AN INTRODUCTION TO A BIOLOGY

A. D. DARBISHIRE

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AND OTHER PAPERS

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BY

A. D. DARBISHIRE

Author of "Breeding and the Mendelian Discovery"

This "Worke, although it may not so compleatly answer thy curious expect, yet let it suffice I have performed my best."

From the prefatory letter to the reader in "An Enucleaire, or Alphabet of Verbes, Neuters, Common and deponents, profitable and necessary to all students of humanitie. Likewise for parsing an helpe extraordinary. Collected and composed by WILLIAM DARBISHIRE, *Philosopher, London,* 1624."

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PREFACE

ARTHUR DUKINFIELD DARBISHIRE, son of Samuel Dukinfield Darbishire, M.D. Oxon., was born in 1879, and educated at Magdalen College School and at Balliol College, Oxford. He worked under Professor Weldon for the Honour School of Zoology, and was placed in the Second Class in 1901. In October of the same year he was appointed Demonstrator in Comparative Anatomy at the University. His interest in the problems of heredity was already beginning to absorb him.

At the instigation of Professor Weldon, the leader of the Biometric school at Oxford, he began a series of breeding experiments with mice, the results of which were published in *Biometrika*.[1] He had a profound admiration for Professor Weldon, and was, not unnaturally, influenced at this time by his hostile attitude towards the Mendelian School. But when he moved to Manchester, where he filled the post of Demonstrator in Zoology at the University from 1902 to 1905, he began to think out on his own lines the problems raised by the Mendelian discovery. Continuing his experiments with mice, he set himself to examine the truth of

[1] "On the result of crossing Japanese Waltzing Mice with European Albino Races." *Biometrika*. Vol. II., No. 1, Nov. 1902; Second Report, Vol. II., No. 2, Feb. 1903; Third Report, Vol. II., Part III., June, 1903; Final Summary, Vol. III., No. 1, Jan. 1904.

the position from which he had started upon them. He had begun as a pupil of the Biometric school with a strong bias against the Mendelian theory. He now worked his way, through difficulty and depression, to a point from which he saw that the contradiction between the two theories was only apparent and was really due to a difference in the point of view from which each party approached the same facts. He defined his position, and cut himself adrift from both schools in his contribution to the debate on heredity at the meeting of the British Association at Cambridge in 1904,[1] and in two papers contributed to the Manchester Literary and Philosophical Society, "On the Supposed Antagonism of Mendelian to Biometric Theories of Heredity" (1905) and "On the Difference between Physiological and Statistical Laws of Heredity" (1906).[2]

He maintained this independent and critical attitude all his life, and upon no man's work, whether of description or interpretation, did he keep a closer critical watch than upon his own. "One's attitude as an investigator," he wrote, "should be one of continual, unceasing and active distrust of oneself." His later experiments with mice, peas, fowls and rabbits were designed to test the Mendelian hypothesis with absolutely no prejudgment of the case. After thirteen years of patient investigation he could write in February, 1915, "I consider the Mendelian principles to be still *sub judice;* and they are so attractive by reason of their simplicity that they need to be under a very

[1] *Vide infra*, p. 162. [2] *Vide infra*, pp. 144 and 167.

stern judge." His book, "Breeding and the Mendelian Discovery," published in 1911, is a clear summary of his work and thought on the problems he had himself investigated. From 1905 to 1911 he held the post of Senior Demonstrator and Lecturer in Zoology at the Royal College of Science. It was at this period that he came in contact with Samuel Butler, first through "Erewhon," then through "Life and Habit," and his conception of evolution underwent a profound change. In his lectures thenceforward he traced the growth of the theory from the Greek philosophers through Buffon and Lamarck up to Darwin, and the philosophical bearings of the theory became for him its paramount interest.

In 1911 he accepted the newly created post of Lecturer in Genetics at the University of Edinburgh. The University Experimental Farm at Fairslacks under the Pentland hills gave him a new field for his investigations in heredity. His friendly association with Professor Cossar Ewart helped to make this post the happiest he had held. He had already become deeply interested by Bergson's thought when Bergson himself came to Edinburgh to give the Gifford lectures in the summer of 1914. He met and talked with Bergson, had the pleasure of introducing him to Butler's theories, and the rare experience of talking about his own speculations to a thinker who saw their drift and value.

At the opening of the war in July, 1914, he was in America, lecturing to the Graduate School of Agriculture at the University of Missouri, Columbia, Missouri. As a result of his lectures the University immediately offered him a Research Professorship at Columbia. He was also offered the Professorship of Zoology at the new University of Vancouver. But he could not reconcile himself to leave England after the outbreak of war. Owing to physical delicacy he was at first pronounced unfit for the Army, and he trained himself for munitions work at the Heriot Watt Engineering College. But he found difficulty in getting suitable employment, and in July, 1915, he tried his luck at a recruiting office and was enrolled as a private in the 14th Argyll and Sutherland Highlanders. He devoted himself to his duties as a soldier with the same zest and the same meticulous attention to detail that marked his work in other spheres, and he won the love and admiration of his comrades. On Christmas Day, in camp at Gailes, he was taken suddenly ill with cerebral meningitis, and he died next morning. Three days after his death he was gazetted Second Lieutenant in the Royal Garrison Artillery.

As a man of science he was perhaps unusual in combining a devotion to pure research (he would have laughed at the phrase, but all his colleagues applied it to him) with a freely speculative interest in his subject. He carried on his investigations with an untiring patience, watchful in self-criticism, recording his results with the greatest care and thoroughness, and persistently postponing interpretation until he had gathered more results. According to his principle, he maintained an absolutely dispassionate attitude as scientific investigator. Those who knew him only in other spheres would not easily believe that he could be dispassionate about anything. His interest in the philosophical basis of his subject was balanced by a close and intimate knowledge of its practical side. He hated and suspected all mutual exclusions, sharp antagonisms, supposed contradictions. Theory and practice were to him as closely connected as good with bad or science with art. He believed there was no such thing as the purely philosophical or purely scientific or purely practical sphere in his subject. He had a passion for gardening and for farming; he liked to be at work in the soil with the hoe or dung-fork, or amongst the animals on a farm. His thorough knowledge of horticultural processes, which contributed so much to the success of his scientific experiments, was due to his own experience in all the arts of the practical gardener. Digging was an art which he carried to perfection after years of practice.

His delight in doing things as well as they could be done gave him unusual powers as a teacher. He took an unconcealed pride in his skill in dissection and in elaborate draughtsmanship. In the laboratory he was always working and learning, and his idea of teaching was not to give information (he often refused it), but to show people how to find out things for themselves. As a lecturer his best qualification was his absorbing interest in the things he had to say. His mind was never more clear, alert, and alive than when he talked to intelligent listeners. He had the instincts of an actor and a good deal of the art; and he could get in touch with his audience at once.

The three greatest influences over his mind and thought were Beethoven, Samuel Butler, and Bergson. They had in common two things that he could not do without: humour, and the sense that the source of life is spirit. Their influence was the quickening influence of one personality on another, and they were a constant stimulus to him in his later speculations, which became increasingly metaphysical in character. He chafed against the orthodoxy of science, and his cherished desire was to make a contribution towards biology in the strict meaning of the term. The whole field of life should be its sphere, he thought; its basis should be philosophical and its method dispassionately critical; and finally, the spirit in which the biologist approaches his subject should be the same spirit of intense interest combined with humility, with which a lover of music hears a symphony of Beethoven, or a lover of life meets one of the transcendent experiences of life itself.

In character he was essentially childlike; generous to a fault, with no arrogance, no malice and no meanness. He had a genius for absurdity, and he used it, as he used his other gifts, with the delight of a child and the skill and thoroughness of an artist. He never made enemies, and he had an infinite capacity for making friends. The men who helped him with his experiments, his laboratory assistants, his gardener, the farmer at Fairslacks were all to him fellow-workers and friends, to whom he delighted to express his gratitude, and with whom he shared, as far as he could, his jests, his interests, and his ideas.

All who knew him will keep in memory a personality alive and young to a rare degree, fulfilling itself in a passion for music, much laughter, a perfectly disinterested love of truth, a delight in producing delight in others, and the keenest possible interest in life itself whichever way it led him.

During the spring and early summer of 1915 my brother was at work upon a book for publication by Messrs. Cassell in the autumn. In May he writes to M. Bergson, "I am hard at work on my book about evolution. I think I am going to call it 'An Introduction to Biology'—Biology as I think it ought to be, not Biology as it is. For the problem of evolution is merely a department of the problem of life, it appears to me. I am convinced that the theory of natural selection is in no sense an explanation of evolution."

The plan of the book is sketched in a prospectus written for the Publishers in June, 1915.

"An Introduction to a Biology

"This book is addressed to all those who are curious about the meaning of life, and is an attempt to put before them the essence of the current mechanistic explanation of the organism and materialistic explanation of the universe, in order that they may examine for themselves these accepted scientific explanations of life. The author does not pretend to set forth a series of conclusions led up to by the stages of a logical argument which follow inevitably and relentlessly from one another, and which compel the reader to reach those conclusions and no other. The reader is not offered a set of irrefragable laws which he must believe, but certain preliminary considerations which, in the opinion of the author, effect a clearing of the ground upon which the foundations of a biology may some day be laid.

"The term Biology is used by the author to signify the interpretation rather than the mere description of life. The Biology, therefore, in the interpretative sense, put before the reader is not a nearly complete fabric the main constructional lines of which have been laid down once and for all, and the completion of which will consist in the filling in of detail; but a tentative indication of the direction in which some approximation to an understanding of life may be sought.

"The main constructive thesis of the book is the idea, which we owe to Samuel Butler, that the details of the process of evolution can be studied most minutely in man, in whose extra-corporeal organs, his weapons, implements, and machinery, evolution is proceeding with great rapidity.

"This study of the evolution of human detachable implements leads on to M. Bergson's thesis that the human intelligence owes its essential traits to the fact that it was developed *pari passu* with the acquisition by man of his control over matter. Upon this conception of the human intelligence rests the main critical thesis of the book, which is the attempt to show that natural selection was acceptable as an explanation of evolution, because it was an explanation in terms of matter, and because it involved the idea that the organism is no more than a machine."

The book was to consist of four long chapters, the contents of which were to be as follows :

I.—The failure of modern interpretative Biology.

II.—The utilitarian origin of the human intelligence.

III.—The consequent acceptability to many minds of a mechanistic theory of the organism and of a materialistic theory of evolution.

IV.—Some suggestions as to the direction in which an understanding of life may be sought.

When he joined the Army two chapters were written, the first revised, the third begun. The fourth chapter, which was to have been the constructive part of the book, is entirely unwritten. In his pocket-book he sketches the plan of the four chapters characteristically in the form of a Beethoven symphony :

Chapter I.—1st Movement.

Chapter II.—*Scherzo.*

Chapter III.—*Adagio.*

Chapter IV.—*Finale.*

Amongst his papers the only direct indications I can trace of the contents of the final chapter are in a note to Professor Dendy, and a letter to Mr. Charles Douglas.

The first, dated June 17, 1915, is a request for a Table at the Marine Biological Laboratory at Plymouth for August. "I wish," he writes, "to make one or two drawings and some observations on the habits of *Carcinus maenas* for the purpose of my new book appearing in the autumn."

I imagine that he intended to support his general belief in the part obscurely played in evolution by the will and intelligence of living things, with records drawn from a close and intimate study of the habits of a particular animal.

The second is a description of a paper which he sent to Mr. Douglas, March 10, 1915, with a view to its publication in the "Transactions of the Highland and Agricultural Society." "The greater part of it," he writes, "is a lecture which I gave to a combined meeting of the Scientific and Agricultural Societies in the University of Aberdeen. It contains in crude form ideas which I hope to express better in about two chapters of a book I hope to have ready by September."

The extract in question is printed (*vide infra*, p. 90) in an Appendix immediately after the unfinished fragment of "An Introduction to a Biology," and I have added to it some notes of a lecture delivered in June, 1915, which illustrate in a technical way one important point raised in the extract. I had also intended to include in this Appendix a letter to M. Bergson, and a reply from him of December, 1912, which touch upon the intended subject-matter of the book. My brother's letter is not to be found, but M. Bergson has most kindly allowed me to print his reply, which suggests its contents.

For the rest, to attempt even an outline of the concluding chapter which my brother left unwritten would be manifestly useless. I have thought that the best thing I could do was to gather together those of his papers, whether published or unpublished, which throw light on his thought and explain his point of view. In the Collection of Notes and Extracts (*vide* p. 104) I have put first in order those notes which were made whilst the book was in progress and which have a direct bearing upon it. The scientific papers are arranged chronologically. As a record of his thought the whole is in one way absurdly incomplete. For except under compulsion he wrote little. He did his thinking in his head, and his papers and lectures, especially in recent years, were given *ex tempore*, often without any notes at all. He was always loath to set down conclusions, for his mind was alive and growing, and conclusions were after all, perhaps, what he never hoped to reach. For all its incompleteness, then, this book may give in some degree a true picture of his mind.

I wish to thank the following for their courtesy in allowing me to reprint papers of my brother's: The Editor of *Nature*, the Editor of *The English Review*, the publisher of *Science Progress*, the Editor of *The Times Literary Supplement*, the Council of the Manchester Literary and Philosophical Society, the Committee of the Manchester University Biological Society, the Editor of the *Manchester University*

Magazine, the Directors of the Highland and Agricultural Society, The President and Council of the Royal Horticultural Society.

I am exceedingly grateful to Professor Curtis, Professor Reed, and Miss M. L. Keene, of the University of Missouri, for their generous help in placing at my disposal their notes of my brother's lectures delivered at Columbia in 1914.

Professor J. A. Thomson, of Aberdeen, has had the kindness to read the proofs of the first part of the book, and I have greatly benefited by his knowledge and insight in a number of corrections.

I owe a special debt to my friend Mr. A. D. Lindsay, of Balliol College, Oxford, for his help and advice throughout the preparation of the book.

HELEN DARBISHIRE.

BOAR'S HILL, OXFORD
August, 1916.

TO

ROBERT BUCHANAN KING

THIS BOOK IS

DEDICATED

An Introduction to a Biology

CHAPTER I

'*You who speculate on the nature of things, I praise you not for knowing the processes which nature ordinarily effects of herself, but rejoice if so be that you know the issue of such things as your mind conceives.*"—LEONARDO DA VINCI.

§ 1. Man is the best starting-point for the study of life.—§ 2. Scientific investigation is the human activity, the critical study of which is the most urgent.—§ 3. Scientific investigation consists of description and interpretation.—§ 4. Interpretation. Biological Laws: the urgency of locating them.—§ 5. Words: the necessity of keeping a sharp eye on the wanderings of their meanings.—§ 6. A glance at our present interpretation of life from the historical quarter.—§ 7. On the fitting of theory to the facts of life.

§ -

THE function of biology, if we adopt the literal, etymological meaning of the word, is to describe and interpret the essential manifestations of life, and to extract from these interpretations a conception, or theory, of life. But the word " biology " has come to be used in certain very much restricted senses, of which it will suffice to mention two. In its commonest signification it merely serves as a convenient common term for the subject matter of both botany and zoology. Another common meaning of it is the study of the habits of a particular animal or plant. With regard to this latter meaning of the word, let it be noted that it applies to the behaviour of any animal except man himself. With regard to the former, the most common meaning of the word, the reader may object that biology, dealing as it does with the comparative anatomy, development, evolution, and many other aspects of animals and plants, so far from dealing with a restricted set of phenomena, ranges over the whole field. There are no other living things than animals and plants ; the study of animals and plants is the province of biology ; therefore biology covers the whole field of life.

The answer to this argument is that the biologist in his efforts to survey the whole field has forgotten one animal, himself. In this way he has committed himself by long habit to the study of those living things of which his knowledge must perforce be external, and has shut himself off from the study of the one living thing which he could know intimately. He has preferred to "cover the whole field" rather than to dig in the spot where, in my belief, the return for cultivation would have been greatest. The course taken by the biologist may be illustrated by the difference between a recent and an early meaning of the verb "to manure." In one of its original, literal, and etymological significations it meant to work the soil with the hand (that is to say, with the spade, which is an extended, detachable part of the hand) ; but the meaning gradually shifted outwards from the process itself to the fertilising effect which the process had upon the soil ; and thence it spread to the other means (besides working with the hand) by which this effect could be produced, namely, the spreading of dung over, and its subsequent incorporation with, the soil. The biologist has chosen to "cover the field" rather than to dig in that little plot, the study of himself. He has preferred extension to depth, and in so doing he has followed the line of least resistance. Anyone can spread dung; it takes a man five years to learn to dig well. Man's attention is so accustomed to look outwards, that it is very difficult for him to turn it inwards upon himself. And so the fertile plot has been very much neglected.

A variety of circumstances has conspired to exaggerate this *extensive* character of biological knowledge to such a pitch that in his anxiety to leave none of the field uncovered the biologist has become careless as to whether the covering is thick enough to be of any use. In choosing subjects for investigation he has set more store on the novelty of the result than on the question whether his results will bring us closer to an understanding of life. And the result of this aiming at the target of novelty has often been that he has hit the bull's-eye of triviality.

This desire for novelty has become so great that there is a danger of the convention growing up that a particular animal or problem is one man's preserve, to trespass upon which shall be a breach of scientific etiquette. And, as it is, the fact that somebody has worked out the development of a particular animal is regarded as an argument *against* the selection of that animal as a subject of embryological research ; but the truth is, surely, that this fact should be an argument *for* such a selection. For in studying again a development which had recently been studied, the investigator would be throwing light upon two problems instead of only one : upon the meaning of the changes of form undergone by the animal in its development ; and upon the trustworthiness of the human intelligence as an investigating mechanism ; and probably more light upon the second than upon the first. For if the investigator made a point of looking neither at the description made by the previous investigator of the development, nor at his interpretation of the facts of which this description was a record until *after* he had completed his own description and interpretation, he would obtain results which would be interesting both to the embryologist and to the philosopher. The solitary confinement in their own studies and laboratories, and the complete isolation of biological workers all over the world, would doubtless be a hindrance to progress in biological inquiry, at any rate on its applied side ; but it is very doubtful whether the opposite extreme, the perfect means of inter-communication now available, is not an equally serious obstacle to such progress.[1] For it ensures the spreading thin rather than the digging deep.

It is not, of course, true that the biologist has entirely excluded man from his attention. He *has* studied him ; but he has done so in the same detached, objective way in which he has studied the

[1] The probable cessation of communication between the countries now at war may effect a partial removal of this obstacle.

other animals. His knowledge of the form of man is derived from corpses; and he investigates his performances on the assumption that the human body is a machine. His attention has been confined to just those attributes of man which can be studied in the same way as those of non-human animals.

This detached, objective method is perhaps the way to establish a science of human physiology and anatomy which shall be useful in the practical science of medicine; but as long as the biologist persists in studying man in the same impersonal way as that in which he studies the other animals, so long will he shut out of his field of inquiry a whole set of vital phenomena. And the manifestations of life thus excluded from investigation are just those which, in my opinion, most deserve the close attention of the biologist at the present time.

The eye of the biologist has ever been turned outwards to the pageant of living things by which he is surrounded. He has forgotten that though he is a spectator, and probably the only spectator endowed with intelligent curiosity, he is also a performer in this pageant.

It is my belief that, so far from excluding from the sphere of inquiry those manifestations of life which can only be studied in man, we should give them a position in the full limelight of our interest. No biology can lay claim to completeness which leaves out of account such essential manifestations of life as human invention, self-expression through painting, poetry, or music, activities and aspirations which constitute the very life which each one of us leads. Surely the way to find out about life is to study life at work; or at play; at any rate, in action. There is no other field at all comparable with that of human activities for such a study. It is only in human life that we can approach close to, and study minutely the very nature of, existence.

§ 2

But there is one human activity which stands out from the rest and demands the most severely critical study. This is the process of investigation itself. It seems to me that one of the most important branches of biology should be a critical study of the process of investigation. It is a perfectly legitimate branch, because this process is indubitably a manifestation of life. It is, moreover, a branch which requires the most careful attention, because if the process of investigation be unsound the results obtained by it will be worthless. Man's theory of life, his biology, is the product of his imagination. Is it not high time, therefore, that he turned his eye inwards and pondered upon the relation between himself and the objects of his investigation? It takes two to make a science. There are two parties to the bargain: the mind of the investigator and the things investigated.

The attention of the investigator perpetually flows outwards, intent upon its object; it does not stop to look at itself; it is all taken up with its prey; none of it is left over to be devoted to its source, the mind. The interests of the investigator do not lie within, but without. He has no misgivings as to the reality of the outside world or the soundness of his explanations of it, because he has no misgivings as to the efficacy of his perceptive and interpretative machinery. Things as *he* sees them are things as they *are*, because his perceptive machinery is infallible. The causes of things as *he* elucidates them are the true causes, because his interpretative machinery is flawless. That, at any rate, would appear to be the tacit assumption upon which he acts.

The reader may object that it is the business of a philosopher and not of the biologist to deal with the process of investigation. But if the philosopher to whom the work is given is not an active investigator, what can he know about investigation? The eye of the philosopher is turned inwards. His interests are within. He is interested in the outside world only as the material upon which his mind feeds, or as a mirror in which it can see itself.

It seems likely, therefore, that that most interesting region which intervenes between the mind and things will fall between two stools. The investigator is interested in the things around him: the philosopher in the mind within; but very few are interested in the relation or interaction between the two. Now the process of investigation is a reciprocal action which takes place between the two poles, mind and object, across the region which intervenes between them. This intervening region is apt to be neglected because men tend, according to the inborn direction of their minds, to cluster around one pole or the other. It must, however, be admitted that there is more of the investigator in the philosopher than of the philosopher in the investigator. This is only natural. We shall see later on that it is the essential characteristic of the mind of man to extend its operation farther and farther outwards towards things. It is more likely, therefore, that the interest of the philosopher should extend outwards across the intervening region towards things, than that the interest of the investigator should extend inwards, across it, to the mind. For both are men.

To the business of investigation there are, as I have said, two parties, and it is especially desirable that the investigator should have his attention drawn to the one in which he is least interested (if indeed he ever gave it a moment's thought), namely, his own mind.

It must be remembered that in our attempts to solve the problems of nature we are not in the same relation to nature as a boy answering an examination paper is to the examiner. We are answering questions in an examination paper which we have ourselves set. The formulation of the problem, in however honest a mind, must involve some dim prevision of its solution. The reader may object that the problem is not formulated in the mind, but set by nature. I venture to think not. What appears to me to happen is another result of that ever outward-streaming nature of our interest, to which reference has already been made. The problems originate in our mind, but no sooner have they taken shape than, all unknown to us because we will not turn the eye inwards and keep guard on our mind, they are swept out, by the current of our interest, to the sea of things. When they have arrived there we become conscious of them for the first time, and so we think that we have found them there.

So strong is this outward current that even the human voice is allowed to be carried out by it. "The facts speak for themselves," we say. But it is an illusion; at any rate, a dangerously misleading figure of speech. Facts do not speak. If the reader should answer and say, "Well, at any rate they speak to me," I would come out and meet him on the same ground and reply, "Very well, then; so do they speak to me, but they do not say the same thing." Facts are like the dolls of the ventriloquist and say what we want them to. Life in the hands of the too self-confident biologist is a very docile doll in the hands of a very skilful ventriloquist.

The discomfort felt by the investigator when he is asked by a layman who is taking, or trying to take, or pretending to take, an interest in his work, and asks him, "What are you trying to prove?" is due to the fact that he sees that this question, unknown to the asker of it, reveals to him what he really is doing. He *ought* not to be trying to prove anything, but to find something out. But very often he knows in his heart that he hopes very much that his pet theory will be proved true by his investigations. And even if he answers, "I am verifying certain hypotheses," he condemns himself out of his own mouth. For "to verify" literally means "to make true."

§ 3

In scientific investigation there are two processes. The phenomena are first to be observed and described; and then, when this is completed, an attempt is made to interpret them. But it is very rare that this chronological sequence is strictly adhered to by the investigator. It is very rare that, as a necessary preparation of himself, he deliberately purges his mind of any preconceived interpretation of the phenomena he is about to describe. Yet if his description is to be unbiased and uncoloured by any interpretation, it is obviously necessary that he should do this. I fear that Interpretation (if I may personify that process for a moment) at the threshold of a piece of research does not hold back and say to Description, "Allow me—Description first." Both rush in together, without ceremony, hand in hand. For they are inseparable companions. And the result is that in the work of Description the hand of Interpretation can always be traced.

In its simplest and least dangerous form the influence of interpretation upon description is seen in the work of the budding investigator, when he first embarks upon a piece of "original research." The subject of inquiry is usually suggested by the lad's teacher, who, in the worst cases, wishes the results of the inquiry to point in a particular direction, and does not conceal his wishes from his pupil, and who in all cases indicates the lines along which, and the methods by which, the research should be carried out. But this is not a dangerous form of the effect of interpretation upon description; because the young investigator soon comes to have views of his own, and rebels against his master. The effect becomes dangerous when it is one's own unconsciously entertained interpretations, which, unknown to us, hold our hand and direct the pencil which we fondly believe to be carrying out the pure, unadulterated work of description.

