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A RETROSPECT[1]

IT is the custom of our association that the annual presidential address should be delivered, not by the actual president, who assumed the responsibilities and honors of his office at the close of the last meeting a year ago, but by his predecessor, who by courtesy is termed the "retiring president" although as a matter of fact he is not "retiring" but "retired." He has to the best of his ability sustained the responsibilities of the presidency and has been relieved of them, he has enjoyed the honor of the position and has retired beyond the range of the spot-light only to be dragged into it once more with even greater responsibilities than before. Professor Dana in his presidential address of nearly seventy years ago describes this situation more eloquently than I can. "In most offices," he said, "the duties terminate with the office, and the thing of the past, the ex-officer, is to the present an unknown quantity. But it is not so with your president. Science . . . sternly drags forward its reluctant presidents to their hardest trial when they have ceased to be, to a judgment after death severer than that of Rhadamanthus." And Professor Asa Gray nearly twenty years later naturally and happily employing a botanical metaphor, compared the president to a biennial plant: "He flourishes for the year in which he comes into existence and performs his appropriate functions as a presiding officer. When the second year comes round, he is expected to blossom out in an address and disappear."

This arrangement has its advantages in that it affords what should be ample time for the preparation of such an address as the occasion and the position demands; for a speaker from this rostrum is confronted with the responsibility of speaking as one having authority, as a representative of science and while he may not have the ability to duly mix "reason with pleasure and wisdom with wit," he may be expected to set forth with surety and clarity the faith that is in him as to the achievements and progress of science, or at least of that department of science which he cultivates. Few can see this responsibility approach with cool, calm composure and assurance and alas! the very fact that one has apparently ample time for the preparation of one's pronouncements

[1] Address of the president of the American Association for the Advancement of Science, Seventy-fifth Annual Meeting, Cincinnati, 1923.

with most of us merely leads to the postponement of that preparation until in the fulness of time it is forced upon one. I confess that this has been my own case and that I am one of those "who time gallops withal." I make this statement not in extenuation but in explanation.

But while I fully appreciate the burden of responsibility that rests upon my shoulders it is with a peculiar satisfaction that I appear here to-night. That I have recently held the highest office in the gift of this association is in itself a source of the greatest pride and satisfaction, but these feelings are enhanced by the fact that I am a witness for the broad spirit of catholicity shown by the association in that it declines to recognize geographical boundaries to scientific endeavor. My presence on this platform is the outcome of a recent meeting of the association on Canadian soil and is to be regarded as a compliment to the association's hosts on that occasion, the University of Toronto and the Royal Canadian Institute. We esteem it an honor that we should have been permitted to act as your hosts, we rejoice in such invasions across our boundary, invasions that tend to maintain and strengthen that *entente cordiale* which, with some slight and temporary perturbations, has characterized the relations of the two countries for well over a century. Let us hope that a century hence a future president from the neighboring Dominion may speak in similar words—omitting reference to perturbations, though that seems almost too much to hope for.

The meeting of 1921 was not the first but the third meeting of the association in a Canadian city, nor am I the first Canadian to be honored with its presidency. At the meeting in Montreal in 1882 the presiding officer was Principal J. W. Dawson, of McGill University, distinguished for his contributions to Canadian zoology and geology and one of those who, like Louis Agassiz, could cultivate with equal success two of the great fields of science that were then included under the term natural history. How these fields have been extended in the forty years that have elapsed and how zoologists and geologists, ever pushing onward the frontiers of their respective territories, have drifted apart, until now each has developed a peculiar dialect which the other finds difficulty in understanding! This is one of the penalties of increasing knowledge.

There is a third president of the association who may perhaps be claimed as a Canadian, I mean Professor T. Sterry Hunt, a distinguished chemist and mineralogist. He was born, it is true, south of the Canadian boundary and the closing years of his life were passed in his native land; but for quarter of a century he was an active and valued member of the Geological Survey of Canada, under Sir William Logan, the first director of that survey. Professor Hunt was the acting president of the association at the Troy meeting in 1870, at which time he was still a member of the Canadian Survey.

I call attention to these facts only to emphasize the broad spirit of fellowship that characterizes this association. Its object is the advancement of science, and it is ready to extend the privileges of its meetings and the stimulus that they bring, wherever, upon this continent, they may be welcome. Canadian scientists and Canadian science have always been as welcome at the association's meetings as that brand of scientist and that brand of science that is produced in the United States. Furthermore arrangements are now on foot whereby it is hoped that the influence of the association in promoting the advancement of science will be extended to the republic that lies south of the Rio Grande and the association is thus justifying its title of American in a fuller and broader sense than that usually attached to that designation. It is working toward the realization of the ideal expressed in its first by-law, which lays down the principle that "The association is American, its field covering North, Central and South America. Inhabitants of any country are eligible to membership." It strives for the advancement of science, wherever cultivated, as a potent factor in civilization.

This broadening out policy is one that has been inherited by the association from its immediate ancestor, The American Association of Geologists and Naturalists. This association, which was primarily one of geologists, was organized in 1840 as the result of the inauguration of geological surveys of various states of the Union. Those engaged in these surveys felt the necessity for cooperation and discussion that there might be uniformity in the presentation of the results of their work. Soon the zoologists and botanists—the naturalists—were drawn in. Some chemists from the first had been included in the association but, in time, their department became a large and important one and finally the meteorologists found a congenial atmosphere in the association. So the scope of the interests of the association broadened out and at its 1847 meeting, held in Boston, it was decided that it should assume a title more indicative of its scope, and at the meeting of 1848, held in Philadelphia, it became the American Assocation for the Advancement of Science and in keeping with the new title it extended its membership to include general physicists, mathematicians, economists and engineers.

