning. I suppose that to each point of one corresponds one point of the other and only one, and inversely; and besides that the coordinates of a point are continuous functions, *otherwise altogether arbitrary*, of the corresponding point. I suppose besides that to each object of the first world corresponds in the second an object of the same nature placed precisely at the corresponding point. I suppose finally that this correspondence fulfilled at the initial instant is maintained indefinitely. We should have no means of distinguishing these two worlds one from the other. The relativity of space is not ordinarily understood in so broad a sense; it is thus, however, that it would be proper to understand it.

If one of these universes is our Euclidean world, what its inhabitants will call straight will be our Euclidean straight; but what the inhabitants of the second world will call straight will be a curve which will have the same properties in relation to the world they inhabit and in relation to the motions that they will call motions without deformation. Their geometry will, therefore, be Euclidean geometry, but their straight will not be our Euclidean straight. It will be its transform by the point-transformation which carries over from our world to theirs. The straights of these men will not be our straights, but they will have among themselves the same relations as our straights to one another. It is in this sense I say their geometry will be ours. If then we wish after all to proclaim that they deceive themselves, that their straight is not the true straight, if we still are unwilling to admit that such an affirmation has no meaning, at least we must confess that these people have no means whatever of recognizing their error.

2. *Qualitative Geometry*

All that is relatively easy to understand, and I have already so often repeated it that I think it needless to expatiate further on the matter. Euclidean space is not a form imposed upon our sensibility, since we can imagine non-Euclidean space; but the two spaces, Euclidean and non-Euclidean, have a common basis, that amorphous continuum of which I spoke in the beginning. From this continuum we can get either Euclidean space or Lobachevskian space, just as we can, by tracing upon it a proper graduation, transform an ungraduated thermometer into a Fahrenheit or a Réaumur thermometer.

And then comes a question: Is not this amorphous continuum that our analysis has allowed to survive a form imposed upon our sensibility? If so, we should have enlarged the prison in which this sensibility is confined, but it would always be a prison.

This continuum has a certain number of properties, exempt from all idea of measurement. The study of these properties is the object of a science which has been cultivated by many great geometers and in particular by Riemann and Betti and which has received the name of analysis situs. In this science abstraction is made of every quantitative idea and, for example, if we ascertain that on a line the point *B* is between the points *A* and *C*, we shall be content with this ascertainment and shall not trouble to know whether the line *ABC* is straight or curved, nor whether the length *AB* is equal to the length *BC*, or whether it is twice as great.

The theorems of analysis situs have, therefore, this peculiarity that they would remain true if the figures were copied by an inexpert draftsman who should grossly change all the proportions and replace the straights by lines more or less sinuous. In mathematical terms, they are not altered by any 'point-transformation' whatsoever. It has often been said that metric geometry was quantitative, while projective geometry was purely qualitative. That is not altogether true. The straight is still distinguished from other lines by properties which remain quantitative in some respects. The real qualitative geometry is, therefore, analysis situs.

The same questions which came up apropos of the truths of Euclidean geometry, come up anew apropos of the theorems of analysis situs. Are they obtainable by deductive reasoning? Are they disguised conventions? Are they experimental verities? Are they the characteristics of a form imposed either upon our sensibility or upon our understanding?

I wish simply to observe that the last two solutions exclude each other. We can not admit at the same time that it is impossible to imagine space of four dimensions and that experience proves to us that space has three dimensions. The experimenter puts to nature a question: Is it this or that? and he can not put it without imagining the two terms of the alternative. If it were impossible to imagine one of these terms, it would be futile and besides impossible to consult experience. There is no need of observation to know that the hand of a watch is not marking the hour 15 on the dial, because we know beforehand that there are only 12, and we could not look at the mark 15 to see if the hand is there, because this mark does not exist.

Note likewise that in analysis situs the empiricists are disembarrassed of one of the gravest objections that can be leveled against them, of that which renders absolutely vain in advance all their efforts to apply their thesis to the verities of Euclidean geometry. These verities are rigorous and all experimentation can only be approximate. In analysis situs approximate experiments may suffice to give a rigorous theorem and, for instance, if it is seen that space can not have either two or less than two dimensions, nor four or more than four, we are certain that it has exactly three, since it could not have two and a half or three and a half.

Of all the theorems of analysis situs, the most important is that which is expressed in saying that space has three dimensions. This it is that we are about to consider, and we shall put the question in these terms: When we say that space has three dimensions, what do we mean?

3. *The Physical Continuum of Several Dimensions*

I have explained in 'Science and Hypothesis' whence we derive the notion of physical continuity and how that of mathematical continuity has arisen from it. It happens that we are capable of distinguishing two impressions one from the other, while each is indistinguishable from a third. Thus we can readily distinguish a weight of 12 grams from a weight of 10 grams, while a weight of 11 grams could neither be distinguished from the one nor the other. Such a statement, translated into symbols, may be written:

$$A = B, \quad B = C, \quad A < C.$$

This would be the formula of the physical continuum, as crude experience gives it to us, whence arises an intolerable contradiction that has been obviated by the introduction of the mathematical continuum. This is a scale of which the steps (commensurable or incommensurable numbers) are infinite in number, but are exterior to one another instead of encroaching on one another as do the elements of the physical continuum, in conformity with the preceding formula.

The physical continuum is, so to speak, a nebula not resolved; the most perfect instruments could not attain to its resolution. Doubtless if we measured the weights with a good balance instead of judging them by the hand, we could distinguish the weight of 11 grams from those of 10 and 12 grams, and our formula would become:

$$A < B, \quad B < C, \quad A < C.$$

But we should always find between A and B and between B and C new elements D and E, such that

$$A = D, \quad D = B, \quad A < B; \quad B = E, \quad E = C, \quad B < C,$$

and the difficulty would only have receded and the nebula would always remain unresolved; the mind alone can resolve it and the mathematical continuum it is which is the nebula resolved into stars.

Yet up to this point we have not introduced the notion of the number of dimensions. What is meant when we say that a mathematical continuum or that a physical continuum has two or three dimensions?

First we must introduce the notion of cut, studying first physical continua. We have seen what characterizes the physical continuum. Each of the elements of this continuum consists of a manifold of impressions; and it may happen either that an element can not be discriminated from another element of the same continuum, if this new element corresponds to a manifold of impressions not sufficiently different, or, on the contrary, that the discrimination is possible; finally it may happen that two elements indistinguishable from a third, may, nevertheless, be distinguished one from the other.

That postulated, if A and B are two distinguishable elements of a continuum C, a series of elements may be found, $E_1, E_2, \cdots, E_n$, all belonging to this same continuum C and such that each of them is indistinguishable from the preceding, that E_1 is indistinguishable from A and E_n indistinguishable from B. Therefore we can go from A to B by a continuous route and without quitting C. If this condition is fulfilled for any two elements A and B of the continuum C, we may say that this continuum C is all in one piece. Now let us distinguish certain of the elements of C which may either be all distinguishable from one another, or themselves form one or several continua. The assemblage of the elements thus chosen arbitrarily among all those of C will form what I shall call the *cut* or the *cuts*.

Take on C any two elements A and B. Either we can also find a series of elements $E_1, E_2, \cdots, E_n$, such: (1) that they all belong to C; (2) that each of them is indistinguishable from the following, E_1 indistinguishable from A and E^n from B; (3) *and besides that none* *of the elements E is indistinguishable from any element of the cut.* Or else, on the contrary, in each of the series $E_1, E_2, \cdots, E_n$ satisfying the first two conditions, there will be an element E indistinguishable from one of the elements of the cut. In the first case we can go from A to B by a continuous route without quitting C and *without meeting the cuts;* in the second case that is impossible.

If then for any two elements A and B of the continuum C, it is always the first case which presents itself, we shall say that C remains all in one piece despite the cuts.

Thus, if we choose the cuts in a certain way, otherwise arbitrary, it may happen either that the continuum remains all in one piece or that it does not remain all in one piece; in this latter hypothesis we shall then say that it is *divided* by the cuts.

It will be noticed that all these definitions are constructed in setting out solely from this very simple fact, that two manifolds of impressions sometimes can be discriminated, sometimes can not be. That postulated, if, to *divide* a continuum, it suffices to consider as cuts a certain number of elements all distinguishable from one another, we say that this continuum *is of one dimension;* if, on the contrary, to divide a continuum, it is necessary to consider as cuts a system of elements themselves forming one or several continua, we shall say that this continuum is *of several dimensions.*

If to divide a continuum C, cuts forming one or several continua of one dimension suffice, we shall say that C is a continuum *of two dimensions;* if cuts suffice which form one or several continua of two dimensions at most, we shall say that C is a continuum *of three dimensions;* and so on.

To justify this definition it is proper to see whether it is in this way that geometers introduce the notion of three dimensions at the beginning of their works. Now, what do we see? Usually they begin by defining surfaces as the boundaries of solids or pieces of space, lines as the boundaries of surfaces, points as the boundaries of lines, and they affirm that the same procedure can not be pushed further.

This is just the idea given above: to divide space, cuts that are called surfaces are necessary; to divide surfaces, cuts that are called lines are necessary; to divide lines, cuts that are called points are necessary; we can go no further, the point can not be divided, so the point is not a continuum. Then lines which can be divided by cuts which are not continua will be continua of one dimension; surfaces which can be divided by continuous cuts of one dimension will be continua of two dimensions; finally space which can be divided by continuous cuts of two dimensions will be a continuum of three dimensions.

Thus the definition I have just given does not differ essentially from the usual definitions; I have only endeavored to give it a form applicable not to the mathematical continuum, but to the physical continuum, which alone is susceptible of representation, and yet to retain all its precision. Moreover, we see that this definition applies not alone to space; that in all which falls under our senses we find the characteristics of the physical continuum, which would allow of the same classification; that it would be easy to find there examples of continua of four, of five, dimensions, in the sense of the preceding definition; such examples occur of themselves to the mind.

I should explain finally, if I had the time, that this science, of which I spoke above and to which Riemann gave the name of analysis situs, teaches us to make distinctions among continua of the same number of dimensions and that the classification of these continua rests also on the consideration of cuts.

From this notion has arisen that of the mathematical continuum of several dimensions in the same way that the physical continuum of one dimension engendered the mathematical continuum of one dimension. The formula

$$A > C, \quad A = B, \quad B = C,$$

which summed up the data of crude experience, implied an intolerable contradiction. To get free from it it was necessary to introduce a new notion while still respecting the essential characteristics of the physical continuum of several dimensions. The mathematical continuum of one dimension admitted of a scale whose divisions, infinite in number, corresponded to the different values, commensurable or not, of one same magnitude. To have the mathematical continuum of n dimensions, it will suffice to take n like scales whose divisions correspond to different values of n independent magnitudes called coordinates. We thus shall have an image of the physical continuum of n dimensions, and this image will be as faithful as it can be after the determination not to allow the contradiction of which I spoke above.

4. *The Notion of Point*

It seems now that the question we put to ourselves at the start is answered. When we say that space has three dimensions, it will be said, we mean that the manifold of points of space satisfies the definition we have just given of the physical continuum of three dimensions. To be content with that would be to suppose that we know what is the manifold of points of space, or even one point of space.

Now that is not as simple as one might think. Every one believes he knows what a point is, and it is just because we know it too well that we think there is no need of defining it. Surely we can not be required to know how to define it, because in going back from definition to definition a time must come when we must stop. But at what moment should we stop?

We shall stop first when we reach an object which falls under our senses or that we can represent to ourselves; definition then will become useless; we do not define the sheep to a child; we say to him: *See* the sheep.

So, then, we should ask ourselves if it is possible to represent to ourselves a point of space. Those who answer yes do not reflect that they represent to themselves in reality a white spot made with the chalk on a blackboard or a black spot made with a pen on white paper, and that they can represent to themselves only an object or rather the impressions that this object made on their senses.

When they try to represent to themselves a point, they represent the impressions that very little objects made them feel. It is needless to add that two different objects, though both very little, may produce extremely different impressions, but I shall not dwell on this difficulty, which would still require some discussion.

But it is not a question of that; it does not suffice to represent *one* point, it is necessary to represent *a certain* point and to have the means of distinguishing it from an *other* point. And in fact, that we may be able to apply to a continuum the rule I have above expounded and by which one may recognize the number of its dimensions, we must rely upon the fact that two elements of this continuum sometimes can and sometimes can not be distinguished. It is necessary therefore that we should in certain cases know how to represent to ourselves *a specific* element and to distinguish it from an *other* element.

The question is to know whether the point that I represented to myself an hour ago is the same as this that I now represent to myself, or whether it is a different point. In other words, how do we know whether the point occupied by the object A at the instant α is the same as the point occupied by the object B at the instant β, or still better, what this means?

I am seated in my room; an object is placed on my table; during a second I do not move, no one touches the object. I am tempted to say that the point A which this object occupied at the beginning of this second is identical with the point B which it occupies at its end. Not at all; from the point A to the point B is 30 kilometers, because the object has been carried along in the motion of the earth. We can not know whether an object, be it large or small, has not changed its absolute position in space, and not only can we not affirm it, but this affirmation has no meaning and in any case can not correspond to any representation.

But then we may ask ourselves if the relative position of an object with regard to other objects has changed or not, and first whether the relative position of this object with regard to our body has changed. If the impressions this object makes upon us have not changed, we shall be inclined to judge that neither has this relative position changed; if they have changed, we shall judge that this object has changed either in state or in relative position. It remains to decide which of the *two*. I have explained in 'Science and Hypothesis' how we have been led to distinguish the changes of position. Moreover, I shall return to that further on. We come to know, therefore, whether the relative position of an object with regard to our body has or has not remained the same.

If now we see that two objects have retained their relative position with regard to our body, we conclude that the relative position of these two objects with regard to one another has not changed; but we reach this conclusion only by indirect reasoning. The only thing that we know directly is the relative position of the objects with regard to our body. *A fortiori* it is only by indirect reasoning that we think we know (and, moreover, this belief is delusive) whether the absolute position of the object has changed.

In a word, the system of coordinate axes to which we naturally refer all exterior objects is a system of axes invariably bound to our body, and carried around with us.

It is impossible to represent to oneself absolute space; when I try to represent to myself simultaneously objects and myself in motion in absolute space, in reality I represent to myself my own self motionless and seeing move around me different objects and a man that is exterior to me, but that I convene to call me.

Will the difficulty be solved if we agree to refer everything to these axes bound to our body? Shall we know then what is a point thus defined by its relative position with regard to ourselves? Many persons will answer yes and will say that they 'localize' exterior objects.

What does this mean? To localize an object simply means to represent to oneself the movements that would be necessary to reach it. I will explain myself. It is not a question of representing the movements themselves in space, but solely of representing to oneself the muscular sensations which accompany these movements and which do not presuppose the preexistence of the notion of space.

If we suppose two different objects which successively occupy the same relative position with regard to ourselves, the impressions that these two objects make upon us will be very different; if we localize them at the same point, this is simply because it is necessary to make the same movements to reach them; apart from that, one can not just see what they could have in common.

But, given an object, we can conceive many different series of movements which equally enable us to reach it. If then we represent to ourselves a point by representing to ourselves the series of muscular sensations which accompany the movements which enable us to reach this point, there will be many ways entirely different of representing to oneself the same point. If one is not satisfied with this solution, but wishes, for instance, to bring in the visual sensations along with the muscular sensations, there will be one or two more ways of representing to oneself this same point and the difficulty will only be increased. In any case the following question comes up: Why do we think that all these representations so different from one another still represent the same point?

Another remark: I have just said that it is to our own body that we naturally refer exterior objects; that we carry about everywhere with us a system of axes to which we refer all the points of space, and that this system of axes seems to be invariably bound to our body. It should be noticed that rigorously we could not speak of axes invariably bound to the body unless the different parts of this body were themselves invariably bound to one another. As this is not the case, we ought, before referring exterior objects to these fictitious axes, to suppose our body brought back to the initial attitude.

(*To be continued.*)

PATHOGENIC PROTOZOA[1]

By Professor GARY N. CALKINS

COLUMBIA UNIVERSITY

AT the present time the importance of protozoa-study is recognized in all branches of biological science where, as single-celled organisms, they illustrate the manifold principles of living things. Thus it is in physiology, in cellular biology, in psychology and in general biology. There is one field, however, a field that is daily growing more extensive, in which the importance of protozoa has only recently been recognized, and this is the field of pathology. To the medical world for the most part, the group of protozoa consists of the few types of parasitic forms that cause human disease, and in this world any one who has a knowledge of *Trypanosoma,* or *Amœba,* or *Plasmodium,* is a student of the *protozoa,* while a deeper knowledge makes him a biologist. At the present time there are many students of the group in this sense, and the relations of protozoa to human welfare bid fair to be the most popular aspect of protozoan study, while in the public mind already the term protozoa is apparently the synonym of some new and fearsome thing. Commissions for the study of protozoan diseases have been appointed in many countries and chairs for the study of protozoology have been established in the universities of Cambridge and London mainly for the study of the pathogenic forms of these unicellular animals. The present paper deals with a few aspects of this more recent field of protozoa work.

Malaria

Few pathologists in good standing gave a thought to protozoa until after the malarial organisms had been worked out and the life history completely known through the researches of biologists and surgeons. Thanks to the work of Laveran, Ross, Grassi and Schaudinn, there is no longer a phase in this disease that is unknown, and the relations of the various symptoms of the malady to stages in the life history of the organisms are perfectly established. I do not need to go into the malaria problem, for the life history of the organisms, their relations to the mosquito *Anopheles,* the coincidence of merozoite 'spore' formation and pyrexial attacks of the disease, are familiar to all who have followed, even remotely, the progress of medical science. I will pass

[1] A lecture delivered at Woods Hole, July 10, 1906.

on rather to less familiar problems that are to-day puzzling the medical world.

Amœbic Dysentery

Since 1860 various forms of *Amœba* have been found in the human intestine. In 1876 Loesch first claimed these rhizopods to be the cause of dysentery, and since that time students of the subject have been about equally divided into supporters of Loesch and his opponents. In recent times, however, the belief is widespread that two forms of the disease occur, one of which, tropical, pernicious, or amœbic dysentery, is invariably accompanied by the rhizopod *Entamœba histolytica,* which Schaudinn distinguishes from the ordinary harmless intestinal amœba *A. coli.* This form becomes a tissue and cell-infecting parasite and to this characteristic it owes its malignancy. Musgrave in the government laboratory at Manila has worked out the organism most carefully and has been successful in producing the disease in monkeys and other animals through cultures free from the ordinary forms of bacteria. The life history of the *Amœba* has been followed by Schaudinn, and in the present place this is perhaps more interesting than the pathological details.

The *Amœba* has little structural detail, a nucleated cell with minute form changes, a slight differentiation into ectoplasm and entoplasm, a great power of reproduction, by division and by budding, leading to masses of the organisms in the intestine and attached organs, where intestinal lesions, liver abscesses and the like, become the characteristic features of the disease. One interesting phenomenon worked out by Schaudinn is the preliminary nuclear metamorphosis before spore formation. In the majority of rhizopods in which the life history is known the formation of conjugating gametes is preceded by fragmentation of the nucleus either by division, or by a kind of nuclear secretion of chromatin. A condition of the cell, known as that of the 'distributed nucleus,' results from this fragmentation and each fragment of chromatin becomes the nucleus of one of the gametes. Unfortunately, Schaudinn gives no figures in his preliminary publication on *Entamœba,* but his description tallies exactly with analogous processes in other rhizopods, notably in *Arcella, Centropyxis, Difflugia* and others.

In *Entamœba* as in these free-living rhizopods the spore-like bodies resulting from this distributed condition of the nucleus conjugate and so bring about a renewal of vitality of the parasite, favorable to the infection of new hosts. Musgrave has shown that these parasites may live a free life in ordinary drinking water and that infection takes place presumably in this way, and his observations indicate that *Entamœba* is a facultative rather than an obligatory parasite of man.

Schaudinn regards *Entamœba coli* as a harmless commensal, but *Entamœba histolytica* as a definite tissue-destroying parasite, a faculty which gives to this form of tropical dysentery its pernicious and often fatal characteristic.

Rabies Furiosa

From dysentery to hydrophobia seems a long jump and yet we may find that these two diseases and with them others apparently as diverse, as small-pox, sleeping sickness, etc., owe their malignancy to the destructive action of protozoa on different tissues of the body. I take up rabies next because the organism of hydrophobia agrees in many of its characteristics with that of dysentery, and belongs, as I believe, on the present evidence, in the group of rhizopods.

Peculiar structures have been known for several years in the brain and nerve cells of animals with rabies. Negri, however, in 1904 was the first to suggest that these are as distinctive of hydrophobia as Plasmodium is of malaria, and he and his assistants succeeded in establishing the conviction that these 'Negri' bodies are sufficiently characteristic to afford a quick and safe diagnosis of rabies. Beyond the suggestion that these bodies are organisms comparable to the small-pox organism as described by Councilman and his collaborators, Negri did not attempt to outline their systematic position. From the apparent absence of nuclei and other structures in the Negri bodies, pathologists have been inclined to interpret them as secretions, or degenerations of a specific type, and very few have been hardy enough to regard them as protozoa. Among these few, however, is Dr. Anna W. Williams, of New York, who spent a summer at Woods Hole three years ago, working on protozoa, and who has done a great deal on pathogenic protozoa; and to her belongs the credit of establishing the protozoon nature of these enigmatical structures.

One of the curious features of pathogenic protozoa is that they are very refractory to the ordinary cytological stains, and do not behave like other cells under hæmatoxylin or methylin blue, etc. This is why the organism of syphilis was overlooked until last year, and this is why Negri failed to get any evidence of cell structure in his rabies organism. In all of his preparations and figures the bodies appear as highly vacuolated structures with no sign of nucleus or differentiated cell body. Dr. Williams, using the Giemsa stain, a method that has been singularly successful in staining delicate pathogenic protozoa, showed that not only do these Negri bodies have a definite structure with nucleus and cytoplasm, but also that a series of nuclear metamorphoses occur which are identical in their consecutive phases with the typical free-living rhizopods. Nuclear fragmentation takes place until as in *Entamœba* or *Centropyxis* or *Amœba*, etc., the cell is filled with chromatin particles. Dr. Williams also showed that reproduction by division and by budding takes place, and she failed only to make out the conjugation processes. From my own study of Dr. Williams's preparations, I have no doubt at all that she is right about the organism, and believe that, under her name *Neurorcytes,* it should be grouped with the rhizopod-like protozoa.

This work on *Neurorcytes* is also very interesting from the side light it throws on the small-pox problem. Here again, in certain phases of variola, we find curious intra-cellular and intra-nuclear bodies having a striking resemblance to the ordinary unstained forms of the Negri bodies. These small-pox bodies were early recognized as characteristic of this disease, and Guarnieri in 1892, believing they were protozoa of a specific kind, named the organism *Cytorcytes variolæ.* Perhaps the majority of pathologists to-day and many biologists, are opposed to this interpretation, and these bodies, like the Negri bodies, are more commonly regarded as specific secretions or degenerations than as protozoa. I have no doubt myself, from long study of these small-pox organisms, that they are protozoa, and believe that with fresh material and by using the stain which Dr. Williams has so successfully used for *Neurorcytes,* the last doubter will be convinced.

At the risk of going somewhat far afield in pathological speculation, let me briefly call attention to one other possibility of amœboid organisms and disease, *viz.,* cancer. For years it has been known that vegetable cells in ordinary edible forms, like the cabbage or turnip, etc., may be stimulated to abnormal multiplication by rhizopod parasites. Such a parasite—*Plasmodiophora brassicæ*—enters the young root cells of a cabbage, stimulates those cells to an unwonted degree of multiplication until great tumors are formed giving rise to the vegetable disease known as 'club-root.' The plant cells become storage reservoirs of the spores of *Plasmodiophora,* and when the plant dies down the spores are liberated in the soil. Now it has been argued that if vegetable cells can be stimulated to abnormal activity, there is no real biological objection to animal cells being similarly stimulated to division by animal parasites, and some pathologists have gone so far even as to see in certain cell inclusions of cancer peculiar bodies which they compared with the *Plasmodiophora* spores. The comparison, however, can not be sustained, and without going into the subject extensively I may say in short that nothing has ever been seen in cancer cells that can be interpreted as a protozoon parasite. This, however, does not weigh against the parasite theory of cancer—a theory which I personally, believe to be the only one that satisfactorily explains the disease. The organism of yellow fever has never been seen, but no one doubts the parasitic nature of that disease and the fact that the virus or the germs of yellow fever pass through the finest Berkefeldt and Chamberland filters indicates that we have to do in this disease with organisms too minute to be seen. Ultra-microscopic protozoa are not the only ones which will do this, for small amœbæ may, under pressure, be forced through some of these filters, while in a few organisms, notably the flagellates, there are some phases in the life history when the individuals become so small that they are no longer discernible with the highest powers of the microscope. This is the case in certain of the trypanosomes and spirochætes, which are now known to cause some of the most malignant of human diseases.

Trypanosomiasis and the 'Sleeping Sickness'

In this country, and indeed in temperate zones generally, there is no dread of trypanosomes, and 'sleeping sickness' is more often the subject of thoughtless jest than of intelligent consideration. In England, where African interests are more keenly followed, a deep interest is taken in this matter, and the Liverpool School of Tropical Medicine deals largely with trypanosomiasis. In his presidential address before the British Association for the Advancement of Science, meeting in South Africa last summer, Colonel Bruce said:

> Trypanosomiasis in man, the 'sleeping sickness,' which occurs on the west coast of Africa, particularly in the basin of the Congo, has within the last few years spread eastward into Uganda, has already swept off some hundreds of thousands of victims, is spreading down the Nile, has spread all round the shores of Lake Victoria and is still spreading southward round lakes Albert and Albert Edward, and now threatens the Transvaal and Zululand.[2]

Many different species of trypanosomes are known, and the normal habitat is the blood. No form of vertebrate is exempt, the blood of fish and amphibia, reptiles, birds and mammals forming suitable culture media for their growth and reproduction. In some stages of their life history they apparently become intra-cellular parasites, lose their flagella and membranes and assume the gregarine-like form. In man they may be either comparatively harmless flagellates, swimming about in the blood plasm, or, by bursting of the capillaries, they may penetrate the membranes of the brain and spinal cord and give rise to the invariably fatal disease of man—'sleeping sickness.' The presence of *Trypanosoma gambiense* in the human blood gives rise to the 'trypanosome fever' of Africa, not much worse apparently than is malaria in this country, but when the parasites enter the nervous system and congregate in the cerebro-spinal fluid, or in the ventricles of the brain, the result is invariably fatal. The result of this nerve-invasion is the appearance of various nervous symptoms like apathy, lassitude, trembling, and, finally, somnolence, increasing to a phase of intense coma and

[2] *Science*, XXII., p. 298, 1905.

death in from three to twelve months. Death usually is due to the disorganization of the nervous system, but in chronic cases it may follow from weakness and emaciation, or, in acute cases, it may result from the blocking of the capillaries by the parasites.

The full life-history of the organism of sleeping sickness is not yet known. In all blood-dwelling protozoa, however, the infection is carried from individual to individual by some intermediate host, usually a blood-sucking invertebrate. *Trypanosoma gambiense* is thus transmitted from man to man by the tsetse fly, *Glossina palpalis; Trypanosoma brucei,* the 'tsetse fly disease' of horses, by *Glossina morsitans; Trypanosoma lewisi* is carried from rat to rat by the louse, *Hæmatopinus; T. ziemanni* from owl to owl by the mosquito, *Culex pipiens.* Nor are the intermediate hosts limited to the insects. *Trypanoplasma borreli,* for example, is transmitted from carp to carp by the fish leech, *Piscicola geometra.*

The history of the trypanosomes in the digestive tracts of these various carriers has been made out in several cases, although by no means in all. In the gut of the leech, of the louse, *Hæmatopinus,* and of the mosquito, three different species of *Trypanosoma* have been worked out by different and competent observers, and in all cases it has been found that this environment is the scene of the most important phases in the life history of the parasite, *viz.,* the conjugation stages, which lead to renewal of vitality. The flagellate parasites thus agree in the main features of their life cycles with the malaria organisms in man and in the insect host *Anopheles.* Indeed, so widespread is this phenomenon that we are justified in assuming, where actual evidence is not forthcoming, that similar important processes take place in all intermediate hosts, and that we must look for conjugation phases of the parasite of Texas fever (cattle) in the tick (*Boophilus bovis*), and of sleeping sickness in the fly (*Glossina palpalis*). On the other hand, we are justified in assuming a protozoon parasite, even though the parasite is not known, in cases where its existence has been proved in an intermediate host. This is the case, for example, in yellow fever, where a definite incubation period of the parasite of about twelve days in the mosquito *Stegomyia fasciata* is known, and where it is fully established that, apart from this mosquito, no other means of transmission of the disease exists.

Associated with the *Trypanosoma* diseases, although not yet established, are those curious maladies of India and similar countries, known as dum-dum fever, kala-azar, splenomegaly, etc. The infection may be general or localized, and curious structures, known as the Donovan-Leischman-Wright bodies, have been observed in the spleen of dum-dum fever patients, in the lesions in 'tropical ulcer,' Delhi boil, 'oriental sore' and the like, and these bodies have been interpreted as stages in the history of some *Trypanosoma.* At the present time, however, it appears much more likely that they are more nearly related to the hæmosporidia than to the flagellates, and Laveran and Mesnil's view assigning them to the genus *Piroplasma,* the same genus as the parasite of Texas fever, may be accepted.

Diseases due to Spirochæta

Closely allied to *Trypanosoma* is the genus *Spirochæta,* a similar flagellate belonging to the order *Monadida.* In its general cork-screw shape it resembles *Spirillum,* one of the bacteria, but differs from this by reason of its plastic body and general mode of life. Many different species have been described, and considerable difference of opinion exists as to whether they should be classed as bacteria or as protozoa. One well-known form—*Spirochæta obermeieri*—has long been recognized as the cause of relapsing fever, and has been recently shown to be the cause of human tick fever in Africa, which is carried from man to man by the tick (*Ornithodorus moubata*). The organism of relapsing fever, however, does not seem to have a typical flagellate structure; there is unmistakable evidence of transverse rather than longitudinal division; its nucleus is distributed like that of a bacillus, and Novy has shown that it reacts like some bacteria during plasmolysis. The need of an intermediate host in the case of African tick fever seems to be the one distinctive protozoan characteristic.

While *Spirochæta obermeieri* is perhaps a doubtful protozoon, there is less doubt in the case of other species, some of which have unquestioned flagellate characters, including typical nuclear structures, longitudinal division and the like, while in a number of species an undulating membrane analogous to that of a trypanosome can be made out. Some of the better known species are: *Spirochæta dentium* of the mouth; *S. refringens* of ulcerating tumors; *S. gallinorum* a blood parasite of fowls, and *S. anodontæ,* parasitic in the crystalline style of the mussel *Anodonta mutabilis.* A similar parasite, described first as *S. balbiani,* is found in the crystalline style of the oyster, but it has so many trypanosome characteristics that it is now called *Trypanosoma balbiani.* It serves to illustrate the close relationship between these two genera.

It is to this group of parasites that *Spirochæta* (*Treponema*) *pallida,* the cause of syphilis, belongs. The organism was discovered a year ago, and since that time has been submitted to the widest range of pathological research. A full life history has been published by Siedlecki and Krzyszakowicz, and some of the stages described by them are strikingly similar to those of *Trypanosoma.* Schaudinn regards it as sufficiently distinct from other *Spirochæta* species to justify a new generic name and calls it *Treponema pallidum.*

The organisms causing the several diseases that I have dealt with in the preceding account fall in three of the four great groups of protozoa, the *Infusoria* alone being unrepresented. The causes of tropical dysentery, of rabies, and possibly of small-pox, are related to the ordinary rhizopod types; the causes of sleeping sickness, trypanosomiasis and syphilis belong to the animal flagellates, and here, possibly, belongs the organism of yellow fever; the causes of malaria, and, probably, of splenomegaly, dum-dum fever and the like belong to the sporozoa.

The effects of these different parasites upon their hosts differ in different cases. Sometimes they poison the host by the liberation of toxins, as in the case of malaria or yellow fever; sometimes by local lesions or general tissue disorganization, as in tropical ulcer, sleeping sickness, syphilis and rabies; or sometimes by the mere mechanical obstruction to normal physiological processes by the accumulation of parasites in capillaries and ducts, while in still other cases the parasites stimulate the latent dividing energy of tissue cells and lead to abnormal tumor-like growths. In many cases knowledge of the organism and of its mode of life has led to preventive measures and to the great saving of human life. In yellow fever, for example, the warfare on mosquitoes will stop entirely its epidemic nature and, thanks to Carroll's brilliant experiments, yellow fever to-day, like malaria or trichinosis, is an advertisement of ignorance or criminal negligence in communities where it exists. Preventive measures hold down the ravages of small-pox, anti-rabic serum lessens the malignancy of hydrophobia, while different specifics are fatal to other kinds of parasitic protozoa, quinine to *Plasmodium malariæ,* mercury to *Treponema pallidum,* and recently Koch's specific to *Trypanosoma.* With the knowledge of these other pathogenic protozoa and of their modes of life investigation will bring out the means of combating them and thus of reducing human suffering, and this prospect if for no other reason, is a full justification of the many commissions that have been appointed, and of the vast sums of money that have been spent, to further protozoan research.

THE MAKING OF THE GRAND CAÑON OF THE COLORADO

By Professor A. R. CROOK

CURATOR, ILLINOIS STATE MUSEUM OF NATURAL HISTORY

THE Grand Cañon of the Colorado furnishes the most impressive illustration of cañon-making forces to be found, and presents a fascinating chapter of world history.

Nearly every visitor to the Cañon attempts an explanation of the manner of its making, and their explanations may be broadly grouped under five types—the explanation of the Indians who for centuries have inhabited the Colorado River region, that of one of the oldest white settlers in Arizona, that of a successful Chicago business man, that of a famous literary man, and finally that of men who have studied various phases of world making long enough to have learned the chief principles involved.

The Indians who have lived in Arizona, generation after generation, might be expected to have some notion of the origin of the Cañon. Their explanation is that a wise chieftain long mourned the death of a beloved wife. Finally, in pity, one of the gods made a great cleft in the earth, took the chieftain to the Happy Hunting Ground to see his wife; and then upon their return, fearing that others might go that way, the god hurled a river in the cleft where it is now flowing, effectually barring the way against intruders. The 'god-made' theory of the Indians is unsatisfactory as the infant science of any race always is.

The explanation of one of the oldest settlers in the region is hardly better. It is that of the widely-known John Hance. One morning he entertained the writer with the picturesque stories which he has poured into the ears of many an amused traveler, telling how thirty years ago he went from the Atlantic coast to Arizona to find room for his energies. In the east he was crowded. In Arizona he found opportunity to expand. A few years ago he used to tell that he dug the cañon, but says that he no longer makes the statement, because a little girl asked him where he put the dirt! The man-made explanation of the white man is no more satisfactory than the god-made theory of the Indian.

A Chicago business man who knows how to make dollars, thinks he knows how the cañon was made, and after visiting it said to the author: "I'll tell you how that cañon was made. Once there was a

Fig. 1. One Mile from the Cañon—Seemingly a Thousand.

Fig. 2. John Hance at Old Grand View Hotel.

gigantic volcano. It exploded with such fury that one side became the finest dust and the other toppled over and now forms the channel through which the river is flowing." Much nearer to the truth than any of these explanations is that of Joaquin Miller. He suggests that the cañon is due to an underground river which worked away like the Green River has done in Mammoth Cave. Finally the roof fell in and behold the Grand Cañon. A picturesque explanation is Miller's and better than any of the others. Its truth consists in representing the river as the active agent, its error in the underground idea.

The geologist knows that the Colorado River cut out the whole cañon and regards the region as the finest example of river work on a grand scale in the world and as the type of all that is gigantic in displacement and denudation. The manner in which the river accomplished its tremendous task can be more easily comprehended by considering a number of facts; first, such as relate to the geography of the region; second, to its topography; third, to its stratigraphy and petrology; fourth, to its denudation and displacements. These different groups are closely interwoven with each other and the facts marshaled in one group are dependent upon those in all of the other groups.

Many tourists returning from the state of Colorado report that they have visited the Grand Cañon of the Colorado when they have simply seen the cañon of the Arkansas River—a cañon which would be a mere scratch in the side of the Grand Cañon. To see the chief cañon in the world they should have journeyed several hundred miles farther towards the southwest, to the region where a cañon 200 miles long has cut off the northwest corner of Arizona from the remainder of that state. Most tourists will now make their visit at the point called Grand Cañon Post Office, while the more fortunate ones will go east to Grand View and west to Cataract Creek. The railroad from the main line has been built to a well-chosen point where the cañon is a dozen miles wide and where for fifty miles to the east and west it is most gigantic and impressive. Within this region it displays all the salient points in its topography, succession of geological formations, different kinds of rocks, and erosion and sculpturing. So that one inquiring about the origin of the cañon can obtain all the factors to the answer in this locality.

Thanks to the good topographic map which the government has issued, one does not have to guess at the distances and depths in the region. One may refer to the map and not be disappointed. Accurate measurements do not rob the depths and distances of their magnificence, nor is exaggeration there necessary for impressiveness.

The new survey shows that the elevation of the plateau through which the river has carved its path is between seven and eight thousand feet above sea level. At the head of the cañon the elevation of the river is twenty-five hundred feet, and by the time it has flowed two hundred miles to the end of the cañon it is only a thousand feet

FIG. 3. NEARLY FIVE THOUSAND FEET OF ROCK STRATA. OPPOSITE, A WALL A DOZEN MILES DISTANT.

above sea level. The distance between the top walls is from three to fifteen miles, and does not accord with the reports and pictures of the early government expeditions to the region. From El Tovar Hotel and the Grand View one sees the cañon at its broadest stretch, where for more than fifty miles it is a dozen miles wide and is filled with buttresses and buttes, towering from two to five thousand feet above the river.

The rim of the northern wall presents a nearly horizontal line. But a bird's-eye view from the western end of the cañon would show the country to the east to be divided into four plateaus, each dipping like cakes of ice in a river till met by its neighbor. The plateau north of the visitor at El Tovar Hotel is one of the blocks that has been raised highest and consequently has brought up the lowest rocks and exposed them to the corroding action of the river.

If with a high buzz-saw we could cut through the country from east to west for three hundred miles and could move out the section nearest us, we should see that the face of the cut opposite is composed of a series of blocks higher towards the west and slipping down towards the east. What we are unable to do in this regard has in a manner been done for us, since for ages the Colorado River has been eating away and has exposed the outlines of the blocks and the various geological formations so plainly that they are easily distinguished.

Seated on the rim in some favorable spot, one sees the different formations spread out as clearly as if diagrammed in a text-book on geology. The Kaibab plateau, just to the north shows six or seven formations belonging to the four great rock systems. At the top is the bluish limestone which weathers into fantastic buttresses and pinnacles, many of them so dizzily perched as to give a constant challenge to the winds or to man to topple them from their precarious position and hurl them a thousand feet below. My companion could hardly find time to sleep, so great was his delight in prying off great blocks of stone and listening to their terrible crunch and roar as they fell hundreds of feet, striking a ledge here and there and finally crushed to powder, or, still in gigantic mass, they rested a thousand or more feet below. This top formation, which Powell called the 'Fortification limestone,' is the 'Upper Aubrey' limestone. It is about five hundred feet thick.

Next below it, can be seen the great white four hundred feet wide band of Upper Aubrey sandstone which stretches like a ribbon in sight for fifty miles and more to the east and west. Its walls are even more precipitous than those of the Aubrey limestone.

Below it are a thousand feet of shelving red sandstone which form the 'Lower Aubrey.' These three formations—the Upper Aubrey limestone and sandstone, and the Lower Aubrey sandstone—constitute the Upper Carboniferous system which is so familiar to the inhabitants of the Mississippi Valley states as the source of coal, but which in this region is not coal bearing.

Following the Upper Carboniferous a shelf of shales leads out to the precipitous wall of the great 'Red Wall' limestone, which, with

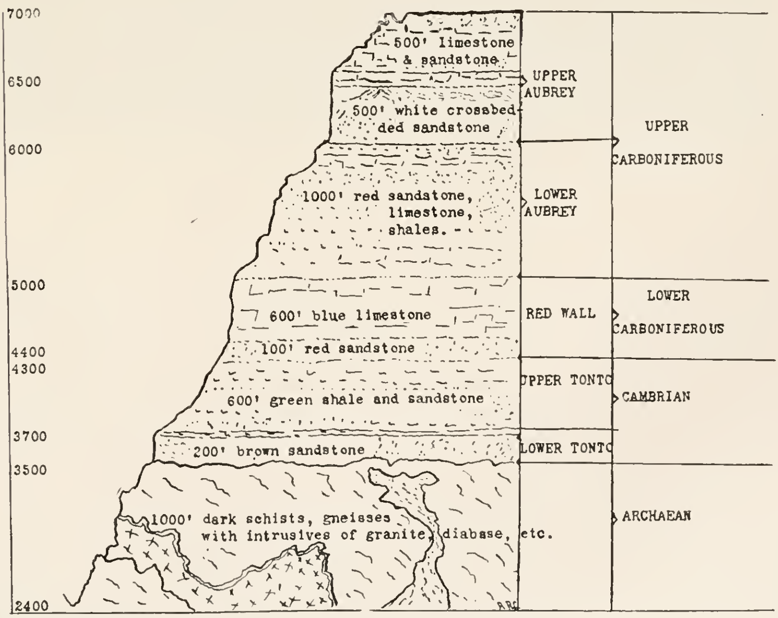


FIG. 4. GEOLOGICAL SECTION AT BRIGHT ANGEL TRAIL.

its six hundred feet of deep red cliffs, presents one of the most striking members of the landscape. The Red Wall constitutes the Lower Carboniferous of the region.

Then there is a terrace—'The Esplanade'—often several miles in width, composed of green shales and sandstones. It is the 'Upper Tonto' formation. It leads by gentle slopes to the brink of another precipice, which is formed by the quartzites and hard sandstones of the 'Lower Tonto.' The Upper and Lower Tonto are the local names of the formation composing the system known the world over as the Cambrian. Together they are about seven hundred feet thick. The Lower Tonto forms the capping of the inner cañon in the Bright Angel region.

FIG. 5. HURLING ROCKS OVER THE RIM. Upper Aubrey Limestones, Sandstone and Lower Aubrey Sandstone seen on Cliff two miles distant.

Below it are jagged, twisted, dark-colored metamorphic rocks, which represent the Archæan—the oldest system of rocks in the world. Tourists call these rocks 'granite,' but they are schists and gneisses. They are in layers and have been subjected to extensive folding. The river has cut through the Archæan rocks to a depth of over a thousand feet and produced walls so precipitous that in most places a sure-footed prospector could not cling to them. Between them the river runs with the greatest speed and fury.

The visitor may see all these formations from the upper rim, but as he descends to the river and passes them in succession, he obtains a clearer idea of their relations and characteristics. They are clearly differentiated by constitution, structure and color. A study of these various strata makes it evident that the topography of the country is largely due to the difference in the kinds of rocks which the river encounters in its journey from the mountains to the sea.

Leaving the immediate vicinity of the cañon and going to the north, the tourist would come upon various younger geological formations which at one time buried the whole region. Although from ten to fifteen thousand feet thick, they no longer remain in the vicinity of the cañon, having been washed away by flowing water after the elevation of the land in early Eocene times. When that was accomplished and the country for miles around was comparatively level, the land was again gradually raised and the river began in Neocene times to cut the outer cañon. When it had cut down to the Tonto terrace—'The

FIG. 6. IN THE HEART OF THE CAÑON. Archaean Rocks capped by Tonto Strata, which dip towards the River.

Esplanade'—elevation ceased and the river wandered back and forth in a lazy manner, widening instead of deepening its channel. Since in that arid region there was not enough rainfall to wash down the side walls, they have remained precipitous, while the main stream has had time to push them back until they are a dozen miles apart. Then, in the Pliocene, slow elevation of the plateau began again, causing the river to run swiftly and renew its carving. This action is proceeding at the present time with the result of deepening the inner cañon.

Standing on the cañon's rim and looking down upon the immense gulf, one marvels that so small a river could have accomplished so gigantic a work. But if he laboriously descends to the brink of the roaring river below and puts his hand into the rushing water and feels its burden of sharp sand driven along with great force, he obtains some conception of the efficiency of the tool. Quartz sand is harder than any of the common minerals in the rocks. Hurled by the rushing water against the sides and bed of the river, it cuts out a path through the rocks as a file does through soft iron.

Volcanic explosions had nothing to do with the making of the cañon. If they had, there would be volcanic rock over all the region instead of in isolated patches, and all the topographic forms would be different. The cañon is not the result of cracking apart of the earth's crust. If it were, the rock layers would dip away from the cañon; opposite sides would not match; they would lack marks of cutting parallel to the

Fig. 7. At Dawn in the Cañon.

bedding; their configuration would be independent of alternating hard and soft layers. The extensive cracking and faulting which does exist has been at right angles to the cañon and has presented different kinds of rocks for the river to work upon, thus producing variously shaped walls. As the river carves into succeeding strata the crust is weakened and various buttresses sink towards the river.

Thus all signs lead to the conclusion that fire did not make the cañon, nor did wind, nor earthquake, but that it was made by the same agent which in an hour carves tiny channels in a garden after a rain storm. That agent was running water, the water of the Colorado as for unnumbered years it has been flowing from snowy Rocky Mountain ridges to the hot sands which border the Gulf of California.

NOTES ON THE DEVELOPMENT OF TELEPHONE SERVICE

BY FRED DE LAND
PITTSBURG, PA.

I. *The Electric-Speaking Telephone*

THE desire to transmit speech over long distances probably dates back to the first wide separation of loved and beloved. That many methods have been suggested for the transmission of speech is of record. That speech has been mechanically or acoustically transmitted over many hundred feet of taut string and practically straight wires, during several hundred years, is true. That the 'lover's telephone' is a toy that has amused several generations is well known. That there were musical (not speech) telephones and sound (not speech) microphones nearly a century ago is an interesting fact. That prior to 1876, many men devoted much thought to the problem of the electrical transmission of speech is granted.

Nevertheless, the facts are that no authentic record has been found proving the existence of a practical method of speech transmission over long-distances, either electrical or mechanical, prior to the invention of the electric-speaking telephone by Alexander Graham Bell.

Moreover, while certain devices not invented by him are in use on telephone lines, each and all are but refinements or conveniences of the system. The broad fundamental method conceived by Alexander Graham Bell, in 1874, underlies the electrical transmission of speech in any form, and in any portion of the world. And Bell's method has been in no wise changed since its promulgation in letters-patent, in 1876, though thousands of the brightest minds in all civilized countries have striven for nearly thirty years to find another way, some other way, any other way, to transmit speech electrically.