Instances of the way in which the work of description, which is the recording of observation, can be distorted by preconceived interpretation will crowd up into the mind of the reader. So great an observer as Buffon, who was an upholder of the doctrine of *evolutio*, according to which the adult animal existed in miniature, but complete in every detail, in the egg, said, "I have opened a great many eggs at different times, both before and after incubation, and I have convinced myself by actual observation that the chicken exists, in its entirety, in the middle of the spot on the yolk, at the moment that the egg leaves the body of the hen."

The improvement of scientific instruments since Buffon's day has doubtless reduced the margin of error, but it has not prevented that which has been seen by means of these improved instruments from bearing a strange resemblance to that which the interpretation already in the mind of many a contemporary observer made him expect to see.

We have seen the manner in which description may be affected by interpretation. We may now glance at the way in which interpretation may be affected by description.

The reason that the investigator has so little misgiving as to the correctness of his interpretations is that, however much he ought to have, he has no misgivings about his observation, nor about his descriptions of things, and we have seen that he does not, as a rule, keep observation and interpretation separate in his mind. So that observation merges imperceptibly into interpretation. No line is drawn to show where the one ends and the other begins; each becomes interpenetrated by the other, and we come to exist in the pleasant delusion that interpretation is conducted in the same bright light as that in which the work of observation is carried on. A curious instance of this mutual interpenetration was afforded by the attitude of the Mendelian and biometric schools to the Mendelian phenomena of heredity, and the interpretation of these phenomena. The Mendelian school were so saturated with certainty as to the reality of the Mendelian phenomena, by daily contact with them, that their certainty spread across the border of ascertained fact, well into the territory of hypothesis. They could not understand how anyone who was familiar with the facts could doubt the hypothesis. The minds of the biometric school, on the other hand, were so full of scepticism as to the hypothesis that they looked with great suspicion upon the facts.

§ 4

The interpretation of a phenomenon consists, in current scientific phraseology, in discovering the laws which govern that phenomenon. If it be the case that the discovery of these laws is the goal of scientific investigation, it is manifestly desirable that we should have a very clear idea of the nature of these laws. It must be admitted at once that nothing could be better adapted to the obscuring of their real nature than the phraseology of scientific textbooks.[1] We are told that such and such phenomena are governed by such and such a law and that this law was discovered by such and such a person. We should conclude from the word "govern" that phenomena are like marionettes whose performances are regulated by things which we call "laws" behind them—at any rate, distinct from them. The discovery of these laws means, literally, the uncovering of them and, as a sort of corollary, the exposing of them to human gaze. This discovery is not supposed to take the form of an actual piercing of the ranks of the marionettes to the laws behind, but to consist in a deduction of the nature of these laws from the performances of the marionettes. But why this delicacy? Why cannot we go behind the scenes and have a look at these laws, and see what they are like? Why should there be any mystery about them? If we are kept in the dark like this we shall begin to think we have been imposed upon.

The truth, in my belief, is that we have been

[1] "The strong family resemblance which is seen both in the human species and amongst animals related to each other is a direct consequence of the existence of the first law of breeding—viz. that 'like begets like,' or 'tends to produce like.' Other laws are in operation at the same time; consequently when an organism comes within the immediate spheres of their action the effects of this and various other laws are modified to harmonise with surrounding conditions." [I have been unable to trace this quotation.—Ed.]

imposed upon. The reason that we are not allowed behind the scenes is that there are no laws to be seen there. But the imposition is the offspring neither of malice nor of fun, but of thoughtlessness. It is the result of preoccupation with the results of investigation, and of inattention to the process of investigation itself.

A little self-examination would reveal the fact that these laws exist, not in the world outside, but in the mind of the investigator. When the biologist —for I am dealing in this book only with the investigation of vital phenomena—thinks or speaks as if he thought that he was discovering the laws determining a set of phenomena, he is, in point of fact, formulating in his own mind a law to express certain sequences or regularities that he observes in these phenomena. It may be objected here that this is common knowledge, and that such phrases as "discovering the laws which govern a set of phenomena" are merely figures of speech, and that no one ever really supposed that phenomena were "governed" by laws or that these laws were subsequently "discovered" by investigators, in the literal sense of these words. Certainly, such phrases are figures of speech; but in my view they are very bad ones, and ought to be dropped. They are bad because they encourage the mind to travel in the direction in which it likes to travel—outwards. These laws are formulated in the mind; but as no watch is kept on the mind they have been caught up in the stream of man's interest and swept outwards to the nether side of things.

If the reader's patience will allow me, I will try to give some idea of the picture called up in the mind, by current scientific phraseology, of the nature and whereabouts of natural laws, by describing the theory of music which I held when I was a boy of about fourteen. At that age music meant to me simply a tune. I thought that a succession of sounds was either a tune or not a tune. If it was, it was part of the order of nature; if it was not, it was simply a noise. Tunes did not differ in merit. Who was I that I should presume to discriminate between things which were not the work of man? I only knew two tunes, "Pop goes the weasel" and Sullivan's "Prithee, pretty maiden, will you marry me?" I thought there existed in the world a certain limited number of tunes; I do not think I speculated as to their origin, or as to where the undiscovered tunes were. But I remember thinking with alarm that if many more men like the discoverers of "Pop goes the weasel" and "Prithee, pretty maiden," arose, all the tunes would soon be discovered. For I wanted very much to discover a tune myself, but I did not know where to look, or how to set about the search; moreover, I had no means of knowing whether, or not, all the tunes had been discovered; in which case it would be no good looking. I thought that all known tunes, together with those which might be unearthed later, had existed for all time, and had, in the literal sense of the word, been *discovered*—that is to say, located and brought to light by a select few who knew how to do it. This is the first stage in the history of my theory of music.

The picture of the laws of Nature which I saw in my mind's eye when, as a young man, I read in scientific books that such and such a person had discovered the laws governing such and such phenomena was very like my boyhood's notion of tunes. I had no reason for supposing that those who wrote what I read had not thought carefully over the meanings of the words they used. I took what I read to be literally true ; I believed that phenomena were governed by these laws ; that they really were passive agents in the hands of the powerful laws which controlled them. These laws had existed from all time, and were part of the order of the Universe. The law of Natural Selection had been discovered ; but there might perhaps be one or two laws of heredity still undiscovered. And just as I had wanted to discover a tune, so, now, I wanted very much to discover a law. In my innocence I believed that if I " discovered " the law " governing " a particular phenomenon, I should have got to the bottom of that phenomenon.

The second stage in the history of my theory of music consisted in abandoning the view I have described, and in rushing to the opposite extreme of believing that tunes were simply invented by musicians, " out of their own heads," as one would have expressed it. This view raised the question why it was that some men could put together notes which the world would recognise as music, whilst others, amongst whom I was compelled to recognise myself, could not. But I imagined that if anyone chose to take the trouble to learn harmony and counterpoint and the rest, and contrived to construct a symphony which he could entice people to listen to now and again, then in the course of time his hearers would get to know it, or rather to recognise parts of it when they heard it again; and that in this way a man's compositions would gradually come to be recognised as music.

[According to this view it will be seen that music was pure invention.

The second stage in the history of my conception of natural law was analogous to the second stage in the history of my conception of music. In it I believed that natural laws were pure inventions of the human mind, and projected by the mind, or rather allowed to escape from the mind, into things.

My third and present stage with regard to music is, to a great extent, a return to the first stage. Music can convey to us part of the order of nature, that part which is alive. The great musicians are the springs through which this message wells up, for us.

My third and present stage with regard to natural law also returns to the first stage, but it returns a very much shorter distance. I believe that the present system of scientific laws relating to life is very much further from portraying the essence of life, than is, for instance, the eighth symphony of Beethoven. But a theory of life cannot be communicated at present by music, and one must make an attempt to communicate it through the inadequate medium of words. The point to which we have now reached in this attempt is that from which we see natural law to be, to a very great extent, a product of the human intellect.][1]

[1] This passage is half deleted in the MS., and was clearly intended to be re-written.

The truth of my contention that biological laws really exist in our own mind, and not in the world outside, governing phenomena and awaiting discovery, is capable of a simple demonstration. If the discovery of these laws really were a detection of the principles underlying phenomena, as the conventional phrase is, and if the deeper we probed the more closely did we approach to fundamental principles, then the deeper we probed the more closely should we agree with one another. The very reverse is demonstrably the case. Our agreement is not directly, but inversely proportional to the depth to which we probe. The truth as it appears to me is, that we are not really burrowing under phenomena at all. We invent laws in our minds about phenomena. Then, looking the other way—i.e. not at our minds—we allow these laws to escape to the other side of, or underneath, the crust of phenomena. And then we experience the thrill of thinking that we have discovered these laws underlying phenomena. The reason that the deeper we probe the more do we disagree, is that the laws that we think we find down there, but really project there, are the products of our minds; nay, almost the portraits of ourselves; for the truest portrait of a man is his conception, or theory, of life. If it be asked why there are not as many laws of, say, heredity as there are men who are interested in it, the answer is that the great majority of these men are content to take their laws secondhand from other men.

In a word, the fact that two men looking, as they believe, below the surface of one and the same phenomenon, can " discover " there two profoundly different laws to explain it, seems to me to prove pretty conclusively that these laws are not detected underneath the phenomenon but projected below it by the men themselves. What a man sees below the surface when he looks down the Holy Well into the waters of Life is not the bottom of the well, but a picture of himself.

There is another reason why laws should have flown out from the mind to the nether side of phenomena. The savage's interpretation of the Universe endowed everything—river, tree, and storm—with souls, and ascribed the form and the performance of these to the nature and activity of their souls. As this interpretation came gradually to be discarded in favour of " scientific " explanations of things, a void was left behind the pageant of phenomena ; and to this void it was natural that the new explanations of things should be drawn.

So that when we feel inclined to congratulate ourselves on our emancipation from the crudities of an animistic interpretation of the Universe, we should ask ourselves whether in fact we have done any more than hand over the government of the Universe from a hierarchy of spirits and demons to one of principles and laws. Very little more, so far as life is concerned, I venture to think. Many an innovation in the past has been no more than the calling of an old thing by a new name.

§ 5

If there is a possibility that words may give a semblance of progress in interpretation, where in reality there is none, it is desirable that some attention should be paid to the relation between words and thought.

Ideally, words are the medium for the communication of thought; thought is the master and words the servants; thought is wealth, words the coinage which facilitates its exchange. But actually it is not so. By subtle and imperceptible manœuvres the word has often got the upper hand, so that thought has come to be at the beck and call of that which should be its slave.

It is probable that thought and language mutually interpenetrate each other, so that at the one extreme there is pure language and at the other extreme pure thought; in the middle an equal mixture of the two; and halfway between the middle and thought, a preponderance of thought over language; and half-way between the middle and language, a preponderance of language over thought. But for purposes of exposition it will be necessary to make an arbitrary excision of the intervening region, and to speak of thought and language as if they were distinct, mutually exclusive things. The problem of the relation of language and thought resolves itself when, reduced to its elements, into the problem of the relation between the word and its meaning. Here again, though the word and its meaning are mutually interpenetrating and interpenetrated, it will be necessary, for the same reason, to treat of them as if they were mutually exclusive.

Before I address myself to the problem I would like to say a word to any philologist who happens to read these words. I can see the smile on his lips at any attempt to deal in a parenthesis with so vast a subject as the relation between language and thought. But if I may explain, I think I can turn the smile from one of contempt to one of at least indulgence. I, too, am appalled by the task of understanding clearly the relation between thought and language. It is because I am thus appalled that I realise that anyone to whose mind the problem of this relation has not yet even presented itself is in grave danger of thinking there are no difficulties because he does not see them. A word and its meaning, especially in the case of ideas (with which, in this book, we have to deal), are united together by a slender, elastic bond which is now contracted, now stretched to its uttermost. The word, if we consider its whole life since its dim origin, has been perpetually changing; so too has its meaning; little in the case of things, much in the case of ideas. So we see the word and its meaning dancing to each other in an airy medium, like a pair of gnats in the lee of a gorse-bush. This, alas! is the simplest case. The more complicated and much more common cases are those in which one word has more than one meaning, or where one meaning has more than one word to express it; these are the cases which, in verbal life, are productive of trouble. I have attempted to condense into one simple picture the relation between words and their meaning. I have tried to convey my conception by a picture because it is a picture of a thing which is far less liable to change than the relation between an idea and the word by means of which it is handed from mind to mind. It may be a poor picture. I do not ask the reader to admit that it helps to make clear the relation between the word and the thing. I ask him to admit that the fact that the difficulty of visually disentangling the movement of a pair, or a harem, of gnats does not present itself to the mind of an ostrich who has buried his head in the comfortable sand of matter hard by, does not prove that this difficulty is non-existent. *De non existentibus. . . .*

The phenomenon of which I have given a picture is the relation of the word and its meaning regarded from a point of view which envisages the whole evolution, the whole life of the word and its meaning from the time of their origin. That was perhaps too much to attempt. Let us examine the changes which take place in this relation from a nearer point of view which only envisages a period of time so short that the word has undergone no changes, or only insignificant changes, in that period. From this point of view the word is seen to remain fixed. When the word denotes a thing like bread, the thing does not change very much more than the word which denotes it. That is to say, the names of *things* fit closely to them; and the word "bread" calls up in the mind the image of bread. But ideas, attributes and processes are not held closely by the words which denote them; they are attached to the word by a bond which varies in length from case to case, but is usually fairly long. The word is the fixed point, the peg to which the leash is attached. The meaning, in such cases, is the sportive goat who is free to wander anywhere within the circle, the radius of which is the length of the leash, and whose chief delight is to browse off and break through the hedge intended to confine him to his proper sphere, in the event of the leash being too weak for its purpose. Do I hear the criticism that I am merely juggling with words? My answer is that I am trying to prevent them juggling with me—not juggling in an active sense, but in a passive one, like a chaperon who lets her charges go where they like, and by not following them, leads them into trouble.

The wanderings of the meanings of a word may be followed in great detail in that fascinating work, the Oxford English Dictionary. The word "curious" was used in its Latin form only in a subjective sense, meaning full of care or pains, careful, assiduous. Amongst the subjective meanings which it came to have later were anxious, concerned, solicitous, careful as to the standard of excellence, difficult to satisfy.

The sense in which I intend the word to be understood on the title-page of this book is one which includes all these meanings. The degradation of the meaning of a good word like "curious" is a common tragedy of verbal life. Its earliest meanings were qualities of the mind which were worthy of nothing but praise. It is now and has long been used in an objective sense merely to denote unusualness in the form or colour of objects.[1] And an attempt has been made to blacken the character

[1] In the "Philosophical Transactions of the Royal Society for 1703" (Vol. XXIII., No. 286) there is "*A* Description *of some* Corals, *and other curious* Submarines *lately sent to* James Petiver, *Apothecary and* Fellow *of the* Royal Society, *from the* Philippine Isles, *by the Reverend* George Joseph Camel."

of such of the meaning as has remained in the mind, in order to stigmatise it, as it were, for its lack of taste in remaining subjective. But in spite of this attempt, its subjective meaning still retains a certain dignity and independence of its own. Curiosity signifies an interest in things which convention has pronounced to be outside the sphere of the legitimate interest of mankind, in things which a conspiracy of silence chooses to ignore. A convention of Modern Science, for instance, is to regard the prying into the workings of the human mind as mere metaphysical curiosity, and the products of such researches as mere metaphysical curiosities. Sir Ray Lankester, K.C.B., F.R.S., for example, in his introduction to "Modern Science and the Illusions of Professor Bergson," says: "I am glad to write a few words by way of preface to Mr. Hugh Elliot's valuable little book, entitled 'Modern Science and the Illusions of Professor Bergson.' I am glad to do this, not merely because I think that the books in which M. Bergson formulates these illusions are worthless and unprofitable matter, causing waste of time and confusion of thought to many of those who are induced to read them, but also because an unmerited importance has been attached to them by a section of the English public, misled by the ingenious and systematic advertisement of M. Bergson by those who amuse themselves with metaphysical curiosities."

Curiosity is a defiance of the conspiracy of silence, a defiance urgently needed when the topic which has been pronounced taboo is the operation of the human mind. The answer to the question "Why is this topic taboo'd by Modern Science?"

is that the eye of science can only look outwards; and the scientific mind regards the philosopher whose eye can look inwards as well, and at all the points along the line between mind and things, merely as a monstrosity.

In the passage quoted above curiosity is probably used in an objective sense to mean the products of subjective curiosity. In the passage which follows the word "curious" is also used in its purely objective sense: "To those who in a thoroughgoing way occupy themselves in collecting and comparing and classifying all the absurdities which have been put forward as 'metaphysics' or 'metaphysical speculation' since the days of Aristotle, this latest effusion has, no doubt, a kind of interest such as a collector may take in a curious species of beetle. To the student of the aberrations and monstrosities of the mind of man, M. Bergson's works will always be documents of value."

The above might be taken as a text for a lecture on the purely objective nature of our interest in life. A curious beetle means a bizarre beetle. It does not mean a fastidious beetle.

The tragedy of the word "curious" is the result of the outward-streaming nature of man's interest. Most of the meaning has been caught up in the current and swept out from the mind, and has made its home amongst things. And even the noun from which the adjective curious is derived has been swept outwards too. When the adjective curious detached itself from its parent substantive, cure meant care. Thus cure first meant the care of the healthy; then the successful treatment of the sick;

and lastly the drug supposed to drive away the malady. We speak of and even believe in a cough cure. So different a meaning has "cure" come to have from that with which it began, that we could say "better use cure in the old sense than rely upon cure in the new."

A word, the meaning of which is very active at the present time, is the verb to "locate." It is used in one sense in Britain, and in another sense in America, where its evolution has been more rapid; but there are signs that the American meaning of the word is spreading eastwards across the Atlantic. To locate, in the British sense, is a transitive verb with a very well-defined, particular meaning; it is the name for a purely subjective process, which takes place in the mind—namely, the finding out of the position of something that Nature or man is trying to conceal from you.[1] By saying that it is a subjective process I mean that, though its immediate object is external to the mind (such as a short circuit in an electrical system, or a malignant growth in the body), the next process is within the mind—namely, the thinking out of some plan of action to be taken when the trouble has been successfully located. In America, however, the idea denoted by the word "to locate" has, first, become generalised and enlarged to mean simply "to find." I was asked last July in St. Louis if I had located a satisfactory natatorium in the town. But the meaning of the word has gone much further than this; it has broken right away from its original

[1] "A useful seaplane reconnaissance located several encampments and two permanent batteries."—*Scotsman*, March, 1915, *re* Dardanelles.

moorings which confined it to the mind, and is used in the passive mood, as synonymous with "situate," to denote a purely objective state. The Century Club is located in West 42nd Street in New York. In this sense it is an intransitive verb; and although in this sense it is generally used in the passive mood, it can also be used in the active mood, so that one can say that one is going to "locate" in Milwaukee. What has happened is this. At one period during its existence on the American continent this word, which originally applied to a process which took place in the mind, extended its application to processes—or rather states—which are outside the mind. Its meaning, like that of so many other words, has been caught up in the Kishon of the mind. The river Kishon swept them away—that ancient river, the river Kishon.

I have used the word "locate" partly because it serves to illustrate the habit of the mind to let its offspring escape from it into the world outside, but also because the British meaning of the word denotes a process which has been neglected by the biologist in his attempt to interpret life. It is essential that he should locate the laws that relate to life. I am not asking him to agree with me in placing them in the mind, but pointing out the necessity of locating them before talking about them.

It was said above that the most troublesome cases are those in which one word has more than one meaning. The reason that these cases lead to confusion is that word and meaning are mutually interpenetrating, so that the flavour given to a word from one meaning is transmitted by that word to another and perfectly distinct meaning. For instance, the word "law" is used for those enactments which in human society adjust the relations between individuals. It is also used, as we know, for those generalisations made by man of regularities or sequences observed in phenomena. But laws, in the latter sense, natural laws as we call them, have been infected through the word as a "carrier," so to speak, with many of the attributes of law in the other sense. To such lengths has this infection proceeded that such absurd expressions as "the phenomenon obeys the law of gravity," or "breaks all the laws of nature," are often heard.

Man's reluctance to keep an eye on the changes which may be taking place in the meaning of a word is part and parcel of his reluctance to turn his eye inwards and examine the operation and furniture of his mind. The result of this reluctance is that he never takes stock of the medium whereby he conveys the results of his thoughts and researches to others, and whereby he receives such results from others. Yet if we are going to set out in earnest to understand life, we must perpetually keep a curious eye upon the relation between the word and its meaning.

The reader may take exception to the sentence, "If we are going to set out in real earnest to understand life," and ask, "Have you the impudence to suggest that we have not yet even started to understand life ? Are you utterly ignorant of Modern Biology ? Or are you wilfully ignoring it ? Do you propose to sweep aside entirely the work of those great investigators who have spent their lives in the service of Biology ? " Let us apply the general conclusions we arrived at with regard to the relation between words and things to the word "Biology." The area covered by this word includes in one, let us say its western, region that great mass of (fairly pure) description of the forms and colours and behaviour of living things. In its eastern region it contains a great mass of interpretation little contaminated by facts ; and, in the intermediate region, mixtures of the two in every conceivable proportion. Now when a man asks himself what he means by Biology he probably calls to mind the great store of (fairly well) established facts which make up the content of descriptive zoology, botany, embryology, cytology, histology and the rest. But if the man who indignantly asked the questions above, merely used the phrase Modern Biology as a bludgeon to frighten me with, he almost certainly did not ask himself what he meant by the word biology ; and in that case he made no attempt to keep the descriptive and interpretative parts of Biology strictly apart and distinct in his mind. Until he has done this, it is impossible to answer the above questions. If he will do this, I will answer that I certainly do not ignore the established facts of biology. I am concerned in this book solely with the interpretation of life ; and I assert that we have not yet begun to understand it.

§ 6

Let us now glance at the relation between the mind of man and the phenomena of life from the historical quarter.

Suppose that when biological problems were first stated, or to speak more accurately, first gradually took shape in the minds of men, suppose that they were wrongly stated, suppose that they took the wrong shape; the solutions of these problems will not be answers to the questions actually posed by Nature herself.

I can illustrate my meaning by the following fable. There was once a man who could only play one tune, "Polly winked her eye." He played it with one finger only. He learnt it one day, not from hearing it, but from the notes, spelling it out with great difficulty. It was in the key of C major. He got the intervals between the notes correctly, allowing that there were no sharps and no flats; but he began on the note below the one he should have begun on.

He always played it like this and seemed to enjoy it. That was how he had ground out the tune for himself; that was how he always thought of it; and that was how it would always exist for him. But the tune was untrue owing to the initial mistake he had made.

Are we certain that in our statement of biological problems we may not have begun on the wrong note? The possibility that we may have done so deserves our earnest consideration. For my part I think we did begin on the wrong note and that, as a consequence, our present statement of biological problems does *not* correspond to the questions posed by Nature herself; and as a further consequence, that though our solutions of those problems may be perfectly correct, they are solutions of problems posed by the human mind. I do not maintain that these problems are entirely fictitious. I do not mean that they bear no sort of relation to the real problems; I think they are a distorted version of the real problems, that they may be said to bear the same sort of relation to the real problems as the tune begun on the wrong note bears to the real tune. But, of course, some biological problems have been stated more correctly than others.

The history of biology is a picture of the evolution of man's endeavour to interpret life. The picture of this evolution, as of all other evolutions, is the picture of a tree: leaves, twigs, branches, trunk, root, rootlets and root-hairs. This tree is the result of the solidification of the stream of mankind's interest in life. The root-hairs are his first vague curiosity and bewilderment; the leaves his most recent publications and opinions; intervening points on the branches, trunk and roots, intervening periods in the history of biology. We will imagine further that discredited observations and unaccepted interpretations were represented in this tree by dead wood, which quickly rotted and fell away.

It seems to me desirable that we should occasionally tear ourselves away from pre-occupation with the high-water mark of our investigations; that we should cease for a moment from our feast upon the leaves, and descending to the ground, reflect at leisure, under its genial shade, upon the form of the tree above us. Reclining there, we should ask ourselves whether the shape of the tree into which the course of the stream of inquiry has solidified, is the right shape, whether some of the wood in it ought not to have been cut out, and whether some that died and has long since disappeared ought not to have survived and altered the shape of the tree. In other words we should ask ourselves, are we working in the right direction ?

For my own part, I believe that at certain points in the history of our attempts to interpret life wrong signals were given and that as a consequence we are at present working along the wrong lines. I am not concerned at present with the nature of this false step ; all I am concerned with now is to express my belief that the satisfaction of the biologist with our current scientific interpretation of life is the satisfaction of the fool with the paradise which he has built.

§ 7

The cocksureness of the scientific biologist should surely be the cause of the gravest misgivings. The more certain a man is that he is right the more probable is it that he is wrong ; because it means that facts are as soft clay in his hands, and his certainty moulds them to his purpose. It is the diffident investigator who tentatively offers us a hypothesis which, in his modest view, brings some of the facts into line, who should inspire us with confidence. It is the theory which seems to fit the facts in places but seems remote from them in others (as, for instance, the theory of sex based on clinical, Mendelian and cytological phenomena and upon the facts of parasitic castration) and not the theory which peremptorily brings all the facts into line, which should seem to us to be likely to be true.