Thus our association had its beginning more than seventy-five years ago and at the close of its inaugural meeting it had a membership list of 461, an excellent showing, especially when it is recalled that in the early years there were certain restrictions of the membership that were subsequently made less definite. From the beginning, however, it has had the support of the leading scientists of the country; on its first council were Professor Jeffries Wyman, Professor Benjamin Peirce, Professor S. S. Haldeman, Professor Joseph Henry and Professor Louis Agassiz, names that we of to-day recall with reverence and admiration, names that will forever stand in shining letters on the records of scientific achievement in this continent. With such men in control of its affairs success was guaranteed to the young association; it at once became the rallying ground for scientists in all departments of research and in turn attracted those who were interested in scientific progress without taking active share therein. For, as Professor Bache remarked at a later meeting, "Who will say that they do not return wiser, better, more zealous according to knowledge from a meeting—with Henry, Peirce or Agassiz?"

This first meeting is of interest too from the standard of papers presented. Foremost among these was an exhibition by Lieutenant Maury of charts of the North Atlantic showing the prevailing winds and currents, deduced from the study of many thousand old log-books, an earnest of what was to develop later into the classical "Physical Geography of the Sea." Lieutenant Maury demonstrated clearly the relation of intensive scientific investigation to practical results, for his charts indicated that the route usually followed by southbound vessels did not allow them to profit by the most favorable winds and by selecting another route, deduced from his observations, and testing it by a number of vessels, it was found that the passage could be made in three quarters the average time taken by vessels following the older recognized route. The introduction of steam navigation in the years that followed detracted greatly from the direct utilitarian importance of Lieutenant Maury's investigations, but he had laid the foundations for our modern science of oceanography and had established principles that, for a time at least, greatly favored commercial intercourse with distant portions of the globe, especially that between this country and the east and that between Great Britain and her Australian colonies. The voyage from Liverpool to Australia in earlier days usually occupied some four months or more, but in 1854 a sailing vessel, following the course advocated by Maury, made the passage in sixty-three days, and that course even in these days of steam, is still largely followed.

Another important paper presented at the first meeting was that on "The Sediment of the Mississippi River" by Dr. M. W. Dickeson and Mr. Andrew Brown, a summary of deductions based on observations extending over eighteen years, and mention should also be made of a series of papers by Agassiz, fresh from his expedition to Lake Superior, on whose shores his practised eye found abundant evidence of glaciation and whose waters yielded to him a rich harvest of fishes which he could compare with the fresh-water fishes of Europe and those of the Spix collection from Brazil that he had already studied.

One may not linger over this first meeting, nor may one pause to note the many interesting contributions presented at later meetings. The activity of the association in these early days was sufficiently great to warrant the holding of two meetings in each of the years 1850 and 1851 and the first of those of 1851, the fifth of the association, was held in this city in the month of May, under the presidency of Professor Alexander Dallas Bache, the distinguished and efficient director of the Coast and Geodetic Survey. The college professor of the fifties was not the migratory bird he has since become, nor were there then the inducements to extensive peregrinations that now exist. The colleges and scientific institutions were ranged along the Atlantic sea-board, the great State Universities, now such important factors in our scientific progress, had not yet arisen, although the University of Michigan had opened her doors in 1841 with a staff of two professors and with eleven students in attendance. All previous meetings of the association and those of the parent society had been held in cities of the Atlantic coast; the May meeting of 1851 was the first held beyond the Alleghanies and in 1851 a journey beyond the Alleghanies was not one to be lightly undertaken, it was an adventure. It may interest you to-night to hear of the expectations of Professor Henry and of the realities he found in attending the first meeting of the association in this city. He confessed that it was the first time that he had been west of the mountains and went on to say that "He expected to see a boundless, magnificent forest world, with scattered dwellings and log-cabin villages and energetic New England-descended inhabitants; he thought to find Cincinnati a thriving frontier town, exhibiting views of neat frame houses with white fronts, 'green doors and brass knockers,' but instead he found himself in a city of palaces, reared as if by magic and rivaling in appearance any city in the eastern states or of Europe." Professor Henry's expectations might have been realized some fifty years earlier; in the meantime Cincinnati had grown to the stature of the Queen city of the West and with her material progress had not failed to make provision for the cultivation of the arts and sciences in such organizations as the Mechanics Institute, the Academy of Natural Sciences, the Mercantile Library Association and the Young Men's Lyceum of Natural History, all of which Professor Henry mentioned with the remark, "These are the pride of Cincinnati—these her noblest works."