Eighteen months passed by after the method conceived and perfected by Alexander Graham Bell became public property, and tens of thousands of Bell's telephones went into daily commercial use, before the first of the many claimants publicly asserted a prior right to the discovery of the art of transmitting speech electrically. Yet, not one could make his apparatus convey speech, except through a utilization of Bell's method and, in some cases, only by using Bell's receiver.

Thus the only reasonable conclusion that men in search of the truth can arrive at is 'that for physical and mathematical reasons it is not possible to have any method except the way that Alexander Graham Bell found.' And that was the sworn testimony unwillingly given by the experts employed by the followers in his footsteps. For the evidence filed in a score of courts should satisfy all honest and unbiased minds that Bell's way is not only 'the only way' in which an electric-speaking telephone can transmit speech, but the absence of any earlier published description of a conception of a method similar in character to that first promulgated by Alexander Graham Bell proves that speech never was successfully transmitted electrically prior to its transmission by him.

After inventors, electricians and scientists had experimented with Bell's telephone for nearly two years, Dr. C. J. Blake told the eminent gentlemen who had gathered in London, in May, 1878, to hear his interesting lecture on the telephone:

> When we consider the complex character of the waves resulting from the production of articulated sounds, and the loss in the excursions of the receiving disk (in the telephone), the wonder grows that this piece of metal can by its mechanical vibration reproduce so clearly and distinctly the delicate shades of quality of the human voice. That this should have been so perfectly accomplished is the result, not of inspiration, but of laborious research, and the instrument of which we reap the benefit to-day is the product, not merely of the genius, but of the patient and persistent labor of Alexander Graham Bell.

In other words, the achievement of the seemingly impossible in the invention of the electric-speaking telephone came not through utilizing the idea or suggestion of another, nor in improving a philosophical or experimental instrument advertised and sold for many years prior to 1875, as a telephone; neither was it gained in a momentary inspiration, nor through automobilic rushes along the by-paths of superficial education.

This creation of a new art followed as the natural outcome of an original and magnificent conception that won the plaudits of scientists in all lands. The invention of the electric-speaking telephone (not the string telephone, nor the make-and-break musical telephone) followed in natural sequence. The combination of diaphragm and electro-magnet was the outgrowth of conception and perfected theory, and formed a practical materialization of both. And conception, theory and apparatus were the honestly earned fruits not only of the inventor's 'intellectual capacity and precision of thought,' but of a thorough knowledge of the essential elements in every factor entering into the problem of speech transmission; a knowledge gained through long and patient research, through many experiments, through financial expenditures that involved personal deprivations and hardships and necessitated the strictest self-denial, and through discouraging criticisms and bitter ridicule on the folly of wasting time and money in inventing 'a scientific toy.'

The magnitude of the masterly conception of creating, controlling and varying the strength or flow of the electric current by the spoken words, and making that current the vehicle for the transmission of the form or quality as well as the pitch and strength of the spoken words, and of delivering at a distant point the same words, with the pitch, strength and quality unchanged; the comprehensive scope of the newly-created art of speech transmission; the remarkable transmitting qualities of the first of all electric-speaking telephones; and the promptness with which the inventor placed before the public a full and complete knowledge of his invention and of the essential steps leading to his application for letters-patent, all go to prove the possession in 1874–6 of an unusual knowledge on the part of Alexander Graham Bell, the more remarkable in view of the slight grasp electricians then possessed of magnetic action and the interrelation of the magnetic field and the electric current.

II. *The Telephone Exhibit at the Centennial Exposition*

In 1874, Alexander Graham Bell evolved his magnificent conception of the transmission of speech over long distances by means of the electric-speaking telephone. Theoretically it was perfect; practically it had no tangible existence. Men eminent in their respective professions, to whom he confided his plans in the autumn of 1874, admitted that while in theory the undulating-current method 'was adequate to the transmission of speech,' yet the electrical effect produced by the vibration of a diaphragm-armature actuated only by the human voice 'would be entirely too small to accomplish the desired end.' In fact, so complete was the absence of practical knowledge concerning the electrical effect that would be produced by causing the spoken word to vibrate an armature in front of an electro-magnet that the experts, most competent to pass upon the value of such an invention as the electric-speaking telephone, testified that the state of the art was such at the date of the patent that it could hardly have been supposed that a magneto-generator moved by a force so slight as the spoken word, 'would under any circumstances be able to generate an electric current which would produce upon a receiving instrument any effect whatever which would be perceptible to the senses.'

Discouraging though the advice and the suggestions of his friends proved, and disheartened though he was by ill-health and the lack of funds to carry on his telephone experiments, never did the inventor allow aught to divert his firm purpose of transforming that marvelous theory into a tangible speech-transmitting telephone.

In 1874, Alexander Graham Bell occupied the chair of vocal physiology in the Boston University, and supplemented his lectures 'by experimental demonstration of the practicability of correcting stammering, stuttering, lisping, burring and other defects of speech.' To a class composed exclusively of teachers of the deaf who had been sent to Boston by various institutions for the deaf throughout the United States, he delivered courses of lectures upon the subject of teaching articulation to deaf pupils, experimentally demonstrating his methods by giving instruction to deaf-mutes. He also had a class of young women who desired to qualify as teachers of articulation in schools for the deaf. Then he had also undertaken the general education of a young deaf-mute child who came to him at the age of five years, in October, 1872.

Thus it was easy to understand how fully occupied the daylight hours were with his professional duties, and how any experimental and research work must necessarily be carried on late at night when most persons had retired. For while 'the income from his professional labors was a fairly good one it was his only means of support.' If he gave up his classes, he would have no income to expend upon experiments, nor even to live upon.

Yet that is what he did in 1875. At the end of the term he dismissed his classes and by the end of July, 1875, had given up all professional work save the education of the deaf lad and occasional lectures at the Boston University which he had long before been paid to deliver, and the income from which he had expended in telephone experiments. Thus the only income he was in receipt of at the end of July was for teaching the little deaf boy. He would not give up trying to solve the problem of speech transmission, and thus he borrowed what money generous friends would loan him, and started in to demonstrate the practicability of his theory.

All the world knows how well he succeeded, and that no better way has been found during all the years that intervene. In October, 1875, he commenced to prepare the specifications for the patent office, and had his application ready for filing before the end of the year. Then he waited on friends in Canada who desired him to take no action that would be prejudicial to patents they proposed taking out in foreign countries. These friends failing to respond to his proposition, he finally decided to wait no longer. In December he submitted his application to the patent attorneys in Washington; it was signed and sworn to in Boston on January 20, 1876, and filed in the Patent Office early on February 14. On March 7, 1876, he was granted the fundamental patent covering both method and apparatus.

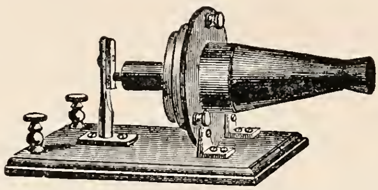
Early in November, 1875, the need of funds to enable him to live forced Alexander Graham Bell to again take up his professional work, and he was soon 'lecturing at various normal schools upon the subject of articulation teaching.' A little later he established a large normal class in Boston, and to be able to properly illustrate his methods, gave free instruction to such deaf-mutes as would serve as subjects for demonstration. Thus by the spring of 1876 he was again in receipt of a fair income and began to repay the sums friends had loaned to him.

On May 10, 1876, at the solicitation of his friends, he read a paper, entitled 'Researches in Telephony,' to the members of the American Academy of Arts and Sciences. Therein he touched upon the inventions of Gray and Reis, and the discoveries of Page, Marrian, Beatson, Gassiot, De la Rive, Matteucci, Guillemin, Wertheim, Wartmann, Janniar, Joule, Laborde, Legat, Poggendorff, DuMoncel, Delezenne, Ferguson, Paul la Cour, Helmholtz, Gore, Sullivan and others, giving due credit to each. Then he described his undulating current and his electric-speaking telephone, for which letters-patent had been granted, and made it clear to his hearers that the essential factor in this problem was not the devising of a definite form of instrument or tube, nor an apparatus having definite structural peculiarities, nor the combining of a certain number of parts into an operating whole. It was to so cause the electric current to flow that the receiver would not only reproduce a few or a majority or about all of the spoken words that impinged on the diaphragm of the transmitter in the form of sound waves, but would reproduce *each* and *all* and *every one* of the variations in the articulations, loudness and pitch and quality, with all their varying characteristics, whether expressed in the slightest whisper, in the soft voice of the cultured woman, in the sonorous rounded sentences of the dignified professor, or in the quick, abrupt remarks of the man of affairs.

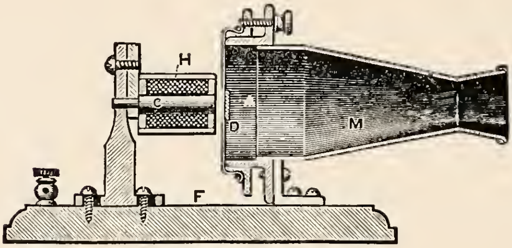
And it may be added that the discovery and practical application of the method so described by which the changes in the current strength in a telephone diaphragm were brought about forms the essential and underlying principle of every successful electric-speaking telephone designed since Alexander Graham Bell created the art of telephony, a composite art, combining magnetism, electricity, acoustics, phonetics, mechanics and engineering.

Gardiner Greene Hubbard was in charge of the Massachusetts educational exhibit at the Centennial Exposition. He insisted on placing the primitive telephones on exhibition in that section if no more suitable place could be secured. Alexander Graham Bell's time was too fully occupied with his professional work to give the subject any attention, and he really did not care whether the telephones were exhibited at the Centennial or not. Class examinations in his school were approaching, and he was far more interested in perfecting the knowledge of the members of his classes who were going forth to instruct deaf children in speech and speech-reading, than in a mere display of an invention that he had completed, patented and had described in a public lecture. An exhibit meant more or less outlay. He was still in debt to his friends, and the funds to repay his friends must come from the income from his school. Therefore the school would be taken care of to the exclusion of the Centennial.

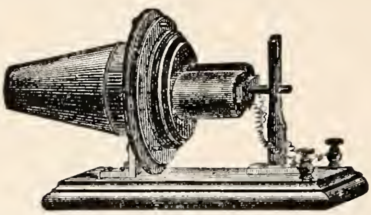
Nevertheless, Mr. Hubbard secured some telephones and certain telegraphic instruments, and placed them on a table in the space allotted to the department of education and science of Massachusetts, which occupied a portion of the gallery at the east end of the main building. This modest display was labeled 'Telegraphic and Telephonic Apparatus. By A. Graham Bell.' It included his system of harmonic telegraphy, and his method of transmitting articulate speech electrically. There were two membrane-diaphragm magneto-telephones, which could be used either as transmitters or as receivers. The only difference was that in one a single-pole electromagnet was used and a double-pole electromagnet in the other. The armature of each electromagnet consisted of a small piece of steel spring glued to the center of the membrane which was three inches in diameter. The castings were of brass, mounted on black-walnut base-boards, and the cones were of japanned tin (Fig. 1).[1] A third form of speaking tele-



Bell's Centennial Single Pole Magneto Telephone.



Section of Same.



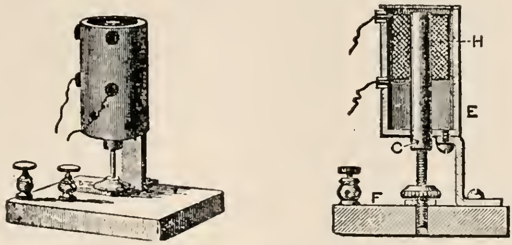
Bell's Centennial Double Pole Magneto Telephone.

Fig. 1.

phone was shown, intended to be used only as a receiving instrument. The electromagnet was enclosed in a hollow box of iron, and a lid of iron was used as an armature. This lid formed a thin circular metallic diaphragm, resting by its edge upon the rim of the iron box, its central portion not quite touching the pole of the electromagnet underneath. This receiver could be placed in circuit with either one of the mem-

[1] The illustrations shown (Figs. 1 to 5) are reproduced with permission from the general brief of the American Bell Telephone Company presented in 'The Telephone Appeals,' before the Supreme Court of the United States, October term, 1886.

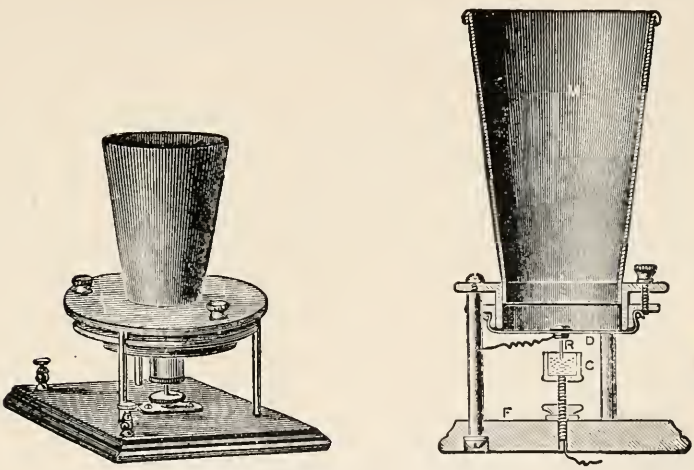
brane telephones. Upon placing the ear against the lid of the box the articulation of the person speaking into the membrane telephone was audible, proceeding from the lid (Fig. 2). A fourth form of



Bell's Centennial Iron Box Magnet Receiver.

FIG. 2.

speaking telephone was shown, intended to be used only as a transmitter. It consisted of a metallic ring supported horizontally, carrying a stretched membrane, to the center of which was fastened a platinum wire dipping into a little cup containing acidulated water and was referred to as the liquid transmitter (Fig. 3). In addition



Bell's Centennial Liquid Transmitter.

FIG. 3.

to these telephones, one of Koenig's manometric capsules was shown, arranged for studying optically the peculiarities of the electrical current generated by the movements of the membrane in the telephone.

Of telegraphic and signaling apparatus there was an excellent display at the Centennial, as well as a historical collection of great value showing the evolution of magnetic and electric signaling. But of other electrical apparatus there was only a meager display. All was included in Group XXV., Instruments of Precision.

Learning that the Centennial judges had arranged for a special inspection of certain telegraph and electrical apparatus on Sunday, June 25, when only those specially invited would be present and the building would be comparatively quiet, Mr. Hubbard sent a telegram requesting Graham Bell to arrive in Philadelphia not later than Sunday morning. But the class examinations were of far more importance just then than any explanation he could give the judges. So Graham Bell decided not to go. Then he received a message that Mr. Hubbard's daughter, Mabel (who a year later became Mrs. Bell), was leaving for Philadelphia, and hurried to the station in Boston to bid her good-bye. Mabel was as anxious as was her father to have Graham Bell explain his invention to the judges at the Centennial, and when she found that his sense of duty to his classes outweighed her influence, woman-like she burst into tears just as the train started. This was more than Graham Bell could stand, so he jumped aboard the moving train and with neither ticket nor baggage went through to Philadelphia.

As the weather was excessively hot in Philadelphia on Saturday evening Mr. Hubbard went to his Washington home 'tired out.' Sunday was another hot day, and after the judges had completed their inspection of all the other exhibits, and 'when it was already late, and the allotted time very nearly exhausted,' and while 'it was very warm, and signs of impatience were becoming manifest on the part of some of those present,' Graham Bell extended an invitation to the judges to inspect his telephones.

After some delay and discussion the judges proceeded to the Massachusetts section, where the apparatus was exhibited on a small table in a narrow space 'between the stairway and the wall.' Accompanying the judges were a large number of distinguished visitors, including the emperor of Brazil. Fortunately, but a short time before, the emperor had visited Graham Bell's school in Boston and had become interested in the method of instruction and also in the telephone. When Graham Bell saw that the emperor was with the judges he did not dream that his brief interview would be recalled. But his majesty cordially greeted him and spoke so enthusiastically about the telephone, that, tired as the judges were, they concluded to investigate thoroughly its merits. And from that moment the future of the telephone was assured.

Chief among the judges was Sir William Thomson (now Lord Kelvin), then and now the world's leading electrical scientist. He listened at the receiver, while Graham Bell's assistant talked into the transmitter. Sir William then went to the distant transmitter and repeated certain lines from Hamlet, which were heard as the receiver passed from one visitor to another. Forgotten was all thought of fatigue, of heat and discomfort, and nearly every visitor was glad of the opportunity of going to the end of the line and talking into that crude transmitter.

The line extended from one part of the building to another, and the transmitting and receiving ends were sufficiently far apart to prevent the possibility of the speaker's voice being directly heard at the receiving end through the air.

Only the membrane magneto transmitter and the iron box receiver, were used. In recounting the enthusiasm that was aroused, Professor Barker said:

I was greatly astonished and delighted to hear *for the first time* the transmission of articulate speech electrically. The mode of operation of the instrument was obvious at once, as soon as it was exhibited: it was one of those marvelously simple inventions that causes one to wonder, on seeing it for the first time, that it had not been invented long before.

At the close of the inspection Sir William Thomson expressed his regret that his wife had not been present to participate in such a marvelous experiment as the electrical transmission of speech, and asked Graham Bell if similar experiments could be enjoyed the following evening. Graham Bell replied that the apparatus was at the disposal of the judges and that they might experiment to their heart's content; but that he must be in Boston Monday morning in order to take care of the class examinations. To him, his school was of far more importance at that moment than 'the scientific toy' he had been chaffed about for many months. He left for Boston that evening and never returned to the Centennial.

That same evening the eminent English scientist, T. Sterry Hunt, wrote to Graham Bell:

I am informed that you leave to-night for Boston, so I take this way of congratulating you on your success to-day. I returned to my hotel with Sir William Thomson, and dined with him. He speaks with much enthusiasm of your achievement. What yesterday he would have declared impossible he has to-day seen realized, and he declares it the most wonderful thing he has seen in America. You speak of it as an embryo invention, but to him it seems already complete: and he declares that, before long, friends will whisper their secrets over the electric wire. Your undulating current he declares a great and happy conception.

On the following day the telephones were removed to the judges' pavilion, and on that Monday evening many experiments were carried on. Wires were first run from a table in the private room of the special jury on instruments of precision to a table placed in the hallway near the main entrance. Owing to the thinness of the partitions, and the possibility of the loud-spoken words being heard over so short a distance, the wires were extended and the transmitter and table were carried some distance out-of-doors. Sir William Thomson and Lady Thomson were present and conversed with each other. Sentences were read from the New York *Tribune,* such as 'the American residents in London have decided to celebrate the Fourth of July,' and as each sentence was received Sir William Thomson would write it down in his note book and then go to the transmitting end of the line and compare what he had heard with what had been read. Most of the routine transmitting was done by Professor Watson, of Ann Arbor, whose voice appeared to transmit most readily. "The particular instruments actually used were the 'membrane telephones' as transmitters and the 'membrane telephone' and 'iron box magneto receiver' as receivers." At the end of the week these instruments were replaced in the exhibit space in the gallery.

During that week thorough experiments were carried out and at their conclusion an award was made to Graham Bell by the judges of the group, while a special report drawn by Sir William Thomson, and a general report prepared by Professor Joseph Henry, secretary of the Smithsonian Institution and chairman of the judges, was published by the government.

Sir William Thomson said, in part:

In addition to his electric-phonetic multiple telegraph, Mr. Graham Bell exhibits apparatus by which he has achieved a result of transcendent scientific interest—the transmission of spoken words by electric currents through a telegraph wire. To obtain this result, or even to make the first step towards it—the transmission of different qualities of sounds, such as the vowel sounds—Mr. Bell perceived that he must produce a variation of strength of current in the telegraph wire as nearly as may be in exact proportion to the velocity of a particle of air moved by the sound; and he invented a method of doing so—a piece of iron attached to a membrane, and thus moved to and fro in the neighborhood of an electro-magnet—which has proved perfectly successful. . . . This, perhaps *the greatest marvel hitherto achieved by the electric telegraph*, has been obtained by appliances of quite a homespun and rudimentary character. With somewhat more advanced plans, and more powerful apparatus, we may confidently expect that Mr. Bell will give us the means of making voice and spoken words audible through the electric wire to an ear hundreds of miles distant.

The chairman, Professor Joseph Henry, in his official report, said:

The telephone of Mr. Bell aims at a still more *remarkable result*, that of transmitting audible speech through long telegraph lines. In the improved instrument the result is produced with striking effect, without the employment of an electrical current other than that produced by the mechanical action of the impulse of the breath as it issues from the lungs in producing articulate sounds. . . . Audible speech has in this way been transmitted to a distance of three hundred miles, perfectly intelligible to those who have become accustomed to the peculiarities of certain of the sounds. . . .

Another of the judges was Professor F. A. P. Barnard, president of Columbia College. A little later he publicly stated that

Of all instruments of precision and research which the group of Centennial judges was called upon to examine, there was none that occasioned greater interest or that they regarded as of higher novelty and importance than the speaking telephone of Professor A. Graham Bell,

and he was confident

that the name of *the inventor of the telephone* would be handed down to posterity with a permanent claim on the gratitude and remembrance of mankind.

Graham Bell was confident that he could transmit speech from Boston to Philadelphia, and, after his class examinations were over, he endeavored to secure the use of a telegraph circuit for that purpose, but failed because all 'were too busy.' Mr. Hubbard endeavored to secure a circuit from Philadelphia, but was unsuccessful for the same reason.

On July 11, 1876, Graham Bell varied the shape of the iron armature by attaching to the membrane a thin disk of Tagger's iron, almost as large as the membrane. The next day he gave one of these telephones to Sir William Thomson. It is said that during the trip home the armature became bent, and useless in that condition. Nevertheless, Sir William used it to illustrate to the members of the British Association for the Advancement of Science how Graham Bell's telephone was the most marvelous of all the wonderful exhibits he had seen in America.

Graham Bell continued his experiments in improving the telephone, and finding that the large iron disk was far superior to the small armatures previously used, he concluded to dispense with the membrane altogether. This he did in October, 1876. On fastening a thin disk of steel in front of the electromagnet, conversation was carried on more easily than ever before. Referring to this improvement, Mr. Storrow said:

> Perhaps the most important contribution which Mr. Bell made towards improving speaking telephony, after the great conception and original instrument of his first patent, consisted in the wonderful sensitiveness and quickness of operation which he introduced into the instrument of the second patent, in consequence of the conviction which he reached by *study*, thought and experiment, that by so proportioning and combining all his parts as to sacrifice absolute strength to absolute quickness, he could obtain the best results; and then his innumerable experiments led him to the surprising discovery that a piece of sheet iron was much quicker and more faithful in following the delicate changes required for speech than the most delicate membrane is.

Nine years later the commissioner of patents, under date of March 3, 1885, wrote:

> Bell's patent was issued on the 17th of March, 1876. At the Centennial Exposition, held at Philadelphia that year (1876), he exhibited his telephone, and it was adjudged by such eminent scientists as Professor Henry, Sir William Thomson, of England, and Professor Gray, one of the contestants herein, to be a success, and the world recognized Bell as the first inventor of a speaking telephone. The indications are that it was not until the promised reward for so important a public service became visible that his claim of priority was called in question by any of the parties to this interference.
>
> That all the efforts of the several contestants who had attempted to produce a speaking telephone were failures seems clear from the record: that Bell was the first to give to the world the art of transmitting articulate speech, and the apparatus by which it could be successfully practised, was substantially conceded for a long period after his success in that behalf was placed beyond doubt. Whether or not these several contestants had the instrumentalities and appliances at that time from which success might have been realized if those instrumentalities had been better understood is of little consequence. The history of their experiments is a history of recorded failures.

III. *Devising the Telephone Exchange System*

Thirty years ago this summer the annual meeting of the British Association for the Advancement of Science was held in Glasgow, Scotland. On that occasion men eminent in their respective professions listened with the deepest interest, while the president, Sir William Thomson (now Lord Kelvin), gave a vivid description of his visit to the Centennial, and stated that the most marvelous of all the wonderful exhibits he had seen in America was a pair of rudely-constructed telephones!

Then he explained to the members how surprising it all seemed when on that memorable Sabbath in June, 1876, to his listening ear came the words spoken at the distant end of the line; and he added:

> All this my own ears heard, spoken to me with unmistakable distinctness by this circular disk armature of just such another little electro-magnet as this which I hold in my hand. . . . This, the greatest by far of all marvels of the electric telegraph, is due to a young countryman of our own, Mr. Graham Bell, now becoming a naturalized citizen of the United States. Who can but admire the hardihood of the invention which devised such a very slight means to realize the mathematical conception that, if electricity is to convey all the delicacies of quality which distinguish articulate speech, the strength of this current must vary continuously and as nearly as may be in simple proportion to the velocity of a particle of air engaged in constituting the sound?

Sir William Thomson was then and is now the leading electrician of the world. And it was this generous endorsement of Alexander Graham Bell's invention that brought the telephone to the attention of scientific bodies in all countries, and led learned men in all lands to investigate its merits and to strive to improve its technical value. For in its remarkable simplicity the invention was a disappointment to many men, until practical experience demonstrated that the more elaborate copies were no more serviceable as speech-transmitting devices than the primitive original instrument. Nor during all the intervening years that have elapsed since 1876 has any inventor or any mechanician or any scientist ever suggested a more complete or a simpler description of the conception of the electric-speaking telephone and its governing principles than Graham Bell embodied in his application for a patent.

Yet that simple invention has exerted a far more potent influence than any of the more attractive fruits of inventive genius in revolutionizing and enriching custom and method in almost every branch of industry, of commerce and of society. And no other invention has so marvelously increased the scope of human usefulness and intelligent activity. With its aid time and distance are virtually eliminated, and Maine and Missouri and Mississippi and Minnesota are distant from each other only the length of a telephone call.

Yet marvelous as was the achievement of inventing the electric-speaking telephone, equally meritorious was the breadth of mind that could entertain at a time when poverty was pressing a prophetic vision of one vast transcontinental telephone system uniting every important village, town and city with wire highways over which messages would speed as quickly as thoughts are spoken.

Had the telephone been sold outright, in place of being leased for use in designated territory, it is very doubtful if a transcontinental system could have been established, or the full intercommunicating value of the telephone developed. For it is one of the few natural monopolies. Foreign telephone experts say that the American telephone system has no equal in scope and efficiency, which is a gratifying endorsement, in view of the fact that the foundations of this American transcontinental system were not laid by men long skilled in an established art, or men who wielded the power inherent in great financial resources, but by men who strove against the combined forces of complete absence of telephonic experience. practise and knowledge, of the destructive power of the elements, and of human greed that would publicly rob vested right and good name.

Had these pioneers comprehended all that was to be endured, the losses, the bitter competition, the costly litigation, how many would have had the courage to imperil funds and business reputation in so hazardous an undertaking? For never before did an industry progress so rapidly as is recorded of the art of telephony, none ever had to face such costly, peculiar and ever-expanding demands, and none was ever so bitterly and so unjustly assailed.

These pioneers soon found that one set of telephone equipment would scarcely be installed by a local company before it would have to be displaced by improved apparatus, if the field was to be held. Or an unexpected marvelous growth in the number of subscribers would compel complete rebuilding of lines and the installation of more improved apparatus. Came storms of wind and sleet wrecking miles of pole line; flashed the lightning, burning out every coil in the plant; came the newly-invented electric lights rendering the service useless after night-fall until circuits were rearranged; came the trolley, making metallic circuits a technically and a judicially determined necessity; all in the brief span of eight years. 'Nothing is constant but change,' was a sentiment readily subscribed to by pioneer telephone men.

Yet notwithstanding discouragements, disasters and hardships, of a character unknown before, the dream of yesterday is the realization of to-day. For now there are nearly three million telephones connected to this one transcontinental system, receiving service over a total wire mileage exceeding five million miles, while the actual cash investment in *new construction alone* expended by the companies forming this great system during the *past five years only* aggregates two hundred millions of dollars.

How came the first commercial telephone exchange to be devised? is a question often asked. The idea of a central exchange telephone system was one of Graham Bell's earliest conceptions in connection with the possible utilization of the telephone. Thus it came in the natural development of so useful a public-service function as telephone service. Prior to the exhibition of the telephone at the Centennial Graham Bell had frequently discussed with his partners, Mr. Hubbard and Mr. Sanders, the great value of a telephone exchange system covering an entire city, while in the lectures delivered in various cities during the winter of 1876–7 Graham Bell outlined many of the principal features that later were embodied in the early telephone exchanges.

However, it should be borne in mind that by reason of the existence in the larger cities of local telegraph central offices or exchanges, operating on lines somewhat similar to the early telephone exchange, the probable usefulness of a telephone system should have appealed to many whose experience with an intercommunicating system might have enabled them to forecast the development of the telephone exchange system. But the contrary appears true.

The central district-telegraph offices were in existence ten years before the invention of Bell's electric-speaking telephone. Through one 'central' the banks and the clearing-house were connected. Stock quotations and the premiums on gold were sent to brokers through another. Many lawyers maintained a 'central' in one city. In a second city the steel mills and factories were thus connected; while in a third city the newspapers maintained a telegraph exchange.

But none of these systems afforded communication other than by electrical apparatus mechanically operated, as, for instance, a dial telegraph, or a printing telegraph system, or an ordinary Morse key and sounder; apparatus and methods in no wise requiring the aid of the complex exchange mechanism known as the modern telephone switchboard. Nor were there any known means prior to 1876, of distant oral communication, aside from the speaking-tube. Yet, in many cities, the telegraph 'central' was the nucleus from which the respective local telephone exchange was evolved.

For after the newspapers and the magazines had made known the high esteem in which Sir William Thomson and other scientists held Graham Bell's telephone, a number of these 'central district' companies, operating electrical communicating circuits investigated the advisability of adding this new device as a side issue, or were invited to make a trial of the telephone in the belief that it would prove a good revenue producer. After investigating the merits of 'Bell's scientific toy' as many called it, some of these electric-service companies could perceive no profit in introducing this 'toy,' placed no commercial value on its serviceability in affording communication over distance, or comprehended its usefulness. And it may be safely stated that, in 1877, less than a score of men foresaw the marvelous future of the telephone or could grasp the meaning of its revolutionizing possibilities, or ever dreamed of such a phenomenal growth in so short a period as a quarter of a century. Even so experienced a man in the world's work as a former president of the Western Union Telegraph Company declared, early in 1877, that 'the telephone could never be of any practical use in business affairs'; while men eminently successful in industrial and commercial circles ridiculed the inventor as 'the man who is trying to make the people believe you can talk through a wire,' and scoffed at his invention.

But Graham Bell's faith in the usefulness and the value of his invention and in its power to eliminate distance in many of the affairs of life never failed. He wasted no time lamenting over evil predictions of failure. To him life was rich in possibilities that come only in the dreams of the unselfish toiler for the welfare of others. And wisdom taught him that brooding never brought fruition. So, early in 1877, he and his partner, Gardiner Greene Hubbard, planned the telephone exchange system somewhat along the lines it developed later, including a trunking-system to connect the different exchanges in the same city, toll lines connecting adjoining cities, suggested the use of aerial cables and underground circuits, as a substitute for the many wires they clearly foresaw would be required in the near future, should the system prove a success, and discussed the advisability of adopting either a fixed rental or flat rate per month or of charging for each 'switch' or call, similar to modern measured-service methods.

And though several companies and many individuals failed in their efforts to successfully introduce the telephone in their respective localities, and gladly seized the opportunity to surrender the license previously obtained for a nominal payment, others were induced to continue in the telephone business, only through the earnest assurance of Graham Bell and his associates that they were planning a comprehensive system continental in its scope, and that, in perfecting these plans they were carefully considering every feature that would enable the public to rapidly derive the greatest possible benefit from Bell's invention.

That all these plans were thoughtfully considered and final decision made on a broad basis are clearly shown in the determination that the telephone should be leased and never sold, and that, while the installation of private lines yielded an immediate profit, the exchange system was the only true field for development. Thus it came about that gradually Graham Bell won staunch supporters to his way of thinking, who comprehended the true function of the telephone and perceived the commercial possibilities in the telephone exchange system. These broad-minded and enthusiastic adherents sought prospective customers among men who had long felt the need of a different method of communication from any then in vogue, and found these patrons on every hand.

As sufficient capital could not be secured in 1877–78 by Graham Bell's friends to enable one company to establish telephone exchange systems in a number of cities, the only recourse open was to afford every encouragement in the establishment of local plants by men of local influence or hustling ability, who shared in the optimistic views of Graham Bell and Mr. Hubbard. While this latter plan would provide the quickest method of meeting the universal demand that Graham Bell was confident would develop, it might not prove the best plan for the public in the long run. For it meant a large number of different companies, governed by as many different policies, and operating under an endless number of systems. While under one company only one policy would prevail, system, equipment and methods would be standardized, and interconnecting lines built as rapidly as the necessary funds could be procured. Thus it was to determine which was the better plan, that Bell licenses were at first issued only for limited periods of five or ten years, with the understanding that at the expiration of the agreed term, the Bell company could take over the local exchange system at cost price, if it so desired.

Again it was quickly perceived that the true value of the telephone was intimately interwoven in the breadth and scope of the exchange system, and that its sphere of usefulness would be seriously curtailed unless combined with all the rights and privileges that a corporation secures from borough and township, county and state, rights that include the building of pole lines on highways and streets, the stringing of aerial circuits and the placing of cables underground. In other words, that the local value of telephone service was in direct ratio to the physical expansion of the exchange system, and in its capacity for meeting the ever-varying requirements of local users. Without the telephone the exchange system would have no value; without the exchange system the telephone would have comparatively little value.

By reason of the very nature of the invention, the right to use the telephone had to be safeguarded as few other patents have been, not only for the protection of the owner, but also that the licensee company might not suffer. A mowing-machine is a self-contained piece of mechanism that can be operated in any field, regardless of the number of or the absence of other mowers. The fewer mowing machines in a county the greater the probable profit of the owner of a machine intended for general use, and the greater its usefulness in a farming community. On the contrary, the fewer telephones in a county the more limited the value of the service to the public. While the greater the number of telephones and the more comprehensive the scope of exchange and toll system, the richer the benefits conferred upon the public, provided all telephones in the county are intercommunicating.

(*To be continued.*)

THE JEWS: A STUDY OF RACE AND ENVIRONMENT

BY DR. MAURICE FISHBERG

NEW YORK CITY

II. *Marriages.*

ONE of the most important causes of the low birth rate of the Jews is their low marriage rate. Only about fifty years ago an unmarried Jew was very rare in Europe, while an old maid was hardly to be met with in the Ghetto. They then followed closely the rabbinical ordinances: "It is the duty of every Israelite to marry as early in life as possible. Eighteen years is the age set by the rabbis; any one remaining unmarried after his twentieth year is said to be cursed by God Himself. Some rabbis urge that children should marry as soon as they reach the age of puberty, *i. e.*, the fourteenth year. A man, who, without any reason, refuses to marry after he has passed his twentieth year is frequently compelled to do so by court."[1] These Talmudical ordinances are not observed to-day by the bulk of European and American Jews, and their marriage rates are much below those of the christian populations among which they live.

Taking first statistics of the crude marriage rate, *i. e.*, the annual number of marriages per 1,000 population, we find in every country in Europe, where data are available, that the rates for the Jews are lower, as can be seen from the following table:

Country.	Period.	No. of Marriages per 1,000 Population.	
		Jews.	Christians.
Algeria	1903	9.12	8.17
Germany	1903	7.10	8.27
Prussia	1904	7.37	8.56
Bavaria	1903	7.80	7.70
European Russia	1897	7.37	9.48
Warsaw, Poland	1901	6.71	8.72
Roumania	1902	10.30	18.70
Hungary	1900	8.04	8.84
Bohemia	1900	7.24	8 26
United States	1886–90	7.40	——

It must be emphasized that even these figures do not give an adequate idea of the low marriage rates of the Jews, because the Jewish population contains a smaller number of children and larger proportion

[1] 'Jewish Encyclopedia,' Vol. VIII., p. 347.

of adults of marriageable ages. If statistics of the number of marriages per 1,000 Jews over fifteen years of age, and especially of unmarried adults, were obtainable, the rates for the Jews as compared with the christians in Europe would show that they are yet less apt to marry than the figures in the above table indicate. Thus it was found in Berlin during the census of 1900 that of all persons over twenty years of age the following percentage were married:

	Men.	Women.
Jewish	51.62 per cent.	52.51 per cent.
Christian	60.38 per cent.	53.83 per cent.

These low marriage rates, which are only a recent social phenomenon among the Jews, are not confined to western Europe. Even in Russia, where the bulk of the 5,000,000 Jews live to-day under strict adherence to their faith and traditions, early marriages are less frequent than among the christians. The poor muscular development of the eastern European Jews, which has in part been attributed to early marriages, will have to be explained by some other causes. The following figures, from the census of Russia of 1897, give the percentage of persons who married at certain ages, both among the Jews and among the general population of the so-called 'Pale of Settlement.'

Age.	Men.		Women.	
	Jews.	General Population.	Jews.	General Population.
20 and less	5.95	31.22	27.76	55.01
21 to 25	43.73	35.64	51.23	31.57
26 to 30	34.05	18.94	12.68	6.96
31 to 35	6.52	5.61	6.69	2.80
36 to 40	3.39	3.30	1.86	1.65
41 to 45	1.75	2.13	0.90	0.97
46 to 50	0.54	1.41	0.81	0.61
51 and over	3.07	1.65	1.07	0.42

It appears from this table that the Jews in Russia marry later in life than the christians in that country. Only 5.95 per cent. of all the Jews who married in 1897 were less than twenty years of age, while about five times as many christians married at this youthful age. Even among the Jewesses only 27.76 per cent. married before reaching twenty, as against double that number, 55.01 per cent., among christian women. Marriages between twenty and thirty, on the other hand, are more frequent among the Jews—77.78 per cent. as against only 54.58 per cent. among christians, and 63.91 per cent. among the Jewesses, and 38.53 per cent. among the christian women. Finally, marriages among persons at advanced ages, over thirty, are contracted in about the same proportions in both groups. In general, Russia has an extraordinarily large number of youthful marriages. Nowhere in western Europe is there to be found more than four per cent. of bridegrooms under twenty years of age. This is of course due to the numerical predominance of agricultural workers with a communistic arrangement of the village community. The Jews in that country live mostly in cities, are either manufacturers, merchants, etc., or skilled mechanics. Marriage among such people must be postponed till business assures a secure income, or till the individual has attained skill in his trade or profession. The low birth rates of the Jews in Russia (compared with the non-Jewish population of that country) is here partly explained. The later an individual marries, the shorter is the period of the married state, and the number of children to be expected is smaller. In fact the census of 1897 showed that there were five per cent. more unmarried Jews than unmarried christians, as can be seen from the following figures:

Social State.	Jews.		Greek Orthodox.	
	Men.	Women.	Men.	Women.
Single	61.20	57.35	55.95	52.09
Married	36.76	36.07	40.28	39.42
Widowed	1.82	6.01	3.68	8.36
Divorced	0.16	0.49	0.03	0.04

In this connection it is interesting to note that the percentage of protogamous marriages is smaller among the Jews in Russia than among the general population. The marriages between bachelors and maids constituted 83.73 per cent. among the general population and only 80.72 per cent. among the Jews. Second marriages are more frequent among the Jews; and contrary to the almost general experience that widowers are more likely to marry maids than widows, the Jewish widowers in Russia more often marry widows. These peculiarities are explained by social customs.

We can find nothing in the above figures which would indicate any racial influence on the marriage rates of the Jews. In fact the evidence indicates that it is purely a social phenomenon. The Jews in eastern Europe marry earlier and have a smaller proportion of celibates than their coreligionists in western Europe, because the latter enjoy on the average a better social and economic prosperity. When compared with their non-Jewish neighbors in eastern Europe, the Jews have a lower marriage rate, because they have very few agricultural laborers, and have, on the other hand, a larger number of merchants, skilled workmen, etc., whose birth rate is lower among all peoples in Europe. In oriental countries, like Palestine, Turkey, Persia, Morocco, Algeria, Tunis, etc., where the Jews live under a primitive culture, and are entirely unaffected by any occidential influences, the Jews to-day marry very early. Husbands at fourteen and wives at the same age are not uncommon. Their birth rate is also above that of the non-Jewish population in those countries, as was shown to be the case in Algeria.

In western Europe and America, where the Jews have been intensely influenced by the occidental social environment, their marriage rates are low. But even here conditions have not always been the same as we find them to-day. Statistics of marriage rates in the beginning of the nineteenth century show conclusively that then the Jews married earlier and had comparatively fewer celibates than the christian population of Germany. Their economic, social and cultural condition at that time was about the same as that of the Jews in eastern Europe to-day. Even as late as 1861 to 1870 Austrian statistics show that 34.3 per cent. of all the Jews who married were less than twenty-four years of age, as against only 17.6 per cent. of christians who married thus early; 23.5 per cent. of the Jewesses were married at this early age, and only 15.1 of christian women. Conditions have changed recently, as was shown above, going hand-in-hand with the change in the sum total of the social, economic and intellectual conditions in which the Jews find themselves at the beginning of the twentieth century.

Consanguineous Marriages

The extraordinarily large number of physical and mental defectives met with among the Jews in Europe has been in part attributed to the frequency of marriage of near kin among them. All available statistical evidence shows that consanguineous marriages are much more often contracted among Jews than among others. Jacobs, adopting Sir George H. Darwin's method, shows that in England 7.5 per cent. of all Jewish marriages are between cousins, while among Englishmen only two per cent. are of this class. Stieda found that in Lorraine the proportion of consanguineous marriages is 1.86 per thousand ordinary marriages among the protestants, 9.97 among catholics, and 23.02 among Jews. In Hungary marriage of near kin can only be contracted after a special permission has been obtained from the civil authorities. The data on the subject in that country are therefore reliable. During 1901 such permission was granted 270 times to Jews and 1,217 times to others. On a basis of the population, it thus appears that the Jews obtained proportionately about five and one half times as many permits as the christians. Among a thousand christian marriages 5.8 were between cousins, while among a thousand Jewish marriages there were 39.3. In Prussia the rates were in 1872–5, among the Jews 23.08 per thousand ordinary marriages; protestants, 14.68, and catholics, 9.98. From figures collected by Treitel in Berlin it appears that during 1900 the proportion of consanguineous marriages was among the Jews 23.3, and among protestants only 6.1, per 1,000 marriages.

It is doubtful whether this inbreeding is the cause of most of the diabetes, insanity, idiocy, deaf-mutism, etc., encountered among the Jews. It is at present the consensus of opinion that consanguineous marriages contracted by healthy individuals are not at all detrimental to the offspring. But when contracted by defectives, the physical or mental defect is likely to appear in a more accentuated form in the progeny. It must in this connection be mentioned that consanguineous marriage among the European population rarely exceeds one per cent. of all marriages and is more frequent, as a rule, in the country than in the city. The Jews, as city dwellers, ought to have a still smaller proportion of marriages of near kin.

Mixed Marriages.

The prevailing opinion that Jews have always refrained from intermarriage with non-Jews is erroneous. Biblical tradition shows clearly that the ancient Hebrews intermarried freely with their Gentile neighbors. Some of the most important patriarchs (Abraham, Joseph, Moses) and kings (Solomon) had gentile wives. The prophets, Ezra and Nehemia, both arraigned the Hebrews for their carelessness in this respect and appealed to them to maintain the purity of the race of Israel. A study of their history shows that the Jews have always more or less intermarried with the races and peoples among whom they lived. This was particularly the case during the Hellenic period, and in Spain for some time before their expulsion. Only during medieval oppression and persecution, rigid confinement in the Ghettos and strict isolation have the Jews married exclusively among themselves.

In recent times the Jews again began to marry with christians in Europe and in America. In some countries this intermarriage has assumed such proportions as to threaten the integrity of Judaism. A survey of the census statistics published in various European countries reveals that wherever the Jews are subject to special adverse legislation, segregated in Ghettos and deprived of friendly social intercourse with their non-Jewish neighbors, very little or no intermarriage occurs. This is particularly the case in oriental countries, like Turkey, Persia, Morocco, etc., and also in semi-oriental countries, like Russia. There intermarriage is prohibited by law; unless the Jew first accepts christianity or mohammedanism, he may not marry a gentile. On the other hand, in western Europe, where he enjoys all the privileges of a free citizen, mixed marriages are of frequent occurrence. In fact, in some countries even more frequent than intermarriage between catholics and protestants.

Country.	Period.	To 100 Pure Jewish Marriages there are Mixed.		
		Husband. Christian.	Wife. Christian.	Total.
Copenhagen	1880–1903	—	—	65.38
Germany	1901–1904	8.01	9.26	17.27
Prussia	1875–1879	5.37	4.46	9.83
"	1880–1884	5.11	5.25	10.36
"	1885–1889	5.89	6.46	12.35
"	1890–1894	6.22	6.34	12.56
"	1895–1899	7.91	9.04	16.95
"	1900–1904	9.02	10.24	19.26
Berlin	1875–1879	16.43	19.64	36.07
"	1895–1899	13.07	21.05	34.12
"	1904	15.10	24.00	39.10
Bavaria	1876–1880	2.18	1.68	3.86
"	1881–1885	1.46	1.79	3.25
"	1886–1890	1.84	2.63	4.47
"	1891–1895	3.41	3.41	6.82
"	1896–1900	2.97	5.87	8.84
"	1903	4.18	3.05	7.23
Hesse	1866–1870	0.50	—	0.50
"	1871–1880	1.82	0.89	2.71
"	1881–1890	2.82	1.59	4.41
"	1891–1900	3.06	1.90	4.96
"	1901–1904	3.16	4.17	7.33
Amsterdam	1899–1901	—	—	9.45
"	1902–1904	—	—	15.08
Hungary	1895–1903	2.98	2.97	5.95
Budapest	1896–1900	7.22	6.11	13.33
"	1901–1902	7.86	6.98	14.84
"	1903–1904	8.22	8.84	17.06

The German censuses are particularly reliable because they have been collected for many years with great care, with a view to eliciting the degree of assimilation of the Jewish population. During the four years 1901 to 1904 there were contracted in Germany 15,635 marriages between Jews and Jewesses, and 2,700 between Jews and christians. The mixed marriages constituted consequently 17.27 per cent. of the pure Jewish marriages, or one sixth of all the Jews who married married christians. It was also found that 8.01 per cent. of all the Jewesses and 9.26 per cent. of all Jews in that country married christians. In other words, every twelfth Jewish bride and every eleventh Jewish bridegroom married a christian.

The largest number of mixed marriages in Germany are contracted in the province of Prussia. There statistics for twenty-eight consecutive years are available (1876–1904), during which 71,160 pure Jewish marriages were contracted; 4,740 between christians and Jewesses and 5,062 between Jews and christian women. It was also established that the number of mixed marriages is constantly on the increase, as can be seen from the following figures:

	Average Annual Number.
1875–1879	239
1895–1899	433
1900–1904	495

In Berlin the proportion of mixed marriages is about twice as large as in Prussia generally. During 1875–9 the average annual number was 101; during 1895–9 it was double, 201; during 1898–1902, 212, and in 1904 it rose to 246. During the last mentioned year 24 per cent. of all the Jews who married married christian women, and 15.1 of the Jewesses married christians. In other words, every fourth Jew and every seventh Jewess in Berlin who married during 1904 married a christian.