If a man came to me to-morrow, full of confidence and certainty, and well pleased with himself, and said, " I have now got a theory which fits *all* the facts," I could think one of two alternative things about him.

I could think that this man, by a miracle of energy, had become acquainted with all the manifestations of sex, and that his theory did fit, down to the very smallest undulation, the surface of the phenomenon, as a glove fits a woman's hand. That is to say, I should think that he had made up the deficit in the facts by the discovery of the remainder, and that he was now able to give a theory which fitted exactly, simply because he had now got all the facts at his disposal.

Or, I could think that he had not made any difference in the stock of facts at his disposal, but had invented a new theory, which fitted the same number of (but probably fewer) facts, only in the sense that a child's hand can be fitted into a steel gauntlet. That is to say, I should think he had made a new theory which really bore very little relation to the facts ; touched them at one or two points but fitted them at none. The theory might be a perfectly consistent one. He would not invent a theory which was not a consistent and organic whole. Indeed it is generally admitted that it is more important that a theory should be consistent with itself than that it should fit the facts closely. For it is considered that the worst thing that can be said against a man who is patiently trying to fit one part of his theory to one set of facts and another part of it to another set, is that the two parts of his theory are not consistent with one another.

If I believed the first alternative I should think that his certainty was due to the perfect fit between theory and phenomenon; if I believed the second I should think that certainty was due to absence of fit between theory and phenomenon. Certainty can be bred in the mind by these two extremes; not by an intermediate stage. Are there any theories concerning vital phenomena about which certainty can be due to perfect fit? Can there be any vital phenomenon that we know so intimately in every undulation of its form, every nuance of its colour, or every phase of its movements that there can be a theory which fits all this exactly? Possibly in the case of some exceedingly simple phenomena (if such exist), certainty is due to perfect fit. But does it seem likely that, for instance, the certainty in the mind of so many that Natural Selection is the explanation of evolution, that tremendous phenomenon of the growth of life, the features of which we can but dimly discern, does it seem likely that this certainty can be due to perfect fit?

An old artist and a young artist both arrived in Venice not long ago, on the same day. At the end of a month the old artist had painted nothing; it was too beautiful; he knew he was not equal to painting a picture which could express to him the manifold magic of the place; he knew that nothing that he could produce would fit reality. The young artist, however, made many sketches, with each of which he was well pleased. He had no difficulty in expressing what *he* saw, because he saw so much less than the old man did. Nor would he know that he saw less because Venice to him would be the

Venice that reached him through *his* eyes; Venice to him was less than it was to the old man. Is not the growth of life more manifold even than Venice? Yet the man of science thinks that he has explained evolution.

Look at the matter from the point of view of the maker of the theory. Suppose he has a theory which fits a set of facts at some points. Man is in a hurry to explain what he sees. Will he not, rather than spend years in gathering new facts, impatiently, but unconsciously, round off the theory to cover the facts already before him?

Then there is a question whether a theory which fitted the facts closely would be acceptable to the mind. Is it not possible that all that the mind can understand, is mind? I mean, it seems likely that a mind is incapable of recognising (a necessary preliminary to understanding) anything but the workings of another mind not very widely different from itself. That is to say, it can only recognise the usual, fairly rigidly fixed features, the deeply ingrained habits of thought—the eyes, nose, mouth and ears, so to speak, which are common to all minds.

When it is remembered that two minds can be so different as to be as unintelligible to one another as is that of the poet to that of the stockbroker, or that of Beethoven to that of Weber, who, when he heard the Seventh Symphony thought that Beethoven was mad; when we see how one mind can be so utterly unintelligible to another, is it conceivable that such a thing as a theory which has no mind (human mind, I mean) in it at all would be even recognisable, much less intelligible, to the mind? Suppose that the mind is suddenly confronted with an apparition, a theory, which fits a phenomenon so closely, down to the smallest crevice, that the theory showed none of the features of the human mind at all but only a cast of those of the phenomenon, the apparition would mean nothing to the mind at all (unless the phenomenon to be explained was a product of the human mind). But if the mind were confronted with an apparition which was sufficiently like a mind to show that it was the offspring of a mind, but also showed, by the indentations on it, what phenomenon that mind had been pressing its face against, then the mind would be confronted with an apparition, a theory (as, for instance, Natural Selection, a theory involving such essentially human ideas as utility and competition) which it could at once understand. In other words, a perfectly fitting theory would be utterly unintelligible to the mind. A condition of the intelligibility of a theory, it seems to me, is that it must not contain too much of the phenomenon (unless that phenomenon happens to be a product of the human mind) or it will be unintelligible. It must, of course, be, or have been, in contact with the phenomenon at one or two points, just to show which phenomenon it is supposed to be explaining. We have, therefore, an argument for the view that the acceptability of a theory is evidence not of the accuracy with which that theory fits the phenomenon, but of its remoteness from it. In other words, an acceptable theory is more likely to be one which resembles the mind, than one which resembles the phenomenon (provided that that phenomenon is not also a product of a mind, such as a murder or a machine).

This argument will probably be uninteresting or meaningless to those who are not accustomed to fitting theory to fact ; and nonsensical to those who are so accustomed. Men who are commonly engaged in the task of making theories to explain things may be classified into three categories according as the things to be explained relate to (1) man ; (2) living things other than man ; (3) not-living things. The first category is occupied chiefly by the criminal lawyer, the second by the biologist, the third by the engineer, the physicist, the chemist, and so forth.

That the lawyer would think it nonsense would be proof of its truth. The lawyer's business is to fit a thing which is entirely human mind, i.e. his theory, to another thing which is also human mind, namely the actions of a particular man or woman. If his theory fitted exactly it would be the exact cast of which the mould was the action of the prisoner ; and so would be immediately acceptable to and intelligible to the mind. (If the reader is inclined to find fault with my metaphor and point out that the cast is not similar to, but the opposite of the mould, I would refer to the well known optical illusion which consists in our inability to say whether a given arrangement of graded light and shadow is a mould or a cast, unless we already know, or have strong reasons for guessing, beforehand, which of the two it is.)

I cannot help thinking that the method of the court of law, with its weighing of evidence and so forth, whilst admirably adapted for the work of finding out what a particular woman or man has or has not done, is not suited to the investigation of extra-human vital processes. In the first place the problems in the two cases are totally different. The function of the court of law is simply to find out what a man has done; and, so far from its being the case that the court is concerned with discovering the cause of the prisoner's action, the fact is that a pretty confident and usually justified visualisation of his motives, i.e. the cause of his probable action, by the jury, is used only as a means of finding out what he did. But the task of the biologist is not to find out whether a particular animal or plant did or did not, on a certain occasion, do a certain thing. The non-possession by non-human animals and plants of a speech intelligible to man makes it impossible for them to lie to us. And, of course, in point of fact, we do know a vast amount about the actions of plants and animals, especially man. For we cannot lie to ourselves about our own actions, though we may about our motives. The task of the biologist is to interpret, to find a meaning in those actions of living things that he knows about; or, to put it so that it embraces the whole domain of life, to explain such manifestations of life as fall within his ken. The application of the method of the court of law to this task seems to me to be one of the most flagrant examples of the use of tools designed for specific practical requirements in the wild-goose chase after truth.

Let us now pass straight on to the third category, that of the physicist and chemist, and leave the biologist out, for reasons which will appear shortly. It is held by M. Bergson, and I agree with his conclusion, that the intellect of man has been developed as a by-product, nay indeed is little more than a product of the process of utilisation and control of matter by man.[1] And whether we incline to the view that this development has been the invention of a conception of matter to fit the rigid features of the mind, or to the view that it has been the modelling of our at first plastic manner of thinking to suit the hard crust of matter ; in a word, whether liquid matter has been poured into and cast in the mould of mind, or liquid mind has been poured into and cast in the mould of matter ; to whichever of these views we incline, we believe that the mind thinks in terms of matter, and that a theory of any manifestation of matter which fits that manifestation closely will be acceptable to and intelligible by man, simply because matter has been moulded on mind, or mind on matter. And therefore to the engineer, the chemist and physicist and other occupants of the third category, my thesis that a theory which fits the facts at all closely will be unintelligible to the mind, will be nonsensical. My thesis will be rightly considered untrue by these men because they are dealing with matter, by means of an instrument, the intellect, designed for the purely practical purpose of dealing with matter ; and by the lawyer because he is dealing with certain phases of human activity by a mechanism, the court of law, designed for that very purpose. The lawyer is fitting human mind to human mind. The physicist and chemist are fitting human mind to matter, upon which the human mind has been moulded. So, of course, to both these classes the

[1] Bergson, "Evolution Créatrice."

closer the fit of the theory to fact, the greater the intelligibility of the theory.

But what about the biologist ? He will think my thesis nonsense too. For not only does he, because he is a man, think most comfortably in terms of matter ; but also because he endeavours to interpret life in terms of physics, chemistry and mechanics, he borrows his ideas about the fitting of theory to fact from the physicist and the rest. Glance for a moment at the series covered by our three categories : (1) lawyer ; (2) biologist ; and (3) physicist. In its broadest sense that series covers (1) man ; (2) life ; and (3) matter. The biologist has started with matter because he is most at home there ; and he has tried to interpret life, and even man, in terms of matter. In a sense, too, he has started from the other end, i.e. from man, in his attempt to interpret life. But he has laid hold of the least vital thing about man that he could have chosen ; for he has done no more than borrow the purely utilitarian, unsympathetic and impersonal method of the court of law to investigate life. And thus life has been enfiladed by a withering fire from both ends ; our theory of life has no life left in it ; and we think of life as machinery, and of the organism as a machine filled with a variety of chemicals, meekly obedient to the laws of chemistry, physics and mechanics.

But to return ; the biologist had to make a theory which fitted. Was he going to wait till he had taken into account all the manifestations of life, especially those exhibited by man ? Not he. Indeed he could not if he would. Preoccupation with the extensive properties of matter almost invariably carries with it an atrophy of sympathy with the varied manifestations of the spirit. The biologist who believes that a living thing is a machine, naturally feels embarrassed in the presence of things which he utterly fails to understand ; so he rules them out of court as "sentiment" or "superstition." Life itself, with its component individualities, no two of which are alike ; with its extravagance and its parsimony, its beauty and its ugliness, its utter misery and its bouncing fun ; all this is a disconcerting spectacle to the biologist who comes to it laden with the ready-made moulds, the concepts which he has borrowed from the chemist, the physicist and the mechanist. In the presence of Life does he throw away his moulds and try to understand it ? Not he. He gathers up his moulds, hurries from the uncongenial atmosphere, and retires to that comfortable sanctum, the recesses of his own mind, where he can think how he likes, and use his moulds ; and having bolted the door against all but the purveyors of such moulds, he constructs a theory of life after his own heart, a machine. "Nature," he says, "is a vast and orderly mechanism, the working of which we can, to a large extent, perceive, foresee and manipulate, so as to bring about certain results and avoid others." And he enjoys "that happiness and prosperity which arises from the occurrence of the expected, the non-occurrence of the unexpected and the determination within ever-expanding limits of what shall occur." [1]

[1] Sir Ray Lankester's Introduction to "Modern Science and the Illusions of Professor Bergson," by Hugh S. R. Elliott, p. x. Longman and Co., 1912.

And when his labours are over he sings :

> " Life is a vale, its paths are dark and rough
> Only because we do not know enough.
> When Science has discovered something more,
> We shall be happier than we were before." [1]

He thinks that he has become far removed from the jagged crust of phenomena in the vale that was dark and rough. And so he has. He thinks that he has burrowed through the soil, which was easy; through the subsoil, which was not so easy; through the intervening strata, which was still more difficult, until at last he has reached fundamental principles and stands upon the Bedrock of certainty itself. But he does not. It is all an illusion. It is true that he has become removed from the crust of phenomena. But it is in the other direction. He has flown upwards on the wings of his imagination, in the machine which he has made, and will soon become a tiny speck, infinitesimal against the evening sky.

I have endeavoured to give some idea of the dangers which beset the path of him who is not satisfied with a mere description of Life, but sets out to understand her and to convey his interpretation to others. If these dangers are so great as to be insuperable, would it not be safer and simpler, we ask in despair, to rest content with a mere description, with a photograph instead of a picture of Life? If we did this, we should certainly make fewer mistakes; but we should not make much progress.

[1] "Lambkin's Remains," p. 20. Published by the proprietors of the J. C. R., at J. Vincent's, 96, High Street, Oxford, 1900.

We could not go far wrong, but we could not go far. No. Let us not play the poltroon's part. Let us locate the dangers and know them so that we may guard against them. Besides, Life challenges us to interpret her. Our best chance of understanding life is to study life in movement, that is to say, in evolution. In our next chapter we shall glance at the evolution of man with a view to finding out what habits of mind he has contracted. Having done this we shall be in a better position to answer the question whether the theory of life, especially the explanation of evolution by natural selection, which constitutes the orthodox biology of the present day, is acceptable to man solely because that theory has been cast in the mould of his habits of thinking; and whether this theory is not, in fact, so remote from life as it really is as scarcely to bear any relation to it at all.

CHAPTER II

"*The lower animals keep all their limbs at home in their own bodies, but many of man's are loose, and lie about detached.*"
—SAMUEL BUTLER.

"*We shall see that the human intellect feels at home among inanimate objects, more especially among solids, where our action finds its fulcrum, and our industry its tools.*"—HENRI BERGSON.

§ 1. Tool and limb the same.—§ 2. Closer analysis of § 1.—§ 3. Still closer analysis of § 1.—§ 4. Evolution extra-corporeal in man.—§ 5. Tool and intelligence knit up together.—§ 6. Evolution of warfare, industry and the chase.—§ 7. Warfare.—§ 8. Industry.—§ 9. Extension of body followed by extension of mind.

§ 1

THE essential difference between man and the other animals is often stated to consist in the fact that man uses tools whilst the non-human animal does not. But I think that the difference between the two can be stated more accurately. The difference consists rather in the fact that the tools used by the wild animal are formed by the modification of its limbs, whilst the tools used by man are made by him out of wood, stone, or metal, or such other solid or tough things as he can find or make. This statement minimises the difference between the limb and the tool. I have called them both tools. Butler, in the sentence which stands at the head of this chapter, calls them both limbs.[1] It does not matter what names we

[1] "Erewhon," A. C. Fifield, 1908, p. 270.

tether them to, so long as we realise that there is no essential difference between, for instance, the claws of the shore-crab and the forceps of the anatomist; and that such difference as there is lies in the fact that the latter can be detached when no longer required and attached when wanted again, while the former is a useful tool only so long as it remains part of the body of the crab.

Mere detachability, however, is not the essential feature of the human implement, because it happens that in the case we have chosen from the other animals the implement can be detached. If the claw of a crab is badly injured it can be voluntarily cast off by the crab, by a deliberate transverse fracture of the limb near the point where it is attached to the body, but not, curiously enough, at a functional joint. A new limb is then grown in the place of the discarded one. It is an interesting but not a surprising fact that a crab has to be in very good health to be able to perform the operation of amputating its own arm in this way. This amputation may also be performed by a crab when its limb is securely held by an adversary.

The essential difference between man and the other animals cannot, therefore, be expressed in the simple statement that man is distinguished from other animals by the fact that he is a tool-using animal. Limb and tool are two names for the same thing. Nor does the difference lie in the fact that this thing, whether we call it limb or tool, is detachable in man and not detachable in other animals. For we have just seen that it is detachable in the crab. The difference lies in the fact that man can lay aside his implement when he has done with it and pick it up when he wants to use it again. And this involves at the moment when he lays it down a looking forward to the time when he will want it again; and, at the moment when he wants it again, a looking back to—that is, a memory of—the time when he laid it down. Here we get our first glimpse of the essential feature which distinguishes the mind of man from that of other animals—namely, the fact that the human mind extends its operation much further forwards and much further backwards than does that of the other animals.

§ 2

This essential identity between the tool (or limb) which is part of the body and the tool which is detachable from it is a very important point to grasp, because it forms the starting point of our study of evolution in man.

I am aware that this thesis conflicts with what may be called a sensible scientific view of the matter. According to that view the claw of the crab corresponds with the hand of the man; the tips of a crab's claws correspond with the tips of a man's fingers, and not to the points of his forceps whilst he is holding them. But this scientific view surely does not express the whole truth. To know the real living thing we must see it in relation to its surroundings and know how it obtains its food, and how it escapes from or defeats its enemies. A specimen of a crab in a bottle shows you the crab accoutred with the full panoply of armour and weapons which protect him from his enemies and from the elements ; and provided with the full set of implements by means of which he obtains what he requires from nature. In fact, the crab is seen with everything by means of which he comes into relation with inanimate and animate nature. But a naked man shut off from his implements is deprived of all this ; he lacks clothing which shelters him from the elements, and if he has no spear and no spade he must shortly starve. So that, looked at from a point of view from which one sees the bond between the organism and its environment, the implements of man do correspond with the limbs of an animal. I am aware that my argument may appear obscure ; but the incidence of the words at my disposal is responsible for much of the obscurity. There is no difficulty in the human sphere ; there are plenty of words for that with which man carries out his work—tool, weapon, implement, instrument, apparatus, machine—each with its own well known incidence of meaning. But in the sphere of extra-human animals the choice of words is much more difficult. For instance, limb does not cover the snout of the hog, which corresponds with a man's potato-fork. Perhaps the best word is organ, which means that with which work is done—i.e. that which effects the bond between the organism and its environment.

The matter may be looked at from the closer, purely sensual aspect. The implement, whilst one is using it, certainly feels as if it were part of oneself. It is true that when one first uses it it does feel like a foreign object in one's hand. But by the time that one can use it well, one comes to feel as if it were part of oneself. The skilled dissector and surgeon soon comes to feel as if the forceps were elongations of his fingers; and in general his dissecting instruments, whilst he is using them, become in a very real sense living parts of his hands. The word "surgeon" is derived from the Greek *cheirourgos*, which literally means one who does (delicate) work with his hands. For the sense by which one traces a nerve or a vessel is a blend of the senses of sight and touch. This sensation of touch must be transmitted to the brain through the instrument as well as through the hand and arm. Therefore, though there *is* a difference between hand and instrument in the faculty of transmitting sensation, the difference is not so great that a hard and fast line can be drawn, *in respect of this faculty*, between instrument and hand. The hand is merely the haft of the tool, that part of the body into which the instrument (as its name implies) is built.

§ 3

"*Life, he urged, lies not in bodily organs, but in the power to use them, and in the use that is made of them—that is to say, in the work they do.*"—SAMUEL BUTLER, "Erewhon Revisited," p. 128.

"The difference between the limb and the implement, between the claw of the crab and the forceps of the anatomist, is a perfectly simple one," it may be objected. "The claw is alive and the forceps are inanimate." The answer to this objection is that the business part of the claw is just as dead as the forceps are; or, if the reverse phrasing is preferred, is no more alive than the forceps, in use, are. Both are made of inanimate matter; both become active at the bidding of the organism to which they are in the one case temporarily, and in the other permanently attached. The crab makes his claws of lime which he elaborates inside himself, whilst man makes a pair of forceps of iron which he elaborates outside himself. But there is no difference in the matter of aliveness between the lime and the iron ; both are of lifeless matter moulded to suit the purpose of the organism which uses them. The living thing has to arm himself with hard matter at the chosen points of himself where he comes in conflict with the world of matter. The particles of matter of which a rhinoceros horn and a man's dagger are built are prisoners made to fight against their comrades—matter impressed into the service of life in its battle with matter. The difference between implement and limb cannot, therefore, I think, be stated to consist in the deadness of the former and the aliveness of the latter.

Our reluctance to admit that the implement of a man is in any sense a part of him is due to our ingrained habit of thinking in terms of matter, which makes us regard the real essential living being as co-extensive with its body. That is why we think the claw to be part of a crab but refuse to admit that the forceps are a (detachable) part of the anatomist. But the essence of an organism, as its name implies, lies in the work which it does ; not in the chemical elements out of which it is made. And as regards work done, the claw of the crab and the forceps of the anatomist are perfectly analogous to one another.

The general conclusion which I believe to be indicated by the foregoing argument is that, though on a materialistic view it is a perfectly simple matter, and the organism ends where the body ends, and there is an end of it, there is probably a mutual interpenetration between the organism and matter; and the difference between man and the other animals is that the latter are obliged to welcome all matter they want to make use of instrumentally within the four walls of their bodies; whilst man is enabled by the invention of the system of detachable organs to keep the great bulk of the matter which he impresses into his service at arm's length.

§ 4

Evolution, therefore, in man consists no longer in a modification of the shape of the parts of his body. At any rate, if this corporeal change is going on, it is taking place very slowly and is not very interesting. Evolution is proceeding with great rapidity in those organs of man which "are loose and lie about detached"; this is the whole argument of the Book of the Machines in Samuel Butler's "Erewhon." Bergson expresses exactly the same idea. "Chaque machine nouvelle étant pour l'homme un nouvel organe,—organe artificiel qui vient prolonger ses organes naturels,—son corps s'en trouva subitement et prodigieusement agrandi . . . etc."[1]

With the origin of man, evolution along the line of man left the plane of corporeal change and rose to that of extra-corporeal change. Before the origin

[1] Institut de France. Académie des Sciences Morales et Politiques. Séance Publique Annuelle, du samedi 12 Décembre, 1914. Discours de M. Henri Bergson, Président.

of man life had to express itself through the arduous medium of modifying its own form; but with the origin of man life had, like an aeroplane just rising from the ground, gathered sufficient momentum to lift itself from the arduous plane of self-modification to the more elastic medium of a supple intelligence which could give form to matter without incorporating that matter with its own body. It is as if the superabundant energy of life, which could only spend a mere fraction of itself through that cumbrous medium, the modification of its own form, burst forth, when once that brake was taken off by the invention of detachable organs, into a wild career of evolution driven onward by the immense reserves of energy pent up within it.

§ 5

It is a commonplace that it was the assumption of the erect posture by man which released his fore limbs from the drudgery and routine of locomotion, and set free his hands for more delicate and versatile use. But it should be remembered that it was because man had preserved the simple pentadactylous hand of his amphibious ancestors, and had resisted every temptation to specialise his hand in the various directions in which it has been differentiated throughout the vertebrate series; it was because the line of life which led to man bided its time in this way that when the intelligence of man had reached the stage at which it could devise and fashion implements, his hands were of a form and of a sensitiveness suitable for the purpose.

Or suppose we do not incline to the view that the ancestors of man bided their time, but believe that it was lack of energy in the line which led to man, which left man with such an unspecialised —that is, with such a backward—hand, then we see in the fact that he was able to profit by this mistake the familiar mark of the good craftsman exhibited at an early age. All fashioning, whether of implements or of sentences, consists fundamentally in the intention to carry out a design. But the finished article is only in part the result of the carrying out of the design as intended ; it is also the result, and often to a very large extent, of our ability to profit by mistakes on the way.

So, whether we incline to the view that man had the ability to profit by the lack of energy on the part of his ancestral line which left the hand undifferentiated, or to the view that the preservation of an undifferentiated type of hand was the result of energy and foresight, the fact remains that it was the possession of this simple type of hand which rendered possible the manufacture and use of tools. But this bald statement does not, I fear, give a very true picture of what happened.

The mind has an inveterate habit of thinking in terms of cause and effect. It likes to see things in pairs, end on ; and to think of the second as being the effect of the first, which is the cause. This form of thought is probably moulded on everyday human action. The cause is the counterpart of our action ; the effect that of the response produced. Our mind is at ease when it can think in terms of a pusher and a pushed ; of cause and effect. And it is natural that we should apply this way of thinking in our pursuit of the truth. So we would like to think that man started the use of tools because he had attained to a certain degree of intelligence; or we would like to think that the use of tools resulted in the sharpening of the intelligence. It is more probable that a slight advance in the one led to a slight advance in the other, which thus got ahead and was able to pull the first after it. But one must beware of forming an image of this process on the analogy of so practical a matter as the progress of two things which are the puller and the pulled by turns, like two children bicycling down a gentle gradient hand in hand.

We must dig deeper in the mind for an analogy, and I suggest that we find it in the relation between seeing and drawing. There is little doubt that the reason why a man cannot draw is not, as the mechanist thinks, that his arm and hand are badly constructed for the purpose of drawing. The fault lies not in the arm but in the mind. Man, according to his usual custom, shifts the cause as far outwards as possible; and the bad workman even puts it in his tools. When a man cannot draw, it is because he cannot see truly. On the other hand it is certain that drawing is a great aid to seeing; in fact, it may almost be said that a man has not truly, fully, and vividly seen a thing until he has drawn it. If, then, a man's drawing cannot improve unless his vision becomes truer, and if the improvement in his vision depends on his drawing better, each is waiting for the other to make a move, and, logically, stagnation should be the result. But, logic or no logic, the fact remains that drawing and seeing, if persevered with, do help each other in a mutual way. Nevertheless, seeing takes the lead. No amount of drawing can make a man a visionary; but no amount of dissuasion and difficulties will prevent a visionary from drawing. It is therefore *within* that we must look for the source of progress.