The first Cincinnati meeting was in itself a notable event as the first invasion by the association of what was then still regarded as the West. But it was made still more notable by two other happenings. At the preceding meeting at New Haven, Professor O. M. Mitchell, to whose enthusiasm the erection of the original Cincinnati Observatory was due, and who was its director until 1859, reported that he had invented and constructed two instruments by which in a single night as many accurate determinations of right ascensions or declinations might be made as were made at the Royal Observatory at Greenwich in a whole year. This was rather a startling claim to be made by one working apart and with few of the resources available at the more richly endowed observatories of the East and of Europe, and a committee was appointed with Professor Peirce as its chairman to investigate the claim and report upon it at the Cincinnati meeting. The committee found that as to the apparatus for observing right ascensions the claim was fully justified and while a sufficient number of observations had not been made with the apparatus for determining declinations to warrant a definite statement regarding it, yet it was regarded as being perfectly correct in the principles of its construction. "The committee," I quote from its report, "are not aware that the history of Astronomical Science exhibits a more astounding instance of great results produced by what would seem to be wholly inadequate means. With the ordinary tools of a common mechanic and with insignificant pecuniary outlay an isolated individual has aspired to rival the highest efforts of the most richly endowed institutions—and his aspirations have been crowned with success." The fame of the Cincinnati Observatory was at once established, for the genius of its director had developed methods of observation that were later adopted by all the leading observatories of the world.

That was the *great* happening at the first Cincinnati meeting. The other one I would mention is of less moment in that it concerns only ourselves; it was the appearance upon the scene for the first time of that supremely important official, known as the permanent secretary. Presidents may and do come and go, each "struts and frets his hour upon the stage, and then he disappears." Not so the permanent secretary, he goes on forever. He came into being at the first Cincinnati meeting in the person of Professor Spencer Baird, and he has continued in existence ever since, in various incarnations it is true, assuming for a time the lineaments of Professor Lovering, then those of Professor F. W. Putnam, then those of Professor L. O. Howard and finally those of the present efficient holder of the office. With each incarnation the responsibilities of the office increased and I need not say, that in each these responsibilities were fully and satisfactorily met.

Thirty years were to elapse before the association again met in Cincinnati, that is to say, it was not until 1881 that a second meeting was held in this city, this time under the presidency of Professor G. J. Brush, of Yale University. We have seen that the first meeting was made memorable by an important change in the administration of the association; the second meeting was made memorable by the adoption of a new constitution involving some important changes in organization. Up to 1875 two sections had been tacitly if not actually recognized in the association, Section A including mathematics, physics and chemistry, and Section B including natural history. In the year mentioned the disruptive tendencies of specialization began to manifest themselves and the chemists segregated in what was officially termed a permanent subsection, a similar action was taken by the anthropologists, one year later the microscopists decided that their highly magnified world required a subsection for itself and five years later still the entomologists deemed it necessary that they should betake themselves to a special hive. For each of these four permanent subsections there was a chairman, while the presiding officers of the two original sections were designated by the more dignified term of vice-presidents.

By the new constitution adopted at the second Cincinnati meeting the permanent subsections were abolished and at the same time the association was divided into nine sections, each of which was presided over by a vice-president, who was required each year to give an address before his section. The nine sections were those of A, Mathematics and Astronomy; B, Physics; C, Chemistry; D, Mechanical Science; E, Geology and Geography; F, Biology; G, Histology and Microscopy; H, Anthropology, and I, Economic Science and Statistics. Mark the significance of this step. It was a recognition of the tendency toward specialization that had become so marked a feature in the science of the day, and established a policy that has prevailed up to the present. That is why the second Cincinnati meeting was a notable one. We are now entering upon the third Cincinnati meeting—that it too may be a notable one is what may be expected from the past, but whether its notability will depend on new developments of policy or on its records of scientific achievement we must wait to see.

The policy of specialization thus inaugurated in 1881 was bound to lead to further developments. The first modification of it, however, was in a retrograde direction, consisting of the absorption of Section G, Histology and Microscopy, into Section F, Zoology, in 1885. Looking back from our present standpoint it is difficult to understand why this section G was ever established and its absorption was a step to the good. But it was not long until Section G was reestablished by the division of the section of biology into sections of zoology and botany (1892). Then followed in 1900 the establishment of Section K for physiology and experimental medicine and in 1906 Section L was created for education and at the same time the title of Section H was changed to anthropology and psychology. Section M for agriculture was established in 1912 and in 1921 astronomy was divorced from mathematics, psychology from anthropology and new sections for historical and philological sciences and for manufactures and commerce were created, bringing the total number of sections up to sixteen. There are still some letters of the alphabet available for future sections.

Nor was this recognition of specialization the only sign of segregation in the association. A geographical segregation was bound to come as the sphere of influence of the association grew. It has come; for in 1914 a Pacific Division was established and in 1920 a Southwestern Division, each with its own constitution and officers, each holding its own annual meeting and yet remaining bound to the parent association by the closest ties of membership and purpose. If the vision that our first by-law calls up is to be realized it is evident that other divisions must be recognized, indeed, as has already been indicated, the establishment of a Mexican Division has already come to be a matter for deliberation. And what a vision it is that our first by-law calls up—a federation of divisions extending from the shores of the Arctic Ocean to Cape Horn, marching under one banner and with one purpose, the advancement of science and civilization!

