ley from a savage wilderness into what it is to-day—then may you find compensation for the want of a past like yours by seeing with prophetic eye a future world power of which this region shall be the seat. If such is to be the outcome of the institutions which we are now building up, then may your present visit be a blessing both to your posterity and ours, by making that power one for good to all mankind. Your deliberation will help to demonstrate to us and to the world at large that the reign of law must supplant that of brute force in the relations of the nations, just as it has supplanted it in the relations of individuals. You will help to show that the war which science is now waging against the sources of disease, pain and misery offers an even nobler field for the exercise of heroic qualities than can that of battle. We hope that when, after your all too fleeting sojourn in our midst, you return to your own shores, you will long feel the influence of the new air you have breathed in an infusion of increased vigor in pursuing your varied labors. And if a new impetus is thus given to the great intellectual movement of the past century, resulting not only in promoting the unification of knowledge, but in widening its field through new combinations of effort on the part of its votaries, the projectors, organizers and supporters of this Congress of Arts and Science will be justified of their labors. SIMON NEWCOMB.

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*THE EVIDENCE OF EVOLUTION.**

MR. PRESIDENT, MEMBERS OF THE UNIVERSITY OF CHICAGO, LADIES AND GENTLEMEN:

The noble aim of university teaching is the lifting up of mankind to a higher appreciation of the ideas of life and truth. It has to cultivate the most intimate connection between theory and practise, between abstract science and actual life. Throughout the world of research this connection is felt to be the real stimulus of the work, the very basis of its existence. American universities and American science have developed themselves on this leading principle, and it is especially on this account that high admiration is given them by their European sisters. Nowhere in this world is the mutual concourse between practise and science so general as here, and nowhere is the influence of the universities so widely felt as in this country. Perfect freedom of thought and investigation, unhampered rights of professing and defending one's conviction, even if it should be wholly contrary to the universal belief, are the high privileges of all real universities. Wealthy citizens spend their possessions in the founding of such institutions, convinced that this is the best way of promoting public welfare. The government liberally supplies funds for scientific research, whenever its application to practical business is clear. Your system of promoting agriculture by means of experiment stations, of scientifically conducted farm-cultures, of inquiries in all parts of the world, and of collecting, introducing and trying all kinds of plants that might become useful crops, is not only admired, but even highly envied by us Europeans.

It is not without hesitation that I have accepted the honorable invitation to speak before this renowned center of learning. The ideas to which I have been conducted by my experiments are to a large degree different from current scientific belief. But I have trusted to your willingness to listen to new facts and divergent convictions, and to your readiness to acknowledge whatever spark of truth might be found in them. Unbiased by prejudice, the calm air of the university and the enthusiasm of

* Convocation address, University of Chicago, September 2, 1904.

youth seeking only truth and convinced that only pure truth can bring real progress are the judges to which I gladly submit my conceptions.

My ideas have grown slowly, and have only reached their definiteness and full development under the protection of the high principles of university freedom. I have needed nearly twenty years to develop them and to gather the evidence by means of which I hope to convince you. I kept my secret until some years ago, and worked only for myself. In this respect old universities, as ours are in Europe, have a distinct advantage over your young American institutions. With you all is sparkling and boiling, with us it is the quietness of solitude, even in the midst of a busy city. But your students and teachers are expected to show what they are doing, and to produce their results at short intervals. In Europe, on the contrary, we are trusted and left free even on this point. Hardly anybody has ever asked me what I was doing, and even those who from time to time visited my garden were content with what I could show them, without telling my real difficulties and my real hopes.

To my mind, this is a high privilege. The solution of the most intricate problems often does not require vast laboratory equipment, but it always requires patience and perseverance. Patience and perseverance in their turn require freedom from all pressure, and especially from the need of publishing early and often unripe results. Even now I would prefer to spend this hour in recounting the obligations which the doctrine of evolution is under to such men as Lamarck and Darwin. I should like to point out how they have freed inquiry from prejudice and drawn the limits between religion and science; how they have caused the principle of evolution to be the ruling idea in the whole dominion of the study of the organic world, and how this idea has been suggestive and successful, comprehensive and hopeful during a whole century of continuous research. Everywhere it is recognized to take the leadership. It has been the means of innumerable discoveries, and whole sciences have been started from it. Embryology and ontogeny, phylogeny and the new conceptions of taxonomy, paleontology of plants and of animals, sociology, history and medicine, and even the life history of the earth on which we live, are in reality in their present form the products of the idea of evolution.