It is probable that the relation between the use of tools and the development of the intelligence was of this mysterious kind. It was a fertile interaction, and so rapidly productive, on both sides, that it may be likened to the case of snails, whose mode of locomotion is so arduous, and opportunities of meeting so few, that they have developed an arrangement (hermaphroditism) whereby when they *do* meet each member of the pair becomes both the father and the mother of a numerous progeny.

Increased skill in the fashioning and use of implements would sharpen the wits of the artificer; and this gain in intelligence would be devoted again to its source, the making and use of tools. And so the two, intimately associated, would advance from conquest to conquest. The first rude implements of flint would give man an advantage over the beasts which unarmed he did not possess. He would thus not only vastly increase his supply of food and of raiment, but also—as did, for instance, the reindeer men of France—discover new sources of material, bone and horn, for his tools. Much later, when he discovered that he could further increase his food supply by sowing the seeds of the sorts that he ate, he would find that the seeds would grow better if he worked the soil with the tools or weapons that he had. He might find that the spear-head was not the best shape for a tilling implement, so he would alter the spear-head and adapt it to a new purpose, or make a rude mattock and see how it worked. A different type of implement would be required for a hatchet, a different one for grinding corn, and so on. How interesting these early experimentings must have been ! How man's whole mind must have been taken up with this new joy of making something ! What new sources of interest were discovered to him when he first made a flint knife !

Not only was this new occupation for his mind operating through his hand absorbingly interesting, it was immensely advantageous too. It not only satisfied his mind, it also brought clothing for his back and food for his larder, and kept the wolf from the door. And, all the time, the fashioning of implements and the growth of his intelligence were playing into each other's hand in a variety of ways.

One thing the invention of the detachable limb did was to set free an immense amount of energy. It is probable (but I do not press this point now) that the making of an implement by the modification of one's own body requires much more highly concentrated, that is to say, much more energy than conducting, outside of oneself, the manufacture of a detachable implement. (If this point seems nonsensical to the reader it is because he is assuming what we are about to discuss, the truth of a mechanistic theory of the organism, and a mechanistic theory of evolution—namely, natural selection.) But whether this is so or not, it is certain that a great amount of energy was saved by getting rid of the necessity of carrying the implement about with one for the whole of one's life. The energy thus set free by the invention of the detachable limb would be put back into capital to be devoted to the further improvement of the liberating invention.

The animal can never put down its tools. Man by gradually devising the detachable tool would gradually come to divide up his time, into that time in which he had his tools in his hand and that time in which he had laid them aside. These leisure moments might be devoted to the imagining of new ways in which he could use his rude flint tools, and new ways in which he could make his tools out of the chunk of flint; as, for instance, the utilisation for skin-scrapers or arrowheads of the flakes which he struck off the core of the flint whilst making his spear or mattock. But it is more probable that such ingenuities would strike him whilst he was actually absorbed in the business of manufacture, and that in his leisure moments he would think to himself how pleasant and how profitable was his work. He might even occasionally and dimly contemplate the wonders of the universe.

But in those hard days these reveries must have been rare, and such flights as his imagination took were not the voiced exaltation of the lark, but the silent intent manœuvring of the hawk. Thought, at first, was all transitive, all intent on the task of escaping beasts of prey and providing for oneself and family.

The existence of man in those early days must have been terribly hard. The very thing which was destined to make him lord of creation might very nearly have led to his undoing altogether. The invention of the detachable limb was made possible by the preservation of the archaic pentadactylous hand of his remote amphibian ancestor. But it seems as if man knew, or the line of life which led to man knew, that man was destined to acquire detachable weapons and implements and armour. For not only the hand but the whole of the body too was left unspecialised, and utterly unprovided with offensive or defensive weapons and useful implements. Man had neither the armour of the crocodile, the portable house of the tortoise, the warm clothing of the bear, the swift hoof of the horse, the horn of the ox, the fangs of the lion, nor poison of the serpent. The non-specialisation of any of his organs as weapons or implements left him utterly defenceless when he was removed from his detachable limbs. Unable to defend himself, he ran to nature for protection and passed much of his life in the dark recesses of caves. Naked and unfurnished in his own person, he had to compensate for his lack of natural clothes, weapons, and tools by a super-bestial ingenuity in the invention of artificial ones.

The exercise of this ingenuity would give him a pull over his enemies by sharpening his intelligence in a way already hinted at. The laying down of the tool when done with involves a looking forward to the time when he will want it again; and the picking of it up, a memory of the use for which it is intended if not of the time when it was laid down. This extension of his attention, forwards and backwards, came to be the chief distinguishing feature of man, as compared with the other animals. He would compete with the beasts of prey not by superior strength but to a certain extent by superior cunning; to a greater extent by his armoury of detachable weapons and implements, and perhaps still more by this new faculty of looking forward—of pro-vision, in fact. The forward extension of his intellectual vision would probably be accompanied by backward extension of it. It may even be that this forward extension of the intellectual vision necessarily involves a corresponding backward extension of it, as a man cannot walk away from a mirror without his image walking away from it in the opposite direction. At any rate, *without* such an extension of the intellectual vision backwards the forward extension of it would not be of much use. *With* it man would begin to provide for the future on the basis of the remembered experience of the past. The domestication of animals, started by accident, continued out of curiosity, and systematised for profit, would stimulate and be stimulated by this faculty of looking forward. So, at a later stage, would the tilling of the soil. It was this long range, therefore, of the intellectual vision of man as compared with that of the other animals, which, although at first the result of need, contributed, along with its source, the use of tools, more than anything else did to make its possessor the lord of creation. The way in which the meaning of the word "pro-vision" has been caught up, like that of the word "cure," in the outward current, and swept from its original subjective moorings in the mind, so that it means no longer an intention, nor the acting upon that intention, but the actual things which we buy from a provision merchant, may serve to illustrate the purely utilitarian character, at the time of its origin, of that extension of the range of interest which is one essential feature of human evolution.

It was not, however, only in a forward and backward direction that the mind of man extended its range as an indirect result of this invention of detachable implements. The invention would also vastly increase the versatility of man. Man by this invention would come to bear the same relation to the other animals as the great actor who can play any part does to the poor one who can only play one. For the invention of the detachable limb carried with it the possibility of possessing a greatly increased number of limbs and this implied so many new points of contact between man and the outside world. The animal whose limb has become the implement for the performance of a particular function is so highly specialised that he is destined to be in contact with the world of matter only at one point. Indeed, the name for the new power with which the detachable limb endowed man—namely versatility—means the ability to turn round and meet the outside world at all, or, at any rate, at many points. It is probable that the only parts of the outside world that interest a crab are those which will sooner or later come to be handled with its claws. The question seems to me to be not whether a crab can be said to possess forceps, but whether a crab is really very much more than a walking pair of forceps.

§ 6

If we attempt to visualise the beginnings of industry, warfare, and the chase, we shall, I think, see that they flow from two or three sources, the products of which unite to form a common stream, or, at any rate, to flow in a common broad channel, which, however, in its turn soon begins to divide up again in the delta of differentiation. Warfare and sport must both have owed their popularity to a great extent to the pleasure of shedding blood. Necessity must have contributed very largely to all three, whilst the insatiable curiosity of man must have played an important part in the origin and development of each, but especially in that of industry. For if Necessity was the mother of Invention, Curiosity was almost certainly its father. But I would prefer to attempt to express the truth with regard to the origin of human activities not in terms of cause and effect, but by suggesting that the relation between necessity and curiosity on the one hand, and adventure and invention on the other was of that obscure, reciprocally fertilising kind already indicated.

The chief objection to this that I can foresee is one which arises from the difficulty of understanding how necessity could be in any way the result of adventure or invention. But the essence of adventure surely is that you risk all in the attempt to gain something, and in the risking of all you may lose an arm which leaves you in the direst necessity or need. Also invention involves the abstraction of the mind from the practical business of provision, and its concentration upon the work in hand; the approximation to the mind of the focus of its interest would soon lead to utter want, unless there were someone to provide for the inventor.

Another objection which may possibly be brought is that love of adventure is not an essentially human characteristic. This objection I should be prepared to admit in part. For it would seem that the adventure which attends the life of the lion is not merely the result of necessity, but is also the result of a love of adventure for its own sake. I cannot think that a lion is as happy eating a horse's head he has been given, in a cage, as eating a zebra's head he has taken, in the wilderness. Still, the love of adventure for its own sake has undoubtedly been accentuated in man; the climber amongst the peaks of the Himalayas is not there in search of food. But is it not possible that our reluctance to grant a love of adventure, in any considerable degree, to animals other than man may be due to our ignorance of their lives; and to that orthodox theory of evolution (natural selection) which obliges us to think that an animal is what it is and does what it does because if it had been ever so slightly less efficient it would have been eliminated in the struggle for existence? For in the cases of the few animals of whose lives we have been given a detailed picture —as, for instance, of the Adélie Penguin, in a delightful book, "Antarctic Penguins," by Mr. Levick —there seems to be no escape from the conclusion that a great many of their actions are inspired by roguish fun, as when the penguins pushed each other into the water, or by the sporting instinct, as when they climb an ice-cliff for the excitement of it and then come down again.

Again, if the reader were to object that invention is not an essentially human attribute, I should be inclined to agree with him. If he argued that a bird's nest was as ingenious an invention as an Indian's wigwam, or an eggshell made out of lime as a pot made out of clay, I would not disagree. For my belief is not so much that the intelligence of man differs from that of the other animals in kind, but rather that it differs from it in range. Both stream outwards; that of the animal a little way to a few things, that of man a long way to many things.

But whatever may have been the relation between, on the one hand, the origins of industry, warfare, and the chase, these three, and, on the other, the springs of human action, it is probable that the early course of each of the three, receiving, as it did, contributions from all of the springs, would be very largely determined by, and would in its turn to a great extent determine those of the other two; as, indeed, would be inevitable in the case of three currents flowing in the same channel.

This degree of community in the origins of industry, warfare, and the chase must in the early days have found its expression in the manufacture, where possible, of a tool which would be equally useful in the provision of food, in tribal aggression and defence, and in the peaceful arts. The rudest flint implement was probably as much weapon as tool. When the working of flint was in its infancy and the manufacture of tools was slow, the man, for instance, who was flaying a kid with an instrument not also adapted for defence would be at a great disadvantage if he were suddenly attacked. But there had soon to be a parting of the ways; and the difference between essential features of the evolutions along the two diverging routes of weapon and tool may be glanced at.

§ 7

In fighting or hunting, a prime necessity is to hit your enemy or your quarry before he can hit you, and the dominion of man over the other animals means nothing more than that he can outrange them. This outranging is effected by the artificial elongation of the arm in two ways: either by the actual elongation of the spear or lance which is too good to throw away, or by throwing a detachable weapon, which may be anything from a stone to an explosive shell.

He who stands upon the parapet of the great earthen fortress upon the crest of the White Horse Hill, in Berkshire, near Uffington, which was Uffa's farm, but nearer to Woolstone, which was Wulfric's, and withdraws his gaze, in space, from the great Vale spread before him, past the green hollow at the foot of the hill, where from five sources the crystal Ock tumbles and splashes out of the hidden face of the chalk (and doubtless supplied the garrison with water), and, casting his mind's eye backwards in time a thousand and few hundred years, to the days when not far from there the hosts of King Alfred defeated the Danes, pictures to himself how attackers of the fortress would have to scramble up its steep sides and how the defenders would hurl them back down into the moat below, and there pound them with stones and transfix them with javelins. He who calls up such a picture to his mind sees for himself how early the cunning of man harnessed to his weapons the auxiliary force of gravity. The bow, the catapult, and the arbalest were subsequent utilisations of other auxiliary forces. But the great step in the increase of range was, of course, brought about by the invention of gunpowder.

That we intuitively think of weapons as part of our arms is shown by the fact that we call them arms,[1] and that we refer to the flying corps, for instance, as a "fourth arm." The whole object of war is to deprive your enemy of the artificial elongation of his arms, to disarm him by killing, wounding, or catching him. All man's offensive and defensive organs are detachable. That is the difference between him and a lion. The only way to disarm the lions in the Zoological Gardens in Antwerp, in case they were released by a shell, was to shoot them.

§ 8

It is natural that in the case of weapons the first thing to be done was to increase the range, and the simplest way of effecting this was the utilisation of external forces. But in the case of implements it was not the maximum distance from, but a convenient proximity to the object which was the desideratum. So that the fashioning of implements had proceeded to great lengths before any other motive force was employed than the energy of man

[1] *See* Skeat, etymology of arm.

himself. And the first utilisation by man of extraneous forces in industry, if we may enlarge the scope of that term to include agriculture, was literally a harnessing; when the burden of supplying the labour required for tilling the soil was shifted from the fore-arm of man on to the shoulder of the ox. One of the greatest inventions ever made by man was the wheel. Without it the impressing of steam, electricity, and petrol into the service of man would not have been possible. The wheel bears the same relation to the extra-corporeal evolution of man as the feather does to the corporeal evolution of birds. Machinery would be impossible for man without the wheel; flight impossible to birds without the feather. And it is a fact that the feather, an annually detached device, which combined, in a manner hitherto undreamt of, the minimum of weight with the maximum of resistance to the air, was invented (or, if the mechanist object, "was grown") by an animal who was much more reptile than bird, and who was discovered fossilised at Solenhofen, and called "Ancient wing" or Archæopteryx by another animal (calling himself "man"), who was destined, in a very few years, to learn to fly himself. The wheel, by taking off the brake imposed by friction and gravity, enabled man to take the burden from his own shoulders and put it in a cart which he or a beast of burden could draw. There seems to be little doubt that the first use to which the horse was put was to harness him to a cart. It was not till a much later date that man learnt to ride the horse. The chronological relation between the invention of the wheel and the domestication of the horse can never be more than subject for guesswork, but it is probable that as a human instrument the cart came before the horse. The need for a strong animal to pull a big cart may have been the chief incentive to the domestication of the horse, but it is known that the cart came before the saddle. Man, by the invention of the wheel, as everywhere else in his battle with matter, has secured an advantage only at the price of commensurate danger. For although in most cases this brake imposed by Nature was a hindrance, there are times when it is a necessity; as when the driver of a tram-car puts, or tries to put, the brake on again when his car is running down a gradient. And in general, although it cannot be doubted that the material welfare of man has been immensely increased by the invention of the wheel, without which the machinery necessary to modern industry would be impossible, it *is* extremely doubtful whether his spiritual welfare has been increased to a like extent; and it is certain that the wheel has been responsible for many horrible deaths and for many miserable lives. True, without the wheel we should be without the automobile and the cinema; but without it we should also have escaped many of the horrors of civilisation. However, it is too late now. We have grown the wheel and we cannot—at any rate, we will not—cut it off. The earliest form of wheel was probably a tree-trunk along which an object too heavy to lift, like the gigantic monoliths of Stonehenge, was rolled. From this clumsy stage a great stride was made forward by the invention of a device, the axle, whereby the wheel went along with the cart; in the activities of peace this invention greatly facilitated the work of transportation; and in warfare it put great power into the hands of the men who used it, by enabling them to do the next best thing to being in more than one place at the same time—namely, to get from one place to another in a very much less time than their adversary. "And the Lord was with Judah; and he drave out the inhabitants of the mountain; but could not drive out the inhabitants of the valley, because they had chariots of iron" (Judges i. 19).

It was not, however, only by taking off the brake imposed by gravity and friction that the wheel let loose the avalanche of man's material prosperity. The potter's wheel gave him utensils which, amongst other things, enabled him to carry and store water, and thus lengthened the apron-strings which bound him to springs and rivers. The spinning-wheel contributed to warmth and comfort.

The wheel could not have been invented before evolution had got on to the extra-corporeal stage in man, because the wheel must, whilst fitting closely round its axle, be absolutely separate from it. Such an organ in a vertebrate would have to consist of a bony disc, with either a hole through its centre, or with an axle which was part of it and which fitted into a horizontal hole in the skeleton, such as the acetabulum. Such an organ could not be used until it was fully grown and had come to consist only of dead bone, because it could not be used until it had become a separate piece from the axle, after which moment no further blood-supply to it would be possible. Up till the moment when the wheel was full-grown, wheel and axle would be of one piece. Once the last drop of blood required for the full development of the wheel had passed into it, the blood-vessels and other tissues in the wheel would begin to atrophy, and a cylindrical separation would take place between axle and wheel, or between axle and socket, and the wheel would be free; and not till then would the wheel be *in esse* a wheel; for then, and not till then, would the wheel possess the essence of the wheel, which is rotability. Up till that critical moment in the life-history of the cyclophorous vertebrate it would be a helpless tetracycle with the brake jammed on. An attempt to move the animal long before the liberation of the wheel would spoil the wheel for future use by wearing it flat in one place; a similar attempt nearer the time of the liberation of the wheel would produce death or grave disorder by hæmorrhage of the axle. And then, when the wheel had become rotable in the fullness of time, the animal could not move of his own accord unless only his hind limbs had been modified into wheels and his front ones were legs; if he were a tetracycle on the level he would remain stationary unless he had domestic animals to pull him about; if he were on the side of a hill he would probably be wrecked. This attempt to visualise the origin of the wheel before evolution had got on to the extra-corporeal stage seems to me to show that the wheel would not have been of any use before the mind had gone far enough to conceive of a cart, and to make the tools good enough to make a cart with. It was not until man had deputed the labour of haulage to his beast of burden that the wheel became an important factor in human economy. For the chariot preceded the wheelbarrow. It was not, therefore, till the man had detached from himself both the matter of which he makes his organs and the power which sets them in motion that the wheel could begin to show itself above the horizon of practicability.

But the early naturalists evidently did not think that the growth of a wheel presented any difficulties to even the lowliest organisms. Certain animalcules possess a circlet consisting of a great many vibratile cilia (delicate whips of protoplasm); and the waves passing rapidly round this circlet, like waves on a field of corn, but produced not, as in the cornfield, by an external agency, the wind, but by the voluntary supination in extremely rapid succession of the cilia composing the circlet, make an excellent imitation of a revolving wheel. This is the description of one of these animalcules by Leeuwenhoek[1] in 1702: "Out of the same sheath appear'd a little Creature, the fore-part of whose Body was roundish, and presently from the same Rotundity proceeded 2 little Wheels that had a swift Gyration. The Limner observing the Rotation of the Wheels, which always ran one and the same way, could not be satisfied with the sight, adding, O, that he could always see such a wonderful kind of a motion." It is a tribute to the early naturalists who took this imitation for the reality and dubbed these animalcules

[1] "Philosophical Transactions of the Royal Society," Vol. XXIII. No. 283.

"Wheelbearers," or Rotifera; for it shows that they thought of the essence of a wheel, not analytically, in terms of matter, as a thing made of hub, spokes and rim; but intuitively, in terms of performance, simply as "that which revolves."

The wheel put power into man's hands in a variety of ways. It cannot be said that the pulley enabled him to lift weights heavier than he had lifted before, for the horizontal monoliths of Stonehenge were probably lifted without its aid. But the pulley certainly facilitated and expedited to a very great extent the labour of lifting. The windmill and the water-wheel are results of the utilisation of tremendous natural forces which could not have been impressed into the service of man (on land) but for the previous invention of the wheel. But the stream and the wind cannot be said to have been subjugated by man to the extent that steam, electricity, petrol and gas have. The stream and the wind serve man at their pleasure and not at his; it is natural, therefore, that it is in association with the more completely domesticated and controllable forces like steam, electricity and the rest that wheels should have attained to the highest stage of their differentiation. How essential a part in those extra-corporeal organs of man which we call machinery the wheel plays is illustrated by the fact that in the picture called up in the mind by the word "machinery" wheels are the dominant feature. How essential a part the wheel played in the origin of machinery may be gathered from the fact, which is, I think, obvious, that without it it is difficult to see how steam, electricity and gas could have been harnessed as motive powers. If man can be said, as I think he undoubtedly can, to be the only animal that has made a fortune, it was the wheel more than anything else which enabled him to do so. That man regards the wheel not as a mere dead device but as an externalisation of himself, and endowed with a vitality which becomes its own, is shown by the way in which the wheel appears to the intuition of the savage and of the poet. It is recorded how some Kaffirs, when they first saw a European wagon, ran along by the little front wheels cheering them for being able to go the same pace as the big back wheels.[1] And the mother of Sisera did not complain of her son's horses, but cried: "Why is his chariot so long in coming? Why tarry the wheels of his chariots?"

§ 9

"What is a man
If his chief good and market of his time
Be but to sleep and feed? A beast, no more.
Sure He that made us with such large discourse,
Looking before, and after, gave us not
That capability and godlike reason
To fust in us unused."—HAMLET.

In this chapter I have suggested a view of human evolution which will, I believe, help us to understand evolution in general—that is to say, the growth of that vast organism (or worker) which we call life; and may possibly bring us nearer to an understanding of the meaning of that growth. I am aware that it may be said that the view which I have taken of human evolution is a merely fanciful one. But

[1] "The Essential Kaffir," by Dudley Kidd.

the fact that it can be labelled with the word "fanciful" does not prove that it is not taken from a point from which we get a very good view of human evolution. I do not, of course, pretend that it is a true view. Human evolution and, still more, life itself, will probably always remain a mystery to us. All that we can hope to do is to approach a little closer to an understanding of these immense, unfathomable things.

To say, as I have said, that man really makes with extended parts of his hands everything which he makes by machinery may seem to be the taking of a deliberately fanciful view of modern manufacture. But the very word we use for this vast business shows that man instinctively regards all this making as, in origin, if not in essence, a making with the hand. Through the hand the body of man has extended outwards, and has become prodigiously enlarged.[1] Through the hand, the mind also has flowed outwards and has become prodigiously enlarged. Or, to state what has happened in other words, the gradual increase of man's control over matter has been accompanied by a gradual extension of the field of operation of his intelligence. Thus man differs from other animals not in the direction of the flow of his attention, for both flow outwards, but in the distance to which that attention reaches. The attention of the hen flows outwards to the handful of objects which constitute her universe; but there it stops. Man, by inventing detachable organs, has made so many new points of contact between himself and the out-

[1] P. 50, *supra.*

side world, and so many new channels through which his mind can flow out into, and run about amongst, external objects. The word "discourse," which occurs in the quotation at the head of this section, expresses exactly what the human mind has done: it has run about in all directions. But in this discoursing man's mind has run along the same tracks as those taken by the progress of his control over matter. The mind at first followed in the paths cut out by the hand; then, looking ever farther ahead, gradually insinuated itself into the position of leader. So that now it is not necessary to make a device with the hand, to see if it will work; the mind is so familiar with the properties of matter that it can foretell whether it will work or not before it has been made. The human mind owes this long range, so to speak, which distinguishes it from that of the other animals, to the apprenticeship which it served in the factory, in the most literal and elemental sense of the word. It was because it learnt to think in terms of matter that it acquired the habit of looking forward, of foretelling. If the earliest object of the mind's interest had been life, it would never have developed the long range which it acquired from its familiarity with inert matter. Can a man and a woman foretell what their children will be like? No. It was because the education of the mind began with a study, for purely utilitarian ends, of the properties of substances like wood, flint and leather, and the behaviour of things like the lever, the spring and the wheel; it was because the mind received its early training in a course of applied mechanics that it was able to acquire the habit of predicting the performance of things, of extending the limits of its vision and of pushing its horizon farther and farther away.

Life during man's defenceless infancy was so hazardous and earnest a thing that, although the mind by its power of foretelling had insinuated itself into the position of leader previously occupied by the hand, the whole activity of the mind, the only weapon man had, had to be concentrated upon securing the victory in the desperate struggle for his very existence. But in the period of rest which followed the decisive victory which the mind, operating through the hand, won over the elements and the wild beasts, the mind, accustomed to ceaseless activity, could not lapse into idleness. It would need a rest, but it would take it, not by doing nothing but by doing something else. Much of it would be needed to retain the ascendancy which he had established, to consolidate the position which he had won. Much of it would be devoted to the peaceful arts of industry and agriculture. But some of it would be free to wander where it listed. Exhausted by actual wandering in space, it would take its rest by imagined wandering in time. Only in the case of men like Alfred the Great are all these three activities to be found within the compass of a single mind. As a rule, a man is destined by the original direction of his soul to be either warrior, husbandman or poet. That, however, is a minor point.

Once the mind has won the leisure which enabled it to wander, there were no limits to its discoursings.

Released from the close application of the very struggle for life, in which a mistake meant certain disaster, the mind, with its vastly increased range, would be directed towards the beginnings and the ends of things. But as a mind, hitherto focused upon the close hard work of existence, could know nothing about either, it would have no difficulty in inventing a consistent theory of the origin and fate of all things—a perfect circle dinted at no point by contact with reality. Puffed up by his victory over the elements and the brutes, man had no conception of his intellectual limitations. Forgetting that he acquired the long range of his intellectual vision by the handling of inanimate objects, he had the temerity to apply it to the interpretation and even the origin of life. He forgot that his mind, now on holiday, although emancipated from service to the hand, still had deeply ingrained in it the habits acquired during its long servitude. Thus he thought of life, and the Universe too, as having been created by a God, just as his own tools, weapons and utensils were created by himself. That he really thought that God was both in form and habit merely a very powerful man, and had created the universe in exactly the same way as a man fashions a pot, is shown by the fact that he explained his own origin by saying that this God created him after His own (God's) image.