So with segregation integration was also taking place. But the principle of segregation that the association felt itself obliged to recognize was not adopted as extensively as some groups of scientists desired and a tendency developed for these groups of specialists to form their own societies independent of the association. It became evident that if such secessions went on the representative character of the association would be endangered. Specialization had come to stay; indeed, it was bound to increase with the growth of the very object to which the association was pledged, the advancement of science. There were advantages for these societies in holding their meetings in conjunction with the association and it was to the advantage of the association that they should do so. That the mutual advantages might be ensured to some extent the council of the association was empowered to enter into relations with certain of these societies, which became designated as affiliated societies and, in time, were granted the privilege of electing one or in some cases two representatives to the council of the association. The number of the affiliated societies has grown prodigiously in recent years and amounts to something over fifty, a fact that may be taken as evidence of the success of this line of policy. The strength of the association does not depend alone upon the size of its membership, but this may be taken as an index of the extent to which it is fulfilling its purposes. Beginning with 461, the membership remained in the neighborhood of 500 until 1870, when a marked growth took place, bringing it up to 2,000 in 1885. Then followed a period of rest lasting until 1900, after which a steady and phenomenal growth occurred until now our membership is approximately 12,000. Surely in such figures we may find reason for congratulation and evidence of the wisdom of the policy laid down by the Council and ably carried out by our late permanent secretary, Dr. L. O. Howard and his successor, Dr. Burton E. Livingston.

Specialization must necessarily accompany progress. When one embarks upon a career of investigation one chooses a stream whose prospect pleases and for a time one floats placidly upon its bosom, following up its course. But soon it is joined by a large tributary and one must decide whether one will follow the right or the left branch. The decision made, one continues one's course, passing tributary after tributary, all of which, like the stream that is being followed, lead into unknown lands and at each a fresh decision must be made. In time the current strengthens, the journey becomes more arduous, difficulties are encountered, but still one keeps on, reaching farther and farther into the unknown and farther and farther from fellow searchers who have chosen other branches. One can not join them if one would, for they are ever advancing, perhaps with even greater rapidity and so one must perforce devote himself to the territory before him, hearing only by chance and at intervals rumors of the discoveries that are being made in other areas. That, it seems to me, is the experience of the investigator expressed in metaphor. The farther he and his associates advance the more they become isolated. New ideas demand new terms in which they may be discussed and so the members of each group come in time to speak a peculiar language and their isolation thus becomes more pronounced, for there is a limit to the number of languages that each of us can understand, some of us, indeed, have but a moderate command of even our native tongue.

And if this be a true statement of conditions, if it be true that even those familiar with the scientific methods find difficulty in appreciating the work of those laboring in other fields, how much more difficult must it be for those who from choice or from lack of opportunity have not had the advantage of a scientific training and yet are deeply interested in the progress and achievements of science. These form a not inconsiderable and important portion of our membership; they come to our meetings to hear something of the latest achievements of science and they listen to addresses largely in an unknown tongue. They ask for bread and are given a stone and profit little from such a monolithic repast. Yet these are the persons that we should endeavor to interest if we are truly and fully pledged to promote the advancement of science. Esoteric science may lead from discovery to discovery but until the significance of its discoveries is made intelligible to what are termed the men in the street it fails to secure popular support. The unintelligible is mysterious and mystery awakens either ridicule or dread.

Much has been spoken and written concerning the need for a popularization of science and something has been done towards its accomplishment, notably the establishment of *Science Service* so ably edited by Dr. Slosson. But is not this very thing a prime duty of this association, devoted as it is to the advancement of science, and does the association live up to the full measure of its responsibilities in this matter? I believe I am right in stating that we have not been so successful in this respect as some of the sister associations in other lands. True, we make some endeavor in providing special evening lectures that are designedly popular and I venture to suppose that the presidential addresses are expected to partake largely of that character. Nor will I be revealing any secrets of policy when I say that the council has given the matter serious consideration, and one may hope that its conjoint wisdom and experience will devise additional means to meet the difficulty. In the meantime it may seem temerarious to suggest measures looking to the betterment of the situation, but a retiring president has privileges and I feel so strongly the necessity for retaining and increasing the interest of what may be termed the lay members of the association in the aims and results of scientific research that I will venture a suggestion. Lack of understanding leads to misunderstanding, and I would beg that those who contribute papers to the sections, and especially the vice-presidents of sections, should in their deliverances bear in mind our lay members and strive for simplicity and perspicuity. Most of us are educators and we have in the meetings of this association opportunities for educating found nowhere else. Let us remember this and take advantage of our opportunities.

These ideas were suggested by the perusal of a number of addresses given by early presidents of the association. There runs through several of them an almost apologetic note, as if it seemed necessary to defend researches into the mysterious phenomena of the universe, since conclusions based on these researches tended to unsettle men's minds by undermining old long-standing beliefs. This was three generations ago and the practical applications of science were neither so frequent nor so striking as they are to-day. The Morse telegraph had been used commercially four years before the first meeting of the association, but the other remarkable applications of electrical energy that have become so much a part of our every day life were as yet undeveloped. Anesthesia had been introduced into surgical practice, but antisepsis, that was to revolutionize surgery, was as yet unknown; indeed, the causation of sepsis, together with that of putrefaction and fermentation was awaiting an explanation by the genius of Pasteur and this explanation was to lead up not only to surgical antisepsis, but to the formulation of the germ theory of disease and the wonderful achievements of modern preventive medicine. How these and other achievements in other departments of science have revolutionized the world! They are tangible evidences of the benefits that science can confer upon mankind, they are recognized as such by the man in the street and he consequently has developed an interest in science and a toleration of its votaries that his forbears of three generations did not possess. Nay, not only does he tolerate science, he encourages it by providing funds for its prosecution, by richly endowing great research laboratories and by bequeathing princely prizes as rewards for important discoveries. The distrust of seventy years ago has given way to trust and the world accepts with tranquillity the shattering of many old beliefs, providing that the necessity for their destruction is vouched for by competent scientific opinion. The theory of relativity, whether or not its full significance is understood, is swallowed without a spasm, even though it may displace the theory of gravitation from what seemed to be its unassailable position; and that the atom, supposed to be the ultimate, indivisible abstraction of human thought, is in reality a more or less complex system of electrons revolving planet-like about a central nucleus, even this idea is accepted without a tremor.