In the other provinces of Germany mixed marriages are also more or less frequent. The record for Hamburg is, during 1896 to 1900, that 5 per cent. of Jewesses and 8.3 per cent. of Jews marry outside of their faith. In Bavaria during the twenty-five years, 1876–1900, it was found that to every 100 pure Jewish marriages contracted, 5.35 were contracted between Jews and christians. That these marriages are on the increase is shown in the table on page 446. The percentage was 3.87 in 1876–80 and 9.0 in 1904. Similarly in Hesse, where in 1866–70 only one in 200 Jews who married, married a christian, mixed marriages have since been on the increase, so that during 1901–4 the mixed marriages amounted to 7.33 per cent. of the pure Jewish marriages. Even in Amsterdam, where the most orthodox Jews are living, and up to about fifty years ago, hardly any Jew married out of his faith, there are to-day a very large number of such unions. During 1899–1901 the mixed marriages constituted 9.45 per cent. and during 1902–3 the proportion increased to 15.08 per cent.

The largest percentage of mixed marriages are contracted in Copenhagen, Denmark, where statistics, recently compiled by Julius Salomon, show that during the twenty-four years, 1880 to 1903, there were contracted 358 pure Jewish marriages and 234 mixed marriages, or 65.36 per cent. of the pure Jewish marriages. How far these marriages are, in that city, in vogue among the Jews is seen by the fact that from 1880 to 1890 the percentage was only 55.17; it rose to 71.03 per cent. during 1891 to 1900, and from 1901 to 1903 it was already 89.74 per cent. In former years we are told that the Jewish rabbis refused to admit children born from mixed marriages to Judaism, but of late years the Jewish community in that city authorized several Jewish physicians to circumcize such new-born boys. This is not confined to Copenhagen alone, but is characteristic of Scandinavia in general. In Denmark there were contracted, during 1873–91, 308 Jewish marriages, of which 187 were pure and 121 mixed, *i. e.*, 64.71 per cent. Since then the increase has been enormous. In the neighboring country, Sweden, it is stated that the number of mixed marriages is much in excess of the number of pure Jewish marriages.

In France and Italy also Jews frequently marry christians. This is particularly the case with the French aristocracy, who often marry Jewish heiresses.[2] In Italy the Jews to-day are thoroughly assimilated, and many observers state that mixed marriages are almost as frequent as pure marriages.

In eastern Europe they are less frequent. In Austria, during 1901, there were contracted 7,576 pure Jewish marriages and 147 mixed marriages. But here we can see how far isolation acts as a preventive of intermarriage. Of the 147 mixed marriages contracted during 1901, 98 were in the city of Vienna and 25 in Bohemia. Although three quarters of all the Austrian Jews live in Galicia, still not a single case of intermarriage was recorded there during that year. It must also be mentioned that in Austria intermarriage between Jews and christians is not permitted by the law, and in many cases of mixed marriages, one of the parties adopts the religion of his or her partner, and the marriage is thus recorded as pure christian or pure Jewish; or one or both declare themselves as dissenters (Konfessionslos) and appear on the registration lists as the marriage of a Jew with a dissenter, or of dissenters. As a result of this condition the available figures do not by any means represent the true condition of affairs.

Up to 1895 intermarriage was entirely prohibited by law in Hungary, unless one of the parties was converted to the religion of his or her partner. Since this law was abolished in 1895, mixed marriages are taking place in large numbers. During the nine years, 1895 to 1903, 3,590 Jews married with christians, and 60,275 with Jews, *i. e.*, 5.95 per cent. of all pure Jewish marriages were mixed. Most of these marriages are contracted in the city of Budapest, where the proportion reached 17.06 per cent. during 1903 and 1904. The steady increase of mixed marriages in that city is well seen from the following figures:

	1896–1900	1901–1902	1903	1904
Jews to christian women..........	6.71	6.98	8.41	7.86
Christians to Jewesses............	7.22	7.86	7.85	7.36

It thus appears that every thirteenth Jew who married in Budapest, during 1904, married a christian.

For English-speaking countries there are no available statistics on the subject of intermarriage, because no religious censuses are taken. In England they occur often among the native Jews, and although among the immigrant Jewish population in London they are less frequent, still they are not as rare as is generally believed. In New South

[2] It has been alleged that most of the mixed marriages are contracted between christian noblemen and rich Jewish heiresses. This is disproved by the figures in the table of mixed marriages. The proportion of Jews who marry christian women is larger than that of christians who marry Jewesses.

Wales it was found, while taking the census of 1901, that of all married Jews, 781 were married to Jewesses and 686, *i. e.*, 87 per cent., were married to christians.[3]

In the history of the Jews in the United States there are many instances of intermarriage between Jews and christians, even in Colonial times. According to Professor Hollander, the well-known 'Ye Jew doctor,' Jacob Lumbrozo in Maryland married a christian woman about 1660.[4] Dembitz shows that "there is no frequenter of the synagogue who either lived in Kentucky or whose ancestors lived there before 1836," and he gives as a cause that the early Jewish settlers disappeared through intermarriage with christians "and the descendants of the early Jewish settlers are known only by their Jewish family names and their oriental (?) features."[5] One has to read detailed accounts of several Jewish families in New York, Pennsylvania, Connecticut, Massachusetts, etc., to be convinced as to the extent of mixed marriages in pre-revolutionary times. The Franks family is particularly interesting: One daughter, Rebecca, married Sir Henry Johnson; another, Mary or Polly, married Andrew Hamilton.[6] About New York, M. J. Kohler says in his work 'Jewish Life in New York before 1800' that "several cases are at hand of intermarriage between Jews and Jewesses to christians and occasional conversions to the prevailing religion."[7] In the 'Biographical Sketches of the Graduates of Yale College,' Vol. II., 1763, two Jews are mentioned, one 'of Jewish extraction' who became a prominent citizen and one of the founders of the episcopal church in Norwalk; the other married a woman of French Huguenot descent. In Ohio also all traces of the early Jewish settlers have been lost. One is mentioned who was married 'out of his faith,' but when he died, in 1821, he asked to be buried with Jewish rites.[8] Speaking of Judah P. Benjamin, of New Orleans, whose wife was a devout catholic and whose daughter married Captain Henri de Bousignac, of the 117th regiment of the French line, Kohler says: "Such intermarriage was, in 1833, not uncommon." A Jewish traveler in New Orleans in 1842 speaks of the synagogue, which merely accommodated fifty persons, and a former "rabbi, a Dutchman, had married a catholic wife, who with difficulty was restrained from sending a crucifix to his grave at his burial."[9]

---

[3] Census of N. S. W., 1901, Bull. No. 14.

[4] Public. Jewish Histor. Soc., I., p. 29.

[5] *Ibid.*, pp. 99–101.

[6] Westcott, 'Historic Mansions,' quoted from Publ. Jew. Histor. Soc., I., pp. 57–58.

[7] *Ibid.*, II., p. 91.

[8] *Ibid.*, VII., p. 43.

[9] *Ibid.*, XII., pp. 68–69.

In more recent times intermarriage of Jews with christians in the United States has continued unabated. In fact, very few of the original Jewish settlers, who were of Spanish and Portuguese origin, have left descendants. Most have been absorbed by intermarriage.[9] For lack of denominational statistics, it is impossible to state accurately the extent of intermarriage between Jews and christians in the United States at the present time. It is known to be very common in the western and southern states, and less so in the eastern states. In a recent paper by Professor M. Schlesinger. of the Hebrew Union College, he quotes Rabbi George Zepin, director of circuit preaching, to the effect that in the northern part of the United States five per cent. is the maximum proportion of mixed marriages, while in the south the proportion ranges from 20 to 50 per cent., 33 per cent. being most nearly correct.[10]

There are no available statistics as to the frequency of mixed marriages in New York City to-day. From the census of the Federation of Churches taken in 1902 of the Twenty-second Assembly District (37th to 55th streets, east side), there were found several cases, about one per cent., of intermarriage. But this does not by any means represent the real conditions. Among many of the older Jewish families in this city we find many cases of intermarriage, and even on the east side, among the immigrant Jews, they are no more rare. In the western states it is very common, and the rabbis have of late actually sounded an alarm as to the danger of mixed marriages.

We can conclude with the words of Arthur Ruppin, who thoroughly studied the problem in Europe: "The best preventive of intermarriage is the Ghetto. In Galicia, Russia, the east end of London and the east side of New York, they are rare. But in countries where the Jews participate in the social and economic life of the general population as equals mixed marriages do occur and are steadily increasing in frequency, as is the case in Prussia, Denmark, Hungary, Italy, the western states, etc." The largest proportion of mixed marriages in Europe is contracted in large cities, as we see from the figures given above is the case in Berlin, Vienna, Budapest, Copenhagen, etc. This is because in large cities the population (Jewish as well as christian) has a higher intellectual standard, and these are better opportunities for people of different faith to come into intimate contact with each other.

---

[10] See Public. Jewish Histor. Soc., Vol. VI., pp. 92–93.

(*To be continued.*)

JOHN STUART MILL

BY PERCY F. BICKNELL

"I FIND it hard to say why I dislike John Stuart Mill," writes Lowell to Leslie Stephen, "but I have an instinct that he has done lots of harm."

"For the sake of the House of Commons at large," says Gladstone in a letter to Mr. W. L. Courtney, Mill's biographer, "I rejoiced in his advent, and deplored his disappearance. He did us all good. In whatever party, whatever form of opinion, I sorrowfully confess that such men are rare." "A wiser, more virtuous man I have never known, and never hope to know," was Mr. John Morley's pronouncement in a speech delivered soon after Mill's death.

To continue a little further these contrasting opinions concerning a philosopher and reformer, the centennial recurrence of whose birthday directs our thoughts upon him at this time, we find Professor Jevons somewhat petulantly exclaiming: "For my part I will no longer consent to live silently under the incubus of bad logic and bad philosophy which Mill's works have laid upon us. . . . In one way or another Mill's intellect was wrecked. The cause of injury may have been the ruthless training which his father imposed upon him in tender years; it may have been his own life-long attempt to reconcile a false empirical philosophy with conflicting truth. But however it arose, Mill's mind was essentially illogical."

"Mill's intellect was essentially of the logical order," declares his biographer and expounder, Sir Leslie Stephen. The late E. L. Godkin called him "the most accomplished of modern dialecticians." Herbert Spencer, referring to Mill's influence on current English philosophical speculation, was of opinion that "by his 'System of Logic' Mr. Mill probably did more than any other writer to awaken it." Henry Sidgwick praised "the unequalled mastery of method which his logical speculations developed."

Now that Mill has been dead a third of a century, it may be worth while to take the occasion of this hundredth anniversary of his birth to review briefly the estimation in which he was held by his contemporaries, and to consider how much and what part of his fame of thirty-three years ago is now alive and likely to survive. His lasting influence, whether for good or for ill, is of course not accurately determinable; for, as Professor Bain has well said, "no calculus can integrate the innumerable little pulses of knowledge and of thought that he has made to vibrate in the minds of his generation"—and, one may add, in the minds of the succeeding generation.

That keen, alert intelligence, trained, as an athlete is trained, to the very maximum of efficiency, if not indeed overtrained, can not but command the admiration of Mill's readers to-day, as it did that of his contemporaries. One can hardly read his remarkable 'Autobiography' without acknowledging in Mill a kind and a degree of intellect so far out of the ordinary as to approach the præter-human—we will not say the superhuman. "Logic-chopping engine," Carlyle may dub his old friend, as he emphatically does in describing the 'Autobiography' to his brother John. "I have never read a more uninteresting book," he declares, "nor should I say a sillier, by a man of sense, integrity, and seriousness of mind. . . . It is wholly the life of a logic-chopping engine, little more of human in it than if it had been done by a thing of mechanized iron. Autobiography of a steam-engine." "A fascinating book it is from beginning to end," said Edward Everett Hale, in reviewing the work at the time of its appearance.

Probably the one book of Mill's that has done more than any other to awaken interest and arouse enthusiasm is his treatise on 'Liberty.' Its merits are well indicated in an adverse criticism that occurs in one of Caroline Fox's letters. "I am reading," she says, "that terrible book of John Mill's on Liberty, so clear and calm and cold; he lays it on one as a tremendous duty to get oneself well contradicted, and admit always a devil's advocate into the presence of your dearest, most sacred truths, as they are apt to grow windy and worthless without such tests, if, indeed, they can stand the shock of argument at all. He looks you through like a basilisk, relentless as fate. . . . No, my dear, I don't agree with Mill, though I, too, should be very glad to have some of my 'ugly opinions' corrected, however painful the process; but Mill makes me shiver, his blade is so keen and so unhesitating." It is exactly this willingness, and more than willingness, to hear the other side, that attracts the young reader, and in fact all ingenuous minds, to Mill. His writings bear the unmistakable stamp of sincerity. "I had always," he tells us, "a humble opinion of my own powers as an original thinker, . . . but thought myself much superior to most of my contemporaries in willingness and ability to learn from everybody; as I found hardly any one who made such a point of examining what was said in defense of all opinions, however new or however old, in the conviction that even if they were errors there might be a substratum of truth underneath them, and that in any case the discussion of what it was that made them plausible, would be a benefit to truth."

In harmony with this spirit of modesty and candor is that peculiar quality in his writings which is at the same time one of their chief merits and a considerable bar to the recognition of their originality. In whatever field of learning he worked, he always sought to knit his thoughts into the body of pre-existing knowledge, and to make his current of speculation flow easily and naturally from sources already familiar to his readers, however widely that current might afterward diverge from the well-worn channels. Thus he was actually at more pains to conceal his originality than most writers are to bring theirs into prominence. In political economy he wrote as an expounder and popularizer of Ricardo, in morals as a disciple and interpreter of Bentham, in the philosophy of mind as a commentator on the works of his father and of Sir William Hamilton, and even in his 'Logic' he is so scrupulously careful to acknowledge indebtedness to earlier thinkers that an undiscerning reader might easily undervalue Mill's contributions in his own name. Recognizing the breadth and fullness of his mind, one is in danger of doing less than justice to its originality.

Mill has been compared with Locke, his influence on the nineteenth century being likened to the earlier philosopher's on the seventeenth. As parts of Locke's teachings have long since passed into the body of common thought and conviction, and have thus lost for us their originality and interest, so there are many of Mill's doctrines that have in this best sense become obsolete, because by general adoption they have ceased to be matter of argument. In addition to the practical reforms he inaugurated or promoted, we may ask at this time, what is his significance and value to us as a philosopher? By example as well as precept he has elevated and purified the utilitarian scheme of ethics. The greatest-happiness principle was with him a religious principle. We may hold that he was fundamentally wrong in his theory of morals, but we can not refuse to applaud his practise. In the abstruser regions of thought, his neat and clear exposition of the experience-philosophy is suggestive rather of the French than of the German school. One can hardly read three pages of German metaphysics without a depressing sense of the futility of human reasoning, whereas a French philosophical treatise fills us with a surprised delight at the efficiency of our own powers. The German is too fond of directing our gaze into fathomless abysses and of leading our feet into bottomless quagmires; the Frenchman conducts us easily and pleasantly over a macadamized road, where all steep ascents are carefully graded, precipitous declivities guarded against by walls and fences, and ugly or disquieting outlooks screened by flowering hedges. As Martineau long ago so admirably expressed it, Mill's distinguishing characteristics as a philosopher are "sharp apprehension of whatever can be rounded off as a finished whole in thought, analytic adroitness in resolving a web of tangled elements and measuring their value in the construction, reasoning equal to any computation by linear coordinates, though not readily flowing into the organic freedom of a living dialectic, remarkable skill in laying out his subject symmetrically before the eye and presenting its successive parts in clear and happy lights. No one has more successfully caught the fortunate gift of the French men-of-letters—the art of making readers think better of their own understanding and less awfully of the topics discussed." The same keen critic points out a glaring self-contradiction in Mill's theorizing. Mill resolves all knowledge into self-knowledge, since we have no cognitive access to either qualities or bodies external to ourselves. On the other hand, however, we know nothing but the phenomena of ourselves, we are but phenomena of the world, and the sensations from which all within us begins are merely the results of outward experience. Thus the pretended *a priori* ideas turn out to be *a posteriori* residues; the volitions that claim to be spontaneities are necessary effects of antecedent causes earlier than we. "And thus," the critic well concludes, "we are landed in this singular result: our only sphere of cognitive reality is subjective: and that is generated from an objective world which we have no reason to believe exists. In our author's theory of cognition, the non-ego disappears in the ego; in his theory of being, the ego lapses back into the non-ego. Idealist in the former, he is materialist in the latter."

We find, then, that in matters of abstract speculation Mill produced little that will live. But where he could bring his thought to the service of humanity, his achievement is noteworthy; and for this we honor him. Even in his contributions to inductive logic, of which he is often called the founder, he was working for the enlightenment of human error in the practical concerns of life; how much more so in his political and economic writings, their greater concreteness makes evident. He was far from being a philosopher for the mere love of 'divine philosophy.' There was no art-for-art's-sake enthusiasm in him. For a luxury of that sort he had too little tendency to passive enjoyment, and too much of the militant, apostolic fervor of the reformer. The will-o'-the-wisp pursuit of ultimate truth for its own sake might well have seemed to him, as did philosophical speculation in general to an eminent contemporary of his, very much like the motions of a squirrel in its cage. Mill's studies demanded a humanitarian motive, and that motive became with him a religion. He himself, in his review of Comte, declares: "Candid persons of all creeds may be willing to admit, that if a person has an ideal object, his attachment and sense of duty towards which are able to control and discipline all his other sentiments and propensities, and prescribe to him a rule of life, that person has a religion." Mill's two chief characteristics, the love of thinking out difficult problems, and the love of mankind, were made to serve each other; and the gratification of these two passions may be regarded as the expression of his natural piety.

His adroitness in applying abstract principles to concrete realities, and thus making attractive to the many those studies of his that might otherwise have repelled even the few, is too well known to require illustration. As Herbert Spencer somewhere makes illuminative use of the shape of a present-day 'milk-jug' to illustrate the irrationality of fashion and convention, so Mill can strengthen an argument by happily introducing the diminishing vogue of fainting-fits among young ladies. And as in small things, so in large. The high degree of common sense inwrought in the philosophy of Mill and Spencer contributes no little to the readableness, the intelligibility and the popularity of their writings. That Mill was nothing but 'a book in breeches,' as he was so often called, can not rightly be made to appear, even in the most learned of his published works. Unless precision and clearness of thought, accuracy of expression, aptness of illustration, breadth of reading and of observation, and constant openness to conviction, constitute the pedant, he was no pedant. One may even wish that there were a little more of the bookish element in him; for, remembering the extent of his reading in both ancient and modern literature, we feel some disappointment at finding in his works so little of that common stock of graceful allusion and happy quotation that might have been expected to adorn and to light up his somewhat sombre pages. Nevertheless he can not properly be called 'a thing of mechanized iron.' If he was the 'steam-engine' that Carlyle pronounced him to be, he was at least an engine of that excellent sort that burns its own smoke, which is more than can be said of Carlyle.

Mr. Frederic Harrison, who knew Mill personally, is emphatic in asserting that his heart was "even richer than his brain." Mr. Morley places Mill's distinction in the "union of stern science with infinite aspiration, of rigorous sense of what is real and practical with bright and luminous hope." All readers will recall the purple patches in 'The Subjection of Women.' In spite of his proof armor of dry logic, the author is more than once carried away by what has been styled 'the logic of feeling.' Mr. Harrison calls him "excessively sensitive and indeed impressionable." As Condorcet said of Turgot, he resembled a volcano clothed in ice. Proofs of this warmth of feeling could be adduced in great number, but a very few must here suffice. For a whole year he took upon himself the duties of his friend and subordinate in the India House, W. T. Thornton, to enable the latter to recruit his health without relinquishing his post. Mill's offer to guarantee the expense of certain early publications of Spencer's and Bain's, and also his generous kindness to Comte, when the French philosopher had fallen on evil days, and at a time when Mill himself was suffering from heavy pecuniary losses, are matters of common knowledge. A considerateness for others, and a depreciation of self, that went even to extremes, may be seen in Mill's conduct on retiring from the India House in 1858 after thirty-five years of service. His friends in the Examiner's Office, including every member of the force, desired to present him with a suitable token of regard. In half an hour after the matter was proposed, subscriptions were eagerly volunteered to the amount of fifty or sixty pounds, while outside contributions were jealously refused, as those in immediate service under the retiring examiner insisted on sharing with no outsiders the pleasure and honor of making this testimonial. But before the gift, an elaborate silver inkstand, could be got ready, the one for whom it was designed caught the scent and was greatly displeased. Approaching the originator of the plan, W. T. Thornton (as will have been surmised), he almost upbraided him, and was really angry, so far as it was in him to cherish anger. He said he hated all such demonstrations; was sure they were never wholly genuine; there were always some who took part in them only because they disliked to refuse; and, in short, he positively would have none of it. With him it was a question of principle, and where a principle was involved he could not give way, despite the obvious awkwardness in store for Thornton and his associates. The matter had gone too far to be dropped altogether, and finally the assistance of Mill's step-daughter, Helen Taylor, was invoked; the inkstand was smuggled into the house without Mill's knowledge, and, thanks to Miss Taylor, instead of being promptly returned, it was in the end promoted to a place of honor in the drawing-room. Mill's excessive devotion to his wife, a devotion that manifests itself in some of his writings as idolatrous worship, proves the warmth of his heart, however clearly it may betray a lamentable clouding of the judgment by a passion to which he, of all men, had seemed least likely to fall a victim.

From the many who knew Mill in his lifetime, abundant testimony could be quoted to prove the charm and purity of his nature, as well as the intellectual and moral stimulus of his personality. "Intimacy with Mr. Mill convinced me," says Henry Fawcett, "that, if he had happened to live at either of the universities, his personal influence would have been no less striking than his intellectual influence. Nothing, perhaps, was so remarkable in his character as his tenderness to the feelings of others, and the deference with which he listened to those in every respect inferior to himself. There never was a man who was more entirely free from that intellectual conceit which breeds disdain. Nothing is so discouraging and heart-breaking to young people as the sneer of an intellectual cynic. A sarcasm about an act of youthful mental enthusiasm not only often casts a fatal chill over the character, but is resented as an injury never to be forgiven. The most humble youth would have found in Mr. Mill the warmest and most kindly sympathy." An anecdote from the same source illustrates another equally admirable trait—an intellectual liberality unusual among scholars devoted to some chosen branch of study. "Some years ago," Fawcett narrates, "I happened to be conversing at Cambridge with three men who were respectively of great eminence in mathematics, classics and physiology. We were discussing the inaugural address which Mr. Mill had just delivered as rector of the St. Andrews University. The mathematician said that he had never seen the advantages to be derived from the study of mathematics so justly and so forcibly described; the same remark was made by the classic about classics, and by the physiologist about natural science. No more fitting homage can probably be offered to the memory of one to whom so many of us are bound by the strongest ties of gratitude and affection, than if, profiting by his example, we endeavor to remember that above all things he was just to his opponents, that he appreciated opinions from which he differed, and that one of his highest claims to our admiration was his general sympathy with all branches of knowledge."

What we of to-day owe to Mill, it seems safe to assert in closing, is not so much the advancement of learning in any particular direction —for the world has already caught up with and assimilated a great part of the new truth uttered by him—as the stimulus of a rarely pure and lofty and strenuous nature, devoted to high ideals for the amelioration of mankind, and unflinchingly courageous in advocating them by example as well as by precept. Industrious and versatile to a degree that astonishes one on surveying the products of his literary and his business activity, he at the same time achieved, in whatever he undertook, a uniformly high quality of workmanship that would be noteworthy even in the most rigorous specialist. To the strenuous youth of this strenuous age (if one may be pardoned for using again a much over-worked adjective) Mill may well serve as a model of nobly directed activity, generous self-sacrifice, and memorable achievement. But in emulating his example, let us first ponder well these words of his from a letter to Caroline Fox: "No one should attempt anything intended to benefit his age, without at first making a stern resolution to take up his cross and to bear it. If he does not begin by counting the cost, all his schemes must end in disappointment; either he will sink under it, as Chatterton, or yield to the counter-current, like Erasmus, or pass his life in disappointment and vexation, as Luther did."

CHANGES OF CLIMATE

By ROBERT DeC. WARD
HARVARD UNIVERSITY

Popular Belief in Climatic Change.—Belief in a change in the climate of one's place of residence, within a few generations, and even within the memory of living men, is widespread. It is confined to no special region or people. It finds support among the most intelligent as well as among the uneducated. Here it may be the view that the climate is growing milder; there, that the winters are becoming more severe; here, that there is increasing aridity; there, that the rainfall is greater. Whenever a season attracts attention because of weather conditions which seem in any way unusual, this belief is strengthened. This popular impression has often found support in the facts of distribution, or the dates of flowering or ripening of certain cereals or fruits. It is asserted that because grapes, or corn, or olives, for example, are now no longer grown in parts of Europe where their cultivation was once an important occupation, we must conclude that the climate has changed from a favorable to an unfavorable one.

Evidences of Climatic Changes within Historical Times.—Evidence is constantly being brought forward of apparent climatic variations of greater or less amount which are now going on. Such reports, largely those of travelers or explorers in little-known regions, are usually based on fluctuations in the extent of inland lakes; on the discovery of abandoned dwelling sites, the ruins of aqueducts and irrigating canals, and the like. Thus we have accounts of a gradual desiccation which seems to have been going on over a large region in Central Asia, during historical times. In eastern Turkestan the lakes have been reported as drying up, Lake Balkash falling one meter in about fifteen years, and Lake Alakul gradually becoming a salt deposit. In his work on Turkestan, Muschketoff gives numerous examples of progressive desiccation, and Rossikoff speaks of the drying up of the lakes on the northern side of the Caucasus. The same thing is reported of lakes in the Pamir. Prince Kropotkin believes that the desiccation of Central Asia in the past drove the inhabitants out on to the lowlands, producing a migration of the lowland peoples and thus bringing on the invasions of Europe during the first centuries of our era. In his recent work on the basin of eastern Persia and Sistan, Huntington believes that, so far as it can be made out, the history of Sistan also indicates a gradual desiccation from early historical times down to the present day. His study of climatic changes in that region is one of the most thorough ever made, for the evidence of archeology, of tradition, of history and of physiography has been carefully matched and found to accord in a very striking manner. Huntington has found evidence of the abandonment of successive village sites as the inhabitants moved further upstream in search of more water. Patches of dead jungle show that vegetation once flourished where aridity now renders plant growth impossible.

In northern Africa certain ancient historical records have been taken by different writers to indicate a general decrease of rainfall during the last 3,000 or more years, the remains of cities and the ruins of irrigating works pointing to a larger population and a greater water supply formerly than at present. The presence of certain animals, now no longer found there, is implied by ancient records, and from this fact also a change of climate is inferred. In his crossing of the Sahara between Algeria and the Niger, Gautier found evidence of a former large population. A gradual desiccation of the region is, therefore, believed to have taken place, but to-day the equatorial rain belt seems to be again advancing farther north, giving an increased rainfall. Gautier divides the history here into three periods, (1) dense population, (2) aridity, and (3) the present change to a steppe character.

Farther south, several lakes have been reported as decreasing in size, *e. g.,* Chad, Ngami and Victoria; and wells and springs as running dry. In the Lake Chad district, Chevalier reports the discovery of vegetable and animal remains which indicate an invasion of the Sudan by a Saharan climate. Neolithic relics indicate the former presence there of prosperous communities. Again, to note another instance, it is often held that a steady decrease in rainfall has taken place over Greece, Syria and other eastern Mediterranean lands, resulting in a gradual and inevitable deterioration and decay of their people. These examples might be multiplied, for reports of climatic changes of one kind or another are numerous from many parts of the globe.

What Meteorological Records show.—As concerns the popular impression regarding change of climate, it is clear at the start that no definite answer can be given on the basis of tradition, or of general impression, or even of the memory of the 'oldest inhabitant.' Human memories are very unreliable things, and there are many reasons for their being particularly untrustworthy in matters of this kind. The only answer of real value must be based on what the instrumental records of temperature, and of rain and snowfall show. Accurate instruments, properly exposed, and carefully read, do not lie; do not forget; are not prejudiced. When such instrumental records, scattered though they are, and difficult as it is to draw general conclusions from them, are carefully examined, from the time when they were first kept, which in a few cases goes back about one hundred and fifty years, there is found no evidence of any progressive change in temperature, or in the amount of rain and snow. Apparent signs of a permanent increase or decrease in one or another element have been fairly easy to explain as due to the method of exposing the thermometer or of setting up the rain gauge. Little care was formerly taken in the construction and location of meteorological instruments. They were usually in cities, and as these cities grew, the temperature of the air was somewhat affected. The rain-gauges were poorly exposed on roofs or in court-yards. The building of a fence or a wall near the thermometer, or the growth of a tree over a rain-gauge, was enough, in many cases, to explain any observed change in the mean temperature or rainfall. Even when the most accurate instrumental records are available, care must be taken to interpret them correctly. Thus, if a rainfall or snowfall record of several years at some station indicates an apparent increase or decrease in the amount of precipitation, it does not necessarily follow that this means a permanent, progressive change in climate, which is to continue indefinitely. It may simply mean that there have been a few years of somewhat more precipitation, and that a period of somewhat less precipitation is to follow.

For the United States, Schott, some twenty years ago, made a careful study of all the older records of temperature and rainfall, including snow, from Maine to California, and found nothing which led to the view of a progressive change in any one direction. There was evidence of slight variations of temperature, occurring with the same characteristics and with considerable uniformity over large areas. These variations have the characteristics of irregular waves, representing slightly warmer and slightly cooler periods, but during the fluctuations the temperature differed by only a degree or two on one side or the other of the mean. Obviously, this is too slight a range to be of any general or practical interest, and in any case, these oscillations give no evidence of a continuous change toward a warmer or a cooler climate. Schott found that these waves of higher and lower temperature followed one another at intervals of about twenty-two years on the Atlantic coast. In the interior the intervals were about seven years. The records of the closing of rivers, the Hudson, for example, to navigation, show no permanent change in the dates for the last hundred years or so.

It has been well pointed out that if a list were carefully compiled of heavy snowstorms, of droughts, of floods, of severe cold, of mild winters, of heavy rains, and of other similar meteorologic phenomena for one of the early-settled sections of the United States, beginning with the date of the first white settlements and extending down to the present day, we should have the following situation: Dividing this list into halves, each division containing an equal number of years, it would be found, speaking in general terms, that for every mild winter in the first half there would be a mild winter in the second; for every long-continued drought in the first division there would be a similar drought in the second; for every 'old-fashioned' winter in the first group there would be an 'old-fashioned' winter in the second. And so on through the list. In other words, weather and climate have not changed from the time of the landing of the earliest pilgrims on the inhospitable shores of New England down to the present day.

Why the Popular Belief in Climatic Changes is Untrustworthy.— Why is the popular belief in a change of climate so widespread and so firmly fixed, when instrumental records all go to show that this belief is erroneous? It is not easy to answer this question satisfactorily, but several possible explanations may be given. The trouble arises chiefly from the fact that we place absolute trust in our memories, and attempt to judge such subtle things as climatic changes on the basis of these memories, which are at best short, defective, and in the highest degree untrustworthy. We are likely to exaggerate past events; to remember a few exceptional seasons which, for one reason or another, made a deep impression on us, and we thus very much overrate some special event. To make use of an illustration given by another, individual severe winters which, as they occur, may be some years apart, seem, when looked back upon from a distance of several years later, to have been close together. It is much as in the case of the telegraph poles along a railroad track. When we are near the individual poles they seem fairly far apart, but when we look down the track, the poles seem to stand close together. The difference in the impressions made upon youthful and adult minds may account for part of this misconception regarding changes of climate. To a youthful mind a heavy snowstorm is a memorable thing. It makes a deep impression, which lasts long and which in later years, when snowstorms are just as heavy, seems to dwarf the recent storms in comparison with the older. The same is true regarding heavy rains, or floods, or droughts.

Changes of residence may account for some of the prevailing ideas about climate. One who was brought up as a child in the country, where snow drifts deeply and where roads are not quickly broken out, and who later removes to a city, where the temperatures are slightly higher, where the houses are warmer, and where the snow is quickly removed from the streets, naturally thinks that the winters are milder and less snowy than when he was a boy. Similarly, a change of residence from a hill to a valley, or *vice versa,* or from the coast to the interior, may easily give the impression of a changing climate. Even in cases where individuals have kept a record of thermometer readings during a long series of years, and are sure that the temperatures are not as low or as high as they used to be, or who are convinced that the rainfall is lighter or heavier than it was some years before, the chances are that the location of the thermometer or the exposure of the rain gauge has been changed sufficiently to account for any observed difference in the readings.

Value of Evidence concerning Changes of Climate.—The body of facts which has been adduced as evidence of progressive changes of climate within historical times is not yet sufficiently large and complete to warrant any general correlation and study of these facts as a whole, especially from the point of view of possible causation. But there are certain considerations which should be borne in mind in dealing with this evidence, certain corrections, so to speak, which should be made for possible controls other than climatic, before conclusions are reached in favor of climatic changes. In the first place, it has been noted above that changes in the distribution of certain fruits and cereals, and in the dates of the harvest, have often been accepted as undoubted evidence of changes in climate. Such a conclusion is by no means inevitable, for it can easily be shown that many changes in the districts of cultivation of various crops naturally result from the fact that grapes, or corn, or olives are in time found to be more profitably grown, or more easily prepared for market in another locality. Thus the area covered by vineyards in northern Europe has been very much restricted in the last few hundred years, because grapes can be grown better and cheaper farther south. Cultivation in one district is abandoned when it is more profitable to import the product from another. It is easy, but not right, to conclude that the climate of the districts first used has changed. Wheat was formerly more generally cultivated far north in the British Isles than is the case at present, because it paid. Later, after a readjustment of the taxes on breadstuffs, it was no longer profitable to grow cereals in that region, and the area thus cultivated diminished. Changes in the facility or in the cost of importation of certain articles of food from a distance are speedily followed by changes in the districts over which these same crops are grown. Similarly, the introduction of some new plant, better suited to the local soil and climate, will result in the replacement of the older product by the newer. In France, Angot has made a careful compilation of the dates of the vintage from the fourteenth century down to the present time, and finds no support for the view so commonly held there that the climate has changed for the worse. The dates of the vintage do, however, indicate some oscillation of the climatic elements. In the period 1775–1875, the average date of the grape harvest in Aubonne was about ten days earlier than during the preceding century, but three days later than during the second century preceding. At the present time, the average date of the grape harvest in Aubonne is exactly the same as at the close of the sixteenth century. After a careful study of the conditions of the date tree, from the fourth century, B. C., Eginitis concludes that the climate of the eastern portion of the Mediterranean basin has not changed appreciably during twenty-three centuries. In China, a comparison of the ancient and present-day conditions of cultivation, of silk production, and of bird migrations, has led Biot to a similar conclusion. In some cases, the reported cultivation of cereals, or other soil products, in certain at present unfavorable climates has been shown to be purely a myth; as in the case of a supposed extended cereal cultivation in Iceland in former times.

Secondly, a good many of the reports by explorers from little-known regions are contradictory. Thus Lake Aral, which was diminishing in area for many years, is recently reported by Berg as increasing. Lake Balkash, which was rapidly drying up, has also begun to fill again. Partly submerged trees are noted as having been seen by Berg, who in June, 1902, found the lake waters quite fresh. As the lake has no outlet, this is an interesting fact. In Africa, Lake Victoria which, it was generally agreed, was sinking in the period 1878–1892, has since shown a tendency to rise. Lake Rukwa, east of Tanganyika, has risen within the last few years. Reports that the Sea of Azov is drying up have been explained as due to a silting up of a lake. Lake Chad is very probably subject to oscillations, sometimes spreading beyond its usual limits as the result of several years of heavy rainfall. Such diverse reports show the need of caution in jumping at conclusions of climatic change. An increased use of water for irrigation may cause the level of water in a lake to fall, as has been the case to some extent in Great Salt Lake. Periodic oscillations, giving higher and then lower water, do not indicate progressive change in one direction. Many writers have thus seen a law in what was really a chance coincidence. Partsch believes that the ancient settlements on the interior lakes of northern Africa show that these lakes contained no more water formerly than they do now. Some have claimed that the supposed desiccation of the climate of northern Africa resulted from deforestation, but no certain evidence exists of the presence or destruction of such forests, and if deforestation did take place, no considerable change of climate could have resulted.

Thirdly, where a progressive desiccation seems to have taken place, the question should be asked, is less rain actually falling, or have the inhabitants less capacity, less energy, less ability than formerly? Is the change from a once cultivated area to a barren expanse the result of decreasing rainfall, or of the emigration of the former inhabitants to other lands? The difference between a country formerly well irrigated and fertile, and a present-day sandy, inhospitable waste may be the result of a former compulsion of the people, by a strong governing power, to till the soil and to irrigate, while now, without that compulsion, no attempt is made to keep up the work. The incapacity of the present inhabitants, or of their rulers, is often responsible for effects which have been interpreted as due to climatic change. It has been shown that where irrigation is resorted to in parts of the districts about the Mediterranean which have been reported to be drying up, there the former fruitfulness returns. In many cases the reports of increasing dryness really concern only the decrease in the water supply from rivers and springs, and it is well known that a change in the cultivation of the soil, or in the extent of the forests, may bring about marked changes in the flow of springs and rivers without any essential change in the actual amount of rainfall. These conditions are particularly likely to occur in regions where there is no snow covering, and where the rain falls in a few months only. In Tripoli, the Vicomte de Mathuisieulx finds that the Latin texts and monuments seem to establish the fact that, so far as atmospheric conditions and soil are concerned, everything is just as it was in ancient times. The present condition of the country is ascribed to the idleness of the Arabs, who have allowed wells to become choked and vegetation to perish. "In a country so little favored by nature, the first requisite is a diligent and hard-working population. The Romans took several centuries to make the land productive by damming rivers and sinking wells in the *wady* beds." In an arid region, man has a hard task if he is to overcome the climatic difficulties of his situation. Irrigation; the choice of suitable crops adapted to arid conditions; steady, thoughtful work, are absolutely essential. To a large extent, an intelligent man may thus overcome many of the obstacles which nature has put in his way. On the other hand, a region of deficient rainfall, once thickly settled and prosperous, may readily become an apparently hopeless desert, even without the intervention of war and pestilence, if man allows the climate to master him.

Lastly, a region whose normal rainfall is at best barely sufficient for man's needs, may be abandoned by its inhabitants during a few years of deficient precipitation, and not again occupied even when, a few years later, normal or excessive rainfall occurs. It is a very striking fact that the districts from which comes most of the evidence of changes of climate within historical times are subtropical or subequatorial, *i. e.*, they are in just those latitudes in which a slightly greater or a slightly less migration of the rain-bringing conditions easily produces a very considerable increase or decrease in the annual rainfall.

It is apparent, on examining the evidence thus far at hand, that the fact of permanent, progressive changes in climate during historical times has not yet been definitely established.

Periodic Oscillations of Climate: Sunspot Period.—The discovery of a distinct eleven-year periodicity in the magnetic phenomena of the earth, naturally led to investigations of similar periods in meteorology. Numerous and varied studies along this line, extending back even into the seventeenth century, but beginning actively about 1870, have been and are still being prosecuted by a considerable number of persons, and the literature on the subject has assumed large proportions. The results, however, have not been satisfactory. The problem is difficult and obscure. It is natural to expect a relation of this sort, and some relation certainly exists. But the results have not come up to expectations. Fluctuations in temperature and rainfall, occurring in an eleven-year period, have been made out for certain stations, but the variations are slight, and it is not yet clear that they are sufficiently marked, uniform and persistent over large areas to make practical application of the periodicity in forecasting possible. In some cases, the relation to sunspot periodicity is open to debate; in others, the results are contradictory.

Köppen has brought forward evidence of a sunspot period in the mean annual temperature, especially in the tropics, the maximum temperatures coming in the years of sunspot minima. The whole amplitude of the variation in the mean annual temperatures, from sunspot minimum to sunspot maximum, is, however, only 1.3° in the tropics and a little less than 1° in the extra-tropics. There are, however, long periods during which there appears to be no influence, or at least, an obscure one, and the relation before 1816 seems to have been opposite to that since then. More recently Nordmann (for the years 1870–1900) has continued Köppen's investigation, using the mean annual temperatures of certain tropical stations, and finds that the mean temperatures run parallel with the sunspot curve, but that the minimum temperatures occur with the sunspot maxima (amplitude 0.7°). This seems to contradict Köppen's conclusion, and also the fact that the sun is hotter at a time of maximum sunspots. The latter difficulty has been explained on the ground that the rainfall and cloudiness, both of which are at a maximum with the sunspot curve, lower the temperature, especially in the tropics. It is obvious that the situation in this matter is rather confusing just at the present time, and that the relation of sunspots and terrestrial temperatures is not wholly clear. The sunspots themselves are probably not the immediate or sole control. "There seems little doubt," says Sir Norman Lockyer, 'that we must look to the study of the solar prominences not only as the primary factors in the magnetic and atmospheric changes in our sun, but as the instigators of the terrestrial variations.' These investigations, however interesting and important they may be to astronomers and physical meteorologists, are really outside the field of climatology.

In 1872 Meldrum, then director of the meteorological observatory at Mauritius, first called attention to a sunspot periodicity in rainfall and in the frequency of tropical cyclones in the South Indian Ocean. The latter are most numerous in years of sunspot maxima, and decrease in frequency with the approach of sunspot minima. Poëy later found a similar relation in the case of the West Indian hurricanes. Meldrum's conclusions regarding rainfall were that, with few exceptions, there is more rain in years of sunspot maxima. This is to be taken only for *means,* and for a majority of stations, and is not to be expected at all stations or in every period. Hill found it to be true of the Indian summer monsoon rains that there seems to be an excess in the first half of the cycle, after the sunspot maximum. The winter rains of northern India, however, show the opposite relation; the minimum following, or coinciding with, the sunspot maximum. Many studies have been made of a possible relation between rainfall and the sunspot period, but the conclusions are not very definite, are sometimes contradictory, and do not yet warrant any general practical application for purposes of forecasting the wet or dry character of a coming year. Particular attention has been paid to the sunspot cycle of rainfall in India, because of the close relation between famines and the summer monsoon rainfall in that country. In 1889 Blanford admitted that the rainfall of India as a whole did not give evidence of the sunspot cycle in the records of the twenty-two years preceding. More recently the Lockyers have studied the variations of rainfall in the region surrounding the Indian Ocean in the light of solar changes in temperature. They find that India has two pulses of rainfall, one near the maximum and the other near the minimum of the sunspot period. The famines of the last fifty years have occurred in the intervals between these two pulses, and these writers believe that if as much had been known in 1836 as is now known, the probability of famines at all the subsequent dates might have been foreseen.

Relations between the sunspot period and various meteorological phenomena other than temperature, rainfall and tropical cyclones have been made the subject of numerous investigations, but on the whole the results are still too uncertain to be of any but a theoretical value. Some promising conclusions seem, however, to have been reached in regard to pressure variations, and their control over other climatic elements.

Brückner's Thirty-five-year Cycle.—Of more importance than the results thus far reached for the sunspot period are those which clearly establish a somewhat longer period of slight fluctuations or oscillations of climate, known as the Brückner cycle, after Professor Brückner, of Berne, who has made a careful investigation of the whole subject of climatic changes and finds evidence of a thirty-five-year periodicity in temperature and rainfall. Brückner began with the long-period oscillations in the level of the Caspian Sea. He then investigated the levels of the rivers flowing into the Caspian, and next the dates of the opening and closing of the rivers of the Russian empire, and finally extended his study over a considerable part of the world, including data concerning mean temperatures, rainfall, grape harvest, severe winters, and the like. The dates of opening and closing of Russian rivers go back in one case to 1559; the dates of vintage to the end of the fourteenth century, and the records of severe winters to about 1000 A.D. In a cycle whose average length is thirty-five years there comes a series of years which are somewhat cooler and also more rainy, and then a series of years which are somewhat warmer and drier. Brückner has found that the price of grain averages 13 per cent. higher in the wetter lustrum than in the drier. This thirty-five-year period is not to be thought of as being a perfectly systematic recurrence, in exactly that term of years. The interval in some cases is twenty years; in others it is fifty. The *average* interval between two cool and moist, or warm and dry periods, is about thirty-five years. Moreover, not only the intervals, but the intensities of the individual periods vary. The mean amplitude of the temperature fluctuation, based on large numbers of data, is a little less than 2°, which makes it greater than that obtained by Köppen for the sun-spot period, and it is natural to expect it at a maximum in continental climates. The fluctuations in rainfall, also, are more marked in interiors than on coasts. The general mean amplitude is 12 per cent., or, excluding exceptional districts, 24 per cent. In western Siberia more than twice as much rain may fall in wet as in dry periods. Regions whose normal rainfall is small are thus most affected. In years of minimum precipitation they may become uninhabitable, and the population may be forced to move away, perhaps never returning, and allowing towns and irrigating works to fall to decay. Slight fluctuations in rainfall are most critical in regions having a normal precipitation barely sufficient for agriculture. The extent of land cultivated, and the returns of agriculture here fluctuate directly with the temporary increase or decrease of rainfall. A supplementary study of the newer rainfall observations for Russia and for the United States, as well as for certain stations in central Europe and eastern Siberia, has given Brückner satisfactory confirmation of his earlier conclusions in the fact that he finds a decrease of rainfall over these districts as a whole, beginning about the middle of the decade 1880–90. The time of the 'boom' in western Kansas and Nebraska, and in eastern Colorado, in the decade 1880–90, followed one of Brückner's wet periods, and the collapse of the 'boom' came when the drier period advanced. Farmers who went out on to the high plains in the years of slightly greater rainfall preceding the boom, and who lost all their capital, and more too, in the vain attempt to raise their grain in the years which followed, could with difficulty be convinced that the climate of the plains had not permanently changed for the worse. The impression left upon their minds, and upon the mind of anyone who saw the country later, was one of decreasing rainfall, unsuccessful agriculture and financial ruin. Within more recent years, in this same region of Kansas, with a somewhat increased rainfall during a wetter cycle, but without any permanent change to a wetter climate, the intelligent choice of cereals better adapted to the soil and climate, and the rational use of the available water supply, have wrought a wonderful change in the aspect and economic value of the state.