This view of the origin of life in all its forms, including man, was satisfying to man for many years, because it was in harmony with his habits of mind and was not contradicted by what he knew about life. It also continued to be satisfying for another reason. Man invented this idea of the origin of life as we now see it with the same instrument, the mind, as that with which he would devise a tent. He therefore not only imagined that living things had been made just as he would make a tent, but he also applied the same criterion of truth to his explanation, as of trustworthiness to his tent. If there was a flaw in the construction of his tent, it would fall about his head in a storm. If there were error in the making of his theory, it would recoil upon him. But it did not recoil upon him; so apparently it was worthy of trust, it was true. Or perhaps he thought, what a great many people think to-day, that it does not really matter whether the foundation of a theory of life is well and truly laid. They think that a flaw in the construction of an aeroplane may be fatal; but they do not think that a flaw in our theory of life can be fatal. But in truth the difference between the two cases is only one of degree. A flaw in the construction of an aeroplane may endanger the life of one man; it may determine whether at a particular moment he will live, or be dashed to the ground. But a flaw in our conception of life may endanger the whole of humanity, and condemn it to an existence from which the sudden end of the airman would be a merciful deliverance. For error in the construction of a machine a man may have to pay down; at any rate, little credit is allowed. For error in the interpretation of life much longer credit may be allowed, but in the end the bill must be met; and in the long interval it may have grown to such a magnitude that it cannot be met. For the first error men pay with their body or out of their pocket; for the second with their soul. Life will revenge herself mercilessly on man if he does not try to understand her, and order his life on the basis of that understanding.

CHAPTER III

§ 1

THE Mosaic account of the origin of the various forms of life which inhabit this planet is re- jected by biologists of the present day on two very different grounds. The first is that the fossil remains of animals which have lived in past ages, some of which have become extinct whilst others have not, and the relation borne by the sequence of these ancient forms to the successive layers of the earth's crust, compel us to believe that the present forms of life have arisen by the gradual modification, in different directions, of pre-existing types. We have thus arrived at a pictorial conception of evolution in the image of a tree, which split early into two stems, the animal and vegetable kingdoms. In this picture the earliest forms of life, whatever they were, are represented by the roots; the origins of the great groups of the animal and vegetable kingdoms, respectively, are represented by the springing of the main branches from the main stem; and the species of the present time are represented by the leaves. There is no evidence from fossils of the common origin of the great groups from a single ancestral form. But we have no need to fathom such deep recesses of the past; it will be quite as much as we can manage if we try to understand the changes which have taken place in so relatively brief a period as that occupied by the origin of a handful of new forms from their parent stock. This, then, is the first ground on which the Mosaic account of creation has been rejected by orthodox biology. It is a ground of fact, and in my opinion it is firm ground.

The second ground on which the Mosaic account has been rejected is a philosophical one ; and although at first glance it appears, by reason of its use-worn surface, to be firm too, it is, in point of fact, a veritable quagmire which has led many an unwary one to a disastrous end. This second ground is that the Mosaic account of creation must be untrue because it is anthropomorphic. Anthropomorphism, strictly speaking, means the endowing of a God with the form of a man. But the meaning of the word has been enlarged at both ends. It has, very properly, been extended at the human end to the endowing of God with attributes which are more essentially human than the mere shape of man—namely, the way in which man does things, and the kind of things he does. It has also been enlarged at the " endowed " end so as to mean the endowing of anything, usually some non-human living thing, with anything human. The extension of the word at this end came about, during the nineteenth century, *pari passu* with the growth of a disbelief in God, especially amongst men of science. There was no god for them to endow with human attributes. It was a pity that a good long word like anthropomorphism should be wasted. So it was used to mean the endowing of anything non-human with any human attribute. You must not say that the lark sings for sheer joy, because that is an anthropomorphic interpretation of the song of the lark. You must not say that the protective coloration or habits of any animal are the result of intention or design in any form, because that would be an anthropomorphic interpretation of the phenomenon. Any ascription of purpose or intention, intelligence or design, to a non-human animal is condemned at once by contemporary orthodox biology on the ground of its radical anthropomorphism. It is true that purpose, intention, intelligence and design are attributes of man. It does not follow from this that they are attributes of man *only*. Yet this conclusion is the central idea of modern biology, the idea that the structure, activities and development of the organism can be explained in terms of physics, chemistry and mechanics, and that the evolution of life is explained not by intelligence, design, purpose or intention, but simply and as it were automatically by the theory of natural selection.

Before we turn our attention to the theory of the organism and to the theory of its evolution, it will be well to examine more closely the way in which the mind has been led astray by the word "anthropomorphism"; for by such an inquiry we shall arm ourselves against similar deception practised by other words.

The great length of the word, and its constant repetition, may in some degree account for its impressive effect and for its anæsthetic influence upon the critical faculty. But be this as it may —and I intend it as no more than a tentative suggestion—there can be no doubt that the word anthropomorphism affords a very good instance of the baneful effect which a word may have upon the course of thought. In its original restricted signification, in which it meant the endowing of God with the form and habits of man, it certainly denoted a grave intellectual misdemeanour, and the epithet anthropomorphic, which very accurately described this process, was rightly regarded as a stigma. But those who were responsible for the extension of the meaning of the word at the "endowed" end, for applying the word anthropomorphic to an entirely different thing—the granting of intelligence, purpose, design and human attributes in general to non-human animals, in order to stigmatise a concession to the "lower animals" which was repugnant to them—were the unconscious perpetrators of a successful fraud. One of the easiest ways to convince an audience of the untruth of an idea you wish to disprove is to apply to that belief a word which has already been brought into discredit and obloquy. If you can persuade the audience that the word fits, the trick is done. In the case of the word anthropomorphism the audience needed no persuasion; they hated the idea that an animal had a soul, many of them hated the idea that they themselves had a soul; they liked to think of the organism as a machine, they liked their mechanical theory of evolution, and they liked a long word. The belief that a non-human animal has an intelligence at all comparable to their own was branded with the word anthropomorphic, and flung into the ash-bin of exploded superstitions. It was not argument which effected the temporary expulsion of this belief; it was abuse. It was the very essence of abuse—which is calling a thing names. Happily, it had all of its ineffectualness too. Another abusive biological epithet is the adjectival form of the name of the great French naturalist, Lamarck. Lamarck's theories may have been farther from or nearer to the truth than subsequent theories of evolution, but the prevalent custom of thinking that any theory or idea can be placed once and for all on the Index of Science by merely calling it Lamarckian is founded upon the theory of abuse, and results in the confusion of the mind. The source of the trouble is inattention to the problem of the relation between the word and its meaning. The word itself does not change, but its meaning does. And unless a sharp eye is kept on the wanderings of the meaning, great confusion may result from the undiscriminating use of the word. Many people are irritated by the statement that one does not understand the sense in which they are using quite a common word; the reason is either that to them each word has its meaning tightly and securely attached to it; or, if they think the meaning has a certain freedom of movement, they avoid the problem of the relation between the word and its meaning as a difficult one (which it is); or the problem of this relation is not present to their minds at all, they do not dissociate word and meaning, and they say in a huff, "I suppose there is no such word as anthropomorphic."

The part played by words, however, in determining the course of human thought is a minor though by no means a negligible one ; but that which has chiefly directed its course is the set of habits with which the human mind began, and especially those which it developed during its acquisition of control over external matter.

Let us see to what extent our modern conception of the organism and our theory of the evolution of life are what the early schooling of the mind would lead us to expect them to be.

To deal first with the organism. It was well-nigh inevitable that the human mind, in casting about for a conception of the organism, for an explanation of its activities, should light upon that one of its offspring which, though not its firstborn, was the apple of its eye—the machine. At any rate, whatever else *might* have happened, this is what *has* happened. René Descartes, the father of physiology, the science whose function it is to discover the causes of the activities of animals (and plants too), was the author of the mechanistic theory of the organism. The essence of this theory is that a living thing does what it does because it is made in the way it is made. The function of an organ is determined by its structure. If we could know everything about the structure of an organism, we could predict how it would behave in response to any imaginable set of stimuli. The analogy used by Descartes to illustrate his theory of the organism is well known. He compared the human body to one of those grottos, devised for the amusement of princes, in which there were waterfalls and fountains, booby-traps and mechanical figures of men which played mock musical instruments, all supplied and worked from an office in the centre of the garden, from which pipes radiated to the various displays, and in which sat the hireling who, by turning on one tap, could turn such and such a fountain on, or by turning another make a mechanical figure play a fiddle. The various activities of the whole apparatus were determined by the diameter of the pipes, by the way in which they were fitted into the waterfalls and figures, by valves in them, and by any ingenuities the inventor chose to devise in them. The man in the central office only had to turn the right taps or levers to direct the water in the desired channel, and, provided there was no leakage, and the joints and wheels were well oiled, he could make any part of the system perform as he wished. The various parts, and the whole, of this system did what it did because of the way in which it was made. If the mechanical 'cello-player was served by the same pipe as that which worked a neighbouring fountain, he had to play when this fountain played.

Descartes compared the nerves (which he believed to be hollow) in the human body to the pipes in such a grotto, the animal spirits (which he believed to be gaseous) to the water which drove the system, and the muscles and limbs to the various displays. The entrances of the tunnels in the nerves into the brain were guarded by doors ; and when the body was in a state of rest the animal spirits were pent up in the cavities of the brain ; but when a stimulus was received from the outside, it was transmitted along a nerve to the brain, the doors guarding the entrance to the nerves supplying those muscles which were required to be set in motion were flung open, the animal spirits rushed out along the nerves and set the muscles in appropriate action. The essence of this explanation of the performances of the human body is that the body is regarded as a machine. The manner of its action is the result of its structure. It is a theory of the living thing which could not but be immediately intelligible. When the human mind invents or encounters the mechanistic theory of the organism, it is confronted with an apparition which it at once recognises as the darling of its adolescence and the symbol of its power—a machine. No wonder it welcomed the theory with open arms.

§ 2

If we look a little closer at the theory we shall see that it is a materialistic one too. As in the case of the machine all action is the result of the form of the solids of which it is made and of the motive force acting upon its different parts, so in the case of the body, according to this theory, all action, however complicated, is the result of the form of the matter of which the body is made—i.e. of the gross structure of the muscles and limbs and the delicate structure of the nerves, and of the interaction of the chemical juices which it contains. It is a materialistic conception of the living thing because it means the belief, held by the great majority of biologists at the present day, that all the manifestations of life are capable of being expressed in terms of matter.

It is true that Descartes endowed the human body with a soul; but it is probable that he did this to avoid trouble with the ecclesiastical authorities. His placing of the soul was a pleasant fancy; he put it in the pineal body because that was the only unpaired structure in the brain, and he called the two diverging strands of nervous tissue which extend downwards from this body the *habenulæ*, and thus gave the picture of the soul as a coachman, perched up on his box, with his reins between his fingers, driving with a cheerful confidence that restive team, the human body.

But to the non-human animal Descartes did not vouchsafe a soul. The orgy of vivisection which followed the enunciation of the mechanistic conception of the organism was justified by those who took part in it on the ground that the cries of their victims were no more than the sounds produced by the disruption of machinery. The difference between the noise produced by kicking a dog and kicking a tin can, according to such men, is due to the difference between the structure of the dog and the conformation of the can. But the cause of the noise in the two cases was the same, the application of the requisite stimulus to an arrangement of material particles; whether such arrangement take the simple form of a tin can, or assume that complexity of structure which is found in the familiar mechanism to which we are accustomed to apply the name of dog.

Modern Biology, the lineal descendant of, and so personally identical with, the mechanistic conception let loose by Descartes about two centuries ago, professes to have explained many vital phenomena in terms of matter—that is to say, in terms of physics, chemistry and mechanics—and it holds out to those who wish to embrace it the hope that in the course of time most, if not all, of the manifestations of life will be interpreted in terms of matter.

It is evident that in this philosophy of life there is no place for the soul. Modern Biology has in this respect gone one better than Descartes. It denies a soul not only to animals other than man, but to man himself. The biologist throws a sop to those who cling to the belief that man has a soul. "We have now," he says, "a scientific conception of that to which the term 'soul' used to be applied. If you wish to continue the use of the word 'soul,' you must not mean any more by it than the sum-total of the activities which are the result of the structure of that complex mechanism, the human body."

According to the scientific view, it will be seen, the soul may be used as an aggregate term for the activities of the various parts of the body, especially the brain. The structure of the matter of which the body is made and the chemical interaction of its gases and juices are the cause of these activities. Matter is the master—the sole determining cause; the soul is only permitted to exist on sufferance, and is reduced to the level of a mere symptom.

But the mechanistic explanation of the activities of the organism will be seen on careful inspection to be a very shallow explanation. Mechanism is certainly the proximate cause of certain simple forms of vital activity, just as the structure and mutual relation of the parts of a man-made machine are the proximate cause of the performance of that machine. But the structure of a machine is no more than the proximate cause of its activity. Structure is only one stage behind action in the causal sequence. True, if the machine were built differently it would act differently; but that does not make the build of the machine the explanation of its activity. The explanation of the performance of a machine surely lies deeper than its structure, and is to be sought in the idea of the machine as it gradually took shape in the mind of the man who invented it to fulfil a particular purpose. May not, then, idea, intention, design, purpose be the explanation of the mechanism which we perceive in the organism? The whole trend of current biological thought is to give an emphatically negative answer to this question. It is not that the modern biologist shrinks from probing deep; on the contrary, he thinks that he has not only touched bottom, but that the foundations of his biology rest upon bedrock itself. But is it not possible that the sensation of security which he thinks is due to his touching bottom is really due to the fact that he feels at home with a machine? Is it likely that man has fathomed the depths of life? It is more likely that his sense of security is an illusion. Man treading water on the ocean of life is more likely to attain to a sense of security by coming to think that he is trundling a bicycle than he is by actually touching bottom.

But if mechanism is not the explanation of the organism, what is the alternative view? If the soul is not the aggregate symptom of the structure of the parts of the body, what is it ? The alternative view is that the form of the body is the effect of the soul; that matter is subservient to spirit; that structure is the result of activity, and not activity the result of structure. Those who hold this alternative view do not think that the mechanistic explanation is false ; they believe that it gives a very true picture of the proximate causes of the mechanical devices made use of by life. They do not deny that a great number of vital processes can be explained in terms of physics, mechanics and chemistry; life, they admit, uses matter to achieve its ends ; but they believe that there is behind these material phenomena which we are beginning to understand, something which is the cause of these phenomena, and which we do not yet even dimly understand.

This, then, is the problem of life as it presents itself when we address ourselves to the theory of the organism. Is the soul a mere aggregate symptom of a mechanism—the body ? Or is the body not rather the instrument of the soul ?

[The MS. breaks off at this point.]

I

Appendix to "An Introduction to a Biology"

Mendelian Practice in the light of Bergson's Biology[1]

I

THE boundary of the territory of biology marches with that of the science of agriculture at one point, or rather in one region, as does the boundary of Germany with that of France. In this region the academic science of biology touches the practical science of agriculture. And this contact has been to a great extent brought about by the development of the ideas of heredity which we owe to Mendel.

But on its other frontier the territory of biology has recently come in contact with that of philosophy. This contact has been effected by the work of M. Bergson; and it has to be confessed that the reception of this friendly invasion has been a sullen, and in many cases a peculiarly offensive defensive, so that the only success hitherto achieved by M. Bergson in the ranks of the biologists has been a few prisoners here and there. This, in my opinion, is the greatest event, so far, in the history of thought in the twentieth century: the realisation by a philosopher—and a philosopher who has already captured the imagination of the imaginative section of mankind—the realisation by a philo-

[1] From a paper read to a joint meeting of the Agricultural Discussion Society and the Scientific Society, Aberdeen University, January 15th, 1915.

sopher that he must take into account the essential facts of life, and especially the phenomenon of the growth of life from its birth on this planet—the phenomenon, namely, of evolution. But M. Bergson does more than this: he is the first philosopher to insist that the philosopher must take into consideration that mass of theory and fact which we call Science.

Kant, it is true, insisted on the absolute necessity of a familiarity with Science to the philosopher. But Science to Kant meant only mathematical science. From Kant's time to the present day Science in all its branches has advanced with such rapidity that it has become more and more difficult for the philosopher to keep pace with it and to assimilate it. The result has been that the gulf between science and philosophy has become ever wider, and the philosopher has become more and more concerned with the theory of knowledge and less and less with things known.

So complete had this unnatural estrangement become that when in 1859 Charles Darwin finally, after a century of unsuccessful efforts by Buffon, Erasmus Darwin, and Lamarck, succeeded in persuading mankind to swallow the pill of evolution by gilding that pill with the materialistic theory of Natural Selection—so complete, I say, had the estrangement between philosophy and science become that when the most far-reaching of biological generalisations was formulated and accepted—I mean the doctrine of evolution—philosophy paid no heed.[1] Not until the publication of M. Bergson's "Evolution Créatrice" was the doctrine of evolution admitted into the circle of philosophy. Nor was the admission of this new-comer a mere formal and belated honour to a distinguished foreigner. M. Bergson incorporated evolution into the very fabric of his philosophy. It might almost be said that he made evolution the foundation of his philosophy. The stone which the

[1] The Author did not forget Herbert Spencer when he wrote these words, though he may have forgotten Hegel.—Ed.

builders rejected the same has become the headstone of the corner.

What, you may ask, has this to do with agriculture? In my opinion, a very great deal—in a general way and in a particular way.

In a general way—viewing the continent of knowledge from a point higher up than an aeroplane can get, but not so far off as the moon—we have seen that the territory of biology has extended its western frontier and has grown into very intimate contact and union with the neighbouring territory of the practical science of agriculture; and on its eastern frontier biology marches with the territory of philosophy. Now it is true that the results of this latter contact have not so far been encouraging. Biologists have, with few exceptions, rejected M. Bergson's speculations as fanciful and vain. The materialistic doctrines of orthodox biology have so coloured thought that a man can get up and say, "War is good: I can justify this statement biologically: the law of the stronger holds good everywhere: those forms survive which are able to procure themselves the most favourable conditions of life and to assert themselves in the universal economy of nature." That is Bernhardi's statement of the doctrine of Natural Selection. I think it is the best concise statement of it that there is. Bernhardi's biological justification for war is based on the materialistic doctrine of Natural Selection.

The history of biology in the nineteenth century is the history of the attempt, doomed to failure, as I believe, from the outset, to express vital phenomena in terms of matter. We are beginning to see what a horrible failure it has been; and I believe that the biologist, if he is to succeed in giving an approximately correct interpretation of life, must, to a large extent, begin over again and follow up the clues given by Samuel Butler and Henri Bergson. If, then, biology is to be affected by the vital philosophy of Bergson (and I for one believe that it is going to be so profoundly affected by it that the course of biological speculation in the near future will be largely determined by that philosophy) it behoves us to consider how that philosophy affects biology on its applied side.

I admit that there is a *prima facie* justification for the question which must have occurred to many of you: "If biology has only just begun to be touched by this new philosophy on one side which we called its eastern frontier, it is not very likely, is it, that the extreme, opposite side of biology—its applied side—will be affected by that philosophy already?"

But, in point of fact, it happens that the particular department of biology upon which at present most light seems to be at once thrown by Bergson's view of life, is the general significance of the Mendelian phenomena. And, as you are aware, it is the principles deduced from these phenomena which have been offered by the biologist to the practical breeder as a key to all, or most of the problems of breeding. And this brings us to the consideration of what I meant by saying that this new philosophy has a bearing on the science of agriculture in a particular way.

Our next business, therefore, is to consider what are the essential features of Bergson's view of life. Bergson believes that life is, in its essence, a thing which flows, a stream which gathers experience as it flows onwards, experience which in the case of *man* is never lost but always retained. We carry with us all of our past, and this past is always liable to crop up in our psychic life. A rolling stone gathers no moss. That is because a stone is a lifeless thing. But a living thing whose real life is made up of its experience is like a snowball which is rolled in the snow; it increases, as it is rolled, with the snow that it gathers, until it becomes big enough to make a snow man of. This flow of our psychic life is regarded by Bergson as that *real time* which we live and in which we move and have our being, as opposed to the mathematical time which is a mere abstraction of the human mind. Our very existence, according to Bergson, is Time. I need not labour the point that our very existence, our real being, is in essence a flow. Anyone who has devoted himself closely to the study of his fellow men, or of himself, must be aware that man's psychic existence is a flow of moods like the procession of shadow, rain, and sunshine across a landscape on a windy day.

Now if we hold a mechanistic interpretation of life—in other words, if we believe Natural Selection to be the explanation of evolution, we believe that the changes which have taken place in the habits and, in general, the actions or activities of animals and plants have been preceded by structural changes in the organs (I might almost say the parts of the machines) which performed those actions. According to this view, changes in form lead the way; changes in activity follow in their wake.

But if you hold a vitalistic interpretation of life, as Lamarck did, and Butler did, and Bergson does, it is just the other way. That which leads the way is changes in activity or attempted action; that which follows behind (because it is brought about by this action) is changes in the organ which attempted to perform the action. Now we know that the more often an action is performed the more fixed does it become. And if we believe that the shape of an organ becomes gradually fixed over the course of countless generations by the repetition for myriads of times of a particular action by that organ, then we believe that the fixation of that character is due to the repeated performance of an act by the organism which possesses it; which performance is an integral part of the very life of the species; which life is the stream made up of the successive generations, i.e. the time lived by that species. In other words, Time has *everything* to do with the fixation of a character. The longer the action, which is the function of a given organ, has been performed, the more fixed will the character of that organ be. So much then for the bearing of Bergson's conception of life on that most important of biological problems, the determination of the causes of the fixation of a character.

Now there is another aspect of this view of life to which I have already referred. It is that this stream, which is the very essence of life, is a perpetually widening stream; that something new is being added to it all the time. An essential feature of life according to Bergson is that in its growth (or its flow) it is perpetually elaborating the absolutely new. So essential a feature of life does Bergson regard *creation* by life that he has embodied it in the apparently paradoxical and brilliant title of the work in which he deals especially with evolution—" Evolution Créatrice." The saying that there's nothing new under the sun expresses that view of change which is perfectly true of *non-living* matter, namely that change consists merely in the redistribution of already existing elements; a redistribution which often, of course, produces an impenetrable illusion of change and of the creation of something new. Nevertheless, it *is* an illusion; and we are thus led to the following conclusion with regard to change in unorganised, i.e. non-living matter: that no more is got out in the effect than was put in in the cause. You put pigs into the machine at one end and you get sausages out at the other. Pigs are not sausages, nor sausages pigs. You have the illusion of change; but, in reality, all that has happened has been a redistribution of elements. When, however, we look at change in the domain of life we are in the presence of the continual creation of the absolutely new. More *is* got out in the effect than was put in in the cause. More *is* got out in the end than was put in in the beginning. So far as this planet is concerned, therefore, we must relegate the saying that there is nothing new under the sun to the limbo of exploded doctrines. For this planet has produced that wonderful thing called life; and where life is, this saying is not true. If for old acquaintance' sake we wish to retain the saying in some form we may say that there is nothing new *in* the sun, where the conditions for the existence of life do not obtain.

There is a third feature in which life differs from unorganised, i.e. non-living matter, according to M. Bergson, namely that in the performances of living things there is an uncertainty which is such that these performances cannot be predicted mathematically. Here is a feature in which life differs profoundly from unorganised bodies, the movements and behaviour of which can be predicted with mathematical precision.

We now come to the fourth and last aspect of Bergson's view of life to which I wish to draw your attention. I have already said that the history of biology in the nineteenth century is the history of the attempt to explain the phenomena of life in terms of chemistry, physics, and mechanics. And you must already have gathered from the whole tenour of what I have said that I agree with Bergson in thinking that this attempt will not be successful. I believe that the living thing is something far more than a highly complex machine filled with a lot of chemicals. How, then, you will ask me, do you account for the great measure of success which has attended the application of chemical, physical, and mechanical principles to the solution of biological problems ? The answer I would give is as follows. I do not for a moment deny that the problem, which confronts the organism, of the getting rid of waste products, or of the secretion of pigment, or the manufacture of bone, is in part a chemical problem, or that the mechanism of the control of the temperature of the organism is a problem which can be expressed in physical terms, or that the child draws in its mother's milk by mechanical means. But I do not believe that the soul is nothing more than a conveniently brief term for the sum-total of the activities of the nervous system. Surely it is the other way round: the nervous system and all it controls are the elaborate instrument of the soul. I do not deny for a moment, therefore, that the organism employs physical, chemical, and mechanical means to carry out its work. But if you think that when you have described these means you have interpreted life, it seems to me that you are resting when your work has only just begun. You have not explained why a baby sucks at its mother's breast when you have described how it does so in the terms of the village pump.