This change of attitude is undoubtedly largely due to an increased appreciation of the value of science as shown by its practical applications. This may not have been the only factor, but it is a potent one. It is impossible to consider the multitudinous and marvelous facilities that have become parts of our daily life without realizing that they are but the practical applications of scientific principles to the control or utilization of natural forces and materials, without, in other words, perceiving that it is to scientific investigation that we are indebted for these advantages. The men who have made these practical applications become known and respected, their names become household words, they are the representatives and high-priests of science and their glory is reflected upon even the most abstruse fields of scientific investigation. The attitude assumed may be expressed thus: "See what great benefits science has conferred! It promises others and therefore it is to be encouraged."

For the present we must perhaps be satisfied with this. For several centuries science was under the ban, dogma was supreme and science, which necessarily found itself in contest with this, was impious and heretical. Truth was standardized and complete and to question that accepted truth was to undermine the foundations of belief. The human mind is conservative in its reactions; habits of thought are as difficult of modification as habits of action and the change from the dogmatic to the scientific habit has been slow; indeed, it is far from complete even now. The utilitarian appeal of science has done much to emancipate it from its thraldom to dogma, but it is not yet universally recognized that the utility of science depends absolutely upon its success in discovering truth. It is only by getting at the true facts and the true principles involved in any problem that the results of science become useful. The scientist is a searcher after truth and it is for that reason that he is able to confer benefits on humanity; it is for that reason that he deserves recognition. Surely he should feel no necessity for an apology for his existence.

But the ultimate truth is elusive. When science establishes a truth that may seem at first to be ultimate it but points the way to another truth lying beyond and it is to the credit of scientific men that they are ready to admit the lack of finality in what has been accomplished, once the vista of the new truth has opened out. This attitude is not easily understood by the layman unfamiliar with the scientific method, and he is apt to imagine that a confession of lack of finality means the condemnation of the older truth as false. This is a misconception that has frequently occurred and, unfortunately, it is a misconception that scientists themselves have aided in creating, by failing to appreciate the popular view-point. In the popular mind the doctrine of evolution is so completely involved in Darwin's exposition of it that it has come to be regarded as the product of his brain. Consequently any acknowledgment that some of Darwin's views may require modification is assumed to imply that the foundations of evolution are shaken. It seems trite to repeat once more the true relation of Darwin's theory to the doctrine of evolution, but there seems to be need for its repetition. Evolution as a theory long antedates Darwin's time; Laplace, to go on farther back, found it in the history of the heavenly bodies, Lyell demonstrated it in the history of the earth, and Goethe, Saint Hilaire and Lamarck saw it in the history of terrestrial organisms. What Darwin did was to give a plausible and convincing explanation of how organic evolution might have occurred, but whether that explanation is or is not the correct one matters not so far as the doctrine of evolution is concerned; that stands unshaken even though Darwin's explanation of how it was brought about be discarded. The evidence in its favor to-day is many times stronger than it was in Darwin's time and it seems incredible that man as a reasoning animal should presume to doubt its validity; such doubts can be based only on ignorance of the evidence or on unreasoning prejudice.

True, it was Darwin who focussed the attention of the world upon the doctrine, by propounding the theory of natural selection as the causal factor in the transmutation of species. The biological world of to-day does not ascribe to that factor the importance that Darwin gave it. Its action can not be denied; it is self-evident to any observer of nature's ways who finds

> that of fifty seeds
> She often brings but one to bear.

It plays an important rôle in the suppression of the unfit rather than in the survival of the fittest, but it can act only on variations sufficiently pronounced to determine life or death. It has been shown in several cases that what seem trivial variations may, under certain conditions, lead to fatal results, but even admitting these, it is difficult to believe that many of the minute differences that distinguish species have selective value. Natural selection acts effectively in the perpetuation of species, but it does not originate them and to that extent the modern biologist may depart from Darwin's standpoint. Darwin was looking for the origin of species, the modern biologist goes a step further and is looking for the origin of variations and the mechanism of heredity problems far beyond Darwin's times. But he stands on the foundation built by Darwin, sinec the whole structure of modern philosophy rests on that foundation.

It would be interesting to sketch the story of the reception of the origin of species as revealed by the records of the association. But the date of its publication was 1859 and before there could be any extended criticism of it the members of the association found themselves face to face with the struggle for the preservation of the Union. From 1861 to 1867 the association held no meetings and by that time the first interest in the "Origin" had somewhat waned. In 1867, however, Professor Newberry in his address, while protesting against the obloquy and scorn that had been heaped on Darwin in many quarters as "peculiarly unjust and in bad taste," states his belief that the theory can not be accepted since, in his opinion, a single case of altruism would overthrow it. He overlooked the fact that individual altruistic sacrifice may benefit the race and it is the race not the individual that nature would perpetuate.

So careful of the type she seems
So careless of the single life.