The following table shows the dates and characters of Brückner's periods:

Warm	1746–1755	1791–1805	1821–1835	1851–1870	
Dry	1756–1770	1781–1805	1826–1840	1856–1870	
Cold	1731–1745	1756–1790	1806–1820	1836–1850	1871–1885
Wet	1736–1755	1771–1780	1806–1825	1841–1855	1871–1885

Interesting confirmation of Brückner's thirty-five-year period has been found by Richter in the variations of the Swiss glaciers, but as these glaciers differ in length, they do not all advance and retreat at the same time. The advance is seen during the cold and damp periods. Supan has pointed out that the Brückner periods appear to hold good in the south polar regions. And Hann's study of the monthly and annual means of rainfall at Padua (1725–1900), Klagenfurt (1813–1900) and Milan (1764–1900) brings to light an alternation of wet and dry periods in harmony with the thirty-five year cycle. It should be noted that Brückner has found certain districts in which the phases and epochs of the climatic cycle are exactly reversed. These exceptional districts are almost altogether limited to marine climates. There is thus a sort of compensation between oceans and continents. The rainier periods on the continents are accompanied by relatively low pressures, while the pressures are high and the period dry over the oceans, and *vice versa*. The cold and rainy periods are also marked by a decrease in all pressure differences. It is obvious that changes in the general distribution of atmospheric pressures over extended areas, of the great centers of high and low pressure, are closely associated with fluctuations in temperature and rainfall. An oscillation of a few hundred miles one way or another may mean the difference between drought or plentiful rainfall over extended areas. These changes in pressure distribution must in some way be associated with changes in the general circulation of the atmosphere, and these again must depend upon some external controlling cause or causes. W. J. S. Lockyer has called attention to the fact that there seems to be a periodicity of about thirty-five years in solar activity, and that this corresponds with the Brückner period. This longer cycle, underlying the sunspot period, alters the time of occurrence of the sunspot maxima in relation to the preceding sunspot minima. He makes out three periods in solar activity, of between three and four years, about eleven, and about thirty-five years, respectively. These are related as 1:3:9.

It is clear that the existence of a thirty-five-year period will account for many of the views that have been advanced in favor of a *progressive* change of climate. A succession of a few years wetter or drier than the normal is likely to lead to the conclusion that the change is permanent. Accurate observations extending over as many years as possible, and discussed without prejudice, are necessary before any conclusions are drawn. Observations for one station during the wetter part of a cycle should not be compared with observations for another station during the drier part of the same, or of another cycle.

Climatic Cycles of Longer Period.—There are evidences of longer climatic cycles than eleven or thirty-five years. Brückner calls attention to the fact that sometimes two of his periods seem to merge into one. Richter shows much the same thing for the Alpine glaciers. James Geikie, in Scotland, has brought forward evidence of several climatic changes in postglacial times. Blytt, in Norway and Sweden, finds some botanical evidence of four great climatic waves since the last glacial period. Brögger estimates that a mean annual temperature between 3° and 4° higher than the present was found in the Christiania Fjord in postglacial time. Lorié, in Holland, finds confirmation of Blytt's views. Gradmann, on botanical evidence, believes in a warmer climate in central Europe after the last ice age, and then a cooler one. Clough concludes that a three hundred-years cycle exists in solar and terrestrial phenomena, the thirty-six year cycle being as it were superimposed upon the longer one. Kingsmill reports a periodicity of three hundred years in droughts and famines in northern China. And so on. As yet, nothing sufficiently definite to warrant discussion here has been brought forward.

Geological Changes in Climate.—Changes of climate in the geological past are known with absolute certainty to have taken place: periods of glacial invasions, as well as periods of more genial conditions. The evidence, and the causes of these changes have been discussed and re-discussed, by writers almost without number, and from all points of view. Changes in the intensity of insolation; in the sun itself; in the conditions of the earth's atmosphere; in the astronomical relations of earth and sun; in the distribution of land and water, in the position of the earth's axis; in the altitude of the land; in the presence of volcanic dust—changes now in cosmic, now in terrestrial conditions—have been suggested, combated, put forward again. None of these hypotheses has prevailed in preference to others. No actual proof of the correctness of this or that theory has been brought forward. No general agreement has been reached. Under these conditions, and in view of the fact that practical climatology is concerned with climatic changes, not of the geological past but of the historical present, this portion of our subject may be dismissed with this brief mention.

Conclusion.—There is a wide-spread popular belief in permanent, progressive changes of climate during a generation or two. This belief is not supported by the facts of meteorological record. Abundant evidence has been adduced in favor of secular changes of climate in historical times. Much of this is unreliable, contradictory, and has been interpreted without sufficient regard to possible controls other than climatic change. Without denying the possibility, or even the probability, of the establishment of the fact of secular changes, there is as yet no sufficient warrant for believing in considerable *permanent changes over large areas.* Dufour, after a thorough study of all available evidence, has concluded that a change of climate has not been proved. There are periodic oscillations of slight amount. An eleven-year period has been made out, with more or less certainty, for some of the meteorological elements, but it has been of no practical importance as yet. A thirty-five year period is less uncertain, but is nevertheless of considerable irregularity, and can not as yet be practically applied in forecasting. Longer periods are suggested, but not made out. As to causes, variations in solar activity are naturally receiving attention, and the results thus far are promising. But climate is a great complex, and complete and satisfactory explanations of all the facts will be difficult, perhaps impossible, to reach. At present, indeed, the facts which call for explanation are still in most cases but poorly determined, and the processes at work are insufficiently understood. Climate is not absolutely a constant. The pendulum swings to the right, and to the left. And its swing is as far to the right as to the left. Each generation lives through a part of one, or two, or even three, oscillations. A snap-shot view of these oscillations makes them seem permanent. As Supan has well said, it was formerly believed that climate changes locally, but progressively and permanently. It is now believed that oscillations of climate are limited in time, but occur over wide areas. Finally, it is clear that man, whether by reforestation or deforestation, by flooding a desert or by draining a swamp, can produce no important or extended modifications of natural climate, which is governed by factors beyond human control.

THE AFRICAN PYGMIES

By S. P. VERNER

THE presence of a group of the African pygmies at the World's Fair at St. Louis attracted considerable attention to these little people. It has also revealed a number of erroneous popular conceptions with reference to them.

The word pygmy, of course, comes from the Greek, being derived from the word denoting a unit of measure, the ell. It was used by various Grecian writers, among them Homer, Herodotus, Heliodotus and Aristotle, to describe a race of small men, about whom tradition had given accounts, and who were usually located toward the sources of the Nile. Historically, then, the word pygmy applies to these Nilotic small peoples, but anthropology has widened the use of the term to include similar peoples scattered all over the globe, and found in many parts of Africa.

Paul du Chaillu was the first eminent modern explorer to find these people. He discovered them in the upper Ogowe basin, west central Africa, in July, 1863. After him others found them in various places. These were Schweinfurth, 1869, on the upper Welle, or Ubangi; Wissmann, 1886, on the upper Kasai; Stanley, 1888, on the upper Aruwimi; while Dr. Donaldson Smith located some south of Abyssinia. Others report them in German Kameruns, in French West Africa, on the borders of Uganda and in the center of the Congo Basin.

The names by which these people are called vary in each locality, but the most widely used term is Batwa. The name Bantu is the word meaning people in a large area of Central Africa. The singular of this is Muntu, meaning a man. These two terms apply to the large or normal people, not to the pygmies. But curiously enough, the name Batwa is the plural for people with the Batwa pygmies, and the singular of this is Mutwa. These last two terms seem to the writer to be diminutives of the words Bantu and Muntu, so that they mean little people and little man, respectively. Sir Harry Johnston, who visited the pygmies in the region where Stanley first found them, spells the name for them Mbute, while Schweinfurth, whose pygmies are not far from those of Stanley and Johnston, calls them Wambutti. It seems to the writer that these are either variations in name or in spelling of the same word. The present governor-general of the Congo, Major Costermans, found some Batwa near Lake Kivu. Wissmann's pygmies

On the left is Bomashulba, a Batwa Pygmy, Age 25 years, Height 4 ft. 7 in., Weight 90 lbs. On right is an ordinary African, Bakuba Tribe; Name, Latuma; Age, 15 years; Height, 5 ft. 2 in. Weight, 135 lbs.

in the upper Kasai are also called Batwa, which is the way Stanley spelled the word for his little neighbors, although these regions are six hundred miles apart.

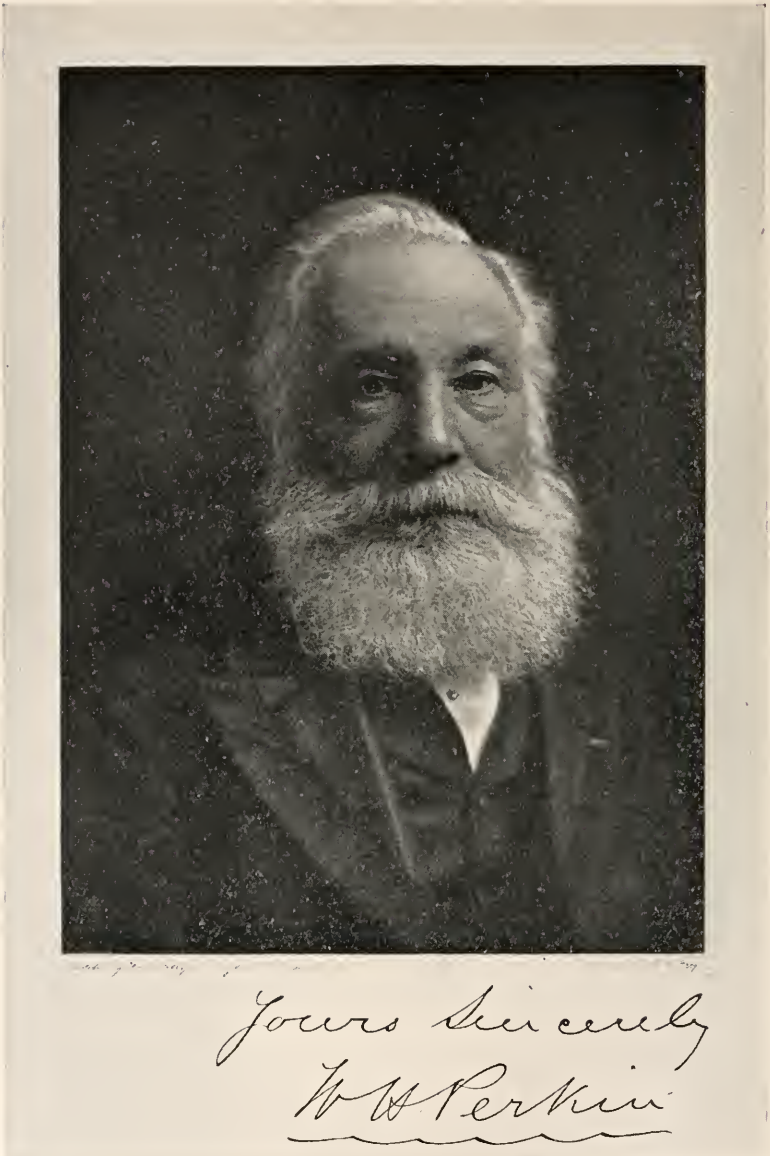
A study of all the writings of explorers reporting discoveries of these people has revealed an average stature of about four feet eight inches for all measured, though the measurements were made upon so few that this average can not be relied upon as a final result.

The plant which furnishes the leaf covering for the huts of the pygmies is the same in the regions, widely apart though they are, explored alike by Stanley and Wissmann. The shape of the house—a rough hemisphere—is also the same. In practically every case the primitive culture of the pygmies is the same, wherever found. The lack of any agriculture in their life is a common characteristic, as are the use of the poisoned arrow and the lack of any centralized tribal organization.

Popular misconceptions about the pygmies are principally as to their height. The general idea having gone abroad that they are the smallest known race of man, there has been produced the impression that they are all veritable Tom Thumbs. Of course, anthropologists know better that this, but the layman can not get clear the difference between a dwarf and a pygmy. Then, too, some travelers have rather unscientifically measured the smallest they could find, and left this as the record of the height of the tribe.

Dr. Mason, of the Smithsonian Institution, and Professor Starr, of Chicago University, concur in making five feet as the limit for the average of a pygmy race. Of course, there will be a few taller than this. and many shorter. It will also be necessary to discriminate against any result of the admixture of alien blood from larger tribes, although there is comparatively little of this going on.

The group at St. Louis came from the region in which the Batwa were found by a number of explorers, though their particular settlement was visited only by the writer and the Reverend W. H. Sheppard, F.R.G.S. These other explorers who found the Batwa in the upper Kasai are the English missionary, Grenfell; the German explorers (under the Congo government), Pogge and Wolff; and Major von Wissmann, who ranks next to Stanley as the explorer of the Congo Basin, and who was subsequently the governor-general of German East Africa. The pygmies at St. Louis were from the forests near Wissmann Falls, the cataracts at the head of the navigation of the Kasai tributary of the Congo. This place is about a thousand miles in the interior of the continent. There are a number of Batwa settlements in the same general district.


Yours sincerely
W H Perkin

THE PROGRESS OF SCIENCE

THE JUBILEE OF THE COAL TAR COLOR INDUSTRY AND SIR WILLIAM HENRY PERKIN

It is not often that a scientific man can take part in the celebration of the fiftieth anniversary of a great discovery that he has made. Perkin, when he was a boy of eighteen, noted a muddy precipitate which occurred in connection with an attempt to produce quinine artificially. The new idea that this substance could be used as a dye-stuff, the courage which led to a patent and further investigations, and the patient persistence with which all practical difficulties were overcome are an unusually clear exhibit of what is meant by genius. This violet color was called 'mauve' and was the forerunner of the auiline dyes and of much besides.

Fifty years ago the dyeing and printing industries were on an extremely empirical basis, the natural dye-stuffs being applied by secret and rule-of-thumb methods. In order to substitute a laboratory product and scientific methods, it was necessary for Perkin to establish a manufactory, which he did with the aid of his father and brother. The production of aniline was put on a commercial basis, and the discovery and application of other dye-stuffs was a comparatively simple matter. Magenta was discovered in France three years later, and subsequently all the colors of the rainbow were produced from aniline, Perkin himself being largely responsible for alizarine, and indirectly for synthetic indigo. As is well known, the artificial production of these dye-stuffs has led to great changes in agriculture and manufactures, one of the most important being the alliance of science and industry in Germany, which has given that country almost a monopoly in the work that originated with Perkin in Great Britain.

The coal tar products now give us flavors and perfumes as well as colors. Saccharine, more than five hundred times as sweet as sugar, was discovered in the laboratory of the Johns Hopkins University. Salicylic and benzoic acids, though natural products, are chiefly produced artificially. Although quinine, which was the subject of Perkin's original research, has not been made by synthesis, the coal-tar products have given us an extraordinary series of drugs—antipyrin, acetanilid, phenocoll, etc. They have given us smokeless powder, photographic films and indirectly nitrates from the atmosphere and the cyanide process of gold extraction.

The scientific advances have been no less remarkable than the industrial developments, and it should be especially noted that to these Perkin has contributed his full share. After acquiring a competence, he sold his manufactory in 1873 and has since devoted himself to scientific research. Professor Nernst has stated that Perkin is the founder of physical chemistry. In connection with his influence on chemistry, it should be remembered that two of his sons have become eminent for work in organic chemistry, both being fellows of the Royal Society.

The scientific and industrial developments following on Perkin's great discovery were adequately celebrated by an international gathering at the Royal Institution on July 26. It was presided over by Professor R. Meldola and various speeches and presentations were made, including the Hoffman and Lavoisier gold medals and degrees from several foreign universities. There was also a dinner, at which Mr. Haldane, secretary for war, proposed the toast of the evening and addresses were made by Professor Emil Fischer, Sir Henry Roscoe and others. Sir William and Lady Perkin held receptions at their home near Harrow and at London. American chemists decided to hold a special celebration of the jubilee, and Sir William and Lady Perkin accepted an invitation to be present at a banquet to be given in New York City, on October 6. Professor Chandler, of Columbia University, presided, and addresses were made by Dr. Hugo Schweitzer, President Ira Remsen, Dr. H. W. Wiley, Professor Walther Nernst and others. Sir William Perkin was presented with the first impression of a Perkin medal, which will hereafter be awarded annually for work in applied chemistry; with a silver tea service, and with honorary membership in the American Chemical Society. There will also be founded in honor of Sir William Perkin a circulating library for American chemists.

THE ROYAL SOCIETY.

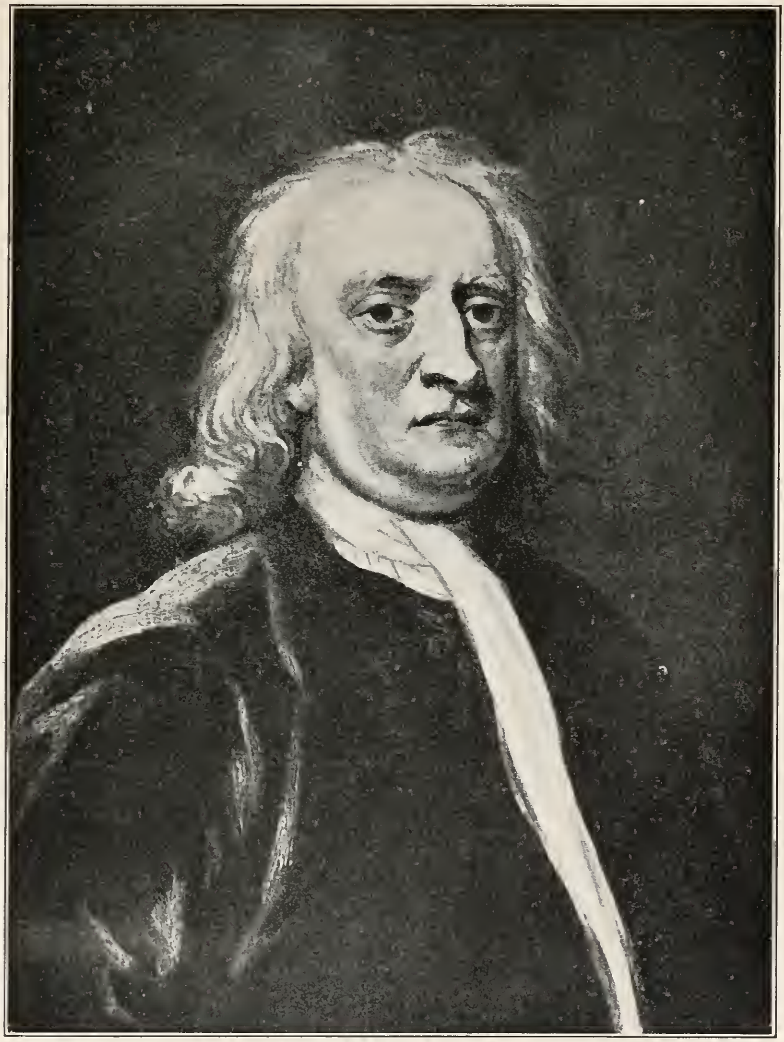
Sir William Huggins, eminent for his contributions to spectroscopic astronomy and president of the Royal Society from 1900 to 1905, has collected into a volume, published by Messrs. Methuen and Company, selections from four of the addresses given at anniversary meetings of the society. The addresses are prefaced by a sketch of the early history of the society, and are followed by a discussion of the place of science in education. The volume contains excellent copies of paintings of fellows of the Royal Society which hang in its halls, and other illustrations, several of which are here reproduced.

Death Mask of Newton.

Reflecting Telescope made by Newton with his own hands in 1671, standing on the bound MSS. of the *Principia*.

The address of 1902 is concerned with the importance of science to the industries of Great Britain, which, it is argued, can only be maintained by making science an essential part of all education. Germany and the United States are quoted as examples that should be followed in their encouragement of research work as a condition of academic degrees. The solution offered appears to be rather optimistic, for Sir William wishes to retain the culture to be derived from classical and literary studies, and to find time for the thorough study of science by improving methods of teaching and by greater devotion to work on the part of the students.

This address had a practical outcome in so far as the council of the Royal Society was lead to draw up a memorandum urging the universities to give greater encouragement to science with a view to its recognition in schools and elsewhere as an essential part of general education, but no very considerable results followed, both Oxford and Cambridge having voted shortly after the presentation of this letter to reject plans for the acceptance of a larger amount of science in the entrance examinations.



SIR ISAAC NEWTON.

President of the Royal Society, 1703–27.

From the painting by T. Vanderbank.

Meeting Room of the Royal Society in Burlington House.

The second address, given in 1903, reviewed the relation of the Royal Society to the special scientific societies. The increase of knowledge and the necessary differentiation of the sciences led to the foundation of the Linnean Society for natural history in 1788 and to the establishment of the Geological Society in 1807, and there are now in London and elsewhere in Great Britain numerous societies devoted to the special sciences. Sir William does not discuss the relations of the Royal Society to the British Association for the Advancement of Science, nor to the British Academy for philosophy, history and philology, which was established during his presidency, after long discussion as to whether the subjects that it covers should be included in the scope of the Royal Society. It appears that efforts were made some ten years ago to form a more or less close affiliation of the principle special societies with the Royal Society, but the plan did not prove feasible. Sir William favors publishing the papers read before the Royal Society both in its transactions and in the publications of the special society, and such a plan is in operation in the case of the Royal Astronomical Society. It is not, however, clear just what is gained by this plan for science, though it might for the time lead scientific men to present their papers before the Royal Society. This discussion has led to the publication of the transactions in two parts, one for the natural sciences and the other for the exact sciences, but it is difficult to see the advantages of publishing, even in two series, papers scarcely one tenth of which would be of interest to any one student. It does not seem that meetings at which papers in all the sciences are read have a useful function at the present time, and this appears to be clearly indicated by the programs and attendance at the ordinary meetings of the Royal Society and of our own National Academy of Sciences.

The third address discusses the relation of the Royal Society to the state and its responsible public duties. The society is not supported by the state, although it administers a government fund for research and publication amounting to £5,000 annually and occupies rooms provided by the government. Sir William takes pride in the fact that the society is a private body of learned men for the promotion of natural knowledge at their own cost. Certainly the British traditions have been well maintained by the Royal Society, Sir William himself being a notable example. He has devoted his life to astronomical research, has built his observatory at his own cost and has worked without a salary of any kind. It is, however, somewhat doubtful whether such traditions can be maintained in the future. In any case, they belong to an aristocracy rather than to a democracy. It will probably be found that even in Great Britain the government will need to employ its scientific men, and will not depend on the voluntary advice and assistance of an independent society.

The fourth address reviews the influence that science, represented by the Royal Society, has had upon the life and thought of the world. It is truly remarkable what a large part of the great scientific advances from Newton to Darwin, and since Darwin, have had their origin in the work of the fellows of the Royal Society.

THE YORK MEETING OF THE BRITISH ASSOCIATION

The British Association for the Advancement of Science has celebrated its foundation in York seventy-five years ago by meeting this year in that city. The attendance at the meeting was 1,959, which is fully as large as the

The Principal Library of the Royal Society.

average, though some 600 fewer than at the meeting in the same city twenty-five years ago, when Sir John Lubbock presided and the fiftieth anniversary of the foundation of the association was celebrated. The president this year was Dr. E. Ray Lankester, director of the British Museum of Natural History. His address consisted of two parts—one a survey of the progress of science during the preceding twenty-five years; the second a discussion of the relations of the government to science. It is an almost impossible task for one man to describe the extraordinary and diverse scientific advances of the past generation. Although the address as printed is very long, numerous important topics are of necessity omitted, and others are perhaps unduly emphasized. It is not certain that Metchnikoff's phagocytic theory of immunity occupies the important place in modern science that is given to it in this address, nor even that radium twenty-five years hence will loom as large as it does now. In his review of the influence of science on the life of the community and its relation to the government, Professor Lankester takes the somewhat pessimistic attitude which appears to be common in Great Britain. He says that political administrators are altogether unaware of the vital importance of science in public affairs and that whole departments of the government in which scientific knowledge is the one thing needful are carried on by ministers and clerks who are ignorant of science and dislike it. Dr. Lankester attributes this ignorance and dislike to "the defective education, both at school and university, of our governing class, as well as to a racial dislike among all classes to the establishment and support by public funds of posts which the average man may not expect to succeed by popular clamor or class privilege in gaining for himself—posts which must be held by men of special training and mental gifts."

Dr. Lankester then enumerates on the other side of the account the establishment of the National Physical Laboratory, the subsidizing of the Marine Biological Association and the endowment of the Lister Institute by Lord Iveagh. He continues: "Many other noble gifts to scientific research have been made in this country during the period on which we are looking back. Let us be thankful for them, and admire the wise munificence of the donors. But none the less we must refuse to rely entirely on such liberality for the development of the army of science, which has to do battle for mankind against the obvious disabilities and sufferings which afflict us and can be removed by knowledge. The organization and finance of this army should be the care of the state."

The British Association will meet next year at Leicester under the presidency of Sir David Gill, astronomer royal in South Africa. The meeting of 1908 will be in Dublin, and in 1909 the association will for the third time visit Canada, meeting in Winnipeg.

SCIENTIFIC ITEMS

Dr. Ludwig Boltzmann, eminent for his work in theoretical physics, of which subject he was professor in the University of Vienna, has committed suicide.

Professor S. F. Earle has resigned the directorship of the Cuban Central Agricultural Station, which was organized in 1902.—Dr. H. C. Wood, for thirty years professor of therapeutics in the University of Pennsylvania and until 1902 clinical professor of diseases of the nervous system, has retired from the active duties of his chair, and has been made professor emeritus.—Dr. A. R. Crook, for the past twelve years professor of mineralogy and economic geology at Northwestern University, has been appointed curator of the Illinois State Museum of Natural History at Springfield.

THE
POPULAR SCIENCE
MONTHLY

DECEMBER, 1906

THE BOGOSLOFS

By President DAVID STARR JORDAN and GEORGE ARCHIBALD CLARK
STANFORD UNIVERSITY

IN the southern portion of Bering Sea, about thirty-seven nautical miles northwest from the island of Unalaska, lies a group of small volcanic islets known as Bogoslof, in Russian, Joanna Bogoslova, St. John, the Theologian. There are now three of these, all of which have risen from the sea, hot and steaming, within historic times. An especial interest attaches to them just now from the fact that the third and largest of the group appeared at about the time of the great earthquake of April 18, 1906.

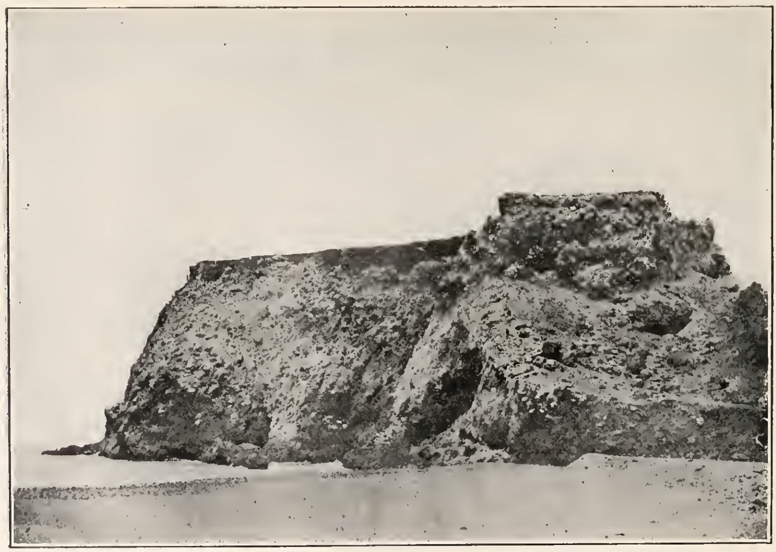
The possibility of a connection between the disturbances at Bogoslof and those which caused the California earthquake is heightened by the fact that the great earthquake rift, which extends through the Coast Range of California for a distance of 200 miles, follows a direction, which, if produced northward to Bering Sea, would pass near the islands of Bogoslof. Again this earthquake rift was largest, and its effects more violent, where it entered the sea in Mendocino County than at any other point throughout its course, the extent of the lateral movement along the crack increasing from about two feet in Monterey County to about 16½ feet at Point Arena, where it finally enters the sea.

In opposition to this view may be placed the improbability that an earthquake rift or fault would extend so far as from the center of California to Bering Sea, a distance of more than 2,000 miles, and through such great depths of water as intervene between Point Arena and Bogoslof. It is also stated that the evidence of the seismograph, so far as understood, favors the idea that the great earthquake was

Old Bogoslof or Castle Island.

confined to California, although its center of disturbance was clearly in the sea in a westerly direction from Cape Mendocino.

It is evident also that the rise of the third Bogoslof was attended by little if any disturbance in the immediate vicinity. The advent of each of the other two islands was marked by earthquake shocks, the fall of volcanic ashes and displays of fire, observed and felt by the people of Iliuliuk on Unalaska Island. The people of this village in 1906 were unaware of the presence of the new island until the news was brought



Fire Island, One of the Old Bogoslof Islands.

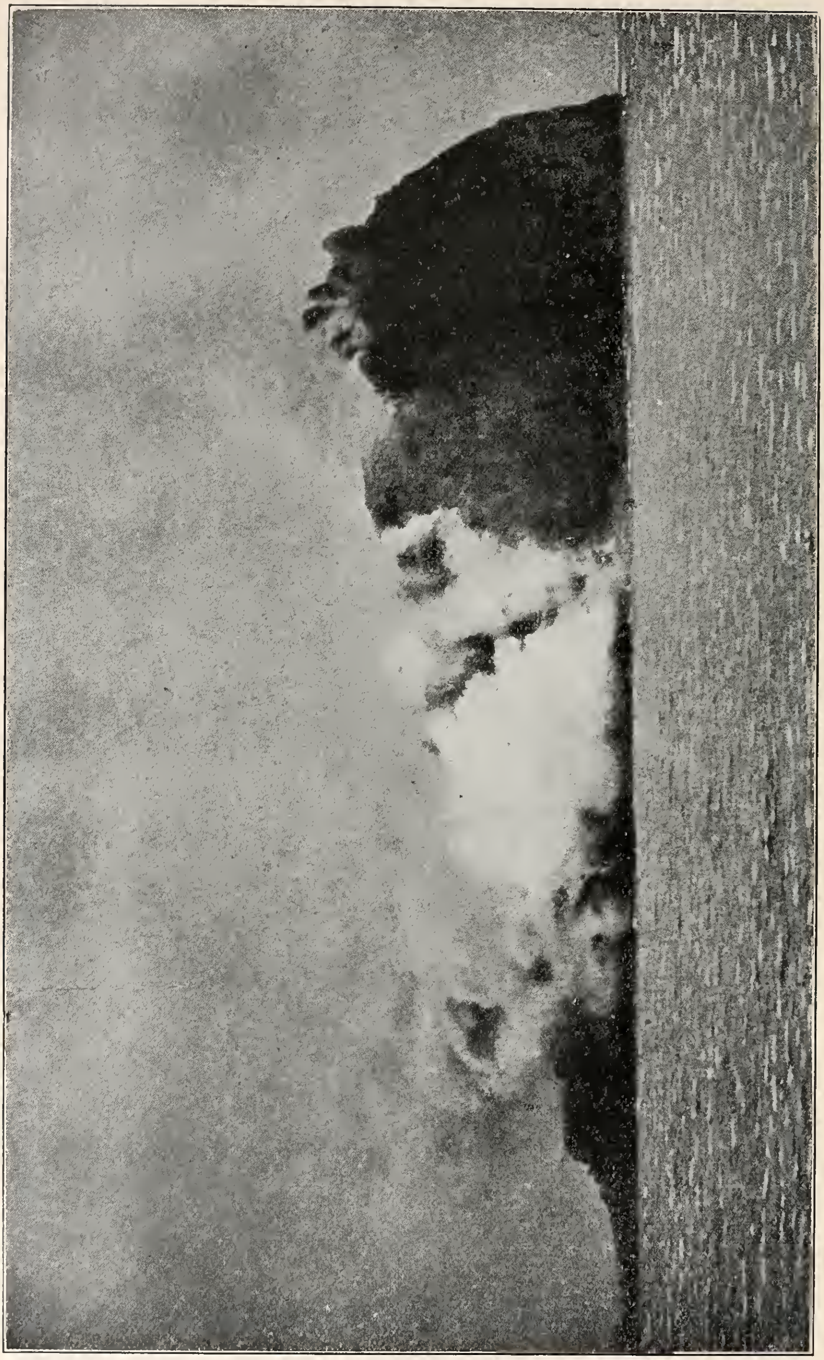
in by vessels touching at the harbor. Earthquake shocks lasting 30 seconds are reported for May 20 and 23 by the keeper of the light at Scotch Cap on Unimak Island, and a 'pretty severe shake' occurred at Dutch Harbor on June 2, but nothing is reported for April or early May, when the new island must have risen. Certainly there could not have been any activity displayed by Makushin or Akutan, both of which volcanoes overlook Unalaska and Dutch Harbor, without being observed by the people of these villages. Perhaps the rise of such an island, in a more or less plastic condition, as it must be, would not necessarily be attended by disturbance in the solid crust of the neighboring islands. On the Pribilof Islands, which had an origin similar to that of the Bogoslofs, no earthquake shock or other disturbance was noted, although

FIRE ISLAND, ONE OF THE OLD BOGOSLOF ISLANDS.

these islands were affected at the time of the rise of New Bogoslof in 1883. The Pribilof group lie 120 miles to the north of the Bogoslofs.

On the whole, however, the weight of evidence at present seems to favor the idea that the Bogoslof disturbance of 1906 was local in character and the coincidence in date with the California earthquake involves no actual relation between the two phenomena.

The writers first saw the islands of Bogoslof in July, 1896, while en route for the Pribilof Islands in connection with the fur seal investigations. The U. S. Fish Commission Steamer *Albatross* attempted to land the commission on Old Bogoslof, but was prevented by the heavy surf, and the thick weather made only a partial view of the islands possible. The vessel afterwards passed the islands on its way to the



New Bogoslof, or Fire Island (1883).

THE NEW BOGOSLOF ISLAND

Commander Islands under more favorable conditions. Dr. Stejneger of the commission obtained some excellent photographs. The senior writer, still later in the same season, passed both islands while on the British gunboat, *Satellite,* on the way from the Russian Islands to Unalaska.

At that time Old Bogoslof, known to the sealers as Castle Island, from its appearance, was cold and dead. It showed in the fog a sheer cliff or hill of ashes about 300 or 400 feet in height, seeming much higher in the uncertain light. It was apparently the home of countless sea birds and a small herd of the gray sea lions (*Eumetopias stelleri*) was hauled out upon one of its slopes.

About half a mile to the northwest lay the islet of New Bogoslof, of about twice the height of the other and considerably greater area. This island was locally known as Fire Island, having but recently ceased to steam and smoke. There was in 1896 no evidence of activity in it, but the water was said to be still warm in the crevices of the rocks. The name Grewingk, in honor of the Russian geographer who compiled an early account of Old Bogoslof, has been given to this island by Mr. Dall.

Both islands were surrounded with deep water. In fact the space occupied by the second island had formerly been safely traversed by vessels. Dredge hauls by the *Albatross* about the islands resulted in the taking of a number of deep sea forms of fishes, among them three 'grenadiers' (*Albatrossia pectoralis, Bogoslovius clarki,* and *Macrourus cinereus*). These were obtained at a depth of 664 fathoms or 3,984 feet.

Conspicuous in the group of islets was an isolated pillar of rock, of considerable height, known as Ship or Sail Rock. It had existed from the earliest times, having been reported as early as 1768. It was seen by Captain Cook in 1778, who mistook it for a ship under sail, hence its name. This was eighteen years before the rise of Old Bogoslof. Ship Rock crumbled and fell in ruins about 1888.

About April, 1906, midway between Old and New Bogoslof, a third island, larger than either of the others appeared. Captain Dirks of Dutch Harbor estimates its size as five times that of New Bogoslof, although the photographs do not seem to bear this out. This new island was first seen by the U. S. Fish Commission Steamer *Albatross,* Captain L. M. Garrett, on May 28, 1906, while on her way to the investigation, under direction of Professor Charles H. Gilbert, of the fisheries of Japan. Soon after this date the U. S. Revenue Cutter *Perry* visited the islands. Photographs were taken under the direction of Lieutenant Hepburn, of the *Albatross,* and these, sent us by Mr. H. H. Taylor, of the North American Commercial Company, are here reproduced, together with photographs of Castle Island and Fire Island, taken by Mr. N. B. Miller of the *Albatross* in 1892.

The early history of these very interesting islands is given by Professor George Davidson in the *Bulletin of the American Geological Society,* Vol. XXII., p. 267, and a detailed and exhaustive account of them by Dr. C. Hart Merriam, profusely illustrated, appears in the Report of the Harriman Expedition of 1899, Vol. II., p. 291–336.

Bogoslof of May, 1906. From New Bogoslof, or Fire Island.

Of the advent of the first island, in 1796, the following account is given in Kotzebue's narrative of discovery in 1817. The story is that of a Russian trader, Kriukof, who found himself with some native hunters forced to seek refuge from storm on the north end of Umnak Island, the island of the Aleutian chain, nearest the Bogoslofs. It was in May and when the storm cleared on the 8th, Kotzebue tells us:

They saw to the N., several miles from land, a column of smoke ascending from the sea; toward evening they observed under the smoke something black, which arose but a little above the surface of the water. During the night fire ascended into the air near the spot, and sometimes so violent, and to such height, that on their island, which was ten miles distant, everything could be distinctly seen by its light. An earthquake shook their island, and a frightful noise echoed from the mountains in the S. The poor hunters were in deadly anxiety; the rising island threw stones towards them, and they every moment expected to perish. At the rising of the sun the quaking ceased, the fire visibly decreased, and they now plainly saw an island of the form of a pointed black

THE THREE BOGOSLOFS, MAY, 1906.

cap. When Kriukof visited the island of Oomnak, a month afterward, he found the new island, which during that time had continued to emit fire, considerably higher. After that time it threw out less fire, but more smoke: it had increased in height and circumference, and often changed its form. For four years no more smoke was seen, and in the eighth year the hunters resolved to visit it, as they observed that many sea lions resorted to it. The water round the island was found warm, and the island itself so hot in many places that they could not tread on it.

The eruption of 1883, which resulted in the rise of New Bogoslof, seems to have had no eye-witnesses and the exact date of its appearance is unknown. Captain Anderson of the schooner *Matthew Turner* saw the new island in September, 1883, and reported that great volumes of steam and smoke, accompanied by showers of ashes, were thrown out from the summit and through fissures in the sides and base, the bright reflections from the heated interior being visible at night. At the time of this eruption a severe earthquake was felt in the sea off Cape Mendocino, apparently in the line of the Portolá-Tomales rift of April, 1906.

The islands were visited in 1884 by the officers of the U. S. Revenue Cutter *Corwin*, and Lieutenant J. C. Cantwell and Surgeon H. W. Yemans made the ascent of New Bogoslof. Lieutenant Cantwell thus describes his experience in the 'Cruise of the *Corwin*':

> The sides of New Bogoslof rise with a gentle slope to the crater. The ascent at first appears easy, but a thin layer of ashes, formed into a crust by the action of rain and moisture, is not strong enough to sustain a man's weight. At every step my feet crushed through the outer covering and I sank at first ankle-deep and later on knee-deep into a soft, almost impalpable dust which arose in clouds and nearly suffocated me. As the summit was reached the heat of the ashes became unbearable, and I was forced to continue the ascent by picking my way over rocks whose surfaces, being exposed to the air, were somewhat cooled and afforded a more secure foothold.
>
> On all sides of the cone there are openings through which steam escaped with more or less energy. I observed from some vents the steam was emitted at regular intervals, while from others it issued with no intermission. Around each vent there was a thick deposit of sulphur which gave off suffocating vapors.

The islands were visited by Drs. C. Hart Merriam and T. C. Mendenhall of the Bering Sea Fur Seal Commission in 1891. Dr. Merriam writes thus of New Bogoslof as seen at that time:

> The new volcano was enveloped in steam, which issued from thousands of small cracks and crannies and poured in vast clouds from a few great fissures and crater-like openings, the principal of which was near the northwest corner, only a few feet above high water mark. From this opening, the shape of which we could not see, it rushed out with a loud roaring noise. So great was the quantity of steam that it completely concealed the upper part of the island except when wafted to and fro by violent gusts of wind. . . . The steam was usually impregnated with fumes of sulphur, and deposits of sulphur, some in very fine needles, were observed along the margins of the cracks.

Of the third Bogoslof, Dr. Charles H. Gilbert, of Stanford University, who was in charge of the work of the *Albatross* when the 'brand new mountain' was first seen on May 28, 1906, writes thus in a personal letter regarding it:

> When I saw it (Bogoslof) in 1890 there were really two small islands about 1½ miles apart, one of them steaming and the other already cooled off. This has been the condition for a number of years, so the hot one had received the name of Fire Island, the cold one, Castle Island. When they came in sight yesterday, we were astonished to find that Fire Island was no longer smoking and that a very large third island had arisen half way between the other two. It was made of jagged, rugged lava and was giving off clouds of steam and smoke

from any number of little craters scattered all over it. Around these craters, the rocks were all crusted with yellow sulphur.

In a later letter, written from Yokohama, Dr. Gilbert said:

I wrote you a full account of Bogoslof, but the letter seems to have miscarried. Our discovery seems to have been corroborated later by some revenue cutter, but if the newspaper report agrees with their findings, very extensive changes took place in the interval between the two visits. When seen by us, the new cone, occupying much of the space between the two older ones, was somewhat higher than either, but was certainly far from 900 feet high—300 feet would be an extreme figure. There was no evidence of a central crater. The steam and fumes were given off most abundantly from cracks and fumaroles on the slopes. About these were heavy incrustations of sulphur. We saw no indications of boiling water, nor did we believe that landing would be impossible.

In an account of the physical history of the Bogoslofs, written in 1899 for the report of the Harriman Expedition, Dr. Grove K. Gilbert, of the U. S. Geological Survey, noting the rapid disintegration of the islands, said:

One might predict that in the next century the name Bogoslof would attach only to a reef or shoal, were it not for the possibility of new eruptions. The pulse of the volcano is so slow that we have noted only two beats in more than a century, but such sluggishness must not be taken as a symptom of death, or even decline, for volcanic organisms are characteristically spasmodic in their activity. Long before the sea has established its perfect sway the arteries of the mountain may again be opened and a new and larger island put forth to contest its supremacy.

Nearly a century elapsed between the arrival of the first and second Bogoslof, only twenty-three years between the second and third.

The floor of the depths of Bering Sea in this region seems to be still unsettled, and astonishing changes may be looked for at any time. If it should prove true that the geological faults of California extend out from this center, a new interest would be attached to the outbreaks of Bogoslof.

NOTES ON THE DEVELOPMENT OF TELEPHONE SERVICE. II.

By FRED DE LAND
PITTSBURG, PA.

IV. *Exploiting The Telephone*

IN the fall of 1876, Gardiner Greene Hubbard began to systematically exploit the electric-speaking telephone invented by Alexander Graham Bell. In the vicinity of Boston a number of private telephone lines were strung, some of which were two or three miles in length, to connect mills and offices or offices and residences. In some instances, where private telegraph lines already existed, the telegraph instruments were replaced with a pair of telephones.

On October 9, 1876, a telephone was attached to each end of a telegraph circuit owned and operated by "The Walworth Manufacturing Company, extending from their office in Boston to their factory in Cambridge, a distance of about two miles. The company's telegraph battery consisting of nine Daniell's cells, were removed from the circuit, and another of ten carbon elements was substituted." It is recorded that "articulate transmission then took place through the wire. The sounds, at first faint and indistinct, became suddently quite loud and intelligible." Another instance of the early practical use of the telephone was in connecting the water works with the central office of the water commissioners, of Cambridge, Mass. On April 4, 1877, a telephone circuit was strung to connect the factory of Charles Williams, Jr., in Court Street, Boston, with his residence in Somerville. This is said to be the first telephone circuit constructed in the United States, the earlier ones being transformed telegraph lines. A number of other private telephone lines were built in and about Boston early in 1877. In fact a number of small contractors found it profitable to string private lines, and strove to secure orders for this class of work. For they would run the circuits on the poles of the telegraph companies without permission, or bracket them to house-tops, to trees, to any place that a bracket or a porcelain knob could be attached, paying no attention to property rights.

In the winter of 1876–77, experimental toll service over telegraph circuits was successful for distances of several hundred miles, even from Boston to New York. In November, 1876, Graham Bell found no difficulty in carrying on conversation over telegraph circuits between New York and Boston, using only a pair of box magneto-telephones, so long as the parallel wires were not in service. "When this happened," he said, "the vocal sounds were very much diminished, but still audible. It seemed, indeed, like talking in a storm. Conversation, though audible, could be carried on with difficulty."

On February 12, 1877, Graham Bell delivered a lecture on the telephone at Salem, Mass., eighteen miles from Boston. At 10:55 P.M., a reporter of the Boston *Globe* turned in his report by telephone and this was the first newspaper report sent by telephone. Previous to the lecture a wire was strung from the lecture hall to a telegraph circuit connected with the editorial rooms. Then a single telephone was placed on each end of the line.

The telephones used in these pioneer lines were of the box magneto type and intended to rest on shelf, table or desk. Several modifications of this form were made. An interior view of the form sent out in April, 1877, is shown in Fig. 4. Then came the wooden hand telephone (Fig. 5) in May, 1877. Owing to its resemblance to a well-known kitchen utensil, it was promptly called 'the butter-stamp telephone.' It was such a decided improvement in shape and convenience over the box telephone that it won its way from the start. Then came the first of the electric-speaking telephone circulars. Three pages contained endorsements by the press and scientists, while the first page contained the following statements that appear unique in the light of later knowledge:

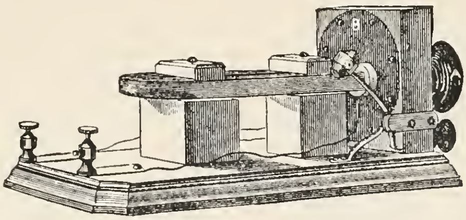


FIG. 4. BOX TELEPHONE, COVER REMOVED.

The Telephone

The proprietors of the telephone, the invention of Alexander Graham Bell, for which patents have been issued by the United States and Great Britain, are now prepared to furnish telephones for the transmission of articulate speech through instruments not more than twenty miles apart. Conversation can be easily carried on after slight practice and with occasional repetition of a word or sentence. On first listening to the telephone, though the sound is perfectly audible, the articulation seems to be indistinct; but after a few trials the ear becomes accustomed to the peculiar sound and finds little difficulty in understanding the words.

The telephone should be set in a quiet place, where there is no noise which would interrupt ordinary conversations.

The advantages of the telephone over the telegraph for local business are

1. That no skilled operator is required, but direct communication may be had by speech without the intervention of a third person.

2. That the communication is much more rapid, the average number of words transmitted a minute by Morse sounder being from fifteen to twenty, by telephone from one to two hundred.

3. That no expense is required either for its operation, maintenance, or repair. It needs no battery, and has no complicated machinery. It is unsurpassed for economy and simplicity.