Instead of telling you of my own work, I should like to sketch the part which of late the scientists of the United States have taken in this work. Mainly in two lines a rapid advancement has been inaugurated in this country. I refer to the pure university studies and the work of the agricultural stations. Highly valuable is the application of science to agriculture in the improvement of races. Each of you knows how this artificial production of races of animals and plants was one of the great sources of evidence on which Darwin founded his theory. But at his time the available evidence was only very scanty when we compare it with the numerous facts and the improved methods which now are the result of half a century's additional work. America and Europe have combined in this line, and the vast amount of facts, heaped up by numerous investigators and numerous well-equipped institutions, has produced quite a new basis for a critical review of Darwin's theory.

I have tried to combine all these too dispersed facts and to bring them together, in order to obtain a fuller proof for the main points of Darwin's conception. In one subordinate point my results have been different from those of Darwin, and it is this point which I have been invited by the

kindness of your president to discuss before you.

Darwin's theory is commonly indicated as the theory of natural selection. This theory is not the theory of descent. The idea of descent with modification, which now is the basis of all evolutionary science, is quite independent of the question how in the single instances the change of one species into another has actually taken place. The theory of descent remains unshaken even if our conception concerning the mode of descent should prove to be in need of revision.

Such a revision seems now to be unavoidable. In Darwin's time little was known concerning the process of variability. It was impossible to make the necessary distinctions. His genius recognized two contrasting elements; one of them he called sports, since they came rarely, unexpectedly and suddenly; the other he designated as individual differences, conveying thereby the notion of their presence in all individuals and at all times, but in variable degrees.

Sports are accidental changes, resulting from unknown causes. In agricultural and horticultural practise they play a large part, and whenever they occur in a useful direction, they are singled out by breeders and become the sources of new races and new varieties. Individual differences are always present, no two persons being exactly alike. In the same way the shepherd recognizes all his sheep by distinct marks, and to find two ears in a field of wheat which can not be distinguished from one another by some peculiarity is a proposition which everybody knows to be impossible. Many highly improved races of forage plants and agricultural crops have been produced by intelligent breeders simply on the ground of these always available dissimilarities. They can be selected and accumulated, augmented and heaped up, until the new race is distinctly preferable to the original strain.

In ordinary agricultural breeding, however, it is very difficult to distinguish sharply between these two principles. Moreover, for practical purposes, this distinction has no definite use. The practise of selection is nearly the same in both cases, and, besides hybridizing, with which we are not now concerned, selection is as yet practically the only means for the breeder to improve his races. Hence it came that at Darwin's time there was no clear distinction between the two types of variations, at least not to such an extent that a theory of the origin of species could confidently rely upon it.

Quetelet's celebrated law of variability was published only some years after the appearance of Darwin's 'Origin of Species.' Variability seemed until then to be free from laws, and nearly everything could be ascribed to it or explained by it. But the renowned Belgian scientist showed that it obeys laws exactly in the same way as the remainder of the phenomena of nature. The law which rules it is the law of probability, and according to this law the occurrence of variations, their frequency and their degree of deviation can be calculated and predicted with the same certainty as the chance of death, of murders, of fires and of all those broad phenomena with which the science of sociology and the practise of insurance are concerned.

The calculations of probable variations based on this most important law did not, however, respond to the demands of evolution. Specific characters are usually sharply defined against one another. They are new and separate units more often than different degrees of the same qualities. Only with such, however, Quetelet's law is concerned. It explains the degrees, but not the origin, of new peculiarities. Moreover, the degrees of deviation are subject

to reversion to mediocrity, always more or less returning in the progeny to the previous state. Species, on the contrary, are usually constant and do not commonly or readily revert into one another. It is assumed that from time to time specific reversions occur, but they are too rare to be comparable with the phenomena which are ruled by the law of probability.

A thorough study of Quetelet's law would no doubt at once have revealed the weak point in Darwin's conception of the process of evolution. But it was published as part of a larger inquiry in the department of anthropology, and for years and years it has been prominent in that science, without, however, being applied to the corresponding phenomena of the life of animals and of plants. Only of late has it freed itself from its bounds, transgressed the old narrow limits, and displayed its prominent and universal importance as one of the fundamental laws of living nature.

In doing so, however, it has become the starting point for a critical review of the very basis of Darwin's conception of the part played by natural selection. It at once became clear that the phenomena which are ruled by this law, and which are bound to such narrow limits, can not be a basis for the explanation of the origin of species. It rules quantities and degrees of qualities, but not the qualities themselves.