Professor Asa Gray in 1872 was the first to express definitely in a presidential address his belief in a process of transmutation in organic life and, as was usual with him, he expresses himself so felicitously that I venture to quote his words. "Organic nature—by which I mean the system and totality of living things and their adaptation to each other and to the world—with all its apparent and, indeed, real stability, should be likened, not to the ocean, which varies only by tidal oscillations from a fixed level to which it is always returning, but rather to a river, so vast that we can neither discern its shores nor reach its sources, and whose onward flow is not less actual because too slow to be observed by ephemeræ which hover over its surface or are borne upon its bosom."

It is interesting to note that before 1859 the question of the permanency of species was already exercising the minds of members of the association. This was a reflection of the discussions on the same question that had earlier agitated the scientific world in Europe. The immutability of species predicated by the definition of the term by Linnaeus was being questioned and the idea of transmutation, later to be known as the doctrine of evolution, was in the air. This, let me repeat, was long before Darwin began to reflect upon the question during the voyage of the "Beagle," long before Wallace in the Malay archipelago began to think along the same lines as Darwin. In 1849 and again in 1850 Professor Agassiz approached the question, indicating the ideas later fully elaborated in his *Essay on Classification* according to which there was a definite plan in organic creation in which each species had its place from which there could be no departure. This position traces back to the influence of Cuvier, which is also manifest in a paper presented by Professor J. D. Dana at the meeting of 1857, in which, confronted with the variability shown by natural species and yet possessed by the idea of their essential immutability, he is driven to a position that forcibly recalls the Platonic theory of ideas or the metaphysical subtleties of scholastic realism, holding that "Species are realities in the system of nature while manifest to us only in individuals." I mention these pronouncements merely to emphasize the fact that the idea of transformationism, that is to say, the idea of evolution, was in men's minds long before the publication of the *Origin of Species*. These pre-Darwinian utterances were, it is true, in opposition to the idea of evolution, but that they should have been made is an indication that that idea was exercising men's minds.

Since evolution has become a fundamental doctrine of modern scientific philosophy it would be of interest to discuss its influence on scientific investigation. The field, is, however, too extensive to permit of adequate treatment of it as a whole and I shall, accordingly, limit my remarks to that portion of it with which I am most familiar and even then but a mere sketch is possible, so numerous and so varied have been the lines of investigation that have opened up since 1859. Unfortunately, here again the records of the association give but little assistance, since, after Professor Agassiz ceased regular attendance on the meetings, zoology for many years was but casually represented and the actual trends of zoological investigation were not clearly revealed by the papers read. But many of the new developments in zoology, its evolution, that is to say, fall within the memory of many of us older men and ample material is available by which the more recent developments may be connected up with the older viewpoints.

In the period immediately preceding Darwin the school of transcendental morphology was at its height and attention was concentrated upon finding the archetype structure, a general plan of organization which could be traced, with adaptive modifications, through the whole scale of animal life. The teachings of this school, led by Saint Hilaire in France, by Oken in Germany and by Owen in England, had a profound influence on zoological thought, especially in two directions. In the first place it divorced morphology, the science of structure, from physiology, the science of function, establishing the former as a special science whose problems were the discovery of homologous parts throughout the range of animal life. Homology was a purely structural idea, function had no place in the determination of the equivalency of parts and so, for the comparative anatomist, function ceased to be of interest.

In the second place by the predication of a structural archetype the transcendentalists opened the way to the idea of the essential unity of animal forms. If there were a primitive archetypal structure by modifications of which the various forms of animal life had been produced, there followed an implication of a definite relationship between these various forms, and the way was smoothed for the reception of the doctrine of evolution.

It is remarkable, however, how small a part comparative anatomy took in the establishment of the doctrine, although it possessed data of great pertinency and in great abundance. Darwin did, of course, make some use of these data, but he was not a comparative anatomist but rather a systematist, and the greater mass of the accumulated anatomical matter was left unapplied. Nor was there that burst of further activity that might have been expected in comparative anatomy after the acceptance of the evolution theory. That may partly be explained by the serious blow dealt the transcendental school by Huxley in 1857 when, with keen logic and trenchant facts, he demolished the vertebrate theory of the skull, a critical and essential part of the transcendental belief. True, comparative anatomy did not languish in the post-Darwinian days; it could not with two such protagonists as Huxley and Gegenbaur, and its efforts were directed toward the substantiation and strengthening of the theory. But a more attractive field was suggested for study and to the investigation of this zoologists flocked as prospectors to a newly discovered gold-field.

The doctrine of evolution predicated a genetic relationship of forms, that is to say, each group of forms, species, genera, families or types each had a pedigree, each had behind it a long series of ancestors by the modification of which it had been developed. As far back as 1828 von Baer in his studies on the development of the chick had been struck with the resemblance which that form in its early stages showed to the embryos of other vertebrate types and expressed this fact in the law that the general characters of the great group to which an embryo belongs appear in development earlier than the special characters. Even earlier (1821) J. F. Meckel had maintained that the development of the individual organism obeys the same laws as the development of the whole animal series; that is to say, the higher animal in its gradual development passes essentially through the permanent organic stages that lie below it. These ideas in the days of the transcendental school could have little effect, though of great interest, but with the acceptance of evolution they took on a new meaning. In 1864 Fritz Müller, who had been studying the development of Crustacea while in exile in Brazil, published his results in a volume termed *Fur Darwin,* and in this expressed the idea that new species might be formed either by deviating from the parental type whilst still on their way towards that type or by passing through the various stages shown by the parents and then progressing beyond them. In the latter case "the historical development of the species will be mirrored in its developmental history." Haeckel in his *Generelle Morphologie* (1866) accepted Müller's idea, terming it the Biogenetic law which was to the effect that each organism in its individual development recapitulates more or less perfectly its ancestral history. If this law be true what an opportunity was offered by embryology for tracing the pedigrees of all sorts of animals and so to embryology the zoologists turned almost *en masse* and the construction of pedigrees, phylogenetic trees they were called, became the fashion. The development of representatives of every group of animals was eagerly studied and many an ardent genealogist concentrated his efforts on the construction of a pedigree for the vertebrates, solving the problem to his own satisfaction time and time again—but it is still unsolved.