The terms for leasing two telephones for social purposes connecting a dwelling-house with any other building will be $20 a year, for business pur-

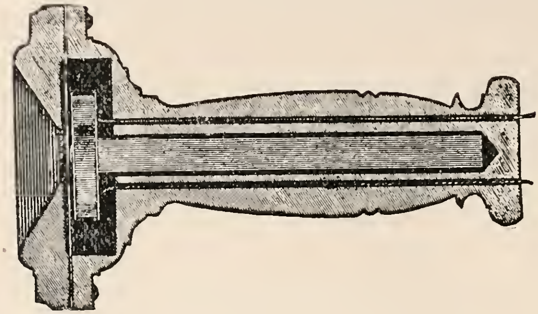


FIG. 5. WOOD HAND TELEPHONE OF MAY, 1877.

poses $40 a year, payable semiannually in advance, with the cost of expressage from Boston, New York, Cincinnati, Chicago, St. Louis, or San Francisco. The instruments will be kept in good working order by the lessors, free of expense, except from injuries resulting from great carelessness.

Several telephones can be placed on the same line at an additional rental of $10 for each instrument; but the use of more than two on the same line where privacy is required is not advised. Any person within ordinary hearing distance can hear the voice calling through the telephone. If a louder call is required one can be furnished for $5.

Telegraph lines will be constructed by the proprietors if desired. The price will vary from $100 to $150 a mile; any good mechanic can construct a line; No. 9 wire costs 8½ cents a pound, 320 pounds to the mile; 34 insulators at 25 cents each; the price of poles and setting varies in every locality; stringing wire $5 per mile; sundries $10 per mile.

Parties leasing the telephone incur no expense beyond the annual rental and the repair of the line wire. On the following pages are extracts from the press and other sources relating to the telephone.

Cambridge, Mass., May, 1877. GARDINER G. HUBBARD.

For further information and orders adress

THOMAS A. WATSON, 109 Court St., Boston.

The work of supplying to customers the hand telephones, referred to in the foregoing circular, was entrusted to Graham Bell's assistant, Mr. Thomas A. Watson, who had entered the employ of the proprietors of the telephone about April 1, 1876. He occupied a small amount of desk room and much bench room in the small factory of Charles Williams, at 109 Court Street, Boston. Here Mr. Watson made up and assembled the parts, as the telephones were called for. Naturally, improvements were the order of the day, and soon a smaller and more attractive mahogany handle magneto-telephone was adopted.

How rapidly 'Bell's toy' began to win its way into public favor is indicated by the statement that on July 31, 1877, or less than four months from the day the first circular was sent out by Mr. Hubbard, 778 telephones had been leased, while in all probability an equal number of experimental telephones had been made by mechanics and scientists who thought that it would be an easy matter to improve upon Bell's method. When the year 1877 closed, there were 5,491 Bell telephones in use.

Naturally this good demand for so serviceable an instrument encouraged 'the proprietors' of the Bell patents to branch out on a little broader basis. As Graham Bell had transferred all right, title and interest to Mr. Hubbard on July 9, 1877, thus placing the control in the latter's hands, on August 1, 1877, Mr. Hubbard organized the Bell Telephone Association, of Boston, without capital stock and served as trustee, while Mr. Sanders acted as treasurer.

Then the development of the exchange business and the assignment of territorial rights began in earnest, and Mr. Hubbard visited all the larger cities seeking to interest men of prominence. But though he journeyed hither and thither, striving to influence capital to favorably consider the telephone as a desirable investment, yet the task of interesting investors in the development of local exchanges proved difficult, and progress was made slowly in the United States.

In Europe some progress was made through elaborate experiments carried on by foreign governments to practically demonstrate the utility of the telephone. On November 28, 1877, it was officially promulgated that 'the introduction of the telephone in the practical telegraph service of the German Empire has been formally accomplished; and the passing of the telephone into practical use may be regarded as satisfactorily completed.' This conclusion was based largely on the excellent results secured on a toll circuit two hundred and thirty miles long, established between Berlin and Prince Bismarck's country residence at Varzin, in Pomerania, early in October, 1877. Probably that was the first official recognition of the practical value of the telephone on the part of a foreign nation. Yet, early in 1877, Mr. Preece, the head of England's telegraph department, had notified his government that Alexander Graham Bell "has rendered it possible to reproduce the human voice with all its modulations at distant points. I have spoken with a person at various distances up to thirty-two miles." In November, 1877, conversation was excellently maintained for two hours between Dover and Calais, a distance of twenty-two miles, by using a telegraph circuit in a submarine cable. Then, in December, 1877, Mr. Preece officially reported having successfully carried on a long conversation through a submarine telegraph cable sixty-seven miles in length, extending from Dublin to Holyhead, by means of hand telephones.

In the United States the first lease for territorial rights was executed on October 24, 1877, with the Telephone and Telegraph Construction Company, of Detroit, Michigan; yet eleven months passed before a telephone exchange was opened in that city.

The second lease was assigned to the District Telephone Company of New Haven, Connecticut, and included the counties of New Haven and Middlesex. The former county was rapidly developed and has the honor of having established within its limits the first two commercial telephone exchanges (at New Haven and at Meriden), the first mutual telephone exchange (Bridgeport), the first private branch exchange system (Ansonia), and the first telephone toll lines regularly connected to operating commercial telephone exchanges.

On February 12, 1878, territory was assigned to the New England Telephone Company, of Boston. On March 8, 1878, a license was granted to the American District Telegraph Company, of St. Louis, and on July 3, 1878, a license was granted for New York City and including a radius of thirty-three miles.

This activity created quite a demand for telephones, and by the end of July, 1878, over 12,000 had been placed. Thus it was deemed wiser to organize a larger and more flexible corporation. So, on July 30, 1878, Mr. Hubbard organized the Bell Telephone Company, of Boston, under the laws of Massachusetts, to manufacture, sell and use telephones outside of New England, and capitalized it at $450,000. Mr. Hubbard was trustee, Mr. Sanders, treasurer, Dr. Bell, electrician, and Thomas A. Watson, superintendent. This company acquired all the patents, property and rights of its predecessor, the Bell Telephone Association, and its headquarters remained at the Williams factory at 109 Court Street.

This second parent company entered into nine formal agreements granting exclusive rights to use Bell telephones in certain specified territory, and did such excellent work in developing the opening of exchanges, that some 22,000 telephones were in service when, on March 13, 1879, it was deemed the wiser plan to organize a new parent company having sufficient financial backing combined with the personal and commercial influence that would enable the business to be handled on the large scale that ripening conditions demanded. So the National Bell Telephone Company was chartered under the laws of the state of Massachusetts, and the company capitalized at $850,000. The executive offices of this parent company were moved to New York, in August, 1878, and located at 66 and 68 Reade Street. A year later the company moved back to Boston and secured offices at 95 Milk Street.

The officials and directors of the National Company were:

W. H. Forbes, President.	Alexander Graham Bell, Electrician.
G. L. Bradley, V.-Pres. and Treasurer.	Francis Blake, Jr., Electrician.
T. N. Vail, General Manager.	O. E. Madden, Supt. of Agencies.

Thomas A. Watson, General Inspector.

Directors

William H. Forbes,	Francis Blake, Jr.,	Charles Eustis Hubbard,
Charles S. Bradley,	Richard S. Fay,	Alexander Cochrane,
Gardiner G. Hubbard,	George Z. Silsbee,	George L. Bradley.
Thomas Sanders,	William G. Saltonstall,	

Executive Committee

W. H. Forbes,	Gardiner G. Hubbard,	Francis Blake, Jr.
R. S. Fay,	Thomas Sanders,	

Now to turn back a year. Before the close of 1878, men successful in other lines of industry perceived that if the crude and limited facilities offered by these early exchanges afforded a service esteemed by the public as a desirable convenience, then the manifold advantages that might be derived from a telephone exchange system embracing all the probable users of telephone service in a town or city, would be in direct ratio to the growth and expansion of that system. Hence, as it appeared evident that this new industry had come to stay, and was quite likely to prove a good revenue producer, capitalists began to look favorably upon 'Bell's toy,' to wonder whether it might not turn out to be an exceedingly valuable invention, and whether it was safe to infringe the Bell patents. Thus it came about that before the close of 1878, a number of promoters who had formerly scoffed at the inventor and his telephone were offering large sums in cash for exclusive rights to operate in given territory, in several cases paying a good premium for the same rights offered for a nominal payment a year previous.

These sudden conversions to implicit belief in the tangible value of the telephone appear the more remarkable when we recall the fact that throughout the United States commercial and financial affairs remained in a depressed condition during the entire year of 1878, and it was an exceedingly difficult matter to get capital interested in any new enterprise. Nevertheless, nearly seventy Bell exchanges were in process of being planned, or were under construction, or were in operation when the year closed. In the following named cities Bell exchanges were in operation at the close of 1878, and, while the number of telephones shown in service is comparatively small, the records show that several of these exchanges had secured from two to four times that number of contracts, and were connecting new subscribers as rapidly as possible.

Albany	250	Indianapolis	150
Baltimore	100	Lowell	200
Boston	150	Meriden	100
Bridgeport	175	New Haven	350
Buffalo	250	New York	250
Chicago	550	Philadelphia	250
Cincinnati	200	St. Louis	325
Columbus	50	Toledo	100
Detroit	150	Troy	100

Then exchanges were in process of construction in Washington, Louisville, New Orleans, Nashville, Cleveland, Springfield, Hartford, Providence and other places.

A very different condition of affairs prevailed in financial and commercial circles a year later, when, in the autumn of 1879, the resumption of specie payment caused a feeling of elation to pervade all branches of industry, and started a remarkable boom in railway construction and in stock speculation that spread throughout the country. Thus it was not surprising that many investors appeared eager to identify themselves with the telephone industry, nor was it so remarkable that as one of the results of this frenzied activity, there were several hundred operating Bell companies when the year of 1879 closed.

Each of these companies operated under its own management, was governed by its own policy, and supplied its service at such rates as each thought best adapted to meet the views of local patrons. There was little or no uniformity in these rates, for the majority had been established not only without regard to the brief but costly experience with low rates that the companies established in 1878 had passed through, but adversely to the sensible suggestions of the parent company to make the service so good that business houses would pay at least a dollar a week for local telephone service. Again, not only were rates established without a due regard for the amount of cash investment that would be required per subscriber, but in entire forgetfulness of two essential factors in determining cost of production and supply: (1) Decrease in plant valuation due to improvements in the art, and (2) the destructive action of the elements.

In some states there was, in 1879, a Bell license for each county, and as each licensee was wholly independent of any and all other licensees, there naturally came to be a great diversity of opinions regarding proper methods of construction and operation, equitable interchange of toll-line traffic, profitable rates and the legal protection that the parent company should insure to its licensees. Furthermore, the broader-minded licensees began to perceive that the telephone business, instead of being merely a local issue, was not only interurban and interstate in character, but continental in scope, and that the healthy growth and ultimate success of these operating companies were largely dependent on the scope and the character of the service supplied, rather than on patent protection. In 1879, it was also foreseen that an amount of capital many times larger than the original estimate called for would have to be invested to place the business on a permanent foundation. Thus the wisdom of consolidating these small county licensees into large state or interstate companies was perceived, and large operating companies controlling exchanges in many counties were in existence before the close of 1879.

Incidentally it is worth while to recall that while some of the pioneers were men to whom too much credit can not be given for the intelligent and persistent manner in which improvements in and extensions to the service were introduced, there were other pioneers whose grasp of the problems they were facing was exceedingly slight, though these latter gentlemen had no hesitation in branding as heretical all views opposed to their own, or in combatting the progressive suggestions of the parent company. Even the technical press was pessimistic in belief concerning the future of the telephone. In 1882, the editor of *The Operator* wrote: "The telephone is almost entirely a local convenience, nearly as much so as gas lighting and horse cars; its monopoly, which is not an oppressive one, rests upon the possession of patents, and must expire with the patents."

That some among the principal Bell licensees had much to learn concerning telephone problems is well illustrated by the published statement of the president of one of the most prominent companies. In 1883, he was quoted concerning the need of long-distance service, as follows:

> In the first place it has never been demonstrated that a continuous wire of one hundred miles is necessary. Such a wire has never been called for or demanded for any purpose, financial or otherwise. How could it pay? You may accept as a fact that no system of telephony which extends beyond a radius of twenty or thirty miles from any city, however large, will prove a paying investment. For purely commercial or financial interests, the telephone will not come into favor as a medium of direct transmission between large cities. The telephone, like any other enterprise, is valuable so long as it pays, and when it ceases to prove profitable in a pecuniary point of view, its scientific uses will not avail much. To sum up the whole matter, the value of the telephone is confined, as I have mentioned, within a certain radius—that is where it has many lines, and beyond that it has no money earning-capacity.

Yet within a year from the publication of that statement a number of toll lines, each more than a hundred miles in length, were in operation, notably one from Denver to Pueblo, in Colorado, one hundred and eleven miles long. This line was built with only 2,619 poles, and cost only about $13,000. Under present methods of construction a line of corresponding length and built in that section of country would probably include 4,900 poles and would cost about $90,000, including a heavy copper circuit.

Another apt illustration is found in the interview given in 1883, by the president of two large Bell companies, and who was called 'the leading practical expert of the country.'

> To talk over the 1,200 miles between Chicago and New York there must be used either a compound wire or an iron one several times as heavy—an impracticable size. The copper wire would require about 36 poles to the mile, and I have roughly estimated its cost at $400,000. Only one wire could be used upon one set of poles, for even at the extreme ends of long cross arms, at such a distance what was said to one wire would be heard on the other and *vice versa*, owing to induction. In fact we find it impracticable for this reason to put more than one wire on one set of poles for distances greater than three miles—four miles being the very limit, even if far apart.

V. *The First Mutual Telephone Exchange*

On Friday evening, April 27, 1877, twenty-nine years ago, Alexander Graham Bell delivered a lecture on his electric-speaking telephone at the opera house in New Haven, Connecticut, and also addressed audiences in Hartford and in Middletown, with the aid of telephones connected to a telegraph circuit loaned by the Atlantic and Pacific Telegraph Company. Mr. Frederick Gower delivered the lecture in Hartford, and Mr. Thomas A. Watson was in charge in Middletown. After giving a number of interesting illustrations of the serviceability of the telephone, and the ease with which conversation could be carried on over considerable distances, Dr. Bell claimed that the time was coming when a telephone in every house would be considered indispensable, that the telephone would displace the telegraph in many business transactions, and that a man of business would have no more difficulty in talking with his agent a hundred miles away than in directing his servant through the speaking-tube. And he further stated that the telephone wires would yet be laid underground, as gas and water pipes are now laid. In Hartford, Mr. Gower explained the telephone and the many ways in which it could be utilized, and said:

> And Smith will have a wire to the central office in Hartford, while his correspondent or branch house in Boston or New York will be similarly connected there. Smith will say to the hole in the wall: Switch me upon 500 State street, Boston. Whereupon the central officer will turn the little lever, and Smith may talk with his friend all day.

Back to that lecture in April, 1877, more than twenty-eight years ago, dates the conception of the movement that resulted in the establishment of the modern telephone exchange. For the earliest among all telephone exchanges were established in Connecticut, Bridgeport claiming the honor of the first mutual telephone exchange system, and Hartford the second; Ansonia, the first private exchange system in which a regular switching system and operator were employed; to New Haven rightfully belongs the honor of having the first commercial telephone exchange ever opened, while to Meriden is credited the second of the commercial exchanges. And the manner in which the telephone was introduced for public use in each place is indicative of the way in which it was first established in many other cities.

During the past twenty years Mr. Thomas B. Doolittle has been one of the most widely-known, capable and companionable telephone men in the country, and has planned a greater mileage of telephone pole lines than any other man. In 1887 Mr. E. J. Hall said: "The first really practical success in talking over long distances was the copper metallic circuit constructed between New York and Boston by Mr. T. B. Doolittle, for the American Bell Telephone Company, in 1883. The distance was about 300 miles, and I call it the first practical success because it was the first circuit that worked at all times regardless of outside electrical disturbances."

In 1874 Mr. Doolittle was a well-known manufacturer of metal goods in Bridgeport, Connecticut. During that year he assisted in the establishment of a social telegraph system, in which some twenty or thirty users of the Morse code were connected by telegraph circuits that terminated in a special switchboard in the Bridgeport office of the Atlantic and Pacific Telegraph Company, the necessary switching being performed by the local telegraph operator.

In June, 1877, the closing of the A. & P. telegraph office, through absorption by the Western Union, temporarily suspended this local service, and necessitated other arrangements. Having become a firm believer in the future of the telephone, Mr. Doolittle secured four pairs of Bell's wooden hand telephones, and placed one pair on each of four lines; then he installed a home-made switching device in his office, and for a signaling device employed a single-stroke bell operating on a gravity-battery current. Then he devised a method of preventing eavesdropping that in May, 1878, was declared 'a great improvement on any other telephone system now in use.' So satisfactory did these instruments prove that other lines were similarly equipped and soon a mutual telephone exchange system was in full operation, that later on formed the nucleus of the commercial exchange opened in Bridgeport by Mr. Doolittle in 1878.

In the fall of 1877 he planned and built a private telephone exchange system for the Ansonia Brass and Copper Company, of Ansonia, that, while in no sense a commercial exchange system, was indirectly of incalculable benefit to the growth and prosperity of the entire commercial telephone industry. For in planning this system Mr. Doolittle decided to use circuits of copper instead of iron, and, after many experiments, produced a hard drawn copper wire of his own adaptation, the drawing of which he supervised in the mills of that company.

Prior to that time it had been found impossible to use copper wire on pole line circuits, as its extreme ductility proved a source of continued elongation in all spans of any length and where the strain was constant. By the Doolittle process the tensile strength of the wire was greatly increased, its elongation reduced to about one per cent., and there was no appreciable change in its conductivity. Yet it required ten years of costly experience with iron wire circuits before the telephone interests fully comprehended the inestimable value of this improvement. In September, 1880, at a conference of telephone men, the representative of a very large wire-drawing mill stated that "copper wire has ceased to be discussed for telephone line use. It is too soft and elongates too readily under exposure. The suitable wire must be tough pure iron, well manipulated to secure flexibility and toughness."

Referring to this pioneer work, Mr. Doolittle wrote:

Hard drawn copper was the result of an adaptation rather than a discovery, although many of its valuable properties were not appreciated until after it had been in service several years. Prior to its introduction for aerial conductors there was very little, if any, call for the hard product. Copper wire was usually annealed after drawing and sold in that form. Copper alloyed with other metal was, and is now, used in the manufacture of hard or spring wire. It is the common knowledge of all who are familiar with the manipulation of copper that the process of drawing it into wire serves to harden the surface. Thus it will be seen that the experiments which resulted in the so-called hard drawn copper wire were based upon a well-known principle, although the application of this principle had never been made use of for the final product. The writer was familiar with this phenomenon of the hardening of drawn copper at the time he entered the field of electricity; therefore, when it was disclosed to him that copper was not only one of the best conductors of electricity, but was the cheapest in conductivity, or per mile ohm, it was only left for him to determine whether or not this hardening process could be made available in order that copper wire should be comparable to iron in its ability to stand the strain of its own weight when strung on poles, and, in addition thereto, the weight of sleet or snow and wind pressure. There was no mathematical road to determine this factor; therefore it was simply a case of 'cut and try.'

It was for producing in 1877 the five hundred pounds of No. 12 B. & S. gauge hard drawn copper wire used in stringing the aerial circuits in the Ansonia Brass and Copper Company's private telephone exchange system that the Franklin Institute awarded to Mr. Doolittle the Edward Longstreth medal of merit.

In connecting up the different offices and mills of the Ansonia Brass and Copper Company he built substantial pole lines, installed a home-made type of switchboard to which all circuits were attached and had a regular operator employed. A pole line was also built to the freight station of the Derby Railroad Company, and a telephone installed there and connected to the main switchboard, thus enabling any department to get in immediate communication with the freight station. This system was completed and in successful operation on December 4, 1877. Then, when the Connecticut Telephone Company came into possession of the territory by virtue of purchasing the rights of earlier Bell licensee systems, and thus possessed the sole right to operate telephone exchanges under the Bell patents, it claimed that this pioneer private exchange was a commercial exchange by reason of connections to outside interests, although calls were exchanged without thought of payment, and thus was infringing the rights of the Connecticut Company. So that exchange was closed. Later on a private branch exchange system was regularly installed for the Ansonia Brass and Copper Company, and now that company is the largest user of telephone equipment and service in Connecticut.

Probably Hartford can be credited with establishing the second mutual telephone exchange system. Three months after Dr. Bell's lecture, an agent for the Bell company called on the principal merchants in Hartford and tried to induce them to utilize the telephone as a business-bringer. On July 19, 1877, the local manager of the Western Union, Mr. G. B. Hubbell, secured the agency of the Bell telephone. On August 9, 1877, the Hartford *Courant* stated: "At the Capital Avenue drug store there is a telephone of simple construction connected with the residence of Dr. Campbell." On August 22 the *Courant* stated that "At the regular meeting of the allopath physicians on Monday evening, experiments were successfully tried with the telephone, and it is proposed to have a system of intercommunication between the doctors established by means of this new invention, so that by reporting to the central office at the Capital Avenue drug store, they can readily exchange views between office and office." In September, 1877, Isaac Smith, owner of the Capital Avenue drug store, had one and possibly two party-lines working in Hartford, and terminating in his store. On October 8, 1877, Smith advertised as follows: "Prof. Bell's Telephone. I am prepared to build and equip telephone lines at moderate rates. Telegraphic lines, with Morse or other instruments, built of the best material. Please call and examine our telephone lines in operation." In November, 1877, Dr. Crane, a dentist in Hartford, had a party-line on which were six physicians and six druggists, including Smith, and on November 15 Crane advertised: "Messages sent direct from my office to the following places by telephone." On January 24, 1878, the *Courant* announced that "When word came to Hartford of the accident on the Connecticut Western, information was dispatched to the central office from whence run wires to many physicians of this city. In a very short space of time and within a few minutes of each other, nearly a score of doctors and surgeons were at the depot."

As a rule, in the beginning the messages sent over these early telephone lines were not switched through, but received at one telephone by 'Central' and repeated to the subscriber through another telephone. For there was one telephone for each circuit terminating in the central office; if there were six subscriber-lines, then there were six hand telephones hanging on the wall of the central office. But this was not the case at Bridgeport, Ansonia, New Haven or Meriden.

In May, 1877, Mr. Edwin T. Holmes used Bell's hand telephone as an accessory to his central-office burglar-alarm system in Boston, one set of wires serving for both purposes. Five of these alarm wires were cut through a small brass telegraphic pin switchboard, enabling a hand telephone to be connected or plugged-in on any line. Mr. F. E. Kinsman, who was then in the employ of Mr. Holmes, said that (in August, 1877) the service was not given by connecting any two circuits together, but that "it was made by the operator taking the message and turning about and talking through the telephone to the party to whom the message was given." Three months later Mr. Holmes installed hand telephones in many of the wholesale and commission houses to enable these subscribers to the system to notify the Holmes central office to tell the express company to call for packages ready for shipment. Then a central switchboard system was installed and, in March, 1878, there were 256 hand telephones in use. The use of hand telephones only is said to have continued in this system for more than twelve years, although the number of subscribers finally exceeded 500.

THE JEWS: A STUDY OF RACE AND ENVIRONMENT. III.

BY DR. MAURICE FISHBERG
NEW YORK CITY

Mixed Marriages between Persons of Different Christian Denominations

THE assumption that Jews and christians refrain from intermarriage because of an inherent racial antipathy existing between the Aryan and the Semite is disproved by the large number of mixed marriages in western Europe and America. All the facts go far to prove that the only reason why they have not intermarried during the middle ages and even as far as the first half of the nineteenth century was the difference of religious belief. It was both the church and the synagogue which discouraged intermarriage between Jews and christians. Not only has the church prohibited intermarriage with Jews, mohammedans and heathens, but even the adherents of the different christian denominations have been thus enjoined. In the beginning of the nineteenth century intermarriage between catholics and protestants was comparatively rare in Europe and America. With the change of conditions characteristic of our age, a spirit of toleration has become dominant, and mixed marriages are to-day more or less common. In some countries in Europe denominational statistics have been compiled, and these are of considerable interest in this connection. In Hungary, where many religious confessions are represented, the following are the rates of intermarriage: To 100 marriages contracted in 1903 between persons of the same creed there are mixed marriages among unitarians, 167.73; protestants, 49.39; reformed church, 48.52; Greek catholic, 42.79; Greek oriental, 16.88; Jews, 7.21. Here we find a connection between the degree of religious toleration and the proportion of mixed marriages. The unitarian church, which does not prohibit its adherents to marry outside of their faith, shows the highest proportion of mixed marriages of all the other denominations. In fact, there were more mixed than pure marriages. Next come the evangelical and reformed denominations, with nearly 50 mixed to 100 pure marriages. The large proportion of mixed marriages among the Greek catholics is due to intermarriage with adherents of the Roman catholic and Greek oriental churches; comparatively few marry protestants. The Jews and the Roman catholics have the lowest percentage of mixed marriages. It must, however, be mentioned in this connection that in Budapest, the capital of Hungary, where social intercourse between Jews and christians is more common, and the Jews are on a higher economic and intellectual plane than their coreligionists in the province, the rate of intermarriage is nearly double, reaching 17 per cent. in 1904, although it is only ten years since they have been legally permitted to marry with christians. In Germany similar conditions prevail. Of a total number of 468,329 marriages contracted during 1901, only 41,014 were between persons of different faith, *i. e.,* only 9.59 mixed marriages to 100 pure marriages. Among the Jews in that country there were during that year contracted 3,878 pure and 658 mixed marriages, or 16.97 per cent., which shows a larger tendency to intermarry among the Jews than among the general population. Taking the three chief religions in Germany, we find the following proportions of mixed marriages:

Religion and Country	Pure Marriages	Mixed Marriages	No. of Mixed to 100 Pure Marriages
Germany (1901).			
Protestants	277,480	39,115	14.10
Catholics	145,141	39,115	26.95
Jews	3,878	658	16.97
Prussia (1901).			
Protestants	172,570	23,794	13.78
Catholics	87,898	23,794	27.07
Jews	2,570	455	17.71
Bavaria (1901).			
Protestants	12,390	4,588	37.03
Catholics	31,739	4,588	14.45
Jews (1902)	447	42	9.40

It is noteworthy that in Germany catholics are more given to intermarry than the protestants, which is exactly the reverse of conditions in Hungary. To 100 pure catholic marriages, during 1901, there were 26.95 catholics who married protestants, while only 14.1 per cent. of protestants were married to catholics, which is even less than the rate of Jewish mixed marriages in that country, 16.97 per cent. In Prussia also the catholics intermarried more than the protestants and Jews, the proportions being, catholics, 27.07 per cent.; protestants, 13.78 per cent., and Jews, 17.71 per cent. In Bavaria, on the other hand, the reverse is true. There the protestants intermarry to a much larger extent than the catholics. The latter had 37.03 per cent. of mixed marriages, as against only 14.45 per cent. among the former, and 9.4 per cent. among the Jews. It appears from these figures that adherents of the religion of the majority of the inhabitants are less likely to marry outside of their faith than persons following the creeds which are in the minority. Thus in Germany where 62.51 per cent. of the population is protestant, and only 36.06 per cent. catholic, the latter are more apt to marry protestants; similarly in Prussia (population, 63.29 per cent. protestant and 35.14 per cent. catholic), the catholics have 27.07 per cent. of marriages with protestants as against only 13.78 per cent. of marriages of protestants with catholics. In Berlin, where the population consists of 84.18 per cent. protestants and only 9.98 per cent. catholics, there were in 1904 only 19.93 mixed marriages of protestants to catholics, as against 323.81 per cent. of catholics to protestants, while in Bavaria, having a majority of catholic population, 70.65 per cent., and only 28.32 per cent. of protestants, it is the protestants who have a higher proportion of mixed marriages, 37.03 per cent., as against only 14.45 per cent. among the catholics. The powerful influence of the majority and its tendency to absorb the minority are thus demonstrated. The immediate cause is, of course, to be sought in the fact that there is often some difficulty to find a suitable partner among the minority, and when one is found among the followers of a different creed, all religious scruples are laid aside.[10]

Intermarriage between persons of different creeds is a recent phenomenon, only one hundred years ago it was quite rare. "In no respect has modern civilization acted more beneficently than as promoter of religious toleration," says Westermarck. "In our time difference of faith discourages sympathy to a much less extent than it did in former ages." [11] In Prussia the number of mixed marriages has quadrupled within the last fifty years, while the number of marriages in general has increased only 70 per cent. during that period. In Bavaria the increase has been more pronounced. During the first half of the nineteenth century they constituted less than three per cent. of the total number of marriages, while to-day one in ten marriages is contracted between persons of different faiths.

All these facts and figures emphasize that it was not any racial antipathy between the so-called Semite and Aryan which kept the Jews of former days from marrying with christians. There were practically no mixed marriages among persons of any religion during medieval days. The same prevails to-day in Russia, where mixed marriages are prohibited by law. With the progress of religious toler-

[10] A somewhat similar phenomenon has been noted among the immigrant population in the United States. The Tenth Census made the interesting deduction that in those portions of the country where a single nationality was numerously represented, as, for instance, the Irish in New York City, there was little intermarriage with other nationalities. But where the nationality was not numerously represented, as the Irish in St. Louis, there was a greater tendency among the men to marry native-born women, or women of other nationality. (R. Mayo-Smith, 'Statistics and Sociology,' pp. 111–112.) The same is true of the Jews in the United States: Very few marry christians in New York City, while in the western and southern states intermarriage is common.

[11] Westermarck, The History of Human Marriage, p. 376.

ance characteristic of our age, the number of mixed marriages has increased among persons belonging to all creeds, including the Jews.

Fertility of Mixed Marriages

Besides the religious and sentimental objections which are being made against intermarriage by Jews and christians, many other arguments were brought forward against them. Some have stated that mixed marriages are infertile, that a large proportion remain sterile and that the average number of children born to each marriage is much below the average of pure marriages. It has also been asserted that intermarriage of different races leads to physical, moral and intellectual deterioration. Only few of the virtues of each are inherited, but most of the vices are transmitted to the progeny.

The fertility of mixed marriages has especially been discussed by Joseph Jacobs, who calculated that even if one tenth of all the Jews and Jewesses married outside of their faith, only a little over two per cent. would be left of the original ten per cent. within six generations or 200 years. This idea prevailed, until recently Arthur Ruppin, after a thorough study of Prussian statistics, showed conclusively that there is no real basis for any such assertion. The fertility of mixed marriages in Prussia is not much below that of pure Jewish marriages.

Superficially, statistics would seem to indicate a lower birth rate of Jews married to non-Jews, as can be seen from the following figures, calculated by J. Thon, showing that in Bavaria the fecundity of the various classes of marriages was as follows:

	Pure		
	Christian	Jewish	Mixed
1876–1900	2.64	3.54	1.58
1902	4.40	2.20	1.38
1903	4.31	2.31	2.11

In Prussia and Hungary the same is shown to be the case, while in the city of Berlin the fertility of mixed marriages appears even lower. The percentage of complete sterility is also stated to be higher among mixed than among pure Jewish couples. In New South Wales 13.41 per cent. of all the Jews married to Jewesses were sterile, while among Jews married to christians 30.55 per cent. were sterile. The average number of children was 3.48 among the general population, 4.06 among the Jews and only 2.01 among Jews married to christians.[12]

All these figures are no safe criterion as to the fertility of mixed marriages, because the usual method applied in calculating the material

[12] Results of Census of N. S. W., 1901, Pt. III., 1902, Pt. V., 1903.

is fallacious. The way fertility is calculated is to divide the number of births in a given year by the number of marriages contracted during the same year. Only very few of the births during any one year are due to the marriages during that year, but are from couples married within the preceding twenty-five years. If the number of mixed marriages did not increase, such a division would more or less accurately give us the average fecundity. But as has been shown above, the number of mixed marriages increases regularly in every country considered, so that the births of the year considered represent the fecundity of a smaller number of marriages than have been contracted during this year. A smaller fertility is thus apparently seen among the mixed marriages. I will illustrate this by figures obtained by Ruppin about conditions in Prussia: During 1901 there were 4.2 births to each christian marriage; 2.80 to each Jewish marriage; and only 1.80 to each marriage of a christian to a Jewess and 1.53 to each marriage of a Jew with a christian woman. But recalling that only a few of these births were the results of marriages contracted during 1901, but represent marriages for about twenty-five years, we are led to investigate further. In 1876 only 256 mixed marriages were contracted in Prussia, and during the twenty-five succeeding years they increased annually, reaching 455 in 1901. If we accordingly calculate the birth rate for 1901 on the basis of the average number of marriages during these twenty-five years (1876–1901), the result is entirely different. Ruppin shows that the rates calculated by this method are 5.07 births to each christian marriage, 2.96 to each Jewish marriage, 2.5 to each marriage of a christian with a Jewess and 2.35 to each marriage of a Jew with a christian woman. The difference is thus not much in favor of pure Jewish marriages when compared with mixed. But even this does not give us a clear picture, because in many mixed marriages one of the parties accepts the religion of the other, and the births are then recorded not as the issue of a mixed marriage, but of a pure christian or pure Jewish marriage, as the case may be. Many births resulting from mixed marriages are consequently missing from the official records, thus reducing the average number of births perceptibly. Considering this and, in addition, the fact that most of the mixed marriages occur in large cities, where the birth rates are much lower than in the country, one is bound to agree with Ruppin that the Prussian official statistics do not support the theory that mixed marriages are less fertile than pure mariages.

There is very little to be said about the alleged physical deterioration of the offspring of mixed marriages, because it has not been proved by any one.[13] Intellectually we do have proofs that the children born to mixed marriages are not below the average of Jewish and christian in Europe. Grant Allen was the first to point out the striking number of distinguished persons of half-Jewish blood as something simply extraordinary.[14] To mention only some of them—Sir John Herschel, the astronomer; Paul Lindau and his brother; G. Ebers, the Egyptologist; Professor Oldenburg, the philologist; Ludovic Halevy, the musician; Paul Heyse; Francis Turner Palgrave, the critic; W. Gifford Palgrave, the traveler; Sir H. Drumond Wolff, Prevost-Paradol; Edwin Booth, the actor; Bret Harte, the novelist, Elic Metchnikoff, the biologist; David Manin; Leon Gambetta; Sir John Millais, the British painter; and many others.

Religion of the Children resulting from Mixed Marriages

The church in many countries often complained that mixed marriages are a net loss to christianity, because the children born to christians married to Jews are more apt to be raised in the tenets of Judaism. The Jews, on the other hand, have always maintained that each marriage of this kind is a distinct loss to Judaism. In fact, it has been pointed out that most of the children are brought up as christians, and that this is a distinct advantage to the Jews, because the race is thus maintained in its desirable purity, unadulterated by the infusion of foreign blood.

From statistical evidence available on the subject, it appears that about 75 per cent. of all the children born to Jews married to christians are baptized immediately at birth, and only 25 per cent. are raised as Jews. This is best seen in Hungary, where the law permitting mixed marriages stipulates that a person intending to marry one of another religion may make provision at the time of making the application for a marriage license about the religion of the children which may be born to them in the future. They may also leave the question open for future consideration, if they so desire. In the latter case it is provided that boys should follow the religion of their father, and girls that of their mother. Of the 3,590 mixed marriages contracted in that country from 1895 to 1903, only 801 have taken advantage of the provision of the law, and decided, at the time they applied for their marriage licenses, about the religious affilia-

---

[13] If the proportion of stillbirths should be taken as an index of the vitality of the new born, then nothing unfavorable can be found in cases of mixed marriages: In Prussia the percentage of stillbirths was found from 1875 to 1899 as follows: Christians, 3.59 per cent.; Jewish, 3.21 per cent.; and mixed, 3.45 per cent. The rates of the mixed are thus about midway between the pure Jewish and pure christian.

[14] *Mind*, Vol. VIII., pp. 504–5.

tion of their future children. Of these 685, or 85.64 per cent., declared that they desired to bring up their children as christians, and only 116, or 14.36 per cent., decided in favor of the Jewish religion. It is a striking fact that even in cases where the parents registered themselves as freethinkers married to Jews, one half declared their intention to raise their children in the tenets of the christian church. The Jews thus lose in Hungary 85.64 per cent. of the children born of mixed marriages, which is a net gain to the christian church.

In New South Wales, also, it was found that most of the children resulting from mixed marriages are raised as christians. Of the children born to Jews married to christian women, 44 were Jewish and 119 christian; of the children of christians married to Jewesses, 35 were christian and 20 Jewish. The net result is that from the children born to Jewish husbands married to christian wives only 26.99 per cent. were Jewish, and from children born to christian husbands married to Jewish wives 36.36 per cent. are Jewish. It is noteworthy that here the mother has more influence than the father in determining the religion of the children.

In Copenhagen, where the number of mixed marriages is very large, Salomon states that most of the children are raised as christians. The same conditions are stated to prevail in Italy and France.

In Prussia this subject has been studied by the census officials for many years. The result is this: About 75 per cent. of all the children are christian, and only 25 per cent. are Jewish. Thus between 1885 and 1900 the figures are as follows:

	Jewish	Christian
1885	24.78 per cent.	75.22 per cent.
1890	25.48 per cent.	74.52 per cent.
1895	24.47 per cent.	75.53 per cent.
1900	24.21 per cent.	75.79 per cent.

These figures represent the children who lived with their parents in the census years mentioned, and show distinctly that only 25 per cent. of them are Jews, although among the parents 50 per cent. of Jews are represented. It is noteworthy that in Prussia it is the husband's religion which has more influence in determining the religious affiliation of the offspring. During the census of 1900 it was found that in families where the mother was Jewish and the father christian only 20.08 per cent. of the children were Jewish, as against 27.67 per cent. in families where the father was Jewish and the mother christian.

It must also be mentioned that this does not represent the entire loss sustained by the Jews through intermarriage. A person who has one parent of christian origin, even if raised as a Jew, is more likely to marry a christian than a Jew when he becomes of age, because socially he comes in intimate contact with his christian relatives. It is also not as difficult for him to be baptized, because he considers himself as much of christian as of Jewish origin. It is Ruppin's opinion that hardly 10 per cent. of the children resulting from mixed marriages remain Jews for any considerable length of time. Of these it is doubtful whether any Jews are left after two or three generations.

"The loss sustained by Judaism through mixed marriages," says Ruppin, "is not to be considered a negligible quantity. In 1901, after five years of legalized intermarriage in Hungary, the proportion of children born to mixed parents was 1.23 per cent. of the total number of Jewish births; in Prussia, after twenty-five years of mixed marriages, it was 10.47 per cent., and in Berlin even 15.15 per cent. of all the Jewish births were of mixed origin. Between 1875 and 1902 14,536 children were born in Prussia from mixed marriages." In fact, Ruppin points out that the loss is much greater than through baptism, which is very much in vogue in Prussia. He shows that in Prussia only about 400 Jews are converted annually to christianity, as against 700 children of half-Jewish blood becoming christians. Only about 25 christians are annually converted to Judaism and 75 children of half-Jewish blood are gained by Judaism through intermarriage. In this connection it is of interest to mention the numerous christian missions to the Jews, which are mostly kept up on the moneys contributed by English-speaking people. It is well known that they meet with but little, if any, success. Their expense is enormous. According to Dr. Kohler,[15] the Berlin Society for Promoting Christianity among the Jews spent more than 117,152 Reichsthaler upon the conversion of 461 Jews during the fifty years of its existence, while the London Society paid between the years 1863 and 1894 from £600 to £3,000 for the conversion of a single Jew. It appears from the figures given above that intermarriage brings much better results for the promotion of christianity among the Jews than missions with their 'costly converts,' who only rarely prove to be desirable acquisitions to christianity.

Dissolution of Marriage

There are but few data about the frequency of divorce among the Jews. In eastern Europe and the orient, the rabbinical law of divorce prevails among the Jewish inhabitants, and any trivial cause often suffices before some rabbis to grant a divorce. The husband can even send a divorce to his wife by proxy through a messenger. Divorces, while not uncommon, considering the ease with which they can be obtained, are not very frequent. Family ties are very strong among the eastern European Jews and only rarely is advantage taken of the rabbinical law to dissolve marriage. The western European Jews do

[15] 'Jewish Encyclopedia,' Vol. IV., p. 252.

not depend on the rabbinical divorce law, but follow the civil laws of the countries in which they live. From the few statistical data available it is seen that during the middle of last century divorce was less frequent among the Jews in Bavaria than among the christian population in that country. The same appears to be the case in Berlin, as can be seen from the following figures:

	Average Annual Number of Divorces per 1,000 Population.		
	1885–1886.	1890–1891.	1895–1896.
Protestants	3.57	3.07	4.73
Catholics	2.71	2.81	3.35
Jews	2.67	2.51	3 26

Although the rates are lower among the Jews than among the christians, still there is to be noted a steady increase in the frequency of divorces among them, from 2.67 in 1885 to 3.26 in 1896. Another way to calculate the divorce rate is by taking the number of divorces in any one year per 1,000 married couples found in a city. In Berlin, during the five years, 1895 to 1899, the rates were 3.75 among the Jews and 5.09 among the christians.

In eastern Europe there are more divorces among the Jews in Hungary and Roumania than among the christians. In Hungary, from 1898 to 1901, the divorces among the Jews constituted 9.51 per cent. of the total number of divorces granted in that country, although the Jews only constituted 4.42 per cent. of the total population. In Roumania 5.83 per cent. of the divorces granted during 1897 were granted to Jews, while only 4.55 per cent. of the total population was Jewish.

There are no statistics about divorces among the Jews in the United States, but it can be stated, without fear of meeting serious contradiction, that it is not uncommon. The divorce courts in New York City are quite often asked by Jews to dissolve their marriage. I am inclined to believe that it is in New York as frequent among Jews as among christians. On the east side of the city the immigrant population very often take advantage of the rabbinical law, and easily obtain divorces. But among the native Jews this never happens; they go to the civil courts for the purpose.

From a few scanty statistics about the causes of divorce among Jews, it is evident that there is very little difference between Jews and Christians in this respect. In Berlin, Ruppin mentions that infelicity on the part of the husband is more frequently a cause among Jews than among christians, while infelicity on the part of the wife is more frequent among christians. Wife desertion is also more frequent among Jews than among christians in Berlin.

It has been stated that divorce is more apt to occur among mixed couples than among pure Jewish couples, and some statistics of the city of Berlin support this opinion. During the ten years, 1892 to 1902, to each 1,000 marriages there were divorces as follows: Jews, 3; christians, 3.91; Jews married to christian women, 10.09; christians married to Jewesses, 11.16. Mixed marriages are thus from three to four times more likely to be divorced than pure marriages. Marriages between christians and Jewesses are more often dissolved than marriages between Jews and christian women. Besides the excessive friction incidental between married couples of different faith, even between catholics married to protestants, it must be recalled that mixed marriages are taking place chiefly in large cities, where divorces are more common than in small towns and in the country. Besides, mixed marriages have lately been increasing, as was shown above, and divorces are more frequent among couples recently married than among those who have successfully passed several years of marital life. Statistics of divorce among mixed couples for a small number of years are, therefore, likely to be fallacious, and for a long period of years there are no available data.

With divorces the influence of the social environment is again evident. In the orient and eastern Europe, where the Jews live under strict adherence to their faith and traditions, participating but little in tendencies of modern life, the sacredness of the family ties is strictly guarded, and divorces are uncommon, although easily obtainable.[16] In western Europe and America, where the Jews are completely under the influence of modern city life, divorces are frequent and are growing in frequency.

---

[16] It will appear from the figures given above about divorces in Hungary and Roumania that they are more frequent among the Jews than among the christians in these countries. But it must be recalled that the christians can not obtain divorces easily according to the civil laws of the country. The Jews, however, are left to themselves in this respect, and they can easily obtain divorces from the rabbis.

PHYSICAL DEGENERACY OR RACE SUICIDE?*

By SIDNEY WEBB

I.

"IF only the devastating torrent of children could be arrested for a few years," wrote one of the most sympathetic friends of progress, not so very long ago, voicing the opinion of the economists from Malthus to Fawcett, "it would bring untold relief."[1] Not many years have passed, and his aspiration is fulfilled. One of his Majesty's Inspectors of Schools, lately revisiting, after some interval, a public elementary school in the center of London, remarked that, since he was there before, the 'babies' class' had ceased to exist. Between 1896 and 1905 the total population of the County of London is estimated to have increased by 300,000 persons. But the total number of children between three and five years of age who were scheduled by the vigilant school-attendance officers positively fell from 179,426 to 174,359. That this scheduling was fairly exhaustive is shown by the fact that there were almost exactly 5,000 fewer children of that age recorded in the London census of 1901 compared with that of 1891. Nor is this either an isolated or a temporary phenomenon. All over England and Wales the birth rate is falling steadily, in a decline which has already lasted thirty years, and which shows no sign of slackening. In 1876, to every 100,000 of the population there were born 3,630 babies. In 1904, to every 100,000 of the population there were born only 2,790—absolutely the lowest number on record since birth registration began.[2]

What does this continuous fall in the birth rate mean? What will be its results upon our economic and social relationships—what upon the future of the race? What ought we to do, and what can we do, to ward off whatever there is of evil in these results? These are questions which seem to be of greater importance to the community than the ephemeral issues of party politics. Yet those in authority are slow to tackle them. The subject has accordingly been under investigation during the past year by a committee of the Fabian Society; and the present article sets forth some of the results—formulated, however, by the present writer in his own way, for which the society as a whole has no responsibility.

* From the London *Times*.

[1] 'The Service of Man,' by J. Cotter Morison, preface, p. xx.

[2] Sixty-seventh Annual Report of the Registrar-General, 1906, p. xix.

1. The decline in the birth rate is not merely the result of an alteration in the ages of the population, or in the number or proportion of married women, or in the ages of these.

It is necessary at the outset to remove one possible explanation. What the Registrar-General gives us is the crude birth rate—that is to say, the exact proportion of births during the year to the total population, whether old or young, married or single. But in comparing these birth rates for different years, we have to remember that important changes may take place, even in a single decade, in (*a*) the proportion between children and adults; (*b*) the proportion between married and unmarried; and (*c*) the proportion between married women of the reproductive age and those above that age. These changes—due, it may be, to emigration or immigration, to economic or social developments, or to mere prolongation of the average life—are sufficient, in themselves, to produce a rise or a fall in the crude birth rate, without there having been any increase or decrease in human fertility. To give one striking instance, the crude birth rate of Ireland per 100,000 population fell from 2,384 in 1881, to 2,348 in 1901. But we happen to know that in the course of these twenty years the proportion of married women of reproductive age to the total population so far diminished that the slight fall in the crude birth rate really represented, not a decline, but a positive increase in fertility. If the Ireland of 1901 had contained a population made up, by ages, sexes and marital conditions, in the same proportion as that of 1881, the recorded births in 1901 would have appeared as a birth rate actually higher by 3 per cent. than that of 1881. We have, therefore, first to ask what are the corresponding figures for England and Wales, eliminating all the elements of variations of age, of postponement of marriage, and of positive refusal to marry.[3]

Now, it so happens that this problem has lately been worked out by the statisticians in a way to remove all uncertainty. Dr. Arthur Newsholme and Dr. T. H. C. Stevenson on the one hand, and Mr. G. Udny Yule on the other, have performed the laborious task of 'correcting' the crude birth rates for differences of age, sex and marital conditions, as regards the census years from 1861 to 1901.[4] The results show a

[3] I have restricted myself throughout to legitimate births. The number of illegitimate births in England and Wales is now only 112 per 10,000 of the population, and their omission does not affect the result. Their inclusion would merely have intensified the force of the argument at all points. The corrected illegitimate birth rate fell between 1861 and 1881 by 21 per cent., and between 1881 and 1901 by 41 per cent.—more than twice as fast as the correct legitimate birth rate.