If we confine ourselves to the chemical activities of the organism we may compare, as Bergson compares, the organism to a retort, and its various juices to the contents of that retort. The retort corresponds to the essentially vital part of the organism and the contents to the inorganic substances in its composition. Doubtless, it would be difficult to draw a hard-and-fast line and say where, within the organism, retort ends and contents begin. But the comparison is only intended to be a rough one and to draw a general distinction, not a sharp line, between the essentially living parts of the body to which chemical principles do not apply; and those parts, or rather contents, of the organism which are not one with the most vital part of the organism, i.e. its individuality (or soul), and which, like for instance cartilage or chitin, or bone or pigment, are the same over a long series of animals. These parts, whether they constitute the scaffolding, like bone, or the clothing, like pigment, we may regard as separable or detachable elements in the body, which do not enter into any close union with the essential life of the organism. With regard to pigments, these constitute, as it were, the outermost, the *most* non-living layer of the not living parts of the organism. They are limited in number, like the paints on the painter's palette; though the variety of pictures which the animal or plant can paint with them on its own wings or petals is without end. It is the picture which it paints, the type of its species, which is individual and unique and alive and part of its very being; the pigments with which the picture is painted are dead and might just as well have been used, and of course *have* been used, by other species to paint different pictures. Bergson's analogy between the organism and a retort does not appear to me to be a perfect one, because we are apt to gather from it that the distinction to be emphasised is between inside and outside, whereas it is, of course, between essentially vital and not-essentially-vital, or rather not-at-all-vital, in fact dead. And we have just seen that some of the chief non-living constituents are the farthest outside, namely, the pigments to which the term "contents" does not apply at all.

Let us now re-state concisely the four main conclusions to which a consideration of M. Bergson's philosophy has led us:—

1. Time is the essential factor concerned in the fixation of the characters of organisms.
2. Life is perpetually creating the absolutely new; more *is* got out in the effect than is put in in the cause.
3. The performances of living things cannot be predicted mathematically.
4. The organism consists of an essentially vital part and of non-living constituent parts.

Now look at the Mendelian principles of Heredity, which are offered to the breeder as an instrument of great value.

(1) One of the chief claims of the Mendelian is that his theory gives, for the first time, a coherent scientific explanation of the fixation of characters. Fixity of a particular character, according to the Mendelian, is due to the fact that the organism which bears that character was the result of the union of two germ-cells, both of which contained the factor for that character. In Mendelian terms, the organism was homozygous for that character. In-

stability or unfixity of a character, on the other hand, is due to the fact that though one of the germ-cells which produced the organism bearing the character in question contained the factor for that character, the other germ-cell contained the other factor of the pair. In Mendelian terms the organism was heterozygous for that character. Fixation, that is to say, is due, according to Mendelian principles, not to the continued breeding to a particular standard over a great number of generations, but to the union of germ-cells bearing the appropriate characters. In other words, time plays no part in the fixation of a character.

(2) Another idea essential to Mendelian principles is that all that the breeder can do is to effect re-combinations of existing characters. The creation of new characters has no place in the Mendelian scheme.

(3) Lastly, the Mendelian principles render possible for the first time the prediction of the result of the union of a given pair of individuals, in cases which have already been subjected to Mendelian analysis; and not only this, but the numerical ratios in which the various characters will appear amongst the offspring resulting from the union can also be predicted.

If these conclusions be compared with the first three conclusions arrived at after a consideration of M. Bergson's biology, it will be seen that they are flatly contradictory. His main conclusions with regard to life are untrue of Mendelian characters. This may mean either (1) that M. Bergson's conclusions are ill-founded, or (2) that the Mendelian characters are dead or, at any rate, appertain to the least vital parts of the organism. I believe the latter alternative to be nearer the truth. If it is nearer the truth, we have, I think, a clue which will enable us to relegate the Mendelian characters to their true position among the characters of living things; and a suggestion which may enable us to determine, without experimentation, which characters are likely to behave in a Mendelian way in heredity and which are not. And it would seem, in general, that Mendelian characters are to be found amongst the contents of the retort and are not exhibited by the retort itself.

II

Notes of a Lecture given in July, 1914, at the Graduate School of Agriculture, held at the University of Missouri, Columbia, Missouri [1]

There is a fundamental difference between character of species and character of varieties.

According to De Vries a varietal character is one which may be borne by any plant or animal. A species character must be borne by all individuals.

Character of a species is the *ensemble* of characters and something unique.

It is like a man's written signature. A varietal character is like a stamped signature. It may be stamped over a large number of individuals.

Varietal characters are all that will be possible of isolation as unit factors; e.g. wrinkledness of peas is also stamped on corn: it is not a species character.

De Vries thinks varietal characters are Mendelian. Probably Mendelian characters are varietal characters.

Four general conclusions with reference to Mendelian results:—

(1) Time is no factor.
(2) Nothing is obtained that is not put in.
(3) Varietal characters are unit factors.
(4) Mathematical expressions can be applied to Mendelian phenomena.

Has time anything to do with the fixation of a character?

Experiment with peas to test this.

There are two varieties of pea-flower in regard to colour: the white pea-flower (the commonest) and the pink pea-

[1] *Vide supra*, Preface, p. xvii.

flower. These crossed give a purple flower, the ancestral form. The experiment explained, on the basis of two sets of factors, thus :—

B = factor producing blue only in the presence of factor for pink, P;

hence Bp = white, and therefore $BPpb$ = purple.

White pea-flower Bp × Pink pea-flower Pb

purple ancestral form
$BPpb$

Purple 9	Pink 3	White 4
$PPBB$, $PPBb$, $PpBB$, $PPBb$, $PpBB$, $PpBb$, $PPBb$, $PpBB$, $PpBb$	$PPbb$ $Ppbb$ $Ppbb$	$ppBB$, $ppBb$, $ppBb$, $ppbb$

This is a convenient way of expressing the results, but it does not really signify anything as to what is actually going on in the plant. If four whites really have these combinations of factors, then it may be tested by crossing the whites with a homozygous pink, and theoretically the results should be :—

PINK (*homozygous*)	×	WHITE	PURPLE	PINK
1. $PPbb$	×	$BBpp$	100%	—
2. $PPbb$	×	$Bbpp$	50%	50%
3. $PPbb$	×	$bbpp$	—	100%

This has been done with a small number of plants, and is being repeated on a large scale by Darbishire. Darbishire found that in the cross (2. above), $PPbb \times Bbpp$, the ratio was about 3 : 1 and not 1 : 1.

All that the 50 : 50 ratio shows, if it be true, is that the Mendelian explanation only accounts for what is happening in gametogenesis of the second generation, and we should not base a general conception of heredity on the conditions obtaining in the second generation. Darbishire obtained seeds from many sources, with the result that pink by white always gave purple, which means that all the white are *BBpp*, i.e. all carry blue in a homozygous condition.

There are no pure whites on the market, if we accept the Mendelian explanation.

Darbishire does not believe that this formula gives us what is really happening. He believes that the union of these two characters sets up a disturbance, causing them to fall back on what they used to be, while later segregation occurs.

The degree of disturbance would be comparable to the length of time the two strains remained isolated.

Darbishire has planned and is now continuing the following experiment :—

Select pure pink with formula *PPbb* and pure white with formula *ppbb*. Cross these :

PPbb × *ppbb*

pink.

Save these seeds, and in next generation make the same cross :

PPbb × *ppbb*

?

If the Mendelian theory is correct, saving the seeds and repeating this cross again and again should never give anything but pink. However, if there is anything in the other view, namely, that a disturbance occurs proportional to the length of time of isolation of the two strains, then sooner or later purple will appear. Darbishire predicts that this is what will happen.

III

Letter from A. D. Darbishire to M. Bergson, of December 6th, 1912, accompanying a copy of his book "Breeding and the Mendelian Discovery." [1]

Letter from M. Bergson to Mr. Darbishire.

Villa Montmorency,
18 *Avenue des Tilleuls,*
Auteuil, Paris,
12. *Dec.* 1912.

Cher Monsieur,—Je tiens à vous remercier pour l'envoi de votre ouvrage "Breeding and the Mendelian Discovery" et pour l'aimable lettre qui l'accompagnait. Quoique surchargé de travail, d'occupations, et de préoccupations en ce moment, je n'ai pu résister à la tentation de lire tout de suite le livre jusqu'au bout, tant les premiers chapitres m'avaient séduit par la précision et l'élégance de l'exposition. Vous avez bien raison de présenter cet ouvrage dans votre préface comme nous donnant une vision *immédiate* des faits, sans hypothèses interposées; mais cette vision, en raison même de sa netteté et de son caractère concret, suggère au lecteur des hypothèses et des idées explicatives. Sur ces hypothèses vous avez eu la discretion de ne pas appuyer dans l'ouvrage lui-même; mais vous les formulez dans la lettre si intéressante que vous avez bien voulu m'écrire. L'idée

[1] This letter, to which I have found many references in correspondence and notes, has not yet been traced. M. Bergson, who has the letter in his possession, promised to let me have it as soon as it could be found, but it has not reached me in time for publication in the present volume. I am greatly indebted to M. Bergson for his kindness in allowing me to print his reply of December 12th, 1912, which gives an indication of the contents of Mr. Darbishire's letter.—Ed.

de voir dans l'hérédité mendelienne un phénomène de *descente* plutôt que de *montée,* et de considérer la domestication en général comme déclanchant certaines potentialités de perte ou de destruction, est originale et suggestive au plus haut point. Je souhaite que vous la repreniez et la développiez tout au long dans l'ouvrage que vous projetez d'écrire sur l'évolution.[1] En attendant je tiens à vous envoyer, avec mes remerciements et mes compliments, l'assurance de mes sentiments dévoués.

H. Bergson.

IV

NOTES AND EXTRACTS

My attitude to the reader not that he must accept this as logical necessity, but may if he like.

* * * *

Biology intermediate between painting and exact science.

* * * *

The spoon, knife, and fork are organs which the civilian has detached and put into the "pool" of the family or restaurant. They are less detached from the soldier.[2]

* * * *

It may have been that curiosity operating through the hand suggested the assumption of the erect posture. According to this view, it would not be the erect posture which set free the hand, but the desire to use the hand which suggested the erect posture.

* * * *

The explanation of the lathe is not its structure, but its history.

* * * *

Farseeingness of Germans, because intellect followed lines of evolution of weapons.

[1] This refers to the work of which a fragment is here published, "An Introduction to a Biology" (*vide supra,* pp. 1-89).

[2] Several of these notes were jotted down while the author was undergoing training as a private soldier.

Description influenced by Interpretation. Hausmann interpreted development of horse in terms of man.

Five nerves have always been described as going to electric organ of Torpedo, because the original investigator described five. There are four.

* * * *

If all description were direct it would be bad enough; but most of it is copied. What an *additional* source of error is here!

* * * *

My dissociation between seeing and understanding: Lying in bed, I discern an outline, blocks of light and shade. I do not know which is light and which shade. I cannot recognise the object. I cannot fit it into a category. Seeing is remembering.

[Correlate my inability to interpret the shadow with the Siamese-twinness of Description and Interpretation.]

* * * *

A theory which touches reality at no point cannot be disproved or proved by an appeal to reality.

* * * *

I see theories as balloons. Their makers inflate them, their admirers over-inflate them; and they generally burst from over-inflation.

* * * *

Those who prefer swimming out of their depth may speculate upon first causes and on what happens after death. But the biologist, whose business is the difficult task of understanding life, must be careful to undertake a much less ambitious and precarious task.

* * * *

Obvious reason for thinking in terms of opposites is that it is thinking in terms of a struggle—perhaps the best instance of the application in the conceptual sphere of habits of thought contracted in the sphere of action.

We think of a thing in a straight line. So Natural Selection explains improvement: it does not explain divarication.

* * * *

To draw the line somewhere. . . . Of all forms of self-expression drawing lines is the most inartistic and the least satisfying.

* * * *

Matthew Arnold says, know what you want to say, and say it as simply as you can. Strange not to see that the idea does not fully exist until it is expressed; but as in the eyes of the law a man's life begins with birth, so an idea dates its origin not from the moment of its first dim conception, but from the moment when it was born, i.e. literally expressed.

* * * *

The secret of —— is that she doesn't externalise. She cannot use a watch. She is the opposite of ——, e.g., who has externalised himself in his museum and his library. None of her interests are external. That is why she has such power: she doesn't dissipate it: she keeps it all in. If one must externalise oneself, one should do it spiritually through art or children.

* * * *

"Original." The sources of the Ock come from rain, but only after that rain has passed through much earth. My drawings come from reality, not direct—I never copy—but through many days of my existence. Beethoven's revelation of life comes from life, but not only the life he has lived in his own person, but from the life he has lived in the persons of his ancestors extending back probably to pre-human ones. A man cannot formulate a conception of life from the conscious observations he has made during his own lifetime. He must formulate and make conscious what is already in him.

"The mantle of A falls upon B" means that B is the next source through which what flowed through A will flow out.

* * * *

There can be no laws of heredity: least of all of hereditary genius. Mendelian characters *may* not be free, but mental ones probably are, and the production of genius must be.

* * * *

Reason why facts of Biology are so specialised—the desire to discover something new, e.g. description of new species. But genius consists not in telling you something nobody knew, but something everyone knows.

* * * *

Originality is the attribute of a man—like Beethoven—who is an instrument in the hands of a power stronger than himself, the spring through which a message is delivered. It is the attribute of the work of the man or of the man himself (for in the case of great men the man and his work are one) who expresses for mankind what they have known all along so deep in their hearts that they did not know they knew it. Originality is the attribute of those men who are born with the gift which they hand on to mankind.

But in current parlance the words "original" and "originality" mean the very opposite of this. Originality is taken to be the doing of something new, or different from the ways in which it has been done. "Original research" means working in a new field and bringing to light facts which have not been discovered before. It ought to mean work which *searches again* into *origins*.

* * * *

Life has got into the very hands least fitted to deal with her—into those of glum octogenarians fondling dry bones with their withered hands.

Why we like fire: the forms of flame are never repeated, i.e. they possess the attribute of life.

*   *   *   *

If you break the bone of your nose so badly that a portion has to be removed, you can obtain a piece of bone from another mammal and have it put in its place. But if you should lose a part of your soul, a much commoner accident, no such operation could be performed.

*   *   *   *

———'s saying, "Pity poor D——— has done nothing." Have I? His idea that one must perpetually be publishing to keep oneself in known-ness is like the cinema bridging over gaps between successive moments. It is extension, not depth.

May 1, 1915.

To G. Herbert Thring, Esq., *Secretary to the Incorporated Society of Authors.*

Dear Sir,—In reply to your circular of last month with regard to the use of the cinematograph for educational purposes, I write to say that I am strongly opposed to the application of the cinema to the study of life, i.e. to biology.

The tendency to make the scientific lecture a sort of music-hall entertainment which carries on the interest from one moment to the next by "experiments" and "lantern slides," and what not, at the minimum expenditure of effort on the part of the listener, requires, in my opinion, no further encouragement. The rôle of the cinema is to amuse even when it covers amusement with the cloak of "interest," and not to stimulate. The cinema encourages the natural tendency of man to seek his pleasures and interest where he has found his instruments, outside rather than within; but it takes us not into the souls of things, but into an unreal, artificial region "midway between things and ourselves, external to things, external also to ourselves."[1]

[1] Bergson, "Laughter." Eng. transl., p. 154.

M. Bergson has expressed himself, in his Gifford Lectures, on the Problem of Personality, very strongly on the lines indicated above.

In short, I should regard the use of the cinematograph in the teaching of biology as fraught with the gravest dangers to the mind.

I am, yours faithfully,
A. D. DARBISHIRE,
Lecturer on Genetics and Demonstrator of Zoology in the University of Edinburgh.

[Notes for a paper read to the Students' Natural History Society at the Royal College of Science, December 19th, 1910.]

The man of science, in announcing his conclusions, says "the facts speak for themselves." But facts do not speak for themselves. They say what we make them say. What, then, is the scientific investigator to do? Is he merely to record facts and leave them to say what anyone who comes across them chooses to make them say, or is he to make them speak? In other words, is it the function of science merely to record, or is its function to record and interpret as well?

I think an answer to this question can be attained by first answering another question with regard to the function of science. Is the sole function of science the elucidation of truth? I think it ought to be; but there is no question that it is not. I mean that many who genuinely believe that they are engaged in laying bare the truth are certainly, in many cases, doing no such thing; and, even when they do accidentally provide a clearer vision of things as they are, they are engaged in this work not *solely* because they wish to get at the truth. They *do* wish to get at the truth, of course; but that is only a symptom of the force that drives the natural philosopher to construct his philosophies.

My thesis is that this force is the same as that which lies behind poetic utterance of every kind, whether it reach us through the eye or the ear. The masterpieces of art and of science are both due to the irrepressible need for the thing for which there is no better word than self-expression. The heroes of science, as well as of art, have always expressed themselves by prodigious performances in the imaginative sphere. "But surely," you may interrupt me, "Darwin's work was great because it discovered the truth about evolution." Well, that is not my view; I do not think he did discover the truth about evolution; he persuaded people to believe that evolution had taken place—a prodigious feat. The great in science are not those who discover new facts, but those who imagine things about the facts discovered by others. This imagining of things about facts is called detecting the principles underlying the facts; but it is nothing of the kind, because there aren't any principles underlying them—and don't you forget it.]

I am not now offering any opinion as to the desirability of imagining things about facts; I am merely stating that the great in science are those who have produced great imaginative works, and not those who discover. Has anyone here not heard of Darwin? How many of you have heard of Giesbrecht? If you have heard the name of Rothschild, it is not because a member of that family has a world-wide reputation as an authority on fleas. . . .

You will gather that my theory is that science provides an opportunity for exercise in the imaginative sphere just as music or painting does. There is one side of the human soul which needs to express itself by creative work of some kind; this may either be done through the medium of those forms of art which appeal to one through the eye, or those which appeal through the ear. I can perhaps give you an idea of the general character of these poetic, i.e. creative utterances by showing you the attempts which I made, at the age of about eight, at creative utterance. I see in them many of the things which I now hear in Beethoven's music. Rushes, complexities, antagonisms, prodigious effort, elemental fun; many of them cast in definite form or design. [Drawings exhibited.] I expressed myself in drawings because I could not in music; similarly I think those who cannot go the whole length of obtaining satisfaction from music obtain it from poetry, which is music cast in words instead of notes. That is why those who really make use of music do not read poetry, and that is why Browning, the least sonorous of poets, was a great lover of music, and Tennyson, the most musical in his poetry, didn't know a note.

And at last—here is my point—science itself is used as a means of creative utterance; the joy a man of science experiences in reaching his conclusions.[1] . . .

[From lectures on Evolution, delivered at the Royal College of Science.]

Evolution cannot be studied out of relation with, and isolated from, the objects of human interest which surround it. I can conceive of no more barren exercise than a study of evolution which is not also a study of life. The two studies cannot be conducted separately. In the future a theory of life, like that of Coleridge, which is not also a theory of evolution, will be of as little value as a theory of evolution like that of de Vries, which is not also a theory of life.

. . . A theory of life is a much bigger thing than a theory of evolution. Evolution is only one of the numerous manifestations of vital activity; but it is that manifestation the study of which will help us most in attaining to a true theory of life. Those of you who have read my paper entitled, "Some Tables for Illustrating Statistical

[1] The MS. breaks off here.

Correlation," may remember that I concluded it with the observation "Directly we can play with machinery we can see how it works. Movement and change enable us to perceive and to understand." I think this represents a very general truth about our relation to natural phenomena. And if you agree that it does you will also agree that our best way of attaining to a true knowledge of life is to study the phenomenon presented by life undergoing change, that is to say, to study evolution.

Whatever may have been thought of any scheme for improving mankind, whether physically or morally, put forward before the time when a belief in evolution had become general, the one thing certain about such a scheme is that it belonged wholly to the realm of fancy. But now that a belief in evolution has become current intellectual coin, the hope of breeding a race of supermen has become more than a Utopian dream, and the early years of this century witnessed the foundation of a department of Eugenics whose ultimate object is the amelioration of the race. The knowledge that we have not always been in the past as we are now gives us hope that we may not always be in the future as we now are.

. . . The plasticity of the organic type is the one thing which gives us hope for the future. Was there not some prophetic significance of this kind in the words spoken by Ophelia in her madness: "They say the owl was a baker's daughter. Lord! we know what we are, but we know not what we may be"?

But Rome was not built in a day, and the change which can be effected in a single generation will be infinitesimally small. And though we cannot hold the extreme form of belief in this plasticity which was entertained by Ophelia, who quotes without comment, but as the context shows with approval, the statement that the owl was a baker's daughter, we may effect some alleviation in the suffering caused by the knowledge of what we are from the fact, now established, that we know not what we may be.

.Darwin and the Origin of Species [1]

Darwin performed the gigantic task of forcing mankind to believe in evolution. So great was this task that it seems almost too much to expect that the theory by means of which he effected this should be true as well. The mere fact that the theory—Natural Selection—which attempted to account for evolution could be understood, or, at any rate, was soon accepted, by nearly everybody invites by itself the suggestion that this theory gives a picture of the causes of evolution which, at any rate, is not complete. For it is not as if evolution were a simple matter; it may be said that we know hardly anything of the factors to which it is due. And the fact that we are all actors in the evolutionary pageant renders it almost impossible for us to visualise the process from a detached standpoint as the master of a pageant does. This brings us to what seems to us to be far and away the most interesting question which can confront the student of life—namely, whether evolution is a process of which a simple mechanistic explanation has been discovered; or whether it is not a mysterious process which we are scarcely able to understand at all yet, but which may, perhaps, be due to deliberate striving on the part of the animals and plants which have taken part and are taking part in it. And many will lean to the latter interpretation, because they find it inconceivable that we should know as much about so vast and complex and close a thing as evolution as we should do if the mechanistic explanation of it by natural selection were true.

Professor Poulton has presented his view of evolution with such eloquence and enthusiasm that we feel almost

[1] Extract from Review, "Charles Darwin and the Origin of Species," by E. B. Poulton, F.R.S.—*The Times* Literary Supplement, Thursday, November 25th, 1909.

forced to lay some stress on the existence of the other view of evolution to which he makes no reference at all; that is, the spiritual (as we may call it) view of evolution of Samuel Butler, as opposed to the mechanistic one of Charles Darwin. The problem of evolution will be brought within range of attack by the man who provides an answer to the question, Which comes first, effort or structure? Does a dog scatter dust with his hind legs because in the past history of his race those dogs have been eliminated which have varied in the *minus* direction with regard to the dust-scraping capacity (which is presumably determined by the molecular structure of a part of the brain) *or* for some reason akin to that which prompts our sanitary habits (such as they are) which we may perhaps never be able to understand? The mechanist may object that these feelings in us are the outcome of the structure of a part of the brain, so that everything is ultimately referable to structure. This may be. But an effort (or other manifestation of the spirit) is none the less an effort for being expressible in mechanical terms. And *the* question of the future will be whether evolution has been brought about by effort or by the elimination of fortuitous structural variations.

Samuel Butler's "Life and Habit" [1]

Perhaps the most conclusive proof, if one were needed, of the arrogance of the human species is to be found in the fact that the man who was the first to succeed in convincing his fellow men of the truth of evolution was the man who first refused to admit that the intelligence of the living things themselves played any part whatsoever in bringing about the changes to which evolution is due. Buffon perceived the fact of evolution; so did Erasmus Darwin; so did Lamarck; but they all believed that the purposive efforts of the living things themselves played at least some

[1] Book notice, *English Review*, March, 1911.

part in bringing it about; and they, likewise, all failed to convince their contemporaries of the fact of evolution. Charles Darwin threw the intelligence of the performers in the evolutionary pageant overboard and put forward, as an explanation of evolution, the theory of natural selection, which went straight to the unimaginative heart of the mid-Victorian.

From that time evolution was accepted as intellectual coinage, with "Evolution" on one face and "Natural Selection" on the other face of the coin. From that time evolution became, in the most strictly literal sense of the words, lifeless and hopeless: lifeless, because, according to the theory of natural selection, living things have not evolved by virtue of any of their essentially vital attributes; and hopeless because, according to the theory of natural selection, no effort of ours can make our children better than their fathers; and because the only way to fight our most formidable enemy, disease, is to expose ourselves to the risk of contracting as many diseases as we can in order that those of us who are susceptible to one or more of them may be eliminated.

Evolution remained this automatic, lifeless thing until 1897, when "Life and Habit" appeared and put the breath of life into its nostrils.

We read "Life and Habit" in the spring of 1909. We had been brought up in the school of Natural Selection; our lectures on Evolution began with Charles Darwin. Evolution as explained by natural selection was a drab thing in which we had to believe. The change wrought in us by reading "Life and Habit" was miraculous. An extraordinary change had also come over the living things we saw. They appeared as they had never appeared before. They wore an uncouth aspect, which was unfamiliar, and yet strangely familiar, to us. They were alive. Evolution from that time became a thing in which it was not a necessity, but a joy, to believe.

About a year later the biological library committee of one of our leading scientific institutions met. Of the large number of works which it considered, one only was rejected —Butler's "Life and Habit." *A Monograph of the Fossil Barrabidæ* was at once passed with subdued murmurs of respectful applause. As were other works of like nature. "Life and Habit" was not merely rejected: it became the subject of merriment. One of the committee opened it at random and exclaimed, "Listen to this," and read a sentence in which "frog" and "soul" occurred. The suggestion that a frog had a soul provoked roars of laughter. To be acceptable to scientific orthodoxy you must not say that a frog has a soul. If you do you will be greeted with laughter. But if you say that a sea-urchin's egg has a *psychont* (which is only another word for soul) you will be treated with deference. The future lies with those who prefer the laughter.