At first, with the pedigree idea so fully in mind, the most striking features of the development of the various forms were sufficient for record, but in time the embryological became more minute, the history of the development was carried farther and farther back until the study of cell lineage came into existence in which step by step, cell division by cell division, the development of various forms was traced from the ovum until the principal organ systems were differentiated. The problem of embryology had changed; it was no longer a question of tracing more or less probable pedigrees, it was the question of the differentiation of tissues and organs of the multicellular organism from the single-celled ovum. It was no longer a search for evidence confirmatory of evolution, it was the beginning of the intensive study of the phenomena that lay behind variation and inheritance.

But in the meantime other lines of study had opened up. In the seventies and eighties of the last century attention was being directed to the remarkable phenomena that were associated with the division of cells and it was found that the process of division was in the immense majority of instances initiated and controlled by the cell nucleus. This body was found to undergo a series of extraordinary changes whereby its constituents were elaborated into a definite number of rod-like bodies termed chromosomes, each of which divided, and half the number thus formed passed into one of the daughter cells and the other half into the other, the nuclei of these cells being reconstituted from the chromosomes so distributed. The germ-cells and especially the sperm-cells proved favorable objects for such studies and their study led naturally to a study of the phenomena associated with the fertilization of the ovum. It would take too much time to recount the many interesting facts that were revealed by such studies; it will suffice for our present purpose to say that they gave very definite assurance for the belief that the chromosomes were the bearers of inheritable qualities, that it was by them that the parental characteristics were handed on to the offspring.

In 1893 there appeared a work by August Weismann, of the University of Freiburg, in which were summed up and elaborated certain doctrines that he had published earlier and which had an important influence on zoological thought. He drew a clear-cut distinction between the tissues which composed the body of an organism, the somatic cells, and the germ cells which served for the perpetuation of the species, and pointed out that there had been from the first a continuity of the germ-plasm; it passed on from generation to generation and was potentially immortal, using that term in a relative sense, whereas the somatic plasm of each generation suffered death and showed no continuity, but was formed anew from the germ-plasm in each generation. These ideas once they were pointed out seemed quite acceptable, and indeed, one might almost say, self-evident, but Weismann added a further idea in maintaining that the germ-plasm, and specific changes acquired by the cally isolated and could be affected only in the most general way by the somatic tissues. Consequently variations could arise only by modifications of the germ-plasm, and specifications acquired by the somatic tissue during the life-time of an individual could not be reproduced in the succeeding generation. Change of structure of the germ plasm was the sole source of variations and these could be perpetuated only by the action of natural selection in some of its forms. At once zoologists were divided into two camps, the Neo-Darwinians, who accepted Weismann's teachings on these matters, and the Neo-Lamarckians, who persisted in believing, as Darwin himself did, that acquired characters could be transmitted. The controversy waged long and in some cases bitterly and, indeed, is with us still.

In all these various lines of investigation zoologists were concerned almost exclusively with the structural side of the problem, the divorce of morphology and physiology which traces back to the transcendental school of comparative anatomists was still in force. In their study of cell-lineage they had seen the differentiation of form and structure appearing in the embryonic cells, but all the refinements of microscopical technique failed to yield an explanation of how and why that differentiation took place. Other methods must be applied and so, following the leadership of Roux, the method employed long before by Trembley, Réaumur and Spallanzani, the experimental method, the method of the physiologists, was revived. The developing ova were subjected to various changes of the environment, they were placed under pressure, the constitution and density of the water in which they were reared was altered, they were subjected to the action of various narcotic and other poisons, they were operated upon in various ways both in the very early and in later stages and the resulting modifications in the differentiation were noted.

Into the details of the results one can not enter now, the important point is the revival of physiological methods for the investigation of morphological problems, the re-marriage of physiology and morphology after a prolonged divorce. The old quarrel between form and function has been stayed, neither the one nor the other is antecedent and determining, but both work together; form can not be understood without a consideration of the function nor function without a full appreciation of structure. The recognition of the interplay of the two is the essential characteristic of modern zoological investigation and more and more the problems first opened up by studies of structure are being attacked along functional, experimental or physiological lines. In its early days the new method was spoken of as if it had created a new science, that of physiological morphology, but it has since become so familiar that any special designation of it is deemed unnecessary.