[4] The decline of human fertility in the United Kingdom and other countries as shown by corrected birth rates, by Arthur Newsholme, M.D., medical officer of health, Brighton; and T. H. C. Stevenson, M.D., assistant medical officer to the Education Committee of the London County Council.

definite progressive fall in the proportion of births after allowing for all differences in the way the populations are made up. If the people of England and Wales had continued during those fifty years to be exactly of the same ages, and to be exactly in the same proportion married and single, the birth rate per 100,000 of the population would have changed to the following extent: 1861, 3,236; 1871, 3,312; 1881, 3,273; 1891, 3,125; 1901, 2,729. That is to say, if the fertility of the married women of equivalent ages had remained the same in 1901 as it had been in 1871, there would have been born 3,312 babies per 100,000 population, instead of 2,729, or just upon 21 per cent. more, equal in the whole of England and Wales to something like 200,000 more than actually saw the light. Why were those 200,000 babies not born?

2. The decline in the birth rate is not confined to the towns, nor (so far as England and Wales is concerned, at least) is it appreciably, if any, greater in the towns than it is in the rural districts.

Human fertility may possibly be normally slightly lower in the towns than in the rural districts, and it is sometimes suggested, especially by German authorities, that the fall in the birth rate is to be accounted for by progressive 'urbanization.' But English statistics afford no support to this hypothesis. It is true that the corrected birth rates of the towns of Northampton, Halifax, Burnley and Blackburn fell off between 1881 and 1901 by no less than 32 per cent., and that of London by 16 per cent. But the corrected birth rate of Cornwall fell off by 29 per cent., that of Rutland by 28 per cent., those of Sussex and Devonshire by 26 per cent., and that of Westmorland by 23 per cent. It is no less significant that, whilst the corrected birth rate of all Ireland actually rose during these twenty years by 3 per cent., that of Dublin rose by 9 per cent. If it was the unhealthy environment of our great towns that was causing a reduction in the number of births, we might expect to find Liverpool, Salford, Manchester and Glasgow—cities of extensive overcrowding, fearful slums and high mortality—heading the list. As a matter of fact, the corrected birth rate between 1881 and 1901 fell off proportionately less in these cities than in any other town, and actually less in proportion than in all but six of the counties. A decline in the birth rate, which does not appear at all in Dublin, appears much less in Liverpool and Manchester, Salford and Glasgow than in Brighton, and appears far more in Westmorland, Rutland, Devonshire and Cornwall than in any of those towns, can hardly be due to 'urbanization.'

On the changes in the marriage and birth rates in England and Wales during the past half-century; with an inquiry as to their probable causes, by G. Udny Yule, Newmarch Lecturer in Statistics, University College, London. Both these papers will be found in the *Journal of the Royal Statistical Society*, April, 1906.

3. The decline in the birth rate is exceptionally marked where the inconvenience of having children is specially felt.

There is not much evidence to be adduced under this head, but what there is is of some significance. Where married women habitually go to work in factories, and where their earnings form an important element in the weekly income of the family, the interruption caused by maternity is probably most acutely felt. The enforcement by the factory and workshops acts of 1891 and 1901 of four weeks' absence from employment after child-birth comes as an additional objection. Moreover, in the factory districts the later age at which children can now become productive wage-earners has certainly rendered large families less economically desirable than of yore. It is, therefore, of some significance that the ten towns in all England in which the relative fall in the birth rate between 1881 and 1901 is most startingly great are Northampton, Halifax, Burnley, Blackburn, Derby, Leicester, Bradford, Oldham, Huddersfield and Bolton—all towns in which an exceptionally large proportion of married women are engaged in factory work, in textiles, hosiery or boots. I can adduce no statistics of the decline in the birth rate among the married women teaching in schools; but it is known to be great.

4. The decline in the birth rate appears to be specially marked in places inhabited by the servant-keeping class.

It is significant that Brighton shows a relatively heavy falling off from a birth rate which was already a low one. But a comparison between various districts of London gives us further indications. Let us take, as a convenient index of relative wealth, the percentage of domestic servants to population. The corrected birth rate of Bethnal-green—the district of London in which there are fewest non-Londoners and in which fewest of the inhabitants keep domestic servants—fell off, between 1881 and 1901, by 12 per cent. (or exactly as much as that of the North Riding of Yorkshire). But that of Hampstead—where most domestic servants are kept—fell off by no less than 36 per cent., and attained the distinction of reaching the lowest of all the corrected birth rates that Dr. Newsholme has computed. Second only to Hampstead in this respect come Kensington and Paddington, which have statistically to be taken together, and which, keeping nearly as high a proportion of domestic servants as Hampstead, saw their corrected birth rates, already lower than that of Hampstead, fall off by 19 per cent., and sink to less than two thirds of that of the Bethnal-green of 1881. It would be interesting to extend this comparison, taking all the districts of London in the order of their average poverty, as shown by such indices as the proportion of the inhabitants who live in one- or two-room tenements, by the rateable value per head, and by the percentage keeping domestic servants. But the variations in the registration areas in nearly all these cases prevent accurate comparison of birth rates between 1881 and 1901. Dr. Newsholme and Dr. Stevenson, on the one hand, and Mr. Udny Yule, on the other, do, indeed, compare the corrected birth rates for 1901 of five separate groups of metropolitan boroughs, arranged in grades of average poverty. This comparison gives us the interesting result that the small group of three 'rich' boroughs have, per 100,000 population (corrected) 2,004 legitimate births; the four groups comprising nineteen intermediate boroughs have almost identical legitimate birth rates of between 2,362 to 2,490 per 100,000; whilst the poorest group of seven boroughs has a legitimate birth rate of no less than 3,078, or 50 per cent. more than that in the 'rich' quarters. From these figures it has been inferred that we are, in London at any rate, multiplying most prolifically from our most inferior stocks. It should, however, be noticed that the group of seven 'poor' boroughs happens to include, not only those containing the greatest numbers of Irish Roman Catholics, but also those in which the great bulk of the Jews are to be found. Practically half the marriages that take place in the registration districts of Whitechapel and Mile-end Old Town are solemnized according to the Jewish rite. It is against all the influences of the Jewish religion, tradition and custom to limit the family, and the birth rate among Jews of all classes and all nationalities is known to be large. We can not, therefore, infer from these statistics, either that the birth rate of the poorest strata of the English race in London is greater than that of the artisan or lower middle class. The remarkable evenness of the corrected birth rate throughout the nineteen 'intermediate' metropolitan boroughs, though they vary from having about 15 up to about 45 per cent. of servant-keeping households, is rather an indication to the contrary. This is in accordance with the fact that the decline in the corrected birth rate appears to be as great in the counties made up preponderatingly of the poorly paid agricultural laborers, as in those districts in which the average level of wages is much higher.[5]

5. The decline in the birth rate appears to be much greater in those sections of the population which give proofs of thrift and foresight than among the population at large.

Here we have to leave the carefully corrected birth rates supplied by Dr. Newsholme, and fall back upon evidence which is statistically less perfect. What would be desirable would be to have precise and 'corrected' birth rates for different years of two sections of the popula-

[5] The failure to take into account the special aggregation of the Jewish and the Irish population in the districts of greatest poverty, and the limitation of the investigation to London, appears to me to diminish the validity of some of Mr. David Heron's implications in the recent publication 'On the Relation of Fertility in Man to Social Status, and on the changes in this relation that have taken place during the last fifty years,' 1906. But these calculations point in the same direction as those cited.

tion, the one comprising those who took thought for the morrow and the other comprising those who did not. Such an exact contrast is, of course, unattainable. But it so happens that we do possess, over a term of nearly forty years, the number of children born in one large sample of the population, selected, it might almost be said, solely by the characteristic of thrift. The Hearts of Oak Friendly Society, the largest centralized benefit society in the kingdom, has now over 272,000 adult male members. This membership belongs to all parts of the United Kingdom, of which it may be said to represent about 3 per cent., or no inconsiderable sample. No one is admitted who is not of good character and in receipt of wages at least 24s. per week, a figure which excludes the agricultural laborer, the unskilled worker in town or country, and even (outside London) the lowest grades of skilled artisans. The society consists, in fact, of the artisan and skilled mechanic class, with some intermixture of the small shopkeeper and others who have risen into the lower middle class. Among its provisions is the 'lying-in benefit,' a payment of 30s. for each confinement of a member's wife. Unfortunately, we do not know either the relative proportions of the members who are married, or the average age of the wives. There is, however, no reason to think that the proportion of married members has appreciably changed, whilst it is believed that the average age of the members as a whole has risen from about 33 to 37.52; and it may possibly be inferred that there is a corresponding increase in the average age of the wives. Judging from the evidence of the Scottish census of 1855,[6] we might in such an event have expected a falling off in the births, due to this assumed difference of age, of at most 15 per cent. Now, what are the facts? From 1866 to 1880 the proportion of lying-in claims to membership rose slowly from 2,176 to 2,472 per 10,000. From 1881 to 1904 it continuously declined, until in the latter year it reached only 1,165 per 10,000 members. The birth rate among the population of a million and a quarter persons, distinguished from the rest, so far as is known, only by one common characteristic, that of thrift, has fallen off between 1881 and 1901 by no less than 46 per cent., or a decline nearly three times as great as that during the same period in England and Wales. Taking the whole period of decline, from 1880 down to the latest year for which I have the statistics, 1904, the falling off is over 52 per cent. A smaller society, the Royal Standard Benefit Society, having 8,225 members and giving a similar benefit, shows similar results. Between 1881 and 1901 the proportion of members claiming the lying-in benefit fell off by more than 56 per cent. If the members of the Hearts of Oak Friendly Society and the Royal Standard Benefit Society had had pro-

[6] See the figures given in 'Fertility, Fecundity and Sterility,' by J. Matthews Duncan, 1871; and those in 'Natality and Fecundity,' by C. J. and J. N. Lewis, 1906, pp. 18, 26 and 33.

portionately as many births in 1904 as the members of 1880 had in that year, there would have been born to them nearly 70,000 babies, instead of 32,000. If the birth rate in these 280,000 families of comparatively prosperous artisans had only fallen in the same degree as that of England and Wales generally, there would have been born to them 58,000 babies instead of 32,000. What was the special influence in these exceptionally thrifty families that prevented the other babies being born?

6. The decline in the birth rate is due to some new cause which was not appreciably operative fifty years ago.

We may, indeed, infer, from the relatively stationary birth-rate, alike of the whole population and of selected classes down to some date between 1861 and 1881, and the steady persistence of the subsequent decline, that the decline is due to some new cause. The same conclusion is reached by the elaborate calculations just published by Mr. Heron.[7]

In 1851, as in 1901, it could have been inferred from a comparison of different districts in the metropolis that 'the more cultured, the more prosperous, healthy, and thrifty classes of the community' were producing fewer children per marriage than the classes of lower social status. But, as regards London in 1851, Mr. Heron is "driven to almost certain conclusion that differences in the mean age of wives were amply sufficient . . . to account for the differential birth rates of districts with divergent social status." The operating cause of a low birth rate was, in fact, at that date, postponement of marriage. We know, however, from Dr. Newsholme's corrected birth rates that no such cause as a greater postponement of mariage, with the corresponding rise in the age of the average wife, has anything to do with the decline in the birth rate now recorded. This decline is due to some cause other than those that were appreciably in operation in 1851.

7. The decline in the birth rate is principally, if not entirely, the result of deliberate volition in the regulation of the marriage state.

The reader can scarcely have read the foregoing statements without coming to the conclusion that the falling-off in the birth rate, which has during the last twenty years deprived England and Wales of some two hundred thousand babies a year, is the result of deliberate intention on the part of the parents. The persistence and universality of the fall in town and country alike; the total absence of any discoverable relation to unhealthy conditions, mental development, the strain of education, town life or physical deterioration of any kind; the remarkable fact that it has been greatest where it is known to be widely desired; the evidence that it accompanies not extreme poverty but social

[7] 'On the Relation of Fertility in Man to Social Status and the changes in this relation that have taken place during the last fifty years.' By David Heron, 1906, p. 20.

well-being, and that it is exceptionally marked where there is foresight and thrift—all this points in one and the same direction.[8]

We may add other evidence. Among the Roman Catholics in the United Kingdom any regulation of the marriage state is strongly forbidden, and has, during recent years, been made the subject of frequent, special animadversion, both privately and from the pulpit. It is significant that Ireland is the only part of the United Kingdom in which the birth rate has not declined; that in Ireland itself it has declined a little in semi-Protestant Belfast, and not at all in Roman Catholic Dublin; and that in the towns of Great Britain the decline is least in Liverpool, Salford, Manchester and Glasgow—towns in which the proportion of Roman Catholics is considerable. Among the principal textile factory towns the decline is least at Preston, which is the one having the largest proportion of Roman Catholics. Among the different metropolitan boroughs—though we can not measure with accuracy the fall in the birth rate—the present rate is highest, and, therefore, in all probability, the fall has been least in those boroughs in which the Irish Roman Catholics (and the Jews who, in this respect, are in the same position) are most numerous. All this is inconsistent with the hypothesis that the decline is due to physical degeneracy, and consistent with that of its being due to deliberate volition. Common report that such deliberate regulation of the marriage state, either with the object of limitation of the family, or (which has the same result) with that of regulating the interval between births, has become widely prevalent during the past quarter of a century—exactly the period of the decline—reaches us from all sides—from doctors and chemists, from the officers of friendly societies and philanthropists working among the poor, and, most significant of all, from those who are engaged in the very extensive business of which this new social practise has given rise. What is needed to complete the demonstration is direct individual evidence.

II.

In the preceding section I have shown, upon statistical evidence that appears to me irresistible in its cumulative force, that the decline in the birth rate which is depriving England and Wales of at least one fifth of every year's normal crop of babies is not accounted for by

[8] It is at any rate consistent with the hypothesis of volitional interference, in view of the fact that illegitimate children are, on an average, certainly less desired than legitimate, that, as already stated, the corrected illegitimate birth rate should have fallen off in England and Wales more than twice as much as the legitimate, and twice as much between 1881 and 1901 as between 1861 and 1881. The figures for Scotland correspond to these. ('Natality and Fecundity,' by C. J. and J. N. Lewis, 1906, p. 54.)

any alteration in the age, sex or marital condition of the population; by any refusal or postponement of marriage; or by any of the effects of 'urbanization' or physical deterioration of sections of the community. The statistical evidence points, in fact, unmistakably to the existence of a volitional regulation of the marriage state that is now ubiquitous throughout England and Wales, among, apparently, a large majority of the population. To verify this inference it seemed necessary to obtain direct individual evidence from a sufficiently large number of persons, taken haphazard from different parts of the country, and from different social grades. This the committee of the Fabian Society set itself to obtain.

The procedure adopted was to obtain a voluntary census from a sufficiently large number of married people who could be relied upon to give frank and truthful answers to a detailed interrogatory. For this information resort was had to between 600 and 700 persons, from whom the committee had grounds of hope that answers would be received. About half of these persons resided in the metropolitan area, the remainder being scattered sparsely over the rest of Great Britain. In social grade, they included a most varied selection of occupations, extending from the skilled artisan to the professional man and the small property owner; omitting, on the one hand, the great army of laborers, and, on the other (with few exceptions), the tiny fraction of the population who have incomes from investments exceeding £1,000 a year. They were, of course, selected without the slightest reference to the subject of the inquiry; so little, indeed, was known about them from this standpoint that more than 20 per cent. of them proved to be unmarried, and thus unable to bear testimony. They were invited to give the information desired without revealing their identity, the form being so arranged as to enable it to be filled up by nothing more easily recognizable than crosses and figures.[9] Altogether 634 forms were sent out. From these there have to be deducted, for one reason or another, 158—viz., 114 bachelors, 30 duplicates (wives of husbands making returns), five which failed to get delivered by post office, two refusals, five returned blank or incomprehensible, and two relating to marriages abroad. Of the 476 remaining, 174 did not reply. Whether these should be added to the number of those who candidly confessed to having taken steps to regulate the births in their families, or to those who had taken no such steps, or in what proportion they should be distributed between the two, the reader must judge for himself. Significant replies were received from 302 persons. But as 14 of the returns included particulars of two marriages, the total number of marriages of which particulars are recorded is 316. In six cases the papers contain references to second marriages of which insufficient particulars

[9] I append the questions asked. (See opposite page.)

are given. These will not, however, materially affect the results. What is recorded here is the result of 316 marriages, and concerns 618 parents—not, of course, an adequate sample of the people of Great Britain, but, being drawn from all parts of the country and from every section of the great 'middle' class, sufficient, perhaps, until more adequate testimony can be obtained, to throw some light on all the previous statistics.

The first division of the marriages is into two classes; marriages with families intentionally limited, and marriages with families not so limited.

In order to avoid clumsy sentences, the term 'limited marriage' will be used to signify a marriage in which the family is intentionally limited, and the term 'unlimited marriage' one in which it has not been so limited. The following table gives all the marriages returned, arranged by the date and classified as limited (L) and unlimited (U),

	Yes.	No.
1. Are you married?		
Those who have been married should return themselves as married. In cases of second marriages each should be dealt with separately. A second paper will be sent if desired.		
2. Is your sex Male?		
3. Age last birthday?		
4. Date of marriage?		
Further returns from persons married before 1870 are not necessary as the period to be investigated goes back only 30 years.		
5. Age of Husband at Marriage?		
6. Age of Wife at Marriage?		
7. Particulars of Children born (including still-born children)		

	Date of Birth.	Sex.	Date of Death.*	
	1.	M. F.		
	2.			
	3.			
	4.			
	5.			
	6.			
	7.			
	8.			

	Yes.	No.
* This is only asked for as relevant to the inquiry in cases of death under five years of age.		
8. Do you expect to have any more (or any) children?....		
9. In your marriage have any steps been taken to render it childless, or to limit the number of children born?...		
10. If yes, during what years have such steps been taken?..		
11. Has there been any exceptional cause (such as the death or serious illness of husband or wife) tending to the limitation of the number of your children? (If possible, state the cause.)		
12. Observations. Any person willing to add any remarks throwing light on the foregoing return is requested to do so.		

together with (1) the number of childless marriages, (2) the number of children born or intended to be born (less deaths up to the age of five years), and the number of marriages in which more children were anticipated. 'One or two' is printed as one and a half.

Date.				Childless.'			Definite Expected Fertility.			More Children Expected.		
	L.	U.	Total.	L.	U.	Ttl.	L.	U.	Total.	L.	U.	Ttl.
1851	—	1	1	—	—	—	—	6	6	—	—	—
1857	—	1	1	—	—	—	—	9	9	—	—	—
1858	—	1	1	—	—	—	—	6	6	—	—	—
1862	—	2	2	—	—	—	—	11	11	—	—	—
1865	—	1	1	—	—	—	—	7	7	—	—	—
1867	1	1	2	—	—	—	1	5	6	—	—	—
1868	1	1	2	—	—	—	6	4	10	—	—	—
1869	1	1	2	—	—	—	7	1	3	—	—	—
1870	—	1	1	—	—	—	—	1	1	—	—	—
1871	—	1	1	—	—	—	—	6	6	—	—	—
1872	—	2	2	—	—	—	—	12	12	—	—	—
1873	3	3	6	—	—	—	10	12	22	—	—	—
1874	—	1	1	—	1	1	—	—	—	—	—	—
1875	4	1	5	1	1	2	6	—	6	—	—	—
1876	2	—	2	—	—	—	6	—	6	—	—	—
1877	3	—	3	—	—	—	13	—	13	—	—	—
1878	6	2	8	—	—	—	28	6	34	—	—	—
1879	6	—	6	2	—	2	19	—	19	—	—	—
1880	3	—	3	—	—	—	12	—	12	—	—	—
1881	2	—	2	—	—	—	7	—	7	—	—	—
1882	1	2	3	—	—	—	7	5	12	—	—	—
1883	6	2	8	—	1	1	16	3	19	—	—	—
1884	6	2	8	—	—	—	28	3	31	—	—	—
1885	8	2	10	—	—	—	31	8	39	—	—	—
1886	3	1	4	—	—	—	8	7	15	—	—	—
1887	6	2	8	—	1	1	20	2	22	1	1	2
1888	6	2	8	—	1	1	22	4	26	1	—	1
1889	10	3	13	—	1	1	23	4	27	2	1	3
1890	8	—	8	2	—	2	14	—	14	—	—	—
1891	6	1	7	1	1	2	15	—	15	1	—	1
1892	11	—	11	3	—	3	20½	—	20½	—	—	—
1893	11	2	13	2	—	2	23	1	24	3	1	4
1894	7	1	8	2	—	2	10	6	16	1	1	2
1895	16	2	18	5	2	7	22½	—	22½	6	1	7
1896	10	2	12	5	1	6	19	5	24	1	1	2
1897	9	—	9	1	—	1	19	—	19	2	—	2
1898	13	1	14	1	—	1	23	3	26	3	1	4
1899	16	4[10]	20	3	1	4	34	3	37	3	2	5
1900	11	1	12	4	—	4	12½	1	13½	5	—	5
1901	9	2	11	2	1	3	12	2	14	3	1	4
1902	9	6[11]	15	1	2	3	15	6	21	4	3	7
1903	9	6	15	1	2	3	15½	5	20½	3	6	9
1904	9	7	16	2	4	6	12½	3	15½	6	7	13
1905	9	3	9	6	3	9	6	—	6	2	2	4
Undated.	4	—	4	1	—	1	10	—	10	1	—	1
	242	74	316	45	23	68	553½	157	710½	48	28	76

It will be seen of the 316 marriages, 74 are returned as unlimited and 242 as limited. But in order to ascertain the real prevalence of voluntary limitation as affecting population, certain deductions should be made. Marriages prior to 1875 may fairly be taken out, since the

[10] One of these gives no information as to children.

decline of the general birth rate only began after that date. This eliminates six limited and 17 unlimited marriages, leaving 236 limited and 57 unlimited. Again, a usual commencement of limitation appears to be after the birth of at least two children. Marriages contracted in 1903, 1904 and 1905 should, therefore be deducted. This leaves 212 limited and 41 unlimited for the period 1875 to 1902, both years included, and including also four marriages the dates of which were not reported, but which almost certainly fall within the period named. But it must be further noted that no less than 13 of the 41 unlimited marriages were childless, and therefore no occasion for limitation arose, unless the parents had desired a childless marriage. This reduces the number of fertile and unlimited marriages during the period 1875 to 1902 to 28 out of 252, or, if the infertile unlimited marriages are deducted, 239.

If we take the decade 1890–99, which may be regarded as the typical period, we find that out of 120 marriages 107 are limited and 13 unlimited, whilst of these 13 five and possibly six were childless at the date of the return. *In this decade, therefore, only seven or possibly eight unlimited fertile marriages are reported out of a total of 120.*

In order to ascertain the effect of limitation on the size of families let us next take the number of children born and living up to five years of age, of all limited marriages from the earliest recorded (1867) to and including 1903.

NUMBER OF CHILDREN OF LIMITED MARRIAGES

Children in family	0	1	2	3	4	5	6	7	8	9	11
Marriages	39	54	59	29	22	11	6	3	2	1	1
Total Children	0	54	118	87	88	55	36	21	16	9	11

That is, a total of 227 marriages and 495 children. But owing to second marriages, which are not in all cases fully detailed, nine children must be added, together with an uncertain number (say six) for two other fruitful marriages mentioned but not reported. Altogether the parents of these (say) 510 children number 452. This, however, ignores expected children.

Taking all limited marriages we may next ascertain what is the probable total *intended* fertility. We can state the number of each limited family in this form:

Number living added to the number intended where stated; and, secondly, number living *plus* an unspecified addition. Cases where the return says 'two or three' more children expected are classified as $2\frac{1}{2}$, and 'three or four' as $3\frac{1}{2}$. We then get the following results:

TOTAL EXPECTED FERTILITY OF LIMITED MARRIAGES

Intended size of family	3	1	2	$2\frac{1}{2}$	3	$3\frac{1}{2}$	4	5	6	7	8	9	11
Completed families	33	35	45	4	26	5	21	11	6	2	2	1	0
Families with indeterminate additions	9	17	12	–	6	–	4	1	–	1	0	0	1
	42	52	57	4	32	5	25	12	6	3	2	1	1

If we assume the unspecified addition to average one and a half children we find that the 242 marriages have yielded, or are intended to yield, a total of 619 children and an average of 2.56 children per marriage.

If we take the typical decade 1890–99 we get the following results:

107 Limited Marriages, 1890–1899

Children living to the age of five	0	1	2	3	4	5
Marriages	25	23	34	15	6	4
Number of completed families ('no more expected')	22	17	19	9	4	4
Not recorded or doubtful	1	3	5	2	1	–
More expected	2	1	4	3	1	–
Number of children expected where indicated	—	2½	9	1	–	–

This gives 118 living children (excluding deaths of any after five years) and 12 or 13 expected, whilst in 11 cases unspecified additions to the families are anticipated, and 12 cases are doubtful. If one additional child is allowed for each doubtful case and one and a half for each unspecified case, this would give 159 children as the fruit of 107 marriages and of 211 parents (allowing for second marriages, in which cases only three persons are concerned in two marriages). This indicates that the offspring of each limited marriage (judging from the period named) is almost precisely one and a half children per marriage. The average number of children to be expected from each marriage, in England and Wales twenty-five years ago, was at least three times as great!

Information as to the causes which had led to limitation was not specifically asked for. But in many papers a large number of valuable details were supplied. Taking all the limited marriages (242) we find the causes indicated as follows:

Causes of Limitation

Economic	38
Sexual ill-health	13
Other ill-health or heredity	19
Disinclination of wife	9
Death of wife	6
Not stated	114
Several causes	43
	242

The death of a parent, of course, is a cause of *limitation* in another sense from that elsewhere employed in this paper.

Analyzing these last again, we find the following causes assigned:

Economic	35	out of	43
Sexual ill-health	11	"	43
Other ill-health or heredity	19	"	43
Disinclination of wife	15	"	43
Death of parent	2	"	43
Other causes	5	"	43

Adding the two together we find that, out of the 128 marriages in which the cause of limitation is stated, the poverty of the parents in relation to their standard of comfort is a factor in 73 cases, sexual ill-health (that is, generally, the disturbing effect of child-bearing) in 24 and the other ill-health of the parents in 38 cases. In 24 cases the disinclination of the wife is a factor, and the death of a parent has in eight cases terminated the marriage. It should be added that in one or two cases of marriages in the earlier years tabulated recent deaths of parents are mentioned which could not have affected the size of the families, and these are not included in the above.

The confidential voluntary census thus taken is, of course, far too small to be, in itself, any proof of a widespread custom. But taken in conjunction with the very extensive statistical evidence already adduced, it seems to me to complete the demonstration. We must, I think, now take it as proved that the principal, if not the sole, cause of the present continuous decline in the birth rate in Great Britain is the deliberate regulation of the marriage state. This practise prevails, it must be inferred, either with the object of family limitation, or merely with that of regulating the intervals between births, among at least one half, and probably among three fourths, of all the married people in Great Britain of reproductive age—not, as is often imagined, only among those above the ranks of labor, but practically among all classes, from the agricultural laborer in sparsely populated districts, and the artisan in the towns, up to the various grades of professional men and even to the wealthy property owners. The result is that after a quarter of a century of this practise, the total number of children born annually in Great Britain is less than four fifths of what it would be if no such interference took place. Nor is the practise confined to this country. The statistics indicate that New South Wales and Victoria have already carried it further than we have, whilst New Zealand is not far behind. Registration in the United States is very imperfect, but it is clear that the American-born inhabitants of New England, and perhaps throughout the whole of the northern states, are rapidly following suit. The same phenomenon is clearly to be traced in the German Empire, especially in Saxony, Hamburg and Berlin, but the German rural districts are as yet unaffected. The Roman Catholic population of Ireland (and of the British cities), as well as those of Canada and Austria, appear to be still almost untouched, but those of Belgium, Bavaria and Italy are beginning to follow in the footsteps of France. The fact that almost every country which has accurate registration is showing a declining birth rate indicates—though, of course, it does not prove—that the practise is becoming ubiquitous.

These clearly proved facts—which we are bound to face whether we like them or not—will appear in different lights to different people. In some quarters it seems to be considered sufficient to dismiss them with moral indignation, real or simulated. Such a judgment appears to the present writer both irrelevant and futile. It is impossible, as Burke has taught us, to draw an indictment against a whole nation. If a course of conduct is habitually and deliberately pursued by vast multitudes of otherwise well-conducted people, forming probably a majority of the whole educated class of the nation, we must assume that it does not conflict with their actual code of morality. They may be intellectually mistaken, but they are not doing what they feel to be wrong. Assuming, as I think we may, that no injury to physical health is necessarily involved—aware, on the contrary, that the result is to spare the wife from an onerous and even dangerous illness, for which in the vast majority of homes no adequate provision in the way of medical attendance, nursing, privacy, rest and freedom from worry can possibly be made—it is, to say the least of it, difficult on any rationalist morality to formulate any blame of a married couple for the deliberate regulation of their family according to their means and opportunities. Apart from some mystic idea of marriage as a 'sacrament,' or, at any rate, as a divinely instituted relation with peculiar religious obligations for which utilitarian reasons can not be given, it does not seem easy to argue that prudent regulation differs essentially from deliberate celibacy from prudential motives. If, as we have for generations been taught by the economists, it is one of the primary obligations of the individual to maintain himself and his family in accordance with his social position and, if possible, to improve that position, the deliberate restriction of his responsibilities within the means which he has of fulfilling them can hardly be counted otherwise than as for righteousness. And when we pass from obligations of the 'self-regarding' class to the wider conception of duty to the community, the ground for blame is, to the ordinary citizen, no more clear. A generation ago, the economists, and, still more, the 'enlightened public opinion' that caught up their words, would have seen in this progressive limitation of population, whether or not it had their approval, the compensating advantage of an uplifting of the economic conditions of the lowest grade of laborers. At any rate, it would have been said, the poorest will thereby be saved from starvation and famine. To those who still believe in the political economy of Ricardo, Nassau Senior, Cairnes and Fawcett—to those, in fact, who still adhere to an industrial system based exclusively on the pecuniary self-interest of the individual and on unshackled freedom of competition—this reasoning must appear as valid to-day as it did a generation ago.

To the present writer the situation appears in a graver light. More accurate knowledge of economic processes denies to this generation the consolation which the 'Early Victorian' economists found in the limitation of population. No such limitation of numbers prevents the lowest grade of workers, if exposed to unfettered individual competition, from the horrors of 'sweating' or the terrors of prolonged lack of employment. On the other hand, with factory acts and trade union 'collective bargaining' maintaining a deliberately fixed national *minimum,* the limitation of numbers, however prudent it may be in individual instances, is, from the national standpoint, seen to be economically as unnecessary as it is proved to be futile even for the purposes for which McCulloch and Mill, Cairnes and Fawcett so ardently desired it.

Nor can we look forward, even if we wished to do so, to the vacuum remaining unfilled. It is, as all experience proves, impossible to exclude the alien immigrant. Moreover, there are in Great Britain, as in all other countries, a sufficient number of persons to whom the prudential considerations affecting the others will not appeal, or will appeal less strongly. In Great Britain at this moment, when half, or perhaps two thirds, of all the married people are regulating their families, children are being freely born to the Irish Roman Catholics and the Polish, Russian and German Jews, on the one hand, and to the thriftless and irresponsible—largely the casual laborers and the other denizens of the one-roomed tenements of our great cities—on the other. This particular 25 per cent. of our population, as Professor Karl Pearson keeps warning us, is producing 50 per cent. of our children. This can hardly result in anything but national deterioration; or, as an alternative, in this country gradually falling to the Irish and the Jews. Finally, there are signs that even these races are becoming influenced. The ultimate future of these islands may be to the Chinese.

The conclusion which the present writer draws from the investigation is, however, one of hope, not of despair. It is something to discover the cause of the phenomenon. Moreover, the cause is one that we can counteract. If the decline in the birth rate had been due to physical degeneracy, whether brought about by 'urbanization' or otherwise, we should not have known how to cope with it. But a deliberately volitional interference, due chiefly to economic motives, can at any moment be influenced partly by a mere alteration of the economic conditions, partly by the opportunity for the play of the other motives which will be thereby afforded.

What seems indispensable and urgent is to alter the economic incidence of child-bearing. Under the present social conditions the birth of children in households maintained on less than three pounds a week (and these form four fifths of the nation) is attended by almost penal consequences. The wife is incapacitated for some months from earning money. For a few weeks she is subject to a painful illness, with some risk. The husband has to provide a lump sum for the necessary medical attendance and domestic service. But this is not all. The parents know that for the next fourteen years they will have to dock themselves and their other children of luxuries and even of some of the necessaries of life, just because there will be another mouth to feed.

To four-fifths of all the households in the land each succeeding baby means the probability of there being less food, less clothing, less house room, less recreation and less opportunity for advancement for every member of the family. Similar considerations appeal even more strongly to a majority of the remaining 20 per cent. of the population, who make up the 'middle' and professional classes. Their higher standard of life, with its requirements in the way of culture and refinement, and with the long and expensive education which it demands for their children, makes the advent even of a third or fourth child—to say nothing of the possibility of a family of eight or twelve—a burden far more psychologically depressing than that of the wage-earner. In order that a due number of children may be born, and that they may be born rather of the self-controlled and foreseeing members of each class than of those who are reckless or improvident, we must alter the balance of considerations in favor of the child producing family.

The question is whether we shall be able to turn round with sufficient sharpness and in time. For we have unconsciously based so much of our social policy—so many of our habits, traditions, prejudices and beliefs—on the assumption that the growth of population is always to be reckoned with, and even feared, that a genuine realization of the contrary position will involve great changes. There are thousands of men thinking themselves educated citizens to-day to whose whole system of social and economic beliefs the discovery will be as subversive as was that announced by Copernicus. We may at last understand what the modern economist means when he tells us that the most valuable of the year's crops, as it is the most costly, is not the wheat harvest or the lambing, but the year's quota of adolescent young men and women enlisted in the productive service of the community; and that the due production and best possible care of this particular product is of far greater consequence to the nation than any other of its occupations. Infant mortality, for instance—that terrible and quite needless slaughter within the first twelve months of one seventh of all the babies that are born—is already appealing to us in a new way, though it is no greater than it was a generation ago. We shall suddenly remember, too, that one-third of all the paupers are young children; and we may then realize that it is, to the community, of far more consequence how it shall bring up this quarter of a million children over whom it has complete power than the exact degree of hardness with which it may choose to treat the adults. Instead of turning out the children to tramp with the father or beg with the mother, whenever these choose to take their discharge from the workhouse, which is the invariable practise to-day, we should rather jump at the chance of 'adopting' these unfortunate beings in order to make worthy citizens of them. Half of the young paupers, moreover, are widows' children, bereft of the breadwinner. For them the community will have to arrange to continue in some form or another the maintenance which the father would have provided had he lived. Above all, we must encourage the thrifty, foreseeing, prudent and self-controlled parents to remove the check which, often unwillingly enough, they at present put on their natural instincts and love of children. We must make it easier for them to undertake family responsibilities. For instance, the arguments against the unlimited provision of medical attendance on the child-bearing mother and her children disappear. We may presently find the leader of the Opposition, if not the Prime Minister, advocating the municipal supply of milk to all infants, and a free meal on demand (as already provided by a far-seeing philanthropist at Paris) to mothers actually nursing their babies. We shall, indeed, have to face the problem of the systematic endowment of motherhood, and place this most indispensable of all professions upon an honorable economic basis. The feeding of all the children at school appears in a new light, and we come, at a stride, appreciably nearer to that not very far distant article in the education code making obligatory in the time-table a new subject—namely, '12 to 1 P.M., table manners (materials provided).' There would be no greater encouragement to parentage in the best members of the middle and upper artisan classes than a great multiplication of maintenance scholarships for secondary, technical and university education, and the multiplication of tax-supported higher schools and colleges at nominal fees, or even free.

Such a revolution in the economic incidence of the burden of child-bearing would leave the way open to the play of the best instincts of mankind. To the vast majority of women, and especially to those of fine type, the rearing of children would be the most attractive occupation, if it offered economic advantages equal to those, say, of school teaching or service in the post office. At present it is ignored as an occupation, unremunerated, and in no way honored by the state. Once the production of healthy, moral and intelligent citizens is revered as a social service and made the subject of deliberate praise and encouragement on the part of the government, it will, we may be sure, attract the best and most patriotic of the citizens. Once set free from the everwhelming economic penalties with which it is at present visited, the rearing of a family may gradually be rendered part of the code of the ordinary citizen's morality. The natural repulsion to interference in marital relations will have free play. The mystic obligations of which the religious-minded feel the force will no longer be confronted by the dead wall of economic necessity. To the present writer it seems that only by some such 'sharp turn' in our way of dealing with these problems can we avoid race deterioration, if not race suicide.

WATERWAY DEFENSES OF THE ATLANTIC COAST

By WILLIAM J. ROE

NEWBURGH, N. Y.

It will be remembered what a spasm of apprehension seized the country when the prospect of war with Spain became imminent. Not only did these fears affect seriously the dwellers in sea-coast cities, but they were shared in to such an extent by those who had been accustomed to plan their summer outings at the sea-shore as to send a very large proportion to the mountains instead. In fact so great was the unreasoning and unreasonable terror that the season of 1898 was quite unremunerative to innkeepers at summer resorts along the coast.

If such was the effect of a declaration of war with Spain, whose sea-power was so notoriously weak, what would it be likely to be in the face of hostilities threatened with a foreign country amply prepared for offensive naval movements? Though the prospect of war with a capable maritime power be ever so remote; though the fashion of modern civilization seems to have been set for arbitration, and congresses of peace have been established with at least the promise of permanency; and though we appear to have entered upon a period of good-will and cordial relations with the most ponderous of nations, still the construction of battle-ships and armored cruisers goes on in every shipyard of every government of the world with increased rather than abated zeal. However much the American public may desire peace, and however determined to exhaust the arts of diplomacy to preserve it, so long as human nature retains any of the virus of the serpent, or the fangs of the wolf, so long will war remain the final appeal of human interest, even though shorn of some of its ferocity as the first resort of inhuman passion. It is, and will continue doubtless for many decades, with nations as with the individual man; that one is always best assured of peace that is best prepared to resist insolence and retaliate forcibly, quickly and effectively upon any form of aggression.

After a long period of lethargy following the civil war the nation has become wisely awakened to the necessity of providing means of defense more in accord with our recent position as a 'world-power.' So have we constructed and are constructing powerful earth-works, replacing the obsolete defenses of stone forts, mounting behind them our guns; so that we planned mines for our harbors, and gathered destructive and far-flying torpedo missiles; and, more important yet, are building day by day huge monsters of the deep, each succeeding one an improvement in strength and fury upon its predecessor, and all vastly the superiors of those famous ships—the Iowa, the Oregon, the Brooklyn and the rest, that so quickly sent the steel-clad hulls of Spain upon the shoals of the Caribbean Sea.

In the state of high efficiency of modern ordnance, while floating guns are able to throw enormously destructive projectiles to such great distances, it is not beyond the range of reason to declare positively that not a single city upon the Atlantic coast is entirely safe from bombardment by a foreign fleet. The extraordinary contingencies of the Spanish war will not, it may be believed, ever return. The next time (which, with our expanding relations, more and more world-wide continually, may come at any moment) that we are called upon to match strengths with an enemy, we may be quite sure it will be with a foe of different caliber than poor, enervated Spain. Her valor, her deep sense of honor, her devotion, fanatical as that of any follower of Mahomet, all were vain and valueless because of—in one phrase—lack of adequate preparation. The next time the American people are called upon to face an enemy upon the high seas, it will not be, we may be sure, to find his nominal fighting power or the speed of his war ships diminished by so large a percentage; nor, to be frank, that our own ordnance, horse-power of engines, general efficiency, shall again surprise ourselves with performances so much better than was expected or claimed.

Do not let us delude ourselves with the undue confidence that all has been done, or is in process of being done, in the way of adequate preparation. For many years to come, though we construct men-of-war in increasing numbers and with increased power, it will still remain that other nations are also increasing their armaments. That 'next time' it may not be one nation, but a coalition of nations. Besides, in these days of swift changes and sudden inventions, the best efforts of designers of floating fortresses may become obsolete almost overnight.

There are some things, however, that by their very nature can not become obsolete; a single nation may for a time, by reason of greater energy, wealth or genius, so dominate in the game of warfare as to checkmate adversaries right and left. America, with her Ericsson and the Monitor, was for a few years as a queen among pawns. But we could not rely, as no nation can rely, upon such marvels. Little more than forty years have passed and the accepted type of battle-ship is the same all over the world; that has replaced the Miantonoma, as that replaced the Monitor, as that displaced the Congress and the Powhatan. Between the banks of oars of the Carthagenian triremes and the sails of Philip II. and Effingham; between the Seventy-four of 1812 and the 'cheese-box on a raft,' great gulfs of mechanical ingenuity are fixed. Yet all the while, under all customs of conflict, all political administrations, in every age, certain changeless principles underlie and determine changing methods of action. These things can never become obsolete; the ingenuity by which Rome taught herself to emulate and at last excel in the first Punic war, and that victory comes not so much by the possession of big forces as by having Drakes and Frobishers pitted against a Medina Sidonia.

In every condition of battle, and especially having reference to our own defense on the Atlantic coast from a powerful adversary on the sea, two great principles assert themselves as essential; first is the establishment of defensive relations, by both fortifications and squadrons, and second, the ability to concentrate swiftly and effectively at any threatened point the full measure of naval effectiveness at our command.

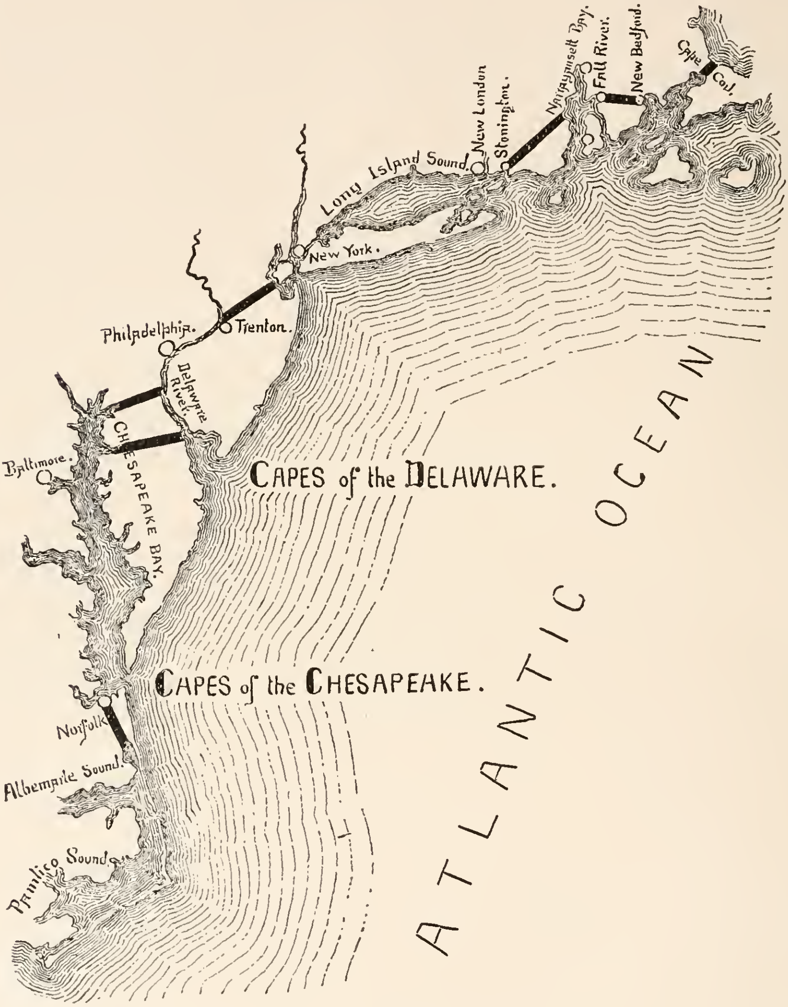
A hostile fleet coming upon our coast for purposes of offense would have the advantage of being able to concentrate at any desired point, to select the city that it sought to doom to destruction or spoliation. The timid citizens of New York, who a few years ago had their fears so excited, may take confort in knowing that of all our great sea-board cities, theirs is probably in least danger of bombardment. Gruesome tales were told of the ease with which foreign war ships could float broadside off Coney Island, to send round shot and shell into Broadway and Fifth Avenue. Calm your fears or assuage them; no doubt a hostile fleet can select and concentrate; but of all exposed points it is least likely to choose New York. The reason for this comparative immunity lies in the fact that the Hudson River empties into the ocean at the apex of a reentrant sea-angle, the base of which is found on a line drawn from the end of Long Island at the east to either the Capes of the Delaware or those of the Chesapeake at the south. At Philadelphia and in Hampton Roads are naval stations, and also at Newport and New London. It is reasonable to assume that at all these would be war vessels, which concentrating would be likely to furnish a force to assail an enemy upon the sea in the rear located off Sandy Hook. Formidable or fortunate would an attacking force be to avoid or avert some form of disaster, if not complete destruction. In war, as in the lightning stroke, energy is apt to take the line of least resistance, and it may, I think, be quite confidently asserted that some other city, not possessed of this advantage, would be the one most exposed to attack; Boston and Portland in the north, and on the south Charleston or Savannah.

We shall not enter into any details concerning movements of land forces, nor do more than call attention to the strength of our present sea-coast batteries. I would not lull you to a too great confidence that that strength is sufficient, nor that even torpedoes, fixed or floating, are certainly effective; nor is it necessary to excite further alarm by suggesting enormous expense involved in the construction of armored forts; these all are foreign to the purpose of this paper, which is simply to point out how easily, and—considering the objects to be achieved—how cheaply, those two principles of defence—mutual defensive relations and concentration of floating forces—can be achieved.