No one denies the extraordinary interest of the Mendelian discoveries. . . . But we hold that he must be a very rash man who accepts without further question the doctrine of gametic purity. Yet it is just in the sphere of interpretation that the Mendelians are so certain. Once in this sphere, we can no longer be guided by facts—if we were dealing with facts we should be in the sphere of discovery—but by "such things as our mind conceives." And one's attitude should be one of continual, unceasing, and active distrust of oneself. The attitude of the Mendelian is different from this. He may reply that he is triumphant only about his discoveries; but we must remember that there is no fixed criterion by which we can say where discovery ends and interpretation begins; and we must be careful not to beg the question by defining discovery as that about which there can be no doubt. . . . We think it high time that the spirit which derives satisfaction from the victory of one opinion over another should be swept from science. There is no place for the party system in science;

because it tends to make the triumph of truth the main object and truth itself a secondary one. We are not arguing that the Mendelian theory is untrue, but that the attitude of anyone daring to say of anything "this is true" should be apologetic rather than victorious.

[From a review, *Nature*, May 23rd, 1907.]

The most fruitful source of progress is a new way of looking at things, and such new points of view result in the destruction of old classifications and the need for new ones. In biology, investigators will soon be classified, not according to the group of animals or plants with which they deal, but according to the particular phase that interests them of the problem of the "fundamental nature of living things," which is the ultimate goal of biological inquiry.

[From a review, *Nature*, August 3rd, 1905.]

This book is not written by a man red-handed, fresh from an encounter with Nature. If his hands needed washing before he wrote, it was to remove the dust of books. Would that the water could have removed the taint of much reading also. The notion that the truth must be sought in books is still widely prevalent, and the present dearth of illiterate men constitutes a serious menace to the advancement of knowledge. . . .

Earnestness is not a sufficient qualification for authorship.

[From a review, entitled "Another Book on Evolution," *Nature*, 1911.]

It would be a most fascinating task to trace the evolution of modern methods of dealing with the problems of life. Differentiation has taken place so extraordinarily quickly. The time is long past when one man can attempt to grapple with the whole problem. Not only so, but the time seems to be past when one man can even be interested in the whole problem. Evolutionists may be broadly classified into those to whom the problem of evolution is the problem of the origin of species and those to whom it is the problem of adaptation. The keynote of de Vries's "Mutationstheorie" is the solution of the problem of species; we even go so far as to say that this is the achievement of de Vries's work. The logical conclusion, the complete working out of the theory of natural selection, is reached in Dr. Archdall Reid's "Principles of Heredity." The interest of the two authors is entirely different. De Vries's interest is in the origin of species, Dr. Reid's in natural selection. Darwin's interest was in both; if we look no further than the title of his chief work we can see this—"On the Origin of Species by Means of Natural Selection."

The fact that these two interests have segregated, and the way in which they have segregated, are both very suggestive, and the direction in which they point is the same. The fact of segregation suggests that the association of the two ideas was unnatural, and that they were not capable of union. The way in which they have segregated confirms this suspicion. For those who devote their attention to the question of species reject natural selection, while those who elaborate the theory of natural selection find no support in the phenomenon of specific difference. All possibility of a reconcilement between the divorced ideas is put an end to by Meyrick, who probably knows more about specific difference than anyone else. In his handbook of British Lepidoptera he says that, in seeking for the most suitable characters by which species may be distinguished, those which can in any way be regarded as useful to the species must be discarded without more ado.

It is not surprising that Darwin's work should have borne fruit which segregated in this way. The case is thoroughly Mendelian. Darwin's work was a cross between a biological theory of evolution and a social and industrial theory of competition. The hybrid, more vigorous than either parent, took the world by storm. We are now witnessing its posterity separating out more or less simply into the two forms which were united in the beginning. Just as every plant in the F_1 generation contains yellow and green peas, and just as it is not until the next that there can be found plants bearing only yellows or only greens, so Darwin's interest was in the "Origin of Species by Natural Selection," while now we find de Vries, who is absorbed entirely with the former, and Reid entirely with the latter. . . .

The experimental method has its limitations no less than its fascination. It is not merely a paradox to say that in biology those things with which we can experiment most are those which to the organism matter least. The reason is that we are not the first to start experimenting. Nature has been there before. For example, the range of continuous variation in an organism may either be the direct result of the constitution of the living substance, or it may have been determined by the most stringent selection acting since life dawned. If, therefore, we institute experiments on variation—for example, the determination of the effect of heat on the range of variation—we may either be studying one of the simple properties of protoplasm or discovering the limits within which natural selection allows the particular organism dealt with to vary under the conditions of heat, e.g., to which we subject it. The really fundamental processes do not lend themselves to experiment. That is how they have become fundamental.

[From a review, *Nature*, April 4th, 1907.]

THE PRINCIPLES OF HEREDITY, WITH SOME APPLICATIONS. *By G. A. Reid.*[1]

The publication of this book marks an epoch in the history of the relation between medicine and biology, inas-

[1] From *Nature*, December 7th, 190 .

much as it is an embodiment of the recognition by medical men that they depend ultimately for a precise knowledge of nature on the professional biologist—who may or may not, at the same time, be a medical man.

The book should be welcomed by doctors as containing in the earlier chapters a straightforward, though rather brief, account of theories of organic evolution, and by biologists as giving a very full account of the medical aspects of these problems, and by both as an interesting collection, under the title of "The Principles of Heredity," of a mass of information and ideas connected with that phenomenon.

The reader may object to the antithesis between medicine and biology; but will, we hope, withdraw his objection when it is explained that all that is meant by it is the antithesis between applied and pure biology. The recognition by medical men of the value to them of the information with which the biologist is able to supply them is unquestionably a good thing; yet it is a curious illustration of the fact that a new movement of opinion cannot stand isolated and alone, cannot be without consequences of one kind or another, that one result of the popularity of the *entente* between the doctor and the biologist may prove harmful to biology, and through it perhaps ultimately to medicine.

The danger is that the biologist pure and simple, the man who works at his subject for the mere joy of investigation and discovery, may cease to exist. So many workers of this type are becoming applied biologists, whether they be sporozoologists devoting themselves to malaria, students of heredity to eugenics, or cytologists to cancer. We do not, of course, complain of the application of biological knowledge; it is obviously fitting and right that as much use should be made of it as possible. But we do complain loudly of the opinion that the application of such knowledge is, or should be, the ultimate goal of him who acquires it. Huxley strongly insisted on the fact that the fruits, useful to mankind, of the tree of natural knowledge fell unsought for and unexpected on the back of the head of some obscure worker under its shade, and never to him who worked there with outstretched palm. Dr. Reid says, p. 331:

"Hitherto the nature of their training has tended to render medical men excessively conservative. Nevertheless, they have already assimilated and put to magnificent practical use one of the two great scientific achievements of the age—Pasteur's discovery of the microbic origin of disease. The other great achievement, Darwin's discovery of the adaptation of species to the environment through natural selection, has hardly been assimilated, and certainly put to no practical use as yet. Both these discoveries should have been made by medical men."

The fact that they were not is an illustration of the truth of Huxley's words.

Let it be emphasised again that we do not hold that the gradual desertion of biologists from the ranks of the pure to those of the applied is other than of the greatest service to mankind. But if this desertion means that the opinion will grow that the natural goal of the young biologist is to obtain a position in applied biology, it is a bad thing for science. The utilitarianism which may lead to the extinction of the pure biologist is to be deplored. If we are going to be utilitarians let us at least be good ones, and let us recognise the demonstrable fact that the only way in which the knowledge and consequent control of nature can be acquired is by encouraging the existence of the type of man who works at his subject for its own sake. Let us have less of the talk about the profound significance of such and such a branch of investigation to the sociologist and the statesman, and more of the frame of mind which finds expression in Bateson's words:—"We are asked sometimes, Is this new knowledge any use? That is a question with which we, here, have fortunately no direct concern. Our business in life is to find things out, and we do not look beyond."

With regard to this utilitarianism, Dr. Reid appears to us to steer the right course in his book, except perhaps that he sails rather too near it when, pointing out that a classical education is inefficient and does not make us like the Greeks and Romans, he says, "the true modern representatives of the great Pagans are not to be found in college halls or country parsonages, but in thinkers and workers like Darwin, Huxley, Kelvin, Cecil Rhodes, the strenuous men who rule Egypt and India. . . ."

Surely the patient inquiring spirit which prompts a man to devote himself to classics is the same as that in the heart of the true man of science. One of the greatest steps forward in the study of heredity itself was made by a monk.

[From a lecture delivered at the Graduate School of Agriculture, Columbia University, Columbia, Missouri, July, 1914.]

. . . There are two essentially different ways in which such a subject as Genetics may be presented. One may either deal with the finished products of investigation; or one may fix one's attention on the machinery of investigation. These two methods can, of course, never be used absolutely separately; like all opposite things, each must contain something of the other—matter a little of spirit and spirit a little of matter, good a little of bad and bad a little of good. But the reason that in theory each should contain the other is that no sane man could be interested in the products of investigation unless he had previously satisfied himself that the machinery of investigation, by means of which those products had been turned out, is sound in construction; and, on the other hand, I think you will agree with me that the pure philosopher is not the man to overhaul this machinery; you want a man who is himself engaged in the business of investigation. I do not, of course, go so far as to say that an investigator, to achieve anything, must be a philosopher as well; nor that a philosopher to achieve anything at all must be an investigator as well; but that the best investigator is he who has a dash of the philosopher in him, and the best philosopher is he who has a dash of the investigator in him.

[From an unpublished paper, "The Art of Breeding," February, 1911.]

Those who take part in discussions as to the most profitable way of spending public money on the improvement of farm live stock may, in nearly all cases, be placed in one of two perfectly distinct categories, namely the scientific and the practical.

The Practical Man believes that the success of such schemes is assured if the planning and the carrying out of the breeding experiments are in the hands of one who has devoted his life to the breeding of live stock. The Man of Science, on the other hand, believes that we have little more to learn from the Practical Man, and that the greatest hope of further improvement lies in the application of scientific methods to the problems of practical breeding.

I believe that neither of these views contains the whole truth; that both of them contain part of it; but that the sum of truth made up by the two falls very far short of the whole truth about the matter. I have intended to suggest in the title of this paper the direction in which the residue is to be sought. . . .

It has not been my lot, though it always has been, and still is, my wish to follow the profession of a farmer. I cannot therefore deal with breeding from the standpoint of one who earns his bread by it. My study of breeding has been from the purely scientific standpoint; and I propose first to offer some general observations on the relation between Science and Agriculture.

The Science of Biology has, in this department of it, the department of genetics, come into very close contact with the science of agriculture. If you are a *pure* biologist, you will say that Biology has stooped down from her pedestal to hold a lamp for poor little *parvenu* Agricultural Science groping in the darkness below, to show her what the laws of heredity really are. If you are a *pure* agriculturist, you will admit that Biology at last seems to be beginning to justify her existence in so far as she seems to be finding out something about the breeding of animals and plants, which may possibly help the agriculturist to improve his stock and his corn. The purely academic biologist is apt to shudder at the thought that his pure and cloistered science should be degraded by being applied to useful ends. On the other hand, the rule-of-thumb breeder is apt to smile contemptuously when he is told that a mere theoriser, a student of biology, can tell *him* anything that he doesn't know about breeding. . . .

You will have gathered from what I have said about the pure biologist and the pure agriculturist that I am not going to bestow upon either of them the blessing which is the expected reward of the pure. We have no use for either of them. There is no question of biology stooping to assist agriculture, or of agriculture condescending to make use of what biology can tell it. There is no question of one being higher and the other lower. They are side by side. But they are very different. Agriculture is practical: when the agriculturist investigates a problem of heredity he does so solely with a view of improving his stock. Biology is disinterested: when the biologist investigates a problem of heredity he does so merely because the problem interests him and because he wants to find out what is going on. . . . It is just because the two are so different that they can be so useful to one another. The man whose contributions to the study of heredity would be most valuable would be a man who combined the experience of the practical breeder with the knowledge of the scientific student of heredity. There are few—I had almost said no such men.

[From a Letter of Application for the post of Director of Experiments in Animal Breeding, . . . February 11th, 1915.]

I do not think that successful breeding is merely a matter of the correct application to practice of a true theory of heredity. Indeed, it is manifestly not so. Successful breeding—or rather, I should say, the most successful breeding that we know, for a much more highly successful breeding is easily conceivable—successful breeding, I say, is the work and self-expression of the individual man, individual enough to conceive an ideal to breed to, and possessing at the same time an intimate knowledge of what is wanted in the breed he is interested in. Breeding, according to my belief, is an art. The relation between the science of heredity and the art of breeding is an extremely important question, both to the student of heredity and to the practical breeder, and I think it is desirable that I should state what my attitude towards this question is.

I was in Aberdeen a month ago, reading a paper on an aspect of heredity to a combined meeting of the Scientific and Agricultural Societies at the University, and on the following day I went with a large party of old students to see Mr. Findlay's herd of Aberdeen-Angus cattle at Aberlour. At the luncheon, a speaker quoted a remark of Mr. Duthie's which Mr. Duthie had made on the occasion of a similar visit to his Shorthorns. This remark was quoted by the speaker and delivered by him with a friendly mock emphasis, as a golden rule which was offered by Mr. Duthie to an audience expectant of useful hints as to the breeding of Shorthorns: "If you are a *good* man you will breed *good* cattle." Everybody, including myself, laughed at it as a genial pleasantry; but since then, I have come to see that it is one of the most pregnant utterances on the subject of breeding that I have heard. It embodies in an epigrammatic way all that is meant by the statement that breeding is an art. It means that it requires a man of force and personality, like Bakewell, for instance, to foresee an ideal and attain to it by methods of which he himself, like the musician, is probably unconscious. It requires an inborn gift to force a new breed, or an improvement of an old one, on the acceptance of mankind, just as it requires an inborn gift to express oneself by means of an arrangement of sounds which mankind will recognise as music. What then is the relation of science to this art of breeding? The answer, in my opinion, is that it is the business of science (to change the simile) to supply the best pigments and brushes wherewith the picture is to be painted; to place at the disposal of the breeder the instruments of precision which will enable him to carry out his work efficiently. The breeder knows, pretty well, the end he wishes to attain; it is the function of science to supply him with the means which shall enable him most expeditiously to reach that end.

II

On the Bearing of Mendelian Principles of Heredity on Current Theories of the Origin of Species

(*Manchester Memoirs, Vol. xlviii.*, 1904, *No.* 24)

A WIDE field of work and speculation has been opened up to the student of evolution by Mendel; and by his followers it has been maintained that it is only by working on the lines laid down by the Abbot of Brünn that a solution of the problem of the origin of species can ultimately be reached. It is the object of this paper to show the rela tion which Mendel's work bears to current theories of organic evolution.

(1) On the Difference between Continuous and Discontinuous Variation

For an account of continuous variation the reader is referred to the Presidential Address to Section D of the British Association, at the Bristol Meeting, by Professor Weldon (Weldon, '98)[1]: discontinuous variation is set forth by Mr. Bateson in his work "Materials for the Study of Variation" (Bateson, '99); all that remains for me to do is to call attention to some of the differences between these two conceptions of variation.

Continuous variation is the name of a phenomenon of everyday observation, namely, the fact that no two individuals of a species (plant or animal) are alike; it is a permanent quality of all animals and plants at all times, tending, as far as we can see, in any direction (and as little

[1] *Vide* List of Authorities, p. 139 *infra*.

teleological in any sense of the word as anything can be imagined to be). Above all, cases of continuous variation can be described by curves of error, a fact which seems to me to have a very deep meaning. Discontinuous variation is the quality which animals and plants possess of giving rise, from time to time, to offspring bearing characters specifically different from those of their parents: in fact, by some these offspring are termed "sports." This "sport" is a new species: that is to say (to use Bateson's words), "Variation, in fact, *is* evolution" ('99, p. 6); but while this can be said of discontinuous, continuous variation is merely the material upon which natural selection operates (if we may thus personify that process). From this it follows that according to the latter view it is impossible to say where one species begins and the other ends; but that according to the former view there is no such difficulty.

Continuous variation may be looked upon as normal, while discontinuous may be regarded as abnormal; but this aspect of the matter is perhaps only justifiable as a help to understanding the difference. For if species have arisen by discontinuous variation it is hardly fair to call it abnormal. (*Vide* Ewart, '99, p. lxxxiv.)

The word "Variation" may represent an abstract or a concrete thing: in the continuous sense it is usually a name given to the whole phenomenon of variation: in the discontinuous, the so-called sport is often spoken of as "*a* variation" (whereas a man with a cephalic index slightly less than his neighbour's would hardly be). This twofold use of the term conveniently recalls the two conceptions of variation.

(2) Galton's Theory of Heredity

I refer to Galton's theory of heredity because I want to show how the Mendelian theory differs from the statistical conception of that phenomenon. Galton said: "The two parents contribute between them on the average one-half, or (0·5) of the total heritage of the offspring; the four grandparents, one quarter, or $(0 \cdot 5)^2$; the eight great-grandparents, one eighth, or $(0 \cdot 5)^3$, and so on. Thus the sum of the ancestral contributions is expressed by the series $\{(0 \cdot 5) + (0 \cdot 5)^2 + (0 \cdot 5)^3, \&c.\}$, which, being equal to 1, accounts for the whole heritage." (Galton, '97, p. 402.)

(3) Mendel's Investigations

Mendel, an account of whose life and a translation of whose work has been given by Bateson (:02*a*), conducted hybridisation experiments with peas in the cloister garden of the monastery at Brünn of which he was Abbot. He had found that peas differed from one another in respect of seven characters, only one of which, for the sake of simplicity, I propose to consider; that is, the colour of the seed: this was either yellow or green. When he crossed a green-seeded pea with a yellow-seeded pea, the hybrid which he obtained was always the same with regard to that character; it was always a yellow-seeded pea. Yellow-seededness therefore was called a *dominant* character; and greenness of seed a *recessive* character. When the hybrids were allowed to breed (they were self-fertilised), a curious result was obtained: 25% of the offspring were green-seeded and 75% yellow-seeded. Now it will be remembered that the hybrid was yellow-seeded; and also that that was the character of one of the parents of the hybrid; so that by mere inspection of the yellow-seededness of this 75%, we cannot tell whether they are all pure yellows, or all hybrids, or some pure yellows and some hybrids. But we can tell this by breeding from these yellow-seeded peas; for it is known that pure yellows breed true, and we have just seen that hybrids produce 25% green-seeded and 75% yellow-seeded peas. Suppose we take actually 75 yellow-seeded peas (the number, of course, does not matter, so long as it is large; but 75 is convenient, as we shall shortly see), and plant some seeds from each plant. What Mendel found was this: 50 of the 75 were hybrids because 25% of their offspring were "green" and 75% "yellow" (which we will now write instead of "green-seeded" and "yellow-seeded"), while the remaining 25 proved themselves to be pure yellows by breeding true—by producing only "yellows." That is to say, the 75 yellow peas are composed of 50 hybrids and 25 dominants; and now that we have at last found out what they are, let us look at the whole result of breeding from the hybrids. We see immediately that:—

25% greens + 75% yellows

is really represented by

25% pure greens 50% hybrid yellows 25% pure yellows

or

25% recessives 50% hybrids 25% dominants.

Mendel experimented with the new hybrids (i.e. the children of the first hybrids) and found that they, too, produced offspring, 25% of which were green and 75% yellow; and he found (though he was working with small numbers: Bateson, :02, p. 57) that, for at most six generations, it was a general rule that hybrids when paired together gave 25% recessives, 50% hybrids, and 25% dominants. This phenomenon is spoken of as *segregation;* which consists in the dispatch by the hybrids, at each generation, of offspring into the dominant and recessive ranks from which (so long as like mates with like) there is no returning.

If we symbolise the dominant character by D, the recessive by R, and the hybrid by DR (and if we imagine for the sake of simplicity that each plant produces 4 seeds), we see that the proportions of D's, DR's, and R's in successive generations are as shown in the table on the following page. The ratio of D's, DR's, and R's can be foretold in any generation n, by this formula:

$$D : DR : R$$
$$2^n - 1 \; : \; 2 \; : \; 2^n - 1.$$

"In the tenth generation, for instance, $2^n - 1 = 1{,}023$. There result, therefore, in each 2,048 plants which arise in

Generation.	*D.*	*DR.*	*R.*	*Ratios.* *D.*	*DR.*	*R.*
1.	1	2	1	1	2	1
	(4 + 2	4	2 + 4) =			
2.	6	4	6	3	2	3
	(24 + 4	8	4 + 24) =			
3.	28	8	28	7	2	7
	(112 + 8	16	8 + 112) =			
4.	120	16	120	15	2	15
	(480 + 16	32	16 + 480) =			
5.	496	32	496	31	2	31

this generation 1,023 with constant dominant character, 1,023 with the recessive character, and only two hybrids" (Bateson, '02, p. 59).

(4) Mendel's Theory to Account for his Results

Mendel knew that green-seeded peas bred true, when self-fertilised or cross-fertilised by other green peas; and that the same was true of the yellow. That is to say, the *germ-cells* of green peas will always produce green peas, and those of yellow will continue to give yellow.

But what happens when a green pea is crossed with a yellow: that is, when a "green" germ-cell meets a "yellow" germ-cell? We know that a yellow-seeded hybrid is produced; but we want to know the condition of its germ-cells. Mendel's hypothesis was that it contained 50% "yellow" germ-cells and 50% "green"; no "greenish-yellow" and no "yellowish-green" germ-cells, but equal numbers of green-producing and yellow-producing gametes; he maintained that no fusion of characters (like a chemical combination) takes place, but merely a mingling (like a mechanical mixture).

The proportions in which the *D*'s, *DR*'s, and *R*'s occur in the offspring of hybrids is certainly accounted for by this theory. Let us (putting aside sex for brevity's sake) imagine what happens when a hybrid is cross- or self-fertilised. Imagine, first, a "green" germ-cell: it is an even chance that it unites with a "green" or "yellow."

$$a.\ G \times G$$
$$b.\ G \times Y$$

Imagine a "yellow": it has an even chance of uniting with a "green" or "yellow."

$$c.\ Y \times G$$
$$d.\ Y \times Y$$

Now *b* and *c* are the same; so that the proportions in which pure "greens," pure "yellows," and hybrids would be produced, as the result of the random unions of the germ-cells of two hybrids, would be $1G : 2GY$ (hybrid): $1Y$, in every four; or, of course, 25% D: 50% DR : 25% R. And this is exactly what happens as the result of actual breeding.

(5) A Device for Explaining Mendel's Theory

I think this theory may be made clearer by a device which has been useful to me: some have imagined that it is intended merely as an instance of the application of the theory of probability: this, however, is not the case; its value, such as it is, lies in the way the process is managed, which has nothing to do with probability.

All that is needed is some red and white counters.

Mendel's conception of the gonad of a hybrid as an organ containing germ-cells, 50% of which bear the dominant character and 50% the recessive, can be easily imitated by a bag containing equal numbers of red and white counters: in fact, the production of the hybrid (or rather its gonad) may be imitated by pouring equal numbers of red and white counters into some convenient receptacle. Now let us pair two such imitation hybrids, using for this purpose two bags or hats, each containing equal numbers of red and white counters. Two vertical lines are drawn on a large sheet of paper; the space between the lines being reserved for pairs, each consisting of a red and a white (*RW*); the space on one side of the two lines for two reds (*RR*), and that on the other for two whites (*WW*). A counter is taken at random out of one hat; then another out of the other hat; and the pair, according to its character (*RR*, *RW* or *WW*), is assigned to the column, on the paper, prepared for it. We should, of course, expect in a large number of trials that there would be 25% *RR*, 50% *RW*, and 25% *WW*; this is, in fact, what happens. Now in playing this game there is only one rule to be observed, which is, "*When a red is drawn with a white the red shall be placed on the top of the white.*" (It is this rule that confers whatever usefulness there is in this device.) Let us make fifty draws and place the result on the paper: the outcome of such a trial taken as I write is 13 *RR*, 26 *RW*, and 11 *WW*; which, for the smallness of the number drawn, is not a bad approximation. Let that colour represent the dominant character, which was placed uppermost in the *RW*'s; let *W* be the recessive and *RW* the hybrid: the observance of the only rule of the game brings out the fact that in the hybrid it is the dominant character which is manifested, while the recessive is hidden. Now, in placing the *RW*'s on the paper one is not likely to absolutely conceal the white; so that, while from our rule we realise that red is the character which the hybrid bears, we are prevented (by the fact that the white is not absolutely concealed) from forgetting the real constitution of the hybrids (or rather their gonads), namely, that they contain germs representing respectively dominant and recessive characters in equal numbers. It will be seen from this illustration that, in making the living cross, all that is done is to mingle (if this word may be used of two objects) the germ-cells together[1] like counters in a hat; and that, in the resultant hybrid, the germ-cells differ from the two which took part in its formation, only in actual number; for the proportions are the same (50% dominant and 50% recessive) and the discontinuous condition of the germ-cells is the same, inasmuch as a germ-cell represents *either* a dominant *or* a recessive, and never partly one and partly the other; in fact, it is no more possible to produce such an intermediate stage than it is possible to get a *pink counter* by shaking up scarlet and white ones in a hat. To look for a moment at the offspring of the hybrids: Mendel says that the "extracted" dominants (the dominant offspring of the hybrids) will always breed true; and that the same is true of the "extracted" recessives: this can be illustrated in our imitation hybridisation experiment by placing half the *RR*'s in one hat and half in another; it is evident that nothing but reds can be got from "matings" from these two hats.