Once revived the method of experimentation was so prolific of results, especially in embryology and in the study of the regeneration of lost parts, that it soon became applied to the most varied lines of zoological research. One can not consider all these even sketchily, but only some of the more important ones may be mentioned. Variation and inheritance were accepted by Darwin as axiomatic forces, they were taken for granted and the theory of natural selection was built upon them as upon foundation stones. The breeding and hybridization of domestic animals and cultivated plants had of course been carried on from time immemorial, but the fundamental laws governing inheritance had not been established. Varieties were obtained by crossing different strains, but it was a hit or miss process, and if desirable varieties did occur they were preserved by a process of artificial selection. It was not until 1889 that Francis Galton attempted the formulation of a law of inheritance and that on quite general lines, but it was sufficient to arouse interest in the matter; experimental breeding was begun and soon the interest grew with the discovery of accounts published in 1866 and 1867 in an obscure periodical, of the remarkable results that had been obtained by Gregor Mendel in the gardens of the Monastery of Brünn. A study of these accounts revealed beyond peradventure the fact that the results of hybridization far from being elusive and fortuitous are governed by definite laws which it was possible to deduce from Mendel's observations. The study of inheritance was thus placed upon a scientific basis and experimental hybridization was carried on extensively by Correns and others in Germany, by Bateson and Darbishire in Great Britain and by Davenport and Castle in this country. Mendel's law was confirmed and extended and, with the discovery of the fruit-fly, *Drosophila,* as a favorable form for experimentation, the brilliant researches of Morgan and his pupils established the location of the bearers of many inheritable characteristics in the chromosomes of the germ cells and even succeeded in indicating the position of these bearers in the individual chromosomes and their relative positions in these! As a result of all these investigations we now have an insight into the *modus operandi* of heredity undreamt of by Darwin and already we have reaped practical benefits from them in the improvements of our strains of wheat, to mention but one example. What the science of eugenics, also based on these researches, may yet do for us is unknown—it holds out great promise.

Another line of research opened up by the experimental method was the study of the determining factors in animal behavior, a study of the responses of living matter to external stimuli such as light, temperature, contact, gravitation and other environmental forces, both physical and chemical. These were essentially studies in comparative psychology and therefore strictly speaking physiological, but, with the breaking down of the old distinction between form and function, it was perceived that they were studies of the adaptation of structure to function and they were undertaken mainly by men who had been trained primarily in morphological methods. They were studies in experimental morphology, studies of the response of the animal mechanism to external influences and they revealed the fact that while in the living substance there was a mechanism (the morphological side) by which responses could be manifested and sources of energy (the physiological side) by which the mechanism might be set in action, there was necessary also "a stimulus external to the responding protoplasm in order that an adaptive or orderly result should occur." (Davenport.) The stress placed by these studies upon a mechanistic explanation of animal activities promptly aroused the opposition of those who still clung to the earlier vitalistic explanation and, as in other cases where fundamental principles are involved, the contention still goes on and will continue until increasing knowledge reveals the truth.

More recently still another line of investigation has attracted those interested primarily in questions of form and structure and in this case also the morphologist has adopted methods and ideas from sister sciences. The physiologists had demonstrated the existence in the body of special organs which manufactured substances influencing in definite ways the chemical activities of the tissues; they had shown that these so-called endocrine organs or ductless glands such as the thyroid, the pituitary and the islands of the pancreas, produced substances necessary for the normal activities of the body and that a lack of these substances resulted in definite and serious disturbances of these activities. Further they had shown that various tissues of the body produced similar substances, hormones, precipitins, secretins and lysins, each with a definite influence upon the activities of some other organ or tissue. All this was purely physiological, but morphologists were not slow to perceive that these substances could be usefully employed in the study of structural changes, and have turned to the study of growth and form as modified by them, have investigated their effects upon the processes of transformation from the larval to the adult stages in *Amphibia,* have sought in their action an explanation of the remarkable modifications in many organs associated with the phenomena of reproduction in higher animals and, quite recently, they have been employed by Dr. Guyer in studies on heredity. This use of endocrine substances in morphological research is but begun, its results may be realized in the future.

I would like to have considered some special instances in which the application of the experimental method has thrown a flood of light upon structural problems, such, for instance, as the fertilization of the ovum, the problem of sex-differentiation, in which direct observation has also played an important part, and the recent attempts of Stockard, Guyer, Detlefsen and others to throw light by refined experimental methods on the old problem of the inheritance of acquired characters, but time is lacking for their proper discussion. With regard to the last example I might add that while these experiments have failed to render a decisive answer to the problem they hold out hope that similar lines of research may at least result in the discovery of a neutral ground on which the contending camps may come together.

So with new methods the fields of investigation have broadened out and knowledge has increased by leaps and bounds. And it is with especial satisfaction that we may note that in these progressive zoological studies the scientists of this continent have always been well in the van, if not in the fore-front of the advancing column. But all through the almost overwhelming flood of new knowledge there runs the guiding clue supplied by the doctrine of evolution. That has been the stimulus and dominating idea in all these studies; without it many, very many of them would never have been conceived and knowledge would have lost thereby. No! Evolution is not dead, nor can it be killed by legislative enactment.

Let me conclude this retrospect with a message for guidance in the future, taken from one who did not always find satisfaction in the advances and applications of science, and all the more impressive on that account. It is not the first instance in which a prophet from whom curses might be expected gave blessings, real or implied, instead. The words are those of Mr. Ruskin. "Go to Nature," he says, "in all singleness of heart and walk with her laboriously and trustingly, having no other thought but how best to penetrate her meaning; and remember her instructions—rejecting nothing, selecting nothing and scorning nothing; believing all things to be right and good and rejoicing always in the truth."

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