With the map before you note the condition of existing waterways along our seaboard. From the eastern end of Long Island a series of continuous inland waterways begins that, with here and there an interruption, does not end till the coast of Florida is reached. Long Island Sound is now or soon will be amply defended from the sea. The East River, New York Bay, the Kill-von-Kull, the Delaware and Raritan Canal, the Delaware River, the Chesapeake and Delaware Canal, the Chesapeake Bay, the Dismal Swamp Canal, Albermarle Sound, Pamlico Sound and beyond, almost continuous lagoons behind the Sea-Islands of North and South Carolina, Georgia and Florida, complete a chain of channels and artificial canals, awaiting only enlargement, and in some cases adequate or extra fortification to render the entire sea-coast—if not impregnable, at least defensible to an extent to which military men have long been alive; but to which the casual citizens (as well as their representatives in congress assembled) have been not only slumbering, but even in wakeful moments deaf, dumb and blind.

We may go even a little farther in the way of defensive suggestion, by including not only the Atlantic sea front, but also the gulf coast. A canal, surveyed and reported upon long ago by the United States Corps of Engineers, could be constructed at a trivial cost from Jacksonville to and down the Suwanee River. That such a canal has not already been constructed is owing to the fact that in a commercial sense it would not pay. Beyond Fisher's Island at the eastern extremity of Long Island Sound, for purposes of ample inland communication, a ship canal would need to be 'built' from near Watch-Hill to some point on Narragansett Bay, and another from near Fall River to New Bedford. Already a canal (designed for small coasting craft) has been projected and some work done, connecting Buzzard's and Cape Cod Bays. The fact stares us in the face, that with a series of channels requiring only construction or enlargement, connecting navigable and defensible inland bodies of water along the coast, nothing—or comparatively nothing—has been done to effect so great a war benefit. Of course this apathy has been due solely to that narrow commercial instinct—the so-called 'timidity of capital'—which has persisted in refusing even to traffic, to say nothing of national defense, the manifest gain that would come from judicious expenditure. Capital will not (as I have heard it expressed) 'sink money in a ditch.'

Some day, perhaps not in the far future, some of these frugal multimillionaires may find occasion to abruptly change their minds. It is 'on the cards,' though they be not yet dealt, not even shuffled. It is not necessary to say that no reference is made especially to Great Britain in quoting a famous phrase: that some day the 'thunders of English cannon may be heard in—St. Angelo?' No, in Boston. When the



splendid Fine Arts building and the Public Library become targets for long range projectiles, and the new 'Old South Church' and Trinity and all the magnificent residences of the 'Back Bay' are crumbling under the 'fire-ball of death,' surely they will be sorry that an expenditure was not demanded, less, far less than the tribute that might be demanded, to save their city from destruction.

Perhaps even before the twentieth century is well out of its youth a hundred millions may not suffice as ransom. That sum, probably much less, spent properly now or within the next five years would go far indeed towards saving their terrors and their pockets.

All this that has been related in perhaps tedious detail has long been under consideration by our department of war. Time and again has laid before the president, the senate and house of representatives the data collected by the engineer corps with painstaking fidelity looking to an end so beneficial. The congress has been asked, urged, implored, in at least one instance where the expenditure required was trifling compared with the defensive result, to construct a deep waterway. Bills, from time to time, have been introduced—five or six in the last fifty years—but nothing has come of any of them of a practical character.

A slight study of the accompanying map showing a portion of our Atlantic seaboard will demonstrate, better perhaps than much argument, the necessity and, inferentially, the effectiveness, of a proposed series of deep water canals, parallel to the coast and connecting one after the other the landlocked and fort-defended rivers, bays and estuaries. From the extreme eastern terminus of the system at Cape Cod Bay, the first of these suggested artificial channels is that which would have its southern end in Buzzard's Bay. Next comes (all being denoted by thick black lines) a similar water communication between New Bedford and Fall River. Still another is proposed between Narragansett Bay and Long Island Sound near Stonington. After this, towards the southeast there is already natural deep-water communication, through the East River and New York harbor. It requires only the widening and deepening of the 'Delaware and Raritan' canal to open a well-defended inland waterway to the Delaware River.

Perhaps at the time we have instanced—not as an alarmist, but as a mere guide-post to possibilities of the future—when a foreign fleet appeared threatening Boston, in New Bedford harbor were a few ironclads. For them to hasten to the threatened point that little strip of sand cut through by a thirty-foot canal would mean perhaps salvation. But with the others cut, how quickly could our fleets gather; one from Newport, another from New London, reinforced—as they speedily would be—by all the naval strength gathered at the New York Navy Yard and at League Island on the Delaware.

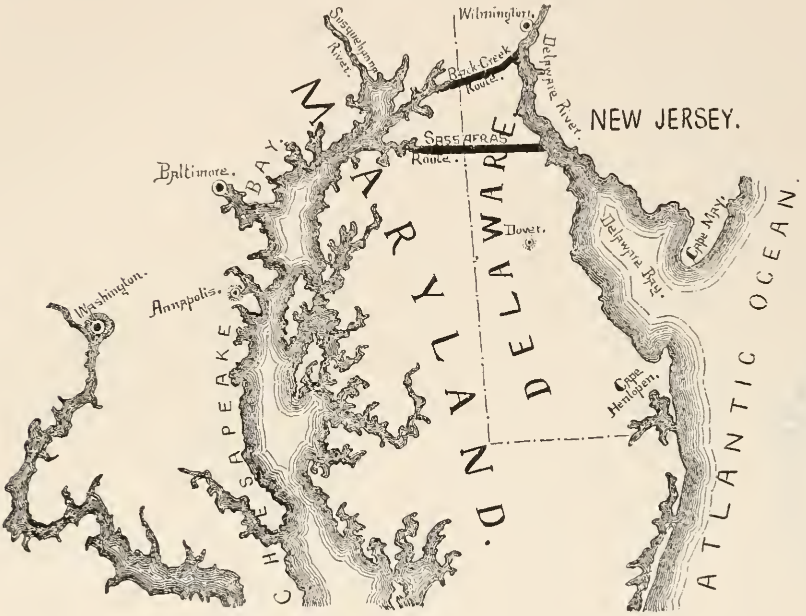
But an even more potential presentation of the advantage of a ship-canal of sufficient depth to enable a war ship to pass through it is found in the projected cutting through of the narrow neck that separates the waters of the Delaware from those of the Chesapeake Bay. The suggested waterways between Long Island Sound and Narragansett Bay, and between Fall River and New Bedford have not even yet been so much as thought of; the Cape Cod cutting, though for over two centuries planned for, and even partially worked, has been regarded solely in the light of a commercial venture. Of all these proposed canals but one has received attention from the United States. This one is the cutting of the divide between the Chesapeake Bay and the Delaware River. In 1894 a commission, authorized by congress and appointed by the president, examined numerous routes already surveyed across the Maryland-Delaware peninsula, with the result that they recommended the route known as the 'Back Creek' or 'Elk River' route, the most northerly of all. Much interest had been taken in a projected canal in Baltimore; but it was wholly in the light of a commercial benefit. The advantage of the route selected—though manifest from a military standpoint—was not perceptible to the practical minds of the traders of the thrifty city. The commission had been required to select that one of six or eight routes (the most southerly being that known as the 'Choptank') which should be most advantageous in ways of commerce as well as those of war.

Baltimore's shippers, willing enough to admit the theoretical benefits of the route selected from a military standpoint, declared with one voice that it possessed none whatever of a commercial character. It would be as well, they said, if not better, to continue to come and go by the old route down the Chesapeake to the capes of Virginia. There was not enough public spirit in congress to incite to action from any purely military considerations; the incentive of private commercial interest being lacking, the project dropped like lead. Ten years passed, and then, at the first session of the fifty-eighth congress a bill was introduced in the house of representatives, and another (at the second session) in the senate, both having for object the purchase by the United States of the 'chartered rights' of the 'Chesapeake and Delaware Canal,' located on the line of the so-called 'Back-Creek,' and the construction of a free ship-canal thereon. Both of these bills failed to get past the committees to which they were referred. Again in 1905 a joint resolution (introduced by Mr. Mudd of Maryland) was referred to the committee on railways and canals. It appeared that this measure was likely to meet the same fate as its predecessors; but interest in the project was aroused in the committee, and intelligent scrutiny; the resolution was modified to the extent that all the so-called 'southern routes' (manifestly of no utility in a military sense) were eliminated, and consideration confined to the two most northerly routes—the 'Back Creek' and the Sassafras. In this shape the joint resolution passed both houses; it was approved by the president, and a commission was appointed to decide which of the two routes was the better, in view of 'probable cost and commercial advantages and military and naval uses of each,' with a view to constructing by the United States of a 'free and open waterway.'

For the first time in the history of these endeavors the question had become a practical one—as to which of two routes was the superior for both commerce and war.

The second of the two maps shows at a glance the situation of the two routes, especially the manifest advantage of the Sassafras, chiefly on account of the greatly decreased distance from the wharves of Baltimore to a point at sea off the capes of the Delaware.

But it is not with the commercial relations of any route that our



interest lies; but rather that by the construction of a ship-canal by one or the other, one link will be securely forged in that chain of waterways by which so much is to be gained in ways of defense of our Atlantic seaboard. Perhaps these defensive advantages could not be better set forth than by quoting in full the 'expert' opinion of General William P. Craighill, former chief of engineers, U. S. A.

Notes upon the Military Considerations concerning a Proposed Ship-Canal from the Delaware to the Chesapeake.

It will be doubted by no one that a deep-water communication between the two bays would be of vast importance in the contingency of war with a maritime nation. Such a connection would provide a means of concentrating the floating defenses of the two bays, and besides this would render more secure the communication between the naval stations of Philadelphia and Norfolk and Washington. Vessels defending a port have two offices to perform, the one being to assist in the direct defense, or to prevent capture or occupation by a hostile force, the other being the prevention or breaking up of blockades. Without a canal a blockade at the Capes of the Delaware would close the port of Philadelphia, or blockade at the Capes of Virginia would close the outward commerce of Baltimore and other ports of the Chesapeake. With the canal built where communication would be secure, neither the ports of Philadelphia nor of Baltimore could be closed unless an effectual blockade were established both at the Delaware and at the Virginia Capes. The disadvantage to the attacking party is obvious, while the defending vessels could concentrate at either outlet, and breaking the blockade at one point would open both ports and render the blockade useless at the other outlet.

When the question of defense is considered in the choice of a route, the elements are rapidity and security of communication. . . . For the purposes of concentration for the defense of the two bays, the Sassafras is superior in regard to rapidity. In this respect there is little or no difference between the Sassafras and the Back Creek.

For security of communication the Sassafras is superior (to the so-called 'lower routes'), as the entire route from Baltimore to Philadelphia can be protected by shore defenses, and the defense can be made or assisted at any point by gun vessels whose light draught would permit them to keep out of water in which they could be rammed by the sea-going warships. The Back-Creek route in this respect is precisely the same as the Sassafras.

The commission, of which General Felix Agnus, of Baltimore, is chairman, has given to the matter of a choice between the two routes all the time and attention that its importance deserves. Public meetings were held during the month of September, in Baltimore, Wilmington and Philadelphia, and great interest manifested, not only on the part of representatives of trade organizations, but by members of congress from the states most directly concerned. This interest was, of course, chiefly commercial, and a keen rivalry was developed between the cities of the Delaware and those of the Chesapeake Bay, the former favoring the line of the present canal, while the Sassafras was advocated by those who foresaw the great benefits that must come to Baltimore from the adoption of this route.

The rivalry, keen as it has been, between the opposed interests of the 'Back-Creek' and the Sassafras routes, has been in all respects honest and good natured; neither side to the controversy having manifested any spirit of jealousy or unwillingness to yield to the findings of the commission.

What that finding may be it is yet too early for conjecture; but in either event, acquiescence will not be withheld, nor all the influences of a united public sentiment, given not 'grudgingly, nor of necessity,' but with generous and cordial assent as to an honest judgment for the public welfare and the nation's good.

THE SIMPLIFICATION OF FRENCH SPELLING

By Professor BRANDER MATTHEWS

COLUMBIA UNIVERSITY

IN February, 1903, the French government appointed a commission to prepare the simplification of French orthography. It consisted of MM. Bernès, Clairin, Comte, Croiset, Devinat, Gréard, Meyer (members of the Superior Council of Education), M. Havet (of the institute), Professors Brunot and Thomas (of the University of Paris), and MM. Carnaud and Cornet (deputies). M. Paul Meyer was the president of the commission and M. Clairin the secretary. The commission made its report in July, 1904, advocating a series of simplifications of French spelling, in accordance with the principle of omitting useless silent letters—the same principle which is guiding the action of the Simplified Spelling Board here in the United States.

The report of this commission was submitted to the French Academy, which charged M. Emile Faguet with the duty of expressing its opinions. As a result the government appointed a second commission, of which M. Faguet is a member and of which the report was written by Professor Brunot. This report is in type, but it has not yet been distributed. M. Meyer has now reprinted his report, prefacing it with a personal paper of his own in which he discusses the present condition of French orthography, explains the historic reasons for its absurdities and points out how it can most easily be improved. His pamphlet, 'Pour la Simplification de notre Orthographe,' is published in Paris, by Delagrave. His statement of the case is curiously like that which has been made in English by the Simplified Spelling Board.

> Ordinarily, spelling is defined as 'the art and science of writing the words of a language correctly, according to established usage.' But that usage becomes established under conditions differing widely, according to the period and the country; and in order to appreciate the value of the orthography of any language it is important in the first place to know the origin of that usage, made permanent in the spelling. Almost everywhere the original idea was that spelling should reflect pronunciation as closely as possible; the phonetic tendency is predominant. But wherever spelling became fixed at an early time, whether by academies or through printers' influence, it ceased gradually to be phonetic in character, because language changed, little by little, in pronunciation as well as in vocabulary and grammar, whereas spelling, once established, paid no attention to these changes. Other causes entered into play which helped gradually to take from the spelling of certain languages the symbol of graphic representation of sounds they originally possessed. One of these causes, and perhaps the most potent, was the pedantry which introduced into the writing of many words so-called etymological letters which were not pronounced. These contradictory tendencies may be seen elsewhere as well as in French. Thus, to cite a single example, English spelling, which was principally phonetic in the sixteenth century, has now become purely conventional, the pronuncia-

tion having undergone since that time considerable change which the written form does not indicate.

M. Paul Meyer then calls attention to the fact that "the great obstacle to the development of a spelling both logical and suited to our language has been the inadequacy of the Latin alphabet, which could not express sounds originating after the Latin period."

In the sixteenth century various expedients were suggested to remedy this poverty of symbols. About 1530, Geoffrey Tory, a printer, introduced the use of the cedilla, already known among the Italians and Spaniards, to indicate the sibilant sound of *c;* but it occurred to no one to employ a similar device to distinguish the two sounds of *g.* Geoffrey Tory also used the *accent aigu* (′), but without giving it exactly the value it has to-day; he made use of it solely to distinguish the *e* pronounced from the *e* mute. The accent grave, which distinguished the open *e* from the closed, was not introduced until much later. In 1562 Ramus succeeded in having the distinctions between *i* and *j,* between *u* and *v,* pass into common use.

He shows clearly that French spelling has suffered from some of the same unfortunate influences which have reduced English spelling to its lamentable condition:

In spite of its lack of uniformity, written French had had until then a phonetic tendency. Unfortunately, there was an antagonistic movement under the influence of humanism, which introduced into the notation of speech certain silent letters to indicate the derivation of words: they wrote *aultre, advocat, doigt, droict, faict, poids, scavoir, soubs, subject,* etc., in order to make the true or supposed etymology of these words visible. This was absurd; there was no need to put an *l* in *autre* to represent that in the Latin *alter,* which was already shown by the *u* (*altre, autre*). These 'superfluities,' so called by the Abbé d'Olivet, editor-in-chief of the third edition of the 'Dictionaire de l'Académie' (1740), in a great many cases, but not in all, have been expunged from the language.

Similar superfluities abound in English still, and they are still defended by arguments like those contained in the preface to the first dictionary of the French Academy (1694). "The Academy adheres to the old spellings accepted among men of letters, because they aid in showing the origin of the words. That is why the academy believes that it ought not to authorize the abridgments which certain individuals, chiefly printers, have made, because these omissions destroy every vestige of the analogy and relation between words that are derived from Latin, or from any other language. Thus the words *corps* and *temps* are given with a *p,* and the words *teste, honneste* with an *s,* to indicate that they come from the Latin *tempus, corpus, testa, honestus.*" As M. Meyer asks, "What value can be given to a spelling founded on such fluctuating principles?" And he quotes Gaston Paris as saying that "the academy, deceived by superficial data, thought it was furthering scientific accuracy by adopting traditional spelling; in reality they followed routine and added to confusion." M. Meyer declares that "what should have been done, had the academy understood its mission, would have been to follow methodically the path taken, instinctively and without purpose, by the writers of the middle ages; a gradual modification of the system of representing sounds was necessary to preserve the connection between spoken and written language. Pronunciation, like vocabulary, alters, insensibly from generation to generation; the written words ought to record each change as it occurs."

Asserting that it is impossible now to inaugurate suddenly a complete series of changes which might readily have been adopted had they been introduced gradually as need arose, M. Meyer tells us that his committee retained existing conventions in so far as they are not in conflict with other conventions no less worthy of consideration, and it denied any wish to establish a phonetic system of spelling. It limited its work to the correction of the most striking irregularities of the present spelling of French. Here again the attitude of this commission of French scholars is seen to be in absolute accord with that taken in America by the Simplified Spelling Board. And the reasons given for action are also almost identical:

1. The esthetic argument: Our spelling is irregular, and gives the language an ugly irregular look.
2. The argument for preservation: It is important to maintain our customary pronunciation upon which the irregularities of our spelling react.
3. The practical argument: These same irregularities make the study of spelling needlessly difficult.

With the contention that certain useless silent letters ought to be retained, because they reveal the derivation, M. Meyer has the impatience of a scholar; and he points out how the existence of these needless letters is dangerous to accuracy of pronunciation:

We write *prompt, promptitude, dompter* (although there was no *p* in the Latin *domitare*), *indomptable*. The Academy says plainly that in the words *indomptable* and *prompt* the *p* is silent. Nevertheless, we hear constantly the pronunciation *dompter, promptitude,* because the school masters who teach French to children, not having the Academy dictionary always at hand, are naturally inclined to pronounce words as they are written. The same cause of error exists in other languages. In English the *g* in *recognize* is pronounced, but formerly it was not even written; the *g* is a pedantic addition which has ended by making its way into the spoken usage. *Fault* and *author* are pronounced as they are written; formerly they were both written and pronounced *faut* and *autor.*

Littré noticed how intimately pronunciation was allied to spelling. They are two forces, he says, which react continually upon each other. When there is no extensive teaching of grammar, and the language is learned orally rather than by the eye, then pronunciation modifies spelling, which follows it closely. When, on the contrary, books play a large part in teaching the mother tongue, spelling influences pronunciation: the tendency is to pronounce all the letters, and traditional pronunciation succumbs in many places to the visible symbols. There are to-day frequent examples of this.

As might be expected, French teachers find the same fault with their illogical spelling that teachers of English find with ours, which is actually far more illogical than theirs and far more irregular. M. Meyer raises their objections with much sympathy:

Although it is to be regretted that children must make such prolonged efforts to learn to write in conformity with obsolete rules a language which they often speak very correctly otherwise, still the misfortune would not be without some compensation if these efforts contributed to the development of their reasoning powers; but they do not. Learning spelling is above all a matter of memory, especially of the visual memory. Reasoning has nothing to do with it, for there is no reason why *apporter* should be written with two *p's*, while *apercevoir* and *apaiser* should have but one; or why the plural of *chou* and six other words should be formed with an *x*, while all other words terminating in *ou* take *s*, following the ordinary usage. On the other hand, the acquisition of a logical spelling would bring the reasoning powers into play far more than the memory.

But the most forcible passages in M. Meyer's own plea for simplification are in response to the various arguments urged in behalf of the existing orthographic confusion:

The objections advanced by the public to all modification of spelling may be grouped under four heads:

1. Every change in spelling distresses us. We dislike to alter our habits. Books printed in a new spelling are distasteful to us. Besides, we have had enough trouble to learn the current spelling; why should we be obliged to learn another?

2. Words in their very form and independently of their sound, have an esthetic beauty, which would be lost as soon as the appearance was modified.

3. The proposed changes would result, in many cases, in causing all trace of the etymology to disappear. We love the *g* of *doigt*, which reminds us of the Latin *digitus;* the *p* of *temps*, behind which we can see the Latin *tempus;* and *physique* written *fisique* seems barbarous to us. Further, these changes would obscure the derivation of words of the same family.

4. These same changes will lead to lamentable confusion, since we shall no longer be able to distinguish *corps* (a body) written without *p* from *cors* (a corn).

It is apropos of arguments of this kind that a great English linguist, A. J. Ellis, said: "These are very sweeping assertions, and those who have given serious attention to the subject for many years feel astonished that any person of ordinary intelligence and linguistic attainments could commit himself to such statements."

1. Let us take up these feeble arguments one by one. We shall not attempt to dispute the fact that any suddenly introduced innovation shocks us. When the fashion of crinolines came in during the Second Empire it seemed at first utterly ridiculous. But people became accustomed to it; the reason it disappeared was not that people disliked it, but because the fashionable dressmakers had to have new styles. It will be somewhat similar with the alterations in spelling that we propose. Assuredly, they will arouse a more general feeling of instinctive and inexplicable opposition than they would have a hundred years ago, for the number of people who know spelling, or at least who have studied it, is infinitely greater than formerly. The changes introduced by the third edition of the academic dictionary do not seem to have been combatted; those which we are proposing would scarcely have met with objections had they been brought to the attention of the revisers of the sixth edition (1835). But the longer we wait the more difficult it will be both to repair the harm done to the language by the bad spelling in vogue to-day, and to overcome the repugnance to any change. . . . It is inconceivable that, out of respect for opinions which are only prejudices, children and foreigners should be forever condemned to commit to memory complicated and contradictory rules whose only result is to pervert the reasoning faculty. Life is too short to waste a part of it in such absurdities. Besides, the transition will be made in a very simple way which will spare the prejudices of the present generation. . . . When we have to write a book, a memorandum, simply a letter, it is quite certain that we shall not stop at every word to ascertain how it ought to be written, according to the new spelling. In this matter changes can not be imposed by law or by decree, like those regulating public accounts. But although the generation that has reached maturity will continue to follow the usage with which it is familiar from childhood, the younger generation and foreigners who have not unalterable habits will learn the new spelling and spare themselves useless labor. Proof-readers, with the help of a printer's dictionary, will conform to it. . . . Thus the change will take place in the course of a generation, without wounding the deep-rooted sentiments of any one.

2. The second objection may be dealt with briefly. It consists in saying

that the way in which words are written evokes an idea of beauty which these same words would lose if they were written otherwise. The people who have this feeling, which is very difficult to analyze, are stylists, caring more for form than for substance, more for words than for ideas; more for the appearance than for the words themselves. Possibly these same people suppose spelling to be immutable in its nature and fixed by law; the exclusive use of recent editions has left them in ignorance of the fact that many of these very words, admirable in their present form, were written differently by the great writers of the sixteenth and seventeenth centuries. It would be interesting to know if these words as they were written in the manuscripts of these authors would be as beautiful or less beautiful.

3. The objection which maintains that the proposed changes will lessen the etymological value of words, has only an appearance of weight. It is the opinion of those people who wish to exhibit their youthful studies. Such an argument never has been and never will be advanced by a philologist. To begin with, let us bear in mind, among other points, that these etymological traces interest only the small number of those who have studied Greek and Latin, and that it is not quite fair to overload spelling with useless letters, merely for the satisfaction of a few students of etymology. In former times this was all very well when only a small minority knew how to read and write. But to-day spelling is intended for every one.

4. The objection which maintains that different words written alike will be mistaken for one another is so childish that I hesitate to make a serious refutation. Certainly there are those who insist that we shall no longer understand each other if we write *poids* (weight) like *pois* (a vegetable). It is always easy to make puns; but the writers, had they spent a moment's reflection on the subject upon which they give their opinions so freely, would have observed that the French language in its present spelling contains a large number of these homonyms, which are essentially different even though similar in pronunciation and spelling. But these homonyms have never been considered a cause for obscurity. Are there human beings so devoid of sense as to confuse the *poids* (weight) with the *pois* (sweet peas), or *petits-pois* (green peas) when the *d* is omitted from the former? Possibly; but theirs is a case for the expert in mental pathology. . . .

To sum up: it will not be disputed that our orthography is an incoherent mixture of spellings belonging to different epochs, often modeled on conflicting systems. Is it actually possible to reform spelling so as to make it absolutely logical? The committee whose conclusions I have reviewed thinks not, believing that so extended a reform would necessitate a complete revision for which the time is not ripe. But we can at least eliminate from our customary spelling the most hideous anomalies, and, in a word, simplify it. To accomplish this all that is necessary in many cases is to reestablish the old forms, unfortunately altered at the time of the Renaissance, notwithstanding objections from many thoughtful men of that period. In adopting this course we are not revolutionizing the language, as our opponents constantly allege, perpetually confusing language with spelling. We do not even propose radical changes in spelling, and are accepting all its conventions, even when these are not entirely satisfactory. We revert to the true history of the language, lost at many points by ill-chosen innovations. Ours is really a work of preservation. I have shown that not a single objection advanced has any weight. One thing is in our way—habit; that we shall overcome.

In the more formal report to which his own incisive essay is prefixed M. Meyer is able to deal with other aspects of the case. He sees that the prospect of successive reforms in spelling will perhaps alarm those accustomed to consider the manner of writing a language as subject to fixed and immutable rules:

But since it is impossible to hinder the progress of an idiom, and since it is as impossible to establish its pronunciation forever as to exclude it from the vocabulary, it must be admitted that spelling is not a permanent and unchangeable institution—that, on the contrary, from one time to another, it must undergo modifications in order to remain in accord with pronunciation. However, even a superficial acquaintance with the history of our language is sufficient to convince one that there is nothing less unalterable than our spelling. Without going back to early times, when writing was subject to no fixed rule, when each one expressed sounds according to his own pronunciation and following exceedingly vague methods—simply taking as the starting point the first edition of the dictionaire of the French Academy (1694), it is noticeable that each new edition of this dictionary has changed the spelling of numerous words. The third edition (1740), of which the Abbé d'Olivet was the editor-in-chief, altered the spelling of about 5,000 words out of 18,000 included in the dictionary. The fourth (1762), the sixth (1835), the seventh (1878), have continued, within narrower limits, it is true, to modify the spelling. But many of these changes so introduced at different periods, most of which merit approval, have the fault of having been proposed without regard for the whole, and without any certain method. In some words silent letters were eliminated, while in certain others they were allowed to remain. At times, even, by a retrogression, the spelling which had been simplified was again complicated.

The French are a logical race and they have not frightened at a theory as easily as the two peoples who speak English. And, therefore, the report of the French commission reveals the fact that they are looking further into the future than any English-speaking committee would dare to do while retaining the hope of ever achieving any practical result. The French commission ventures to hint at more radical reforms than have entered the minds of our own Simplified Spelling Board. Yet these final suggestions of theirs are as significant of the trend of scientific opinion as they are interesting in themselves:

These are the changes we propose, and which we hope will not be deemed excessive. The committee is not at all insensible to the objections which may be advanced against its work. The chief one is that the proposed alterations are not the result of a system of spelling logically devised, all of whose elements are rigorously coordinated. But it was not the business of the committee to create a new system of spelling; they were simply authorized to remove as far as possible the anomalies which complicate our spelling and render the study of it so difficult for children and foreigners. The committee has had, therefore, to use as a basis the present system of spelling, which represents a bygone condition of the language—and to restrict itself to regulating this system. They themselves admit that they have not even succeeded fully in this modest attempt. In the cases where a rational and uniform notation could not be obtained except by creating new conventional spellings, or at the price of too numerous changes, they refrained, leaving the present spelling intact in spite of its defects. But their self-imposed restraint does not bar subsequent changes. They foresee in the future reforms more general than those they endeavored to prepare by partial changes. Many of the members have even expressed the hope which ought to be recorded here, that some day a new committee composed not only of grammarians but also of phonetic experts may be set to work to develop a system of spelling better adapted than ours to the present state of the language, and sufficiently elastic to follow it through its inevitable changes.

But from now on, important advantages will be secured if the moderate propositions of the committee are accepted. At any rate, the teaching of the language will be greatly facilitated; the number of exceptions that the pupils must learn will be noticeably diminished. Our language will be more easily acquired by foreigners. Finally, by the suppression of inconsistent and obscure forms which make the real pronounciation doubtful, it will be made possible to teach in our schools, that greatly neglected subject, orthoepy. This teaching alone is able to prevent errors in pronunciation which, individual at first, finish by becoming general.

THE VALUE OF SCIENCE

BY M. H. POINCARÉ

MEMBER OF THE INSTITUTE OF FRANCE

5. *The Notion of Displacement*

I HAVE shown in 'Science and Hypothesis' the preponderant rôle played by the movements of our body in the genesis of the notion of space. For a being completely immovable there would be neither space nor geometry; in vain would exterior objects be displaced about him, the variations which these displacements would make in his impressions would not be attributed by this being to changes of position, but to simple changes of state; this being would have no means of distinguishing these two sorts of changes, and this distinction, fundamental for us, would have no meaning for him.

The movements that we impress upon our members have as effect the varying of the impressions produced on our senses by external objects; other causes may likewise make them vary; but we are led to distinguish the changes produced by our own motions and we easily discriminate them for two reasons: (1) because they are voluntary; (2) because they are accompanied by muscular sensations.

So we naturally divide the changes that our impressions may undergo into two categories to which perhaps I have given an inappropriate designation: (1) the internal changes, which are voluntary and accompanied by muscular sensations; (2) the external changes, having the opposite characteristics.

We then observe that among the external changes are some which can be corrected, thanks to an internal change which brings everything back to the primitive state; others can not be corrected in this way (it is thus that when an exterior object is displaced, we may then by changing our own position replace ourselves as regards this object in the same relative position as before, so as to reestablish the original aggregate of impressions; if this object was not displaced, but changed its state, that is impossible). Thence comes a new distinction among external changes: those which may be so corrected we call changes of position; and the others, changes of state.

Think, for example, of a sphere with one hemisphere blue and the other red; it first presents to us the blue hemisphere, then it so revolves as to present the red hemisphere. Now think of a spherical vase containing a blue liquid which becomes red in consequence of a chemical reaction. In both cases the sensation of red has replaced that of blue; our senses have experienced the same impressions which have succeeded each other in the same order, and yet these two changes are regarded by us as very different; the first is a displacement, the second a change of state. Why? Because in the first case it is sufficient for me to go around the sphere to place myself opposite the red hemisphere and reestablish the original red sensation.

Still more; if the two hemispheres, in place of being red and blue, had been yellow and green, how should I have interpreted the revolution of the sphere? Before, the red succeeded the blue, now the green succeeds the yellow; and yet I say that the two spheres have undergone the same revolution, that each has turned about its axis; yet I can not say that the green is to yellow as the red is to blue; how then am I led to decide that the two spheres have undergone the *same* displacement? Evidently because, in one case as in the other, I am able to reestablish the original sensation by going around the sphere, by making the same movements, and I know that I have made the same movements because I have felt the same muscular sensations; to know it, I do not need, therefore, to know geometry in advance and to represent to myself the movements of my body in geometric space.

Another example: An object is displaced before my eye; its image was first formed at the center of the retina; then it is formed at the border; the old sensation was carried to me by a nerve fiber ending at the center of the retina; the new sensation is carried to me by *another* nerve fiber starting from the border of the retina; these two sensations are qualitatively different; otherwise, how could I distinguish them?

Why then am I led to decide that these two sensations, qualitatively different, represent the same image, which has been displaced? It is because I *can follow the object with the eye* and by a displacement of the eye, voluntary and accompanied by muscular sensations, bring back the image to the center of the retina and reestablish the primitive sensation.

I suppose that the image of a red object has gone from the center A to the border B of the retina, then that the image of a blue object goes in its turn from the center A to the border B of the retina; I shall decide that these two objects have undergone the *same* displacement. Why? Because in both cases I shall have been able to reestablish the primitive sensation, and that to do it I shall have had to execute the *same* movement of the eye, and I shall know that my eye has executed the same movement because I shall have felt the *same* muscular sensations.

If I could not move my eye, should I have any reason to suppose that the sensation of red at the center of the retina is to the sensation of red at the border of the retina as that of blue at the center is to that of blue at the border? I should only have four sensations qualitatively different, and if I were asked if they are connected by the proportion I have just stated, the question would seem to me ridiculous, just as if I were asked if there is an analogous proportion between an auditory sensation, a tactile sensation and an olfactory sensation.

Let us now consider the internal changes, that is, those which are produced by the voluntary movements of our body and which are accompanied by muscular changes. They give rise to the two following observations, analogous to those we have just made on the subject of external changes.

1. I may suppose that my body has moved from one point to another but that the same *attitude* is retained; all the parts of the body have therefore retained or resumed the same *relative* situation, although their absolute situation in space may have varied. I may suppose that not only has the position of my body changed, but that its attitude is no longer the same, that, for instance, my arms which before were folded are now stretched out.

I should therefore distinguish the simple changes of position without change of attitude, and the changes of attitude. Both would appear to me under form of muscular sensations. How then am I led to distinguish them? It is that the first may serve to correct an external change, and that the others can not, or at least can only give an imperfect correction.

This fact I proceed to explain as I would explain it to some one who already knew geometry, but it need not thence be concluded that it is necessary already to know geometry to make this distinction; before knowing geometry I ascertain the fact (experimentally, so to speak), without being able to explain it. But merely to make the distinction between the two kinds of change, I do not need to *explain* the fact, it suffices me *to ascertain* it.

However that may be, the explanation is easy. Suppose that an exterior object is displaced; if we wish the different parts of our body to resume with regard to this object their initial relative position, it is necessary that these different parts should have resumed likewise their initial relative position with regard to one another. Only the internal changes which satisfy this latter condition will be capable of correcting the external change produced by the displacement of that object. If, therefore, the relative position of my eye with regard to my finger has changed, I shall still be able to replace the eye in its initial relative situation with regard to the object and reestablish thus the primitive visual sensations, but then the relative position of the finger with regard to the object will have changed and the tactile sensations will not be reestablished.

2. We ascertain likewise that the same external change may be corrected by two internal changes corresponding to different muscular sensations. Here again I can ascertain this without knowing geometry: and I have no need of anything else; but I proceed to give the explanation of the fact employing geometrical language. To go from

the position A to the position B I may take several routes. To the first of these routes will correspond a series S of muscular sensations; to a second route will correspond another series S'' of muscular sensations which generally will be completely different, since other muscles will be used.

How am I led to regard these two series S and S'' as corresponding to the same displacement AB? It is because these two series are capable of correcting the same external change. Apart from that, they have nothing in common.

Let us now consider two external changes: α and β, which shall be, for instance, the rotation of a sphere half blue, half red, and that of a sphere half yellow, half green; these two changes have nothing in common, since the one is for us the passing of blue into red and the other the passing of yellow into green. Consider, on the other hand, two series of internal changes S and S''; like the others, they will have nothing in common. And yet I say that α and β correspond to the same displacement, and that S and S'' correspond also to the same displacement. Why? Simply because S can correct β as well as α and because α can be corrected by S'' as well as by S. And then a question suggests itself: If I have ascertained that S corrects α and β and that S'' corrects α, am I certain that S'' likewise corrects β? Experiment alone can teach us whether this law is verified. If it were not verified, at least approximately, there would be no geometry, there would be no space, because we should have no more interest in classifying the internal and external changes as I have just done, and, for instance, in distinguishing changes of state from changes of position.

It is interesting to see what has been the rôle of experience in all this. It has shown me that a certain law is approximately verified. It has not told me *wherefore* space is, and that it satisfies the condition in question. I knew in fact, before all experience, that space satisfied this condition or that it would not be; nor have I any right to say that experience told me that geometry is possible; I very well see that geometry is possible, since it does not imply contradiction; experience only tells me that geometry is useful.

6. *Visual Space*

Although motor impressions have had, as I have just explained, an altogether preponderant influence in the genesis of the notion of space, which never would have taken birth without them, it will not be without interest to examine also the rôle of visual impressions and to investigate how many dimensions 'visual space' has, and for that purpose to apply to these impressions the definition of § 3.

A first difficulty presents itself: consider a red color sensation affecting a certain point of the retina; and on the other hand a blue color sensation affecting the same point of the retina. It is necessary that we have some means of recognizing that these two sensations, qualitatively different, have something in common. Now, according to the considerations expounded in the preceding paragraph, we have been able to recognize this only by the movements of the eye and the observations to which they have given rise. If the eye were immovable, or if we were unconscious of its movements, we should not have been able to recognize that these two sensations, of different quality, had something in common; we should not have been able to disengage from them what gives them a geometric character. The visual sensations, without the muscular sensations, would have nothing geometric, so that it may be said there is no pure visual space.

To do away with this difficulty, consider only sensations of the same nature, red sensations for instance, differing one from another only as regards the point of the retina that they affect. It is clear that I have no reason for making such an arbitrary choice among all the possible visual sensations, for the purpose of uniting in the same class all the sensations of the same color, whatever may be the point of the retina affected. I should never have dreamt of it, had I not before learned, by the means we have just seen, to distinguish changes of state from changes of position, that is, if my eye were immovable. Two sensations of the same color affecting two different parts of the retina would have appeared to me as qualitatively distinct, just as two sensations of different color.

In restricting myself to red sensations, I therefore impose upon myself an artificial limitation and I neglect systematically one whole side of the question; but it is only by this artifice that I am able to analyze visual space without mingling any motor sensation.

Imagine a line traced on the retina and dividing in two its surface; and set apart the red sensations affecting a point of this line, or those differing from them too little to be distinguished from them. The aggregate of these sensations will form a sort of cut that I shall call C, and it is clear that this cut suffices to divide the manifold of possible red sensations, and that if I take two red sensations affecting two points situated on one side and the other of the line, I can not pass from one of these sensations to the other in a continuous way without passing at a certain moment through a sensation belonging to the cut.

If, therefore, the cut has n dimensions, the total manifold of my red sensations, or, if you wish, the whole visual space, will have $n + 1$.

Now, I distinguish the red sensations affecting a point of the cut C. The assemblage of these sensations will form a new cut C'. It is clear that this *will divide* the cut C, always giving to the word divide the same meaning.

If, therefore, the cut C' has n dimensions, the cut C will have $n + 1$ and the whole of visual space $n + 2$.

If all the red sensations affecting the same point of the retina were regarded as identical, the cut C' reducing to a single element would have 0 dimension, and visual space would have 2.

And yet most often it is said that the eye gives us the sense of a third dimension, and enables us in a certain measure to recognize the distance of objects. When we seek to analyze this feeling, we ascertain that it reduces either to the consciousness of the convergence of the eyes, or to that of the effort of accommodation which the ciliary muscle makes to focus the image.

Two red sensations affecting the same point of the retina will therefore be regarded as identical only if they are accompanied by the same sensation of convergence and also by the same sensation of effort of accommodation or at least by sensations of convergence and accommodation so slightly different as to be indistinguishable.

On this account the cut C' is itself a continuum and the cut C has more than one dimension.

But it happens precisely that experience teaches us that when two visual sensations are accompanied by the same sensation of convergence, they are likewise accompanied by the same sensation of accommodation. If then we form a new cut C'' with all those of the sensations of the cut C', which are accompanied by a certain sensation of convergence, in accordance with the preceding law they will all be indistinguishable and may be regarded as identical. Therefore C'' will not be a continuum and will have 0 dimension; and as C'' divides C' it will thence result that C' has one, C two and *the whole visual space three dimensions.*

But would it be the same if experience had taught us the contrary and if a certain sensation of convergence were not always accompanied by the same sensation of accommodation? In this case two sensations affecting the same point of the retina and accompanied by the same sense of convergence, two sensations which consequently would both appertain to the cut C'' could nevertheless be distinguished since they would be accompanied by two different sensations of accommodation. Therefore C'' would be in its turn a continuum and would have one dimension (at least); then C' would have two, C three and *the whole visual space would have four dimensions.*

Will it then be said that it is experience which teaches us that space has three dimensions, since it is in setting out from an experimental law that we have come to attribute three to it? But we have therein performed, so to speak, only an experiment in physiology; and as also it would suffice to fit over the eyes glasses of suitable construction to put an end to the accord between the feelings of convergence and of accommodation, are we to say that putting on spectacles is enough to make space have four dimensions and that the optician who constructed them has given one more dimension to space? Evidently not; all we can say is that experience has taught us that it is convenient to attribute three dimensions to space.

But visual space is only one part of space, and in even the notion of this space there is something artificial, as I have explained at the beginning. The real space is motor space and this it is that we shall examine in the following chapter.

Chapter IV. Space and its Three Dimensions

§ 1. *The Group of Displacements*

Let us sum up briefly the results obtained. We proposed to investigate what was meant in saying that space has three dimensions and we have asked first what is a physical continuum and when it may be said to have n dimensions. If we consider different systems of impressions and compare them with one another, we often recognize that two of these systems of impressions are indistinguishable (which is ordinarily expressed in saying that they are too close to one another, and that our senses are too crude, for us to distinguish them) and we ascertain besides that two of these systems can sometimes be discriminated from one another though indistinguishable from a third system. In that case we say the manifold of these systems of impressions forms a physical continuum C. And each of these systems is called an *element* of the continuum C.

How many dimensions has this continuum? Take first two elements A and B of C, and suppose there exists a series Σ of elements, all belonging to the continuum C, of such a sort that A and B are the two extreme terms of this series and that each term of the series is indistinguishable from the preceding. If such a series Σ can be found, we say that A and B are joined to one another; and if any two elements of C are joined to one another, we say that C is all of one piece.

Now take on the continuum C a certain number of elements in a way altogether arbitrary. The aggregate of these elements will be called a *cut*. Among the various series Σ which join A to B, we shall distinguish those of which an element is indistinguishable from one of the elements of the cut (we shall say that these are they which *cut* the cut) and those of which *all* the elements are distinguishable from all those of the cut. If *all* the series Σ which join A to B cut the cut, we shall say that A and B are *separated* by the cut, and that the cut *divides* C. If we can not find on C two elements which are separated by the cut, we shall say that the cut *does not divide* C.

These definitions laid down, if the continuum C can be divided by cuts which do not themselves form a continuum, this continuum C has only one dimension; in the contrary case it has several. If a cut forming a continuum of 1 dimension suffices to divide C, C will have 2 dimensions; if a cut forming a continuum of 2 dimensions suffices, C will have 3 dimensions, etc. Thanks to these definitions, we can always recognize how many dimensions any physical continuum has. It only remains to find a physical continuum which is, so to speak, equivalent to space, of such a sort that to every point of space corresponds an element of this continuum, and that to points of space very near one another correspond indistinguishable elements. Space will have then as many dimensions as this continuum.

The intermediation of this physical continuum, capable of representation, is indispensable; because we can not represent space to ourselves, and that for a multitude of reasons. Space is a mathematical continuum, it is infinite, and we can represent to ourselves only physical continua and finite objects. The different elements of space, which we call points, are all alike, and, to apply our definition, it is necessary that we know how to distinguish the elements from one another, at least if they are not too close. Finally absolute space is nonsense, and it is necessary for us to begin by referring space to a system of axes invariably bound to our body (which we must always suppose put back in the initial attitude).

Then I have sought to form with our visual sensations a physical continuum equivalent to space; that certainly is easy and this example is particularly appropriate for the discussion of the number of dimensions; this discussion has enabled us to see in what measure it is allowable to say that 'visual space' has three dimensions. Only this solution is incomplete and artificial. I have explained why, and it is not on visual space, but on motor space that it is necessary to bring our efforts to bear. I have then recalled what is the origin of the distinction we make between changes of position and changes of state. Among the changes which occur in our impressions, we distinguish, first the *internal* changes, voluntary and accompanied by muscular sensations, and the *external* changes, having opposite characteristics. We ascertain that it may happen that an external change may be *corrected* by an internal change which reestablishes the primitive sensations. The external changes capable of being corrected by an internal change are called *changes of position,* those not capable of it are called *changes of state.* The internal changes capable of correcting an external change are called *displacements of the whole body;* the others are called *changes of attitude.*

Now let α and β be two external changes, α' and β' two internal changes. Suppose that α may be corrected either by α' or by β', and that α' can correct either α or β; experience tells us then that β' can likewise correct β. In this case we say that α and β correspond to the *same* displacement and also that α' and β' correspond to the *same* displacement. That postulated, we can imagine a physical continuum which we shall call *the continuum or group of displacements* and which we shall define in the following manner. The elements of this continuum shall be the internal changes capable of correcting an external change. Two of these internal changes α' and β' shall be regarded as indistinguishable: (1) if they are so naturally, that is, if they are too close to one another; (2) if α' is capable of correcting the same external change as a third internal change naturally indistinguishable from β'. In this second case, they will be, so to speak, indistinguishable by convention, I mean by agreeing to disregard circumstances which might distinguish them.

Our continuum is now entirely defined, since we know its elements and have fixed under what conditions they may be regarded as indistinguishable. We thus have all that is necessary to apply our definition and determine how many dimensions this continuum has. We shall recognize that it has *six*. The continuum of displacements is, therefore, not equivalent to space, since the number of dimensions is not the same; it is only related to space. Now how do we know that this continuum of displacements has six dimensions? We know it *by experience.*

It would be easy to describe the experiments by which we could arrive at this result. It would be seen that in this continuum cuts can be made which divide it and which are continua; that these cuts themselves can be divided by other cuts of the second order which yet are continua, and that this would stop only after cuts of the sixth order which would no longer be continua. From our definitions that would mean that the group of displacements has six dimensions.

That would be easy, I have said, but that would be rather long; and would it not be a little superficial? This group of displacements, we have seen, is related to space, and space could be deduced from it, but it is not equivalent to space, since it has not the same number of dimensions; and when we shall have shown how the notion of this continuum can be formed and how that of space may be deduced from it, it might always be asked why space of three dimensions is much more familiar to us than this continuum of six dimensions, and consequently doubted whether it was by this detour that the notion of space was formed in the human mind.

§ 2. *Identity of Two Points*

What is a point? How do we know whether two points of space are identical or different? Or, in other words, when I say: The object *A* occupied at the instant α the point which the object *B* occupies at the instant β, what does that mean?

Such is the problem we set ourselves in the preceding chapter, § 4. As I have explained it, it is not a question of comparing the positions of the objects *A* and *B* in absolute space; the question then would manifestly have no meaning. It is a question of comparing the positions of these two objects with regard to axes invariably bound to my body, supposing always this body replaced in the same attitude.