Species, however, are not in the main distinguished from their allies by quantities, nor by degrees; the very qualities may differ. The higher animals and plants are not only taller and heavier than their long-forgotten unicellular forefathers; they surpass them in large numbers of special characters, which must have been acquired by their ancestors in the lapse of time. How such characters have been brought about is the real question with which the theory of evolution is concerned. Now if they can not be explained by the slow and gradual accumulation of individual variations, evidently the second alternative of Darwin's original proposition remains. This was based on the sports, on those rare and sudden changes which from time to time are seen to occur amongst cultivated plants, and which in these cases give rise to new strains. If such strains can be proved to offer a better analogy to real systematic species, and if the sudden changes can be shown to occur in nature as well as they are known to occur in the cultivated condition, then in truth Darwinism can afford to lose the individual variations as a basis. Then there will be two vast dominions of variability, sharply limited, and sharply contrasted with one another. One of them will be ruled by Quetelet's law of probability, and by the unavoidable and continuous occurrence of reversions. It will reign supreme in the sciences of anthropology and sociology. Outside of these, the other will become a new domain of investigation, and will ask to be designated by a new name. Fortunately, however, a real new designation is not required, since previous to Darwin's writings the same questions were largely discussed, and since in these discussions a distinct name for the sudden and accidental changes of species into one another was regularly used. At that time they were called 'mutations,' and the phenomenon of mutability was more or less clearly distinguished from that of variability in a more limited sense. Especially in France a serious scientific conflict raged on this point about the middle of the last century, and its near relation to religious questions secured it a large interest. Jordan and Godron were the leaders and numerous distinguished botanists and zoologists enrolled themselves under their banners. They cleared part of the way for Darwin and collected a large amount of valuable evidence. Their facts pleaded for the sharp and abrupt delimitation of their

species, and asked for another explanation than that which was derived from the ordinary, slow and continuous variations.

Their evidence, however, was not complete enough to command the decision in their behalf. The direct proof of the sudden changes could not be offered by them, and they allowed themselves to be driven to the acceptance of supernatural causes on this account. Thereby, however, they lost their influence upon the progress of science, and soon fell into oblivion.

Instead of following this historical line, however, I have now to point out one of the weightiest objections against the conception of the origin of species by means of slow and gradual changes. It is an objection which has been brought forward against Darwin from the very beginning, which has never relented, and which often has threatened to impair the whole theory of descent. It is the incompatibility of the results concerning the age of life on this earth, as propounded by physicists and astronomers, with the demand made by the theory of descent.

The deductions made by Lord Kelvin and others from the central heat of the earth, from the rate of the production of the calcareous deposits, from the increase of the amount of salt in the water of the seas, and from various other sources, indicate an age for the inhabitable surface of the earth of some millions of years only. The most probable estimates lie between twenty and forty millions of years. The evolutionists of the gradual line, however, had supposed many thousands of millions of years to be the smallest amount that would account for the whole range of evolution, from the very first beginning until the appearance of mankind.

This large discrepancy has always been a source of doubt and a weapon in the hands of the opponents of the evolutionary idea, and it is especially in this country that much good work has been done to overcome this difficulty. The theory of descent had to be remolded. On this point conviction has grown in America during the last decades with increasing rapidity. Cope's works stand prominent amongst all, and much valuable discussion and evidence has been brought together.

The decision, however, could only be gained by a direct study of the supposed mutations, but no distinct cases of mutability were at hand to provide the material. Discussions took the place of inquiry, and a vast amount of literature has broadly pictured all the possibilities and all the more or less plausible explanations without being able to give proof or disproof.

In this most discouraging state of things I concluded that the only way to get out of the prevailing confusion was to return to the method of direct experimental inquiry. Slow and gradual changes were accepted to be invisible or nearly so; mutations, however, would be clear and sharp, although of rare occurrence. I determined to start on a search for them, and tried a large number of species, partly native forms of my own country and partly from different sources. Each of them had to be tried as to its constancy, and large numbers of seedlings had to be produced and compared. The chance of finding what I wanted was of course very small, and consequently the number of the experiments had to be increased as far as possible.

Fortune has been propitious to me. It has brought into my garden a series of mutations of the same kind as those which are known to occur in horticulture, and moreover it has afforded me an instance of mutability such as would be supposed to occur in nature. The sudden changes, which until yet were limited to the experience of the breeders, proved to be accessible to direct experimental work. They can not yet in truth be produced artificially,

but, on the other hand, their occurrence can be predicted in some cases with enough probability to justify the trial. Color changes in flowers, double flowers, regular forms from labiate types, and others have been produced more or less at will in my garden, and under conditions which allowed of a close scientific study. The suddenness of the changes and the perfection of the display of the new characters from the very beginning were the most striking results.

These facts, however, only gave an experimental proof of phenomena which were historically known to occur in horticulture. They threw light upon the way in which cultivated plants usually produce new forms, but between them and the real origin of species in nature the old gap evidently remained.

This gap, however, had to be filled out. Darwin's theory had concluded with an analogy, and this analogy had to be replaced by direct observation.

Success has attended my efforts even on this point. It has brought into my hands a species which has been taken in the very act of producing new forms. This species has now been observed in its wild locality during eighteen years, and it has steadily continued to repeat the phenomenon. I have brought it into my garden, and here, under my very eyes, the production of new species has been going on, rather increasing in rate than diminishing. At once it rendered superfluous all considerations and all more or less fantastical explanations, replacing them by simple fact. It opened the way for further investigations, giving nearly certainty of a future discovery of analogous processes. Whether it is *the* type of the production of species in nature or only *one* of a more or less large group of types can not yet be decided, but this is of no importance in the present state of the subject. The fact is that it has become possible to see species originate, and that this origin is sudden and obeys distinct laws.

The species which yielded these important results is an American plant. It is a native of the United States, and nearly allied to some of the most common and most beautiful among the wild flowering plants of this country. It is an evening primrose, and by a strange but fortunate coincidence bears the name of the great French founder of the theory of evolution. It is called 'Lamarck's evening primrose,' and produces crowns of large and bright yellow flowers, which have even secured it a place amongst our beloved garden plants.

The most interesting result which the observation and culture of this plant have brought to light is a fact which is in direct opposition to the current belief. Ordinarily it is assumed that new species arise by a series of changes in which all the individuals of a locality are equally concerned. The whole group is supposed to be modified in a distinct direction by the agency of the environmental forces. All individuals from time to time intercross, and are thereby assumed to keep equal pace in the line of modification, no single one being allowed to go distinctly ahead of the others. The whole family gradually changes, and the consequence would be that the old form disappears in the same degree as the new makes its appearance.

This easy and plausible conception, however, is plainly contradicted by the new facts. There is neither a gradual modification nor a common change of all the individuals. On the contrary, the main group remains wholly unaffected by the production of new species. After eighteen years it is absolutely the same as at the beginning, and even the same as is found elsewhere in localities where no mutability has been observed. It neither disappears nor dies out,

nor is it ever diminished or changed in the slightest degree.

Moreover, according to the current conception, a changing species would commonly be modified into only one other form, or at best become split into two different types, separated from one another by flowering at different seasons, or by some other evident means of isolation. My evening primrose, however, produces in the same locality, and at the same time, from the same group of plants, quite a number of new forms, diverging from their prototype in different directions.

Thence we must conclude that new species are produced sideways by other forms, and that this change only affects the product, and not the producer. The same original form can in this way give birth to numerous others, and this single fact at once gives an explanation of all those cases in which species comprise numbers of subspecies, or genera large series of nearly allied forms. Numerous other distinct features of our prevailing classification may find on the same ground an easy and quite natural explanation.

To my mind, however, the real significance of the new facts is not to be found in the substitution of a new conception for the now prevailing ideas; it lies in the new ways which it opens for scientific research. The origin of species is no longer to be considered as something beyond our experience. It reaches within the limits of direct observation and experiment. Its only real difficulty is the rarity of its occurrence; but this, of course, may be overcome by persevering research. Mutability is manifestly an exceptional state of things if compared with the ordinary constancy. But it must occur in nature here and there, and probably even in our immediate vicinity. It has only to be sought for, and as soon as this is done on a sufficiently large scale the study of the origin of species will become an experimental science.

New lines of work and new prospects will then be opened, and the application of new discoveries and new laws on forage crops and industrial plants will largely reward the patience and perseverance required by the present initial scientific studies.

HUGO DE VRIES.

SCIENTIFIC BOOKS.

The Direction of Hair in Animals and Man. By WALTER KIDD, M.D., F.Z.S. London, Adam and Charles Black. 1903.

Dr. Kidd's recent work on the 'Direction of Hair in Animals and Man' is to a certain extent a compilation of his numerous previous works on the same general topic, to which is added a considerable amount of theoretical discussion. It is not intended to be an exhaustive treatment of the subject, but rather a discussion of those particular conditions which seem to substantiate the doctrines of Lamarck.

Three principles governing hair direction are pointed out:

1. That the simple and uniform hair slope of primitive mammals (*i. e.*, a general slope from cephalic toward caudal extremity of the body and from the proximal toward the distal end of the limbs) is not easily departed from in the individual development of any animal.

2. That there are certain modifications in this primitive arrangement that are due to morphological changes in the animal exhibiting them.

3. That all of the remaining phenomena of hair direction are to be explained by the action of mechanical forces on the surface of the body.

The first of these principles receives a brief discussion in which it is stated that the primitive hair slope corresponds to the direction of overlapping of the scales, which it is assumed covered the bodies of the earliest mammals. This law accounts for the slope of the major part of the hairy covering of any mammal. The existence of such a condition is