I suppose that between the instants α and β I have moved neither my body nor my eye, as I know from my muscular sense. Nor have I moved either my head, my arm or my hand. I ascertain that at the instant α impressions that I attributed to the object A were transmitted to me, some by one of the fibers of my optic nerve, the others by one of the sensitive tactile nerves of my finger; I ascertain that at the instant β other impressions which I attribute to the object B are transmitted to me, some by this same fiber of the optic nerve, the others by this same tactile nerve.

Here I must pause for an explanation; how am I told that this impression which I attribute to A, and that which I attribute to B, impressions which are qualitatively different, are transmitted to me by the same nerve? Must we suppose, to take for example the visual sensations, that A produces two simultaneous sensations, a sensation purely luminous a and a colored sensation a', that B produces in the same way simultaneously a luminous sensation b and a colored sensation b', that if these different sensations are transmitted to me by the same retinal fiber, a is identical with b, but that in general the colored sensations a' and b' produced by different bodies are different? In that case it would be the identity of the sensation a which accompanies a' with the sensation b which accompanies b', which would tell that all these sensations are transmitted to me by the same fiber.

However it may be with this hypothesis and although I am led to prefer to it others considerably more complicated, it is certain that we are told in some way that there is something in common between these sensations $a + a'$ and $b + b'$, without which we should have no means of recognizing that the object B has taken the place of the object A.

Therefore I do not further insist and I recall the hypothesis I have just made: I suppose that I have ascertained that the impressions which I attribute to B are transmitted to me at the instant β by the same fibers, optic as well as tactile, which, at the instant α, had transmitted to me the impressions that I attributed to A. If it is so, we shall not hesitate to declare that the point occupied by B at the instant β is identical with the point occupied by A at the instant α.

I have just enunciated two conditions for these points being identical; one is relative to sight, the other to touch. Let us consider them separately. The first is necessary, but is not sufficient. The second is at once necessary and sufficient. A person knowing geometry could easily explain this in the following manner: Let 0 be the point of the retina where is formed at the instant α the image of the body A; let M be the point of space occupied at the instant α by this body A; let M' be the point of space occupied at the instant β by the body B. For this body B to form its image in 0, it is not necessary that the points M and M' coincide; since vision acts at a distance, it suffices for the three points 0 M M' to be in a straight line. This condition that the two objects form their image on 0 is therefore necessary, but not sufficient for the points M and M' to coincide. Let now P be the point occupied by my finger and where it remains, since it does not budge. As touch does not act at a distance, if the body A touches my finger at the instant α, it is because M and P coincide; if B touches my finger at the instant β, it is because M' and P coincide. Therefore M and M' coincide. Thus this condition that if A touches my finger at the instant α, B touches it at the instant β, is at once necessary and sufficient for M and M' to coincide.

But we who, as yet, do not know geometry can not reason thus; all that we can do is to ascertain experimentally that the first condition relative to sight may be fulfilled without the second, which is relative to touch, but that the second can not be fulfilled without the first.

Suppose experience had taught us the contrary, as might well be; this hypothesis contains nothing absurd. Suppose, therefore, that we had ascertained experimentally that the condition relative to touch may be fulfilled without that of sight being fulfilled, and that, on the contrary, that of sight can not be fulfilled without that of touch being also. It is clear that if this were so we should conclude that it is touch which may be exercised at a distance, and that sight does not operate at a distance.

But this is not all; up to this time I have supposed that to determine the place of an object, I have made use only of my eye and a single finger; but I could just as well have employed other means, for example, all my other fingers.

I suppose that my first finger receives at the instant α a tactile impression which I attribute to the object A. I make a series of movements, corresponding to a series S of muscular sensations. After these movements, at the instant α, my *second* finger receives a tactile impression that I attribute likewise to A. Afterwards, at the instant β, without my having budged, as my muscular sense tells me, this same second finger transmits to me anew a tactile impression which I attribute this time to the object B; I then make a series of movements, corresponding to a series S' of muscular sensations. I know that this series S' is the inverse of the series S and corresponds to contrary movements. I know this because many previous experiences have shown me that if I made successively the two series of movements corresponding to S and to S', the primitive impressions would be reestablished, in other words, that the two series mutually compensate. That settled, should I expect that at the instant β', when the second series of movements is ended, my *first finger* would feel a tactile impression attributable to the object B?

To answer this question, those already knowing geometry would reason as follows: There are chances that the object A has not budged, between the instants α and α', nor the object B between the instants β and β'; assume this. At the instant α, the object A occupied a certain point M of space. Now at this instant it touched my first finger, and *as touch does not operate at a distance,* my first finger was likewise at the point M. I afterward made the series S of movements and at the end of this series, at the instant α', I ascertained that the object A touched my second finger. I thence conclude that this second finger was then at M, that is, that the movements S had the result of bringing the second finger to the place of the first. At the instant β the object B has come in contact with my second finger: as I have not budged, this second finger has remained at M; therefore the object B has come to M; by hypothesis it does not budge up to the instant β'. But between the instants β and β' I have made the movements S'; as these movements are the inverse of the movements S, they must have for effect bringing the first finger in the place of the second. At the instant β' this first finger will, therefore, be at M; and as the object B, is likewise at M, this object B will touch my first finger. To the question put, the answer should, therefore, be yes.

We who do not yet know geometry can not reason thus; but we ascertain that this anticipation is ordinarily realized; and we can always explain the exceptions by saying that the object A has moved between the instants α and α', or the object B between the instants β and β'.

But could not experience have given a contrary result? Would this contrary result have been absurd in itself? Evidently not. What should we have done then if experience had given this contrary result? Would all geometry thus have become impossible? Not the least in the world. We should have contented ourselves with concluding that *touch can operate at a distance.*

When I say, touch does not operate at a distance, but sight operates at a distance, this assertion has only one meaning, which is as follows: To recognize whether B occupies at the instant β the point occupied by A at the instant α, I can use a multitude of different criteria. In one my eye intervenes, in another my first finger, in another my second finger, etc. Well, it is sufficient for the criterion relative to one of my fingers to be satisfied in order that all the others should be satisfied, but it is not sufficient that the criterion relative to the eye should be. This is the sense of my assertion, I content myself with affirming an experimental fact which is ordinarily verified.

At the end of the preceding chapter we analyzed visual space; we saw that to engender this space it is necessary to bring in the retinal sensations, the sensation of convergence and the sensation of accommodation; that if these last two were not always in accord, visual space would have four dimensions in place of three; we also saw that if we brought in only the retinal sensations, we should obtain 'simple visual space,' of only two dimensions. On the other hand, consider tactile space, limiting ourselves to the sensations of a single finger, that is in sum the assemblage of positions this finger can occupy. This tactile space that we shall analyze in the following section and which consequently I ask permission not to consider further for the moment, this tactile space, I say, has three dimensions. Why has space properly so called as many dimensions as tactile space and more than simple visual space? It is because touch does not operate at a distance, while vision does operate at a distance. These two assertions have the same meaning and we have just seen what this is.

Now I return to a point over which I passed rapidly in order not to interrupt the discussion. How do we know that the impressions made on our retina by A at the instant α and B at the instant β are transmitted by the same retinal fiber, although these impressions are qualitatively different? I have suggested a simple hypothesis, while adding that other hypotheses, decidedly more complex, would seem to me more probably true. Here then are these hypotheses, of which I have already said a word. How do we know that the impressions produced by the red object A at the instant α, and by the blue object B at the instant β, if these two objects have been imaged on the same point of the retina, have something in common? The simple hypothesis above made may be rejected and we may suppose that these two impressions, qualitatively different, are transmitted by two different though contiguous nervous fibers. What means have I then of knowing that these fibers are contiguous? It is probable that we should have none, if the eye were immovable. It is the movements of the eye which have told us that there is the same relation between the sensation of blue at the point A and the sensation of blue at the point B of the retina as between the sensation of red at the point A and the sensation of red at the point B. They have shown us, in fact, that the same movements, corresponding to the same muscular sensations, carry us from the first to the second, or from the third to the fourth. I do not emphasize these considerations, which belong, as one sees, to the question of local signs raised by Lotze.

VESUVIUS DURING THE EARLY MIDDLE AGES

By Dr. Charles R. Eastman
HARVARD UNIVERSITY

IT is certain that Vesuvius, prior to the Plinian eruption of 79 A. D., by far the most tragic, and one of the three most violent in Italian history, was regarded as an entirely extinct volcano. The details of this eruption, the sequence of its phenomena, and its peculiarly destructive effects, are familiar to us from contemporary sources, and from the memorials written in large characters by the mountain itself over the ruined cities at its base. From the date of this catastrophe onward for over fifteen hundred years, when the period of modern investigation begins, our knowledge of Vesuvian history depends upon more or less casual mention, and upon brief notices of eruptions in monastic chronicles.

Owing to the scantiness of our information, little attention has been paid by students to the long interval separating the two most violent paroxysms known to have shaken the mountain. Yet, inadequate as the records are, their importance is of the first order. They register for us the dates of major disturbances, at least, extending over a period of sixteen centuries, and afford some means for estimating the intensity of volcanic action in the Naples district for a still longer period. Moreover, they furnish data for reconstructing the probable form of the mountain in antiquity, and for detecting the amount of change it has undergone since Plinian times. Nor should it be forgotten that the early topographic descriptions that have come down to us offer interesting points of comparison with the present condition of the stately guardian of the Bay of Naples.

Thus it appears that the original sources of information, which are all that need concern us in the present article, acquaint us not only with the actual history of Vesuvius since the first century of our era, but, taken in connection with other facts, throw a fresh coloring upon the accounts of the 'burnt mountain' that have survived from classic times. Two of the points just enumerated will repay further inquiry: first, the chronology of eruptions during the early middle ages; and secondly, the probable form of Somma-Vesuvius in antiquity. One reason why a review of the chronology seems desirable is because the dates of medieval eruptions are often confused, and the authorities for them incorrectly given, or more frequently omitted. It will be profitable, therefore, to take a brief survey of the original sources, but without entering into the voluminous literature of the Plinian disaster, which belongs in a separate category. Note, however, that some of the titles included under it are luminous for an understanding of early Somma-Vesuvius history, and the same may be said of Cassius Dio's account of the second eruption (203 or 204), which also falls outside the period of our inquiry.

The whole history of Italy under the Goths is contained in Cassiodorus and Procopius, although the dry compendium of Marcellinus Comes is not without value for the chronology of certain facts. These three writers are our only informants[1] of the eruption that fell at the beginning of the dark ages, in 472, and a word or two concerning them may not be inapt. Magnus Aurelius Cassiodorus, a great Roman noble of wealth, learning and astute statesmanship, born in 480 and reputed to have lived nearly a century, occupies a position throughout the reign of Theodoric scarcely less prominent than that of the king himself, whose chief counsellor he was. His writings, especially the 'Variæ,' or collection of state papers, are of incalculable value for Italian history under Gothic rule, and contain a wealth of curious detail concerning political, social and moral conditions, and general life of the period. In these official papers, the secretary frequently intersperses comments, from an obviously personal point of view, upon any subject that interests him, often displaying remarkable erudition. One of his marked tendencies is a passion for natural history, which he touches upon with naïve ardor, yet displaying withal acute observation. Many a random note occurs relating to birds, beasts or fishes, as witness for example his excursus on the elephant, faintly suggestive of Ctesias, or his description of the 'exormiston,' identified by Dr. Theodore Gill with a Leptocephalus. Little wonder is it, therefore, that we find in these 'Variæ' (iv., 50) an interesting digression on Vesuvius, apropos of an eruption commonly assigned to the year 512, but which, according to Mommsen,[2] must have taken place from one to five years earlier. The date of this event is accordingly best written 507/511. Allusion is also made to the far more severe eruption of 472, remarkable for its heavy discharge of ashes, carried to an enormous distance. For years afterward at Constantinople a solemn fast was held on the sixth of November in memory of that day when the heavens were darkened, and the greater part of Asia Minor was rocking with frightful earthquake shocks. In another letter (iii., 47) he refers to an eruption of one of

[1] Pious imagination of later days has added much fanciful embroidery to the accounts of this and other early eruptions, coupled with the miraculous intervention of Naples' patron saint. The curious will find entertaining reading in the various lives of St. Januarius, as, for instance, that by Girolamo, 1733.

[2] 'Monumenta Germaniæ Historica, Auct. Antiq.,' Vol. XII. (1894), p. 137. We shall refer hereinafter to the folio volumes of the 'Scriptores' series under the abbreviation *M. G. H., SS.*

the Lipari Islands, the date of which is said to have coincided with Hannibal's death, 183 B. C.

So far Cassiodorus. In Procopius (fl. 495–565) we are confronted with a very different sort of personage, yet one recognized as chief authority for the events of the reign of Justinian. His position in literature is defined by Hodgkin, in his 'Italy and her Invaders,' in following terms:

> After so many generations of decline, here, at length, the intellect of Hellas produces a historian, who, though not equal doubtless to her greatest names, would certainly have been greeted by Herodotus and Thucydides as a true brother of their craft. Procopius has a very clear idea of how history ought to be written. Each of his books, on the Persian, the Vandal, and the Gothic wars, is a work of art, symmetrical, well proportioned, and with a distinct unity of subject. His style is dignified but not pompous, his narrative vivid, his language pure. . . . He exhibits a considerable amount of learning, but without pedantry: and resembles Herodotus in his eager, almost child-like interest in the strange customs and uncouth religions of barbarian nations.

Such appears to be a conservative estimate of Procopius the Cæsarean. He has transmitted to us a vivid pen picture of Vesuvius as observed by him during a four months' sojourn at Naples in 537, at which time an eruption was threatened, though none actually occurred until nearly a century and a half thereafter. Thus it happens that the value of Procopius lies in his excellent topographic description (*de Bello Gothico,* ii., 4), together with hearsay accounts of the two preceding disturbances (*ibid.,* iii., 35). One of the features of the last which we gather from him is that ashes were carried as far as Tripoli. Lava flows are distinctly mentioned both by him and by Cassiodorus as an accompaniment of these eruptions, a fact often overlooked by modern geologists. The Byzantine historian will have further claim to our attention later on.

Following close upon the fall of the Gothic kingdom came the Lombard invasion, which marks the most ill-starred period of Italian history. But little direct and contemporary testimony to historical facts has come down to us from the Lombards, but as their rule approached its end, a native historian arose who preserves the memory of foreign mastery, and ranks as the most distinguished writer of this early part of the middle ages in Italy. This historian is Paul the Deacon (720–*c.* 787), to whom we are chiefly indebted for a history of the Lombards and a revision of Eutropius. Both of these writings contain mention of Vesuvian eruptions, and it is interesting to note that we find in them the earliest suggestion that the original Somma crater had been shattered by the Plinian catastrophe.[3] Paulus Diaconus, in his 'Historia Langobardorum,' and the Roman 'Liber Pontificalis,' a compilation due to many hands and extending over a number of cen-

[3] 'Hist. Lang.,' edited by Muratori, *R. I. S.,* Vol. V., p. 59.

turies, are our only sources relating to the ash-eruption of 685, which continued 'mense martio per dies aliquot.' This is reckoned as the fifth in the list of recorded disturbances.

There now ensues a period of dire poverty in Italian chronicles, and we hear nothing further of Vesuvius for nearly three hundred years. Then are found meager and incidental notices of two eruptions, during the earlier of which lava flows reached the sea, a symptom of high intensity, only twice repeated in modern times. The unique authority for these events is Petrus Damianus, a prolific and most singular polemical writer of the early eleventh century, monk and cardinal, whom Balzani describes as having 'treated in prose and verse every possible subject, whether in literature, homilies, lives of saints, political or religious treatises.[4] Some confusion exists regarding the dates of these eruptions. This arises from the fact that the years in which they occurred are unmentioned, although names are given instead of petty princes with whose deaths they synchronized, the coincidence being interpreted in a manner usual to the times. Later on we find an extract of Peter's account appearing in a postscript to Leo of Marsi's 'Chronicon' under date of 1049, where, unfortunately, the name of the reigning duke of Naples, John III., is omitted, thus leaving it uncertain which one of the numerous family of Capuan princes was intended. Recently the tangle has been unraveled by the Neapolitan historian Capasso,[5] who is no doubt correct in assigning the events in question to the years 968 (in lieu of 982) and autumn of 999, respectively. By a fortunate chance, the original draft of Leo of Marsi's chronicle is still preserved at Munich, and, with its erasures and numerous additions, clearly shows what use was made by the author of his materials in preparing finished copy.

For a brief mention of the eighth recorded eruption we are indebted to a wandering monk, Rodulphus Glaber, of whom little is known except that he lived at various monasteries, including those of Bèze and Cluny, after having traveled extensively in Italy. His history, published 1047, is not without value for contemporary events, and is regarded as reliable in the main, hence no reason appears for doubting his account of a violent eruption in 1007. By some authors the passage has been understood to read seven years *before* instead of after the millenium, hence the earlier date is often incorrectly given in catalogues of eruptions. Mabillon recalls that the year 1006 was

[4] Ugo Balzani, 'Le cronache italiane nel medio evo,' 2d ed. (Milan, 1900). The complete works of Petrus Damianus are edited by Migne, 'Patrolog. lat.,' Vols. CXLIV., CXLV. (Paris, 1853). Cf. Opusculis xix., c. 9 et 10.

[5] B. Capasso, 'Monumenta Neapolitani Ducatus,' Vol. I., p. 114. For an account of the Pandulf line of princes, see the article by M. Schipa in *Archiv. Storico Prov. Napoletane*, ann. XII. (1887), p. 254, and compare the genealogical table given in Pflugk-Harttung's 'Iter Italicum,' p. 711.

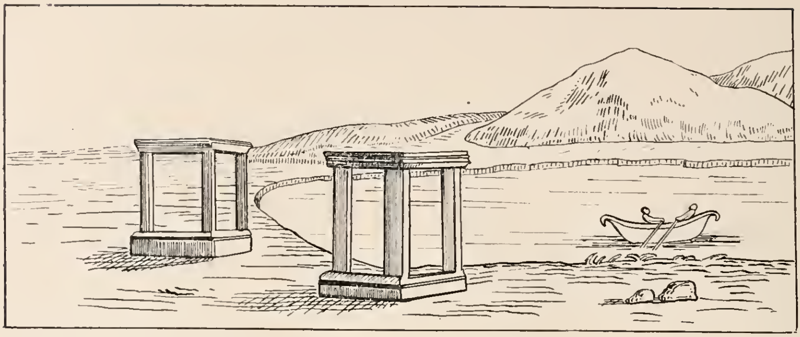


Fig. 1. Vesuvius as represented in Roman Times. From facsimile engraving of a Pompeian fresco in 'Pitture d' Ercolano e dintorni.'

memorable for a number of calamitous happenings, and suggests that the injury wrought by Vesuvius of which Rodulphus informs us may have fallen properly about that time. Bombs were projected on that occasion to a distance of three miles, issuing from a greater number of orifices than usual, and the noxious gases accompanying the eruption rendered the country round about uninhabitable.[6]

An interval of thirty years ensued before the ninth eruption (1037), which was succeeded by a little over a century's repose. After 1139 no further disturbance is known to have taken place until early in the sixteenth century, although Mount Epomeo, in the neighboring island of Ischia, was active in the year 1302. Brief records of the ninth and tenth eruptions are found in monastic chronicles, compiled in near-by abbeys, and of noteworthy importance. Foremost should be mentioned the monastic histories written during the eleventh century at Monte Cassino, a famous abbey of ancient foundation and mother of all Benedictine monasteries, which shone like a light in the dark ages. Of

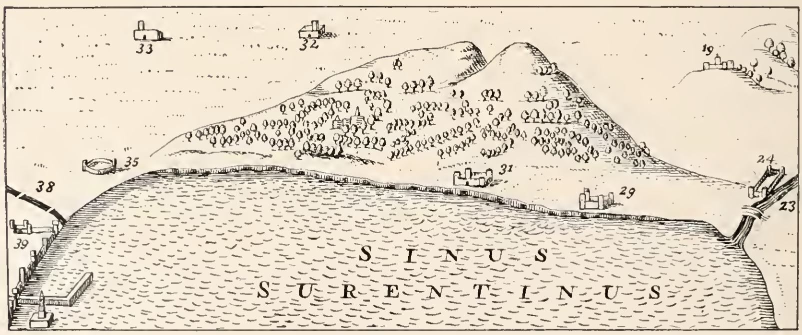


Fig. 2. Vesuvius about the Year 1500. From the earliest known engraving of the mountain and surroundings, printed 1514. 19, Palma; 23, Sarnus fl.; 24, Pompeii; 29, Turre Nunciatæ; 31, Herculaneum; 32, Marilianum'; 33, Pomilianum'; 35, Palæpolis; 38, Sebethus fl.; 39, Neapolis.

[6] *M. G. H.*, *SS.*, Vol. VII., p. 61.

interest for our present purpose are the 'Annales Casinenses' (1000–1212),[7] a compilation by various monks whose names are unknown; the 'Chronicon Casinensis'[8] of Leo of Marsi, better known, after becoming

Fig. 3 Attempted Restoration of Vesuvius prior to the Eruption of 1631. From Mecatti, after an early print. *A*, Barra; *B*, Massa di Somma; *C*, Maria dell' Arco; *D*, S. Sebastino; *E*, S. Giovanni a Teduccio; *F*, S. Maria del Soccorso; *G*, Pietra Bianca; *H*, Portici; *I*, Resina; *K*, Torre del Greco; *L*, S. Maria a Pugliano; *M*, Torre dell' Annunziata; *N*, Camaldoli della Torre; *O*, Torre Scassata; *P*, Boscotrecase; *Q*, Sarno.

[7] *M. G. H., SS.*, Vol. XIX., pp. 306, 309.
[8] *Ibid.*, Vol. VII., pp. 670, 684.

Bishop of Ostia, as Leo Ostiensis, one of the most sober and important of Italian historians; the 'Annales Cavenses' (569–1318),[9] produced by another famous monastery near Salerno; and finally the 'Chronicon' (1102–1140)[10] of Falco of Benevento, notary, judge and papal chancellor, to whom posterity is indebted for precious information. These contemporary sources contain all that is known of the ninth and tenth Vesuvian eruptions. Details are wanting, but it is said of the former that it happened in January, 1037, and lava flows reached the sea; the duration of the latter (1139) is stated in one account to have been eight, in another, forty days. Critical estimates of the documents above referred to will be found in various works dealing with the sources of medieval history, amongst which it will be sufficient to mention an article by Hirsch on 'Desiderius of Monte Cassino.'[11] Our review of the chronology of eruptions in the early middle ages is now completed.

There remains to be considered a question that has often been asked, and variously answered: was the form of Somma-Vesuvius essentially the same in antiquity as we know it to-day, or were the ancients acquainted with only a single crateriform summit whose broken wall now partially encircles the newer cone? The only reason for raising the inquiry at all is that neither by direct statement nor by implication do any of the ancient authors allude to Vesuvius as a double-peaked mountain, and the older topographic descriptions can with difficulty be reconciled with the present form of the volcano. It appears indeed passing strange that Strabo, Pliny, Cassius Dio and Procopius should all have remained silent respecting the most salient feature of Vesuvius as viewed from the west, in case its twin peaks presented to their eyes, as they do to ours, almost identical outlines. Yet, accepting their accounts at face value, the only conclusion possible is that the younger cone has been entirely built up during the middle ages, a far shorter interval than is demanded by geologic evidence. A time allowance of barely a thousand years (or at the most fifteen hundred, if we leave Procopius out of the reckoning and admit the correctness of Leone di Ambrogio's figure of a double summit in 1514) for the formation of the central cone is absurdly inadequate, the number of eruptions contributing towards it too few, and their intensity too slight, to have performed the work. This we know from the present slow rate of accumulation, and from the relatively unimportant changes wrought by even paroxysmal eruptions. And it may well be doubted whether the convulsion of 79 A. D. was of more violent character than those of 1631 and 1906, these three exceeding all others in intensity.

---

[9] *Ibid.*, Vol. III., p. 189.

[10] Muratori, *R. I. S.*, Vol. V., p. 128.

[11] *Forsch. deutsch. Gesch.*, Vol. VII., 1867, pp. 1–112.

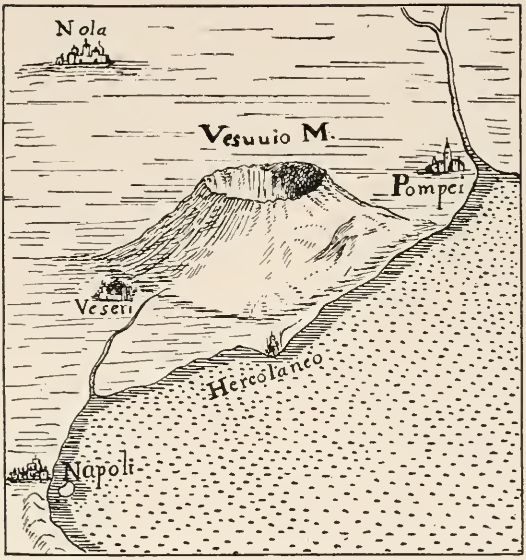


FIG. 4. ATTEMPTED RESTORATION OF THE PARTHENOPIAN VOLCANO AS IT APPEARED IN STRABO'S TIME. After Pellegrino, 1651. (The site of Veseri is conjectural.)

The problem of the geologist is to determine the past condition of things from what he is able to find out from the present. Nevertheless, the tendency of popular opinion has been to subordinate geologic to documentary evidence, and the majority of standard works continue to uphold the view that Vesuvius proper was non-existent at the time Herculaneum and Pompeii were overwhelmed. As positive a statement of this view as any is the following, from Professor Phillips' excellent work on 'Vesuvius':

Somma, the broken crest of a greater and earlier volcanic crater, has been unmoved in place, unchanged in form and height, through eighteen centuries; a grand and awful fragment left after the poetic 'struggle of earth and sky,' and full of peculiar records of the combat. Vesuvius, born of Somma, and seated within the encircling grasp of its parent, is a variable heap thrown up from time to time, and again, not seldom, by a greater effort of the same force, tossed away into air. . . . Thus two classes of forms arise in the history of Vesuvius: one may be called the old or Somma form, left after violent and exhaustive efforts of the volcano; the other the new form, in which Vesuvius takes a place unrecorded in ancient history (p. 174).

Equally confident is the tone assumed by Professor Judd, in his volume on 'Volcanoes,' in the International Science Series:

Nothing is more certain than the fact that the Vesuvius upon which the ancient Romans and the Greek settlers of southern Italy looked, was a mountain differing entirely in its form and appearance from that with which we are familiar. The Vesuvius known to the ancients was a great truncated cone, having a diameter at its base of eight or nine miles, and a height of about

4,000 feet. The summit of this mountain was formed by a circular depressed plain, nearly three miles in diameter, within which the gladiator Spartacus, with his followers, were besieged by a Roman army (p. 83).

The above description is reinforced by a figure of a truncate colossus, supposed to represent Vesuvius in the time of Strabo, a graphic portrayal that has been popular ever since the first attempt in this direction was essayed by Camillo Percgrino,[12] in 1651. Strange as it may seem, some writers have been misled into supposing that such was the actual form presented by the mountain in the middle of the seventeenth century. As a matter of fact, all these fanciful restorations of the Somma form of crater, however cleverly they may interpret geological evidence, and to that extent suggest remote prehistoric conditions, can not be considered as having any real historical foundation. For we have no right to interpret literary documents in a manner wholly discordant with what is known of the structure and behavior of the mountain itself, but rather should first seek to establish their credibility by scrutinizing them in the light of ascertained facts. If it has been easy to misconstrue Braccini's account of the crater in the first quarter of the seventeenth century, should we not be wary of accepting the usual rendering of ancient authors? And who is so bold as to claim that the huge truncate cone of which Strabo is commonly understood to speak finds any visible support in Pompeiian wall frescoes, of which several representing Vesuvius in a more or less idealized fashion have been discovered? Impressionistic as all of these paintings are, it is not difficult to perceive that the local scene which caught the artist's fancy was after all not very different from that which still meets our gaze from within or hard by the disinterred city.[13]

We may affirm, then, this conclusion: there is no good reason to suppose that Vesuvius appeared materially different in the yesterday of one or two thousand years ago than it does to-day. The summit of the younger cone, still partially encircled by the ancient Somma rim, has been undergoing comparatively slight modification throughout probably the whole course of human history. And we must perforce believe it to have been existent even before the race of man had appeared on the face of the earth, and had begun to acquire dominion over it.

---

[12] 'Discorsi della Campania Felice,' p. 309. (Naples, 1651.)

[13] For a recent and interesting discussion of this whole matter, and also of the events of the Plinian eruption, one may consult the following: Enrico Cocchia, 'La forma del Vesuvio,' an essay reprinted in Volume III. of his 'Saggi Filologici' (Naples, 1902); and S. Herrlich, 'Die antike Ueberlieferung über den Vesuv-Ausbruch im Jahre 79.' *Beitr. zur alten Gesch.*, Vol. IV. (1904), pp. 209–226.

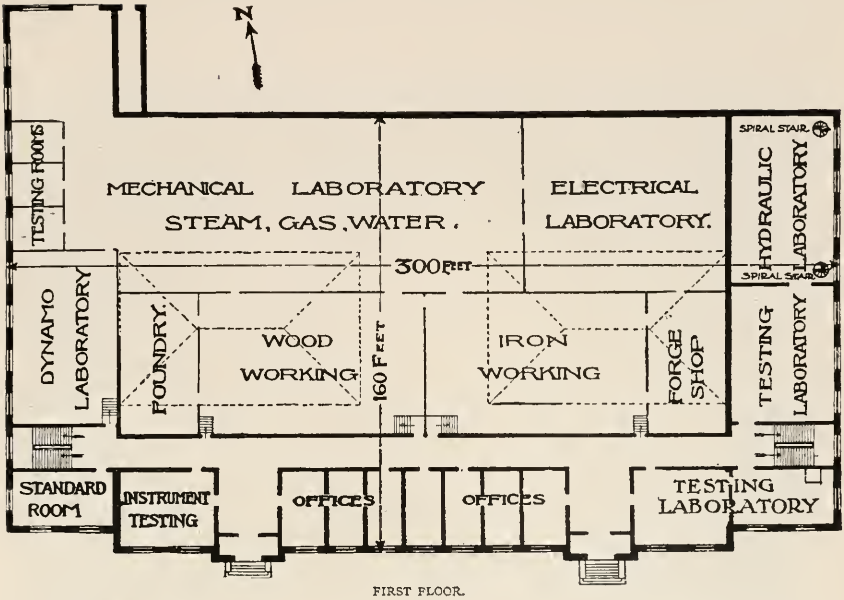
THE PROGRESS OF SCIENCE.

THE NEW ENGINEERING BUILDING OF THE UNIVERSITY OF PENNSYLVANIA

The new engineering building of the University of Pennsylvania was dedicated on October 19, in the presence of delegates from over one hundred scientific institutions and societies and representatives of six leading foreign nations. The building was open for inspection in the morning, and after luncheon had been served in the building the formal ceremonies took place. Provost Harrison accepted the building on behalf of the trustees, thanking especially Professors Spangler and Marburg, the heads of the departments of mechanical, electrical and civil engineering that occupy the building, the architects, Messrs. Cope and Stewardson, the workmen and the numerous donors who had made the building possible. The degree of doctor of science was conferred on a number of eminent engineers, and the principal addresses were made by Mr. Frederick W. Taylor, the president of the American Society of Mechanical Engineers, and Dr. Alexander C. Humphreys, president of the Stevens Institute of Technology.

The building, a view of which and a general plan of the first floor are shown in the accompanying illustrations, is the largest of the seventy buildings now occupied by the University of Pennsylvania, having a frontage of 300 feet and a depth of 210 feet. The cost, including equipment, was almost one million dollars. It is of fire-proof construction, and the equipment is of the most modern and approved type. The exterior is of dark brick, with limestone trimmings, and the general architectural treatment is in the English-Georgian school, in accord with the later university halls. There are three stories, the total floor area being 128,000 square feet. The heating is by direct steam; the ventilation by electrically-driven fans, and the lighting by electricity. The steam for the engines is supplied from the central station of the university, and, after being used by the engines, is sent into the heating system of the building. There are two principal entrances leading to the main hallway, which extends east and west the entire length of the building to staircases at both extremities. The basement contains locker rooms, lavatories, machinery for heating and ventilating, storage battery rooms, laboratories for geodetic and hydraulic work, and for the testing of the materials of construction. On the first floor, adjacent to the main entrance, are the offices of the heads of departments, the eastern part of the building being devoted entirely to the civil engineering department, and the western part to the mechanical engineering department. Accommodation is also provided for physical and hydraulic testing, instrument testing and for special work in mechanical and electrical engineering. Rooms are likewise set aside for dynamos and electric motors, steam and gas engines, refrigerating apparatus, hydraulic motors, boiler testing, pattern-making, wood and iron working, foundry and machine shops, etc. On the second floor is a reference library and reading room, a students' assembly room, rooms for the use of instructors and for lectures and recitations. The rear portion of this floor is devoted almost wholly to drawing rooms. A room for the use of the engineering societies, a general supply store, and the library stack oc-

The Engineering Building of the University of Pennsylvania



GROUND PLAN OF THE ENGINEERING BUILDING, UNIVERSITY OF PENNSYLVANIA

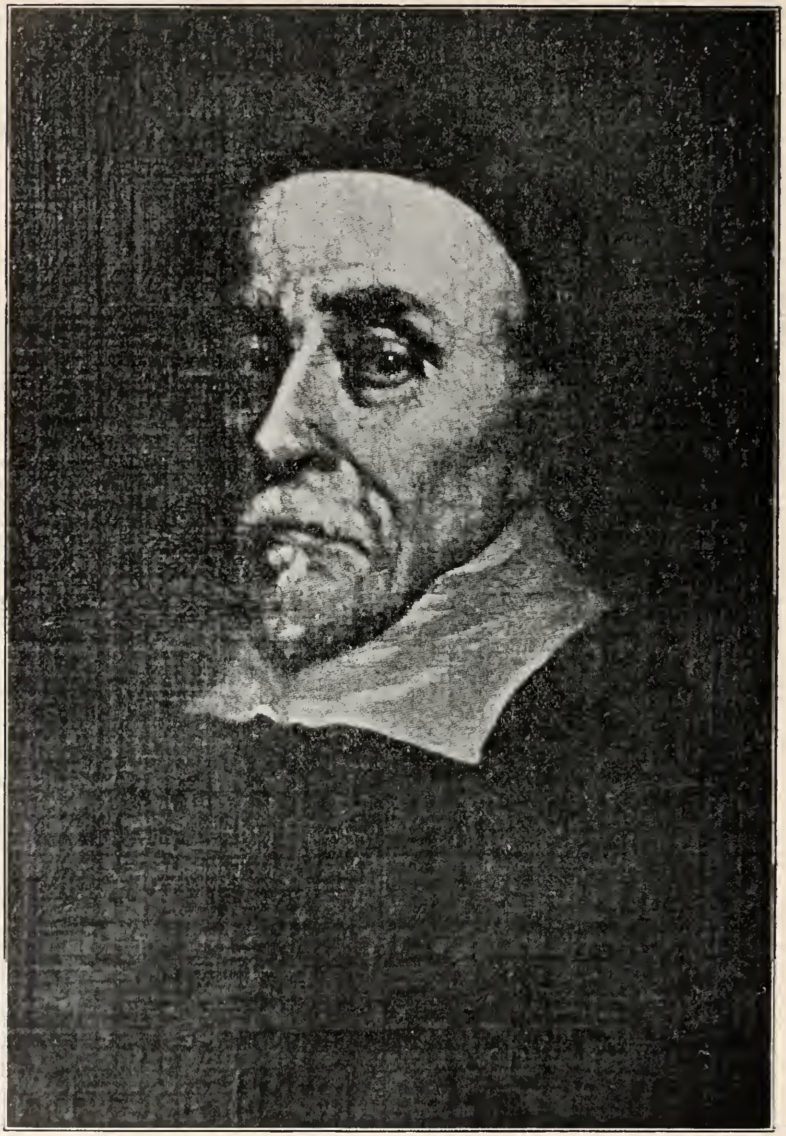
cupy the middle portion. In the east and west wings ample space is assigned to the engineering museums, while the rear of this floor is set aside exclusively for additional drawing rooms, which, like those just beneath, will have the full advantage of a north light.

The engineering department of the university was established in 1874, but the constant increase of numbers in the classes of the departments has necessitated their moving into more spacious quarters three times since their founding. The departments this year have a total enrollment of nearly six hundred students, and a teaching force of forty.

As Mr. Taylor said at the close of his address: "Philadelphia is the center of the largest and most diversified group of engineering and manufacturing enterprises in this country. The engineering schools of the University of Pennsylvania already stand high; but it seems to me that the opportunity lies open to them even more than to their famous medical and law schools to stand at the very top. This magnificent building, equipped as it is with the latest and best of everything, is the first and a great step towards this end. But after all, your largest possibility and one which does not exist for, and can not be created by, any other American university, lies in the opportunity for bringing your students into close touch and personal contact with the men who are working in and managing the great industrial establishments of Philadelphia."

THE HARVEIAN ORATION

THE Harveian oration, delivered annually before the Royal College of Physicians, London, was given on October 18 by Dr. William Osler, regius professor of medicine in the University of Oxford, and formerly professor of medicine in the Johns Hopkins University, who chose as his subject 'The Growth of Truth: as illustrated in the discovery of the circulation of the blood.' With his felicity of expression and wealth of knowledge of the history of medicine and science, Dr. Osler reviewed again the instructive story of the discovery. Though rehearsed now for two hundred and fifty years by Harveian orators, the story loses in Dr. Osler's



WILLIAM HARVEY.

From a painting by De Reyn, in the rooms of the Royal Society; reproduced in Sir William Huggins's 'The Royal Society.'

hands none of its interest and is well worth the repetition.

By the terms of Harvey's bequest, founding the lectureship, the orator is among other things enjoined to exhort the fellows of the college 'to search out and study the secrets of nature by way of experiment.' The discovery of the circulation of the blood is itself a conspicuous illustration of the fruitful application of the experimental method to a fundamental physiological problem, as well as a type of the evolution of scientific truth through the various stages, from the accumulation of facts and observations to a universally accepted explanation. The discovery of the general circulation is one of the important landmarks in the history of science. It evidences, on the one hand, in a peculiarly striking manner, the overthrow of authority and the birth of the modern scientific spirit. On the other hand, in the discovery and more particularly in the methods by which it was attained are laid the foundations of experimental physiology and medicine, which have contributed so incalculably to human welfare.

The sixteenth century had witnessed notable contributions to the knowledge of the structure of the human body. The work of Vesalius, Fallopius and Fabricius had brought about numerous corrections in the anatomy of Galen, which had been taught in the schools with but little change for fourteen centuries. The Galenical physiology remained, however, in complete authority on all matters pertaining to the functions of the body. It taught that the liver is the source of the blood, one kind of which flowed to the right ventricle and thence by way of the veins to the lungs and the general system, the other by way of the arteries from the left ventricle to the lungs and general system. It was supposed that the blood flowed directly from the right to the left ventricle through the pervious septum of the heart. The muscular function of the heart in propelling the blood was unknown. Such were the prevailing conceptions in Harvey's time. While the lesser circulation had been fully described, its function was supposed to be the nutrition of the lungs. It may be doubted whether any one had even glimmerings of the greater or systemic circulation. The demonstration of this was Harvey's great work, all the more notable since the estimation of the significance of a scientific discovery is determined by the state of knowledge at the time when it is made.

After studying medicine at Padua under Fabricius, the foremost anatomist of the day, Harvey returned to London as a general practitioner, and in the course of time was appointed Lumleian lecturer to the Royal College of Physicians. It was before the president, censors and fellows of the college and an unusually large company of laymen that, on April 17, 1616, he demonstrated the structure and action of the heart and propounded his new doctrine of the general circulation. The formal announcement of the discovery in the *De Motu Cordis et Sanguinis* was, however, deferred for twelve years, furnishing an interesting instance of delay in the publication of a great discovery through fear of the prejudices of men. The motive which actuated Harvey is not far to seek. So complete was the dominance of the authority of Hippocrates, Galen and Columbus that few would have had the temerity to question doctrines which had the sanction of the ancients and were sealed by general acceptance.

The new theory was as usual given a tardy recognition, and in some parts, notably in France, was met with open hostility. Owing, however, to the care and accuracy in observation and experimentation, to the fullness of the array of evidence adduced, and to the brilliancy of the demonstration, its truth could not long be denied. He himself lived to see his discovery all but universally accepted, and Hobbes's oft-quoted statement is well-nigh literally true that Harvey, 'conquering envy, hath established a new doctrine in his lifetime.' Complete and universal acceptance was, however, not obtained until late in the century, even after Malpighi had in 1661 by means of the newly invented microscope completed the demonstration of the circulation by tracing the course of the blood in the capillaries. In contrast with the tenacity with which traditional conceptions have been adhered to in the past, as illustrated in this discovery and its reception, and the whole history of science is strewn with more bitter struggles for acceptance, it is gratifying to note the receptivity of the public mind to new theories in our day and the tolerance and readiness with which they are accorded a hearing.

The significance of Harvey's work lay not merely in the discovery of the circulation of the blood and the profound changes which it wrought in the conceptions of the functions of the body and in medical thought and practise, great though these were. It lay more perhaps in its contribution to the development of scientific method. In the words of the Harveian orator, "here for the first time a great physiological problem was approached from the experimental side by a man with a modern scientific mind who could weigh evidence and not go beyond it, and who had the sense to let the conclusions emerge naturally but firmly from the observations. To the age of the hearer, in which men had heard, and heard only, had succeeded the age of the eye, in which men had seen, and had been content only to see. But at last came the age of the hand—the thinking, devising, planning hand; the hand as an instrument of mind now reintroduced into the world in a modest little monograph of seventy-two pages, from which we may date the beginning of experimental medicine."

SCIENTIFIC ITEMS

Sir David Gill, astronomer royal in South Africa, has retired.—Dr. Wilhelm Waldeyer, professor of anatomy at Berlin, and secretary of the Berlin Academy of Sciences, has celebrated his seventieth birthday.—The sum of about $5,000 has been subscribed for the foundation for the advancement of geographical study in honor of Dr. von Neumayer.—Advantage will be taken of the twentieth anniversary of the isolation of fluorine to present a gold medal to M. Moissan.—In acknowledgment of his work in metallurgical research, Professor Henry M. Howe, of Columbia University, has received from the Russian emperor the order of the Knighthood of St. Stanislas.

Dr. Sven Hedin, who by order of the government was denied access to Tibet from the side of India, is making good his entry into western Tibet from Chinese Turkestan.—Mr. Walter Wellman and Major Hersey have returned to this country. The former will go to Paris in six weeks to continue his supervision of the changes in his airship. Major Hersey will accompany the Chicago *Record-Herald* expedition in its attempt to reach the Pole next summer.—Captain Roald Amundsen sailed on November 8, on the Scandinavian-American steamer *Hellig Olaf*, for Christiania, where the records of his magnetic observations in the Arctic will be worked out. Captain Amundsen has presented his entire collection to the Norwegian government. The new king of Norway has conferred upon him the highest decoration of the kingdom, the grand cross and cordon of St. Olaf.

A scientific session of the National Academy of Sciences was held at the Harvard Medical School, Boston, beginning on Tuesday, November 20.—The American Association for the Advancement of Science and the twenty or more national scientific societies affiliated with it will meet in New York City during convocation week, beginning on December 27. It is expected that this will be the largest and most important meeting of scientific men ever held in America.

INDEX

NAMES OF CONTRIBUTORS ARE PRINTED IN SMALL CAPITALS

Vol. LXIX. No. 1 JULY, 1906

THE POPULAR SCIENCE MONTHLY.

EDITED BY J. McKEEN CATTELL

CONTENTS

THE SCIENCE PRESS

LANCASTER, PA. GARRISON, N. Y.

NEW YORK: SUB-STATION 84

SINGLE NUMBER, 30 CENTS YEARLY SUBSCRIPTION, $3.00

The Popular Science Monthly

Entered in the Post Office in Lancaster, Pa., as second-class matter.

CONTENTS OF MAY NUMBER

CONTENTS OF JUNE NUMBER

OF THE NEW ERA PRESS PRINTING COMPANY

VOL. LXIX. No. 2 AUGUST, 1906

THE POPULAR SCIENCE MONTHLY.

EDITED BY J. McKEEN CATTELL

CONTENTS

THE SCIENCE PRESS

LANCASTER, PA. GARRISON, N. Y.

NEW YORK: SUB-STATION 84

SINGLE NUMBER, 30 CENTS YEARLY SUBSCRIPTION, $3.00

The Popular Science Monthly

Entered in the Post Office in Lancaster, Pa., as second-class matter.

CONTENTS OF JUNE NUMBER

CONTENTS OF JULY NUMBER

OF THE NEW ERA PRESS PRINTING COMPANY

VOL. LXIX. No. 3 SEPTEMBER, 1906

THE POPULAR SCIENCE MONTHLY.

EDITED BY J. McKEEN CATTELL

CONTENTS

THE SCIENCE PRESS

LANCASTER, PA. GARRISON, N. Y.

NEW YORK: SUB-STATION 84

SINGLE NUMBER, 30 CENTS YEARLY SUBSCRIPTION, $3.00

The Popular Science Monthly

Entered in the Post Office in Lancaster, Pa., as second-class matter.

CONTENTS OF JULY NUMBER

CONTENTS OF AUGUST NUMBER

OF THE NEW ERA PRESS PRINTING COMPANY

Vol. LXIX. No. 4 OCTOBER, 1906

THE POPULAR SCIENCE MONTHLY.

EDITED BY J. McKEEN CATTELL

CONTENTS

THE SCIENCE PRESS

LANCASTER, PA. GARRISON, N. Y.

NEW YORK: Sub-Station 84

Single Number, 30 Cents Yearly Subscription, $3.00

The Popular Science Monthly

Entered in the Post Office in Lancaster, Pa., as second-class matter.

CONTENTS OF AUGUST NUMBER

CONTENTS OF SEPTEMBER NUMBER

Vol. LXIX. No. 5 NOVEMBER, 1906

THE POPULAR SCIENCE MONTHLY.

EDITED BY J. McKEEN CATTELL

CONTENTS

THE SCIENCE PRESS

LANCASTER, PA. GARRISON, N. Y.

NEW YORK: Sub-Station 84

Single Number, 30 Cents Yearly Subscription, $3.00

VOL. LXIX. No. 6 DECEMBER, 1906

THE POPULAR SCIENCE MONTHLY.

EDITED BY J. McKEEN CATTELL

CONTENTS

THE SCIENCE PRESS

LANCASTER, PA. GARRISON, N. Y.

NEW YORK: SUB-STATION 84

SINGLE NUMBER, 30 CENTS YEARLY SUBSCRIPTION, $3.00

The Popular Science Monthly

Entered in the Post Office in Lancaster, Pa., as second-class matter.

CONTENTS OF OCTOBER NUMBER

CONTENTS OF NOVEMBER NUMBER