It is also a fundamental part of the Mendelian principle (in fact, it seems to me to be its foundation-stone) that the "extracted" hybrids will produce the same kind, and proportions of the three kinds, of offspring as the first hybrid; for if half the *RW*'s are put into one hat and half into another it is evident that random matings will give 25% *RR*, 50% *RW*, and 25% *WW* as before, and, what is more, that they will continue to do so (so long as we keep up the number of counters) for however long we continue the process, that is to say, for howsoever many generations it is carried on.

(6) A Point of Difference between Galton's and Mendel's Theory

I do not propose to discuss here the difference between the Mendelian principles and the statistical conception of

[1] Of course the germ-cells fuse: it is the character-bearing elements which are thus mingled.

inheritance, but to consider one part of the hypothesis put forward by Mendel, which is at variance with Galton's theory. I refer to the phenomenon of segregation. We have seen what Mendel says (*see* Bateson, :02, p. 57). But this is flatly contradicted by the Galtonian generalisation, according to which the greater number of generations a given hybrid is from the first hybrid (i.e., of course, also from the parents of the hybrids), the fewer pure recessive and dominant forms is it likely to produce when mated with another hybrid of its own generation (Darbishire, :04, pp. 23 *et seq.*).

I refer to this point (which at first sight may appear insignificant, but in reality is not) because it seems to me to afford a means of deciding between the relative validity of the two theories, inasmuch as it is a matter about which the Mendelian and Galtonian predictions are totally at variance ; and because I think the time has not yet come for such statements, as, for example, this from the pen of Professor Castle (:03*a*, p. 228) : " It " (Mendel's theory) " thus meets the two-fold requirement of a scientific theory, a statement of phenomena and an *explanation of them ;* the 'law of ancestral heredity' attempts only the first of these two things, and even here fails lamentably. It will be thus seen that the claims of Mendel's law are much greater than those of Galton's law." The italics are mine (*see* Karl Pearson, " Grammar of Science," p. 121 ; and also, in this connection, Pearson, :04).

(7) New Conceptions based on Mendel's Investigations

I will only refer to three of these : the curious reader is referred to Bateson's " Mendel's Principles of Heredity," p. 26.

(*a*) The first of them is " the purity of the gametes in regard to certain characters " (Bateson, :02, p. 26). This generalisation is based on the often repeated fact, that (to take an example) in respect of the colour of the seed a germ-cell of a pea, whether it be contained by the pure yellow or pure green race, or by the hybrid or any of its descendants, *is absolutely pure.* And it is believed that it will continue to be pure in respect of these characters until a specific upheaval takes place, when a new character will arise by the process of discontinuous variation.

(*b*) The second is the conception of *unit-characters* (l.c., pp. 27–28) in respect of which the gamete is pure. Such units seem to correspond to that in the adult organism which Weismann sought in the germ. These unit-characters, as we have seen, usually exist in pairs in such a way that one is dominant and the other recessive; and this fact is recognised by naming such characters allelomorphs. It is hardly necessary to say that the germ contains not merely one allelomorph (as we have been imagining for the sake of simplicity), but very many; in fact, Mendel recognised seven such pairs in his peas, and this must be the merest fraction of the actual number that exists.

(*c*) We are thus indirectly led to the conception of "*compound characters,* borne by one gamete, transmitted entire as a single character so long as fertilisation only occurs between like gametes" (l.c., p. 29). The reader is here strongly advised to refer to the first thirty-five pages of Bateson's book (Bateson, :02*a*).

(8) The Relation between these New Conceptions and the Theory of the Origin of Species by Discontinuous Variation (*a*, Gametic Purity; *b*, Unit-characters; *c*, Compound Characters).

Let us suppose a new character to arise by discontinuous variation; for example, the possession of a trunk in a race of previously snoutless elephants. According to the statistical view of heredity this new character would be soon swamped by the, so to speak, normal trunkless ancestry of the new form itself and of its mate; for one of two courses is open to the new form: it may either unite with another like it, which would depend on a series of contingencies — the production of two such beasts at the same time, at the same place, of opposite sexes, and the condition that they were not averse to one another; or it might unite with a trunkless relative. But even in the former case the offspring would have a smaller trunk than its two parents. And if this smaller trunk were to be perpetuated its owner would have to unite with another trunk-bearing variety, which therefore would have to arise at the proper time, be of the opposite sex, and in the neighbourhood; but even if the smaller trunk were lucky enough to find such a one its offspring would be less trunked even than itself! If the original trunked variety paired with a trunkless relative the swamping would be ever so much faster. But I leave the reader to pursue this argumentation for himself—suffice it to say that these trunk-bearing sports would on this view be very soon wiped out.

But if we adopt the conceptions of gametic purity and unit-characters, there is no reason, when once the variation has arisen, why it should not be perpetuated. For its germ-cells represent trunk-bearing elephants; if it mated with a similar beast its offspring would all be trunk-bearing and in the same degree; if, on the other hand, it met a trunkless form, the result of such a union—the hybrid, in other words—would have a trunk if the possession of that organ were dominant, and would not if it were recessive; but whichever of these was the case, 25% of the next generation would be true-breeding trunk-bearers; and so on. This illustration may be crude, but I hope it shows the kind of way in which, according to these new conceptions, such a variation might be perpetuated; while according to the biometric view of heredity this would not be the case.

We have seen that a single unit-character continues to produce its like so long as it unites with its like until a new variation arises from it; but, when we come to consider (*c*) *Compound characters,* we find that new characters can arise in another way than by a discontinuous variation.

I will take an example with which I am familiar. When a yellow-and-white Japanese waltzing mouse is crossed with an albino, a hybrid is produced which is unlike either parent, being, with some exceptions, hardly distinguishable from the common house-mouse (*see* Bateson, :02, note on p. 55 and pp. 24 and 25). This hybrid (I have bred some 350) is never an albino, and it never waltzes: albinism and waltzing therefore are recessive characters; and pigmentation and normal progression are the corresponding dominant characters. So that we are crossing a creature—the albino—possessing normality of progression (*D*) and albinism (*R*) with another beast which exhibits waltzing movements (*R*) and the presence of pigment (*D*). Let us consider the offspring of hybrids thus produced from the point of view of the two pairs of allelomorphs. First, with regard to colour, we should expect 25% albinos which should breed true: this is in fact what we get. Secondly, with regard to their progression, we should expect to find 25% waltzing mice: this is very roughly what happens. I have been unable to determine if they breed true (on the Mendelian hypothesis they should, of course). Now let us look at the offspring of hybrids from *both* points of view at the same time: one mouse in every four is an albino; one in every four is a waltzer, so we should expect one in every sixteen to be an *albino waltzer*. Now these albino waltzers are new things; and, what is more, they should breed true, because both their characters (*A* and *W*) are recessive. What has happened is that we have taken the recessive character—albinism—from one parent of the hybrid, and the recessive character—"waltzing"—from the other; and through the mediation of the hybrid united them in one individual—the new albino waltzer—which will produce nothing but offspring like itself, because its gametes are pure.[1] I have

[1] I am not sure that this case is not, strictly speaking, an example of a synthetical variation; at any rate, it is a very simple instance of the argument set forth in Bateson, :02*a*, p. 29.

bred several examples of this new species, but have so far been unable to obtain young from them.

The reader may suspect that there is something peculiar about these two characters—albinism and waltzing—that the former is one that is likely to have arisen as a sport, and that the latter is, in a way, pathological, and that both are of such a kind as to be very quickly eliminated in the struggle for existence: my opinion is that such a suspicion is of great interest. But I do not propose to discuss the source of such variations here: all I have tried to do is to sketch the relation which exists between Mendelian Principles and current theories of the origin of species.

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III

On the Supposed Antagonism of Mendelian to Biometric Theories of Heredity

(*Manchester Memoirs, Vol. xlix.*, 1905, *No.* 6)

I[1]

Dissensions in scientific matters may be said to be of two kinds, of which one is a disagreement about fact and can be settled by an appeal to fact, while the other is the conflict of theoretical interpretations which cannot be so easily concluded. When Owen said that an ape's brain had not a hippocampus minor and Huxley asserted that it had, Flower announced that he had an ape's brain in his pocket; and the dissection of the brain put an end to the discussion. But in the second form of controversy no such touchstone can be applied, and in the debate on heredity at Cambridge this year Mendelian maize-cobs were displayed in vain. Of this kind of controversy there are again two sorts, one in which the theories put forward by the opposite factions are mutually exclusive, and another in which, while there is apparent incompatibility, the truth of both of the hypotheses is ultimately demonstrable. It remains to be seen to which of these subdivisions the Cambridge debate,[2] and the wider discussion of which it was the outcome, are to be assigned. If the two theories are mutually exclusive, which is right? If, on the other hand, they are not, how

[1] This essay is so arranged that if the reader is not interested in the phenomenon of hybridisation he may leave out Part 2.

[2] Reported in *Nature*, Vol. 70, pp. 538–9.

do they fit in with one another? These are questions which "the general inquiring public" may be expected to ask and to which the specially trained biologist may be expected to supply an answer.

It is the thesis of the present essay to demonstrate the compatibility of Mendelian and biometric theory and to account for their apparent antagonism.

A few words as to the spirit and scope of this essay seem to me to be necessary. There are two methods of scientific criticism, if, indeed, one of them can be justly called scientific. One arises from a determination to crush a theory, while the other consists in the postponement of the attack until every endeavour has been made to appreciate the exact point of view of the upholders of that theory, and in a willingness to put off the attack for ever if the theory should not be found wanting after all. The most flagrant example of the first kind of criticism, on which I can lay hands, flowed from the pen of a writer who, after having misrepresented the theory he was attacking by declaring that it was "an essential part of the Mendelian hypothesis that the (so-called "extracted") recessive individual which is produced by pairing two first crosses is in every respect similar to the original pure recessive,"[1] concludes with these words: "This mouse is clearly not a pure dominant, because it produces albinos; it is not a dominant hybrid, because it has pink eyes; and it cannot be a recessive, because when paired with an albino it produces some black-eyed forms." It is evident from this quotation that the stimulus which actuated the author was a desire to stultify and refute Mendelian theory at all costs, and that he did not make the smallest attempt to discover what Mendelian theory really was or to put himself in the position of those who held it to be true. For an example of the second form of criticism I suggest that the reader may turn to the following pages; in them I shall do my best to discover the most essential character-

[1] A. D. Darbishire, *Biometrika*, Vol. 2, pp. 282–5.

istics of Mendelian and biometric theory and so to put myself in a position to discuss their mutual relationship.

With regard to the scope of this essay, there is one point I wish to emphasise: it may be that some critic will lay down this pamphlet with the remark that all that he has read may be very true, but that the fact remains that the only thing which "matters" is the mass-phenomenon; or another may declare that the key which will unlock the secret of heredity can only be obtained by a study of the properties of the germ-cells. I do not propose to express an opinion on either of these contentions, but I wish most strongly to insist that when a man has made either of them, he has stepped from one country into another. After discussing the mutual relation of two theories, he suddenly asserts that after all it is only one of them that matters—as one who during a discussion of the evidence for and against the existence of mental activity after the death of the brain should declare that after all, belief in such a survival was a great comfort to many: both questions may be worth discussing, but they should be discussed separately. In this essay I propose to treat of the mutual relations of the two theories purely as theories, without touching on the question of their possible value to the pure or applied biologist.

The reader may easily convince himself by a perusal of the literature on this subject that the self-same facts are interpreted by the rival schools of thought in the light of their own theories; and if he looks for recognition from either party that there may be something of truth in the opinions of their opponents, he will search in vain. I do not propose to discuss the opposite points of view, because I believe that the remedy for the present inconclusiveness of the discussion lies very deep, and is to be found in the clear appreciation of the fundamental relation between the biometric and Mendelian points of view.[1]

[1] The reader who wishes to follow the discussion of the facts at first hand will find the necessary references to four cases in the Appendix (*see* p. 161).

An Introduction to a Biology

II

At a time when I did not clearly see this relation, I had before me some data which convinced me that the Mendelian interpretation of the phenomenon of segregation was wrong, and that the facts were striking evidence of the truth of Galton's theory.

There are two attributes of a heterozygote which are said to follow from the theoretical constitution of its gonad; one is that a quarter of the population produced by the union of heterozygotes consists of individuals bearing the recessive character, and the other is that half the population produced by mating heterozygotes with recessives consists of recessives. My hybrids [1] were tested for these two properties and the results were not denied to be in accord with Mendelian expectation. But this result was not conclusive in favour of that theory only, because the proportion of recessives demanded by Mendelian theory in the case of the first property was identical, and in the case of the second only slightly less than that which follows from the truth of Galton's generalisation.[2]

The Galtonian prediction of the number of albinos that will be produced by two hybrids (H), each of which is the offspring of a pure-bred waltzing and a pure-bred albino mouse, is ·25 of their generation; while on that theory the proportion of albinos in a generation resulting from the union of hybrids with albinos (A) is ·53125, if we only calculate as far back as the great-grandparental generation.

We have seen, therefore, that hybrids were mated with hybrids, and that they were also mated with albinos. In this way two kinds of hybrids were produced which could not be distinguished from one another by their outward appearance, but differed in the amount of their albino ancestry; for while the one kind, which we may call HH, resulted from the union of two hybrids, the other, HA,

[1] *Biometrika*, Vol. 3, pp. 30–33.

[2] Francis Galton, "Proc. Roy. Soc.," Vol. 61, p. 402, line 13.

was the offspring of a hybrid and an albino. I took those individuals to be hybrids which resembled the first crosses (F_1) in coat and eye colour—i.e. in the possession of a coloured coat and pigmented eye.

In mating hybrids of this generation (F_2) I did not previously look up their ancestry in books containing their genealogical record, so that mice of the categories *HH* and *HA* were mated at random; in this way three kinds of crosses were made: $HH \times HH$, $HH \times HA$, and $HA \times HA$. In each type of union a hybrid was mated with a hybrid, as I believed at the time; and as on Mendelian theory there is no difference between the gametic constitution of *DR* produced by $DR \times DR$ and *DR* with parentage $DR \times RR$, I argued [1] that, if that theory were true, each type of union would produce a fraternity, half of which would be composed of hybrids, a quarter of which would be composed of pink-eyed mice with coloured coats, while the remaining quarter would consist of albinos; on the other hand, it was evident that on Galton's theory of heredity the proportion would not only not be the same in each of the three cases, but would differ in direct proportion to the amount of albino blood in the parents of the population. The subjoined table gives, together with the actual result, the proportions of albinos as predicted by the two theories. In calculating the Galtonian prediction I have not taken into account any generations more remote than the great-great-grandparental.[2]

Type of Parentage.	*Number of Young.*	*Mendelian Expectation.*	*Galtonian Expectation.*	*Actual Result.*
HH × HH	93	25%	9·37%	10·75%
HH × HA	107	25%	17·96%	18·69%
HA × HA	121	25%	26·56%	24·79%

[1] *Biometrika*, Vol. 3, pp. 23–5.

[2] I thank Mr. J. T. Wadsworth for checking this simple calculation.

Before continuing my argument, I should like to dwell for a little on the circumstances which led me to believe that I was dealing in F_2 with hybrids, and, indeed, with heterozygotes in the strict Mendelian sense of the term. In the first place, these putative hybrids bore the same features of coat and eye colour as the F_1 hybrids exhibited; secondly, they formed 50% of the F_2 generation; and lastly, at least two upholders of Mendelian theory[1] had asserted that the heterozygote was represented in my experiment by the coloured mice with pigmented eyes.

To resume the thread: these results were brought before the notice of a student of heredity whose first question was, Had there ever resulted from the union of two hybrids a family in which there were no albinos?—a query to which an affirmative answer was given. Then the following argument was used by my critic: "The only proof that a given individual is a hybrid is one which is based on an examination of its gametic constitution; in the case of your mice you have no right to say that a grey mouse with black eyes is a hybrid until you have mated it with an albino and obtained albino young in the litter thus produced; until this has been done there is no evidence that it is not a dominant. In the case you have just shown me you mated coloured mice with dark eyes without making this test, and by this neglect many dominants may have been included among them, and you see that this suggestion, against the truth of which you have no evidence, accounts just as well as Galtonian theory for the difference in the proportions of albinos in the three kinds of matings." I replied that the test of the true heterozygote nature of the apparent hybrids should be made, though I did not believe the suggested Mendelian interpretation of this apparently conclusive anti-Mendelian result.

The test has been applied with the most remarkable result; but before giving an account of it I propose to describe

[1] Castle and Allen, "Proc. Amer. Acad. Arts and Sci.," Vol. 38, No. 21.

my reasons for adhering to the interpretation of the facts of this case which I held at first. I believed that all the individuals in F_2 with pigmented coats and eyes were hybrids, and that when mated with albinos they would all of them give some albinos; that the hybrids of F_2 differed from those of F_1 only in degree—namely, that while it was the property of the latter when mated together to produce as nearly as possible one albino in every four in their litters, the former had a less albino-producing capacity; and that the hybrids of F_3 would each be capable of producing still fewer albinos, and so on with succeeding generations. My belief was that the hybrids of a given generation—say F_5—would be all the same with regard to their albino-producing capacity, but would differ from those of F_4 in having a smaller one. Before dealing with the origin and meaning of this view I will relate how, by the application of the test suggested, it was shown to be erroneous. The test of the real heterozygote nature of the hybrids was made, as suggested, by mating them with albinos. In all cases but two I found, as I had expected, that albinos were produced; and I ascribed the absence of albinos from the other two litters to chance, and had no doubt that I had only to mate them with albinos again to obtain the required proof of their hybridity. But the next litters from the two mice, mated thus, contained no albinos. So that it began to look very much as if these apparent hybrids really were dominants. This fact in itself pointed to the truth of the suggestion that mice with coloured coats and eyes were of two kinds—hybrids and dominants; but coupled with the result of mating the gametically tested hybrids *inter se* it afforded fairly complete confirmation of that hypothesis, for of the 92 young resulting from such union, 14 had a pink eye and coloured coat, 58 a dark eye with coloured coat, while 20 were albinos.

I wish particularly to remind the reader that I do not think that these numbers are large enough to draw any numerical conclusions from; they are introduced solely as part of an argument which is intended to show how I came to see that the facts summarised in the Table (*vide* p. 148) were equally in accord with Mendelian and Galtonian theory; and their value in this respect will not be in the least impaired if it ultimately turns out that the proportion in which the three categories of coat and eye colour occur are "in every respect discordant with" Mendelian prediction.

That the two mice which gave no albinos in the two matings referred to really are dominant is, I think, placed beyond doubt by the fact that in a third mating with albinos they have failed to produce anything but coloured mice with black eyes.[1] That the remaining mice are heterozygotes is also beyond doubt, and that a quarter of the population produced by breeding them together is composed of albinos remains to be demonstrated. The Table on p. 148 therefore is in no wise a refutation of Mendelian theory; at any rate, the suggested Mendelian interpretation of the facts cannot be regarded as disproved. Nevertheless, the facts are no less in accord with Galtonian theory, though in a different way than I at first held; the manner in which I believed that the truth of that theory would be borne out was, as I have said before, and as I wish to emphasise again, by the gradual diminution of the albino-producing capacity of *each* hybrid of successive generations. It now appears that the manner in which that theory is borne out by these facts is by the gradual invasion of the "hybrid" ranks in successive generations by dominant individuals bearing the external hybrid characteristics; whether there will appear among the "dominants" or even recessives a compensating number of hybrids, or even whether this is demanded by Mendelian theory, is a question of fact which does not affect my argument. What I want to point out is that I fell into the error of believing that that which was

[1] Besides these two mice which have been tested thrice there are three which have been tested twice, two bucks and a doe, with the same result.

true of the whole population was also true of the individual —a mistake which, I believe, was due to an attempt to discover whether certain phenomena were evidence in favour of the one or the other of two theories without appreciating the essential character of either theory and much less their mutual relation; to a failure, in short, to realise that a biometric formula of heredity is true only of large masses, the component units of which in most cases unite at random, while the Mendelian theory is an attempt to account for the hereditary phenomena exhibited by the union of individuals carefully selected, by a theory of the constitution of their germ-cells. It is perhaps not unnatural, though it is certainly unjustifiable, that when an experimenter is thinking of a set of facts before him now in terms of the one theory and now in terms of the other without having clearly fixed the peculiar characters of each theory to its proper owner, he should get these characters misplaced; that he should add to the real character of the biometric theory one which it does not possess—that of applicability to the individual. I have discussed this particular error of judgment at some length because it illustrates the kind of mistake a student of heredity at the present time may make unless he realises the exact nature, at which I have so far only hinted, of the two theories whose compatibility with fact he is testing. Were I not persuaded that mine is not the only case in which this or a similar kind of error has been made, I should not have described it; and it is because I believe that the misunderstandings and arguments at cross purposes, which have lately characterised discussions on heredity, will be things of the past when the relation between biometric and Mendelian theory is clearly seen, that I set forth the following considerations.

III

From a point of view which commands a wide range of our experience, our knowledge may be divided into two distinct classes, according as we are dealing collectively with a vast number of things—with a mass phenomenon; or with the individual units which make up that mass. These two kinds of knowledge are radically different, and are distinguished from one another by the same characters as those which are peculiar to the two meanings of the statement that a thing happens by chance; for when we make this statement we may either be referring to the method by which I decided whether to write "biometric" or "Mendelian" first in the title of this paper, or to the result of a very great number of tosses—an approximation to 50% heads and 50% tails, which is close in proportion as the number of trials is great. The first difference that I mention between these two meanings of chance, as illustrative of the characters of the two classes into which we have divided our aspect of things, is that, while in the case of the first it is impossible to predict the result of a single trial, there is nothing easier to foretell than the result of a very large number; nothing is more uncertain than the former, nothing more certain than the latter. A second difference between these two groups is that that which is true of the mass is not necessarily true of all the component individuals, though it may be of some; in the case of coin-tossing, the statement that the result of an infinitely large number of trials is an equal number of heads and tails is contradicted at every single toss, though this would not be the case if some of the coins we tossed had half the head and half the tail on each side of the coin.[1] Is it necessary to add that from the fact that what is true of the mass is not true of the individual it does not follow that assertions about the individual are antagonistic to statements about the mass? It is important to realise this truth because it is seldom done and appar-

[1] I have found Galton's apparatus for illustrating the origin of the curve of frequency ("Natural Inheritance," p. 63, Fig. 7) very useful for explaining the difference between these two classes. An example of the first is afforded by allowing a single shot to run down the inclined board; an example of the second by displaying the result of a thousand such events.

ently difficult, the difficulty resulting from the extreme difference of the two points of view. A midge walking across a picture of a meadow done by the three-colour process would assert that it was traversing a white plain, over which were distributed patches of different sizes and three colours—red, blue, and yellow; a child would maintain that it was walking across a picture of a field; each would be convinced that he was right and the other wrong; yet that both were right could be recognised by any man able to use a magnifying lens. This leads us to a third feature of the relation between our two classes (which results from the fact that our knowledge has probably developed along those lines that our point of view has made most valuable), namely, that in proportion as our knowledge of the component units is small so is our knowledge of the mass result great.

To take an example of these two ways of looking at things. The climate of a country or of a long period of time is a mass-phenomenon; the particular climatic condition of a certain day is referred to as the weather.[1] It is, though it may be becoming less, impossible to predict the weather with precision; but the nature of the climate of a given country or long period of time is a matter of tolerable certainty. Yet the statement that the summer is warm does not exclude the possibility of a frost in May. That our practical knowledge of the elements is confined to the climate is evident from the fact that, having procured, we begin to put on warmer clothing at a certain period of the year; but if our intelligence were so sharpened, or our meteorological instruments so improved that we could pre-

[1] "By *climate* we mean the sum total of the meteorological phenomena that characterise the average condition of the atmosphere at any one place on the earth's surface. That which we call *weather* is only one phase in the succession of phenomena whose complete cycle, recurring with greater or less uniformity every year, constitutes the climate of any locality." P. 1.—J. Hann's "Handbook of Climatology," transl. by R. de C. Ward, 1903

dict the exact state of the weather a fortnight in advance, we should not procure the warmer raiment until we knew that it would be needed.

Another phenomenon which may be looked at from these two points of view is that of the causation of heat; it is believed that the heat of a substance is occasioned by the mean speed at which the molecules of which it is composed are travelling. To deal with the three differences between our two aspects of things in turn; it is evident first that, while our ignorance of the speed of an individual molecule is so great that we try to conceal it by saying that it is determined by chance, our knowledge of the average speed of myriads of them is so accurate that certain laws of thermodynamics have been formulated. Secondly, it has been calculated that a curve representing the frequency of the various speeds spread over the molecules is an ordinary curve of error; so that although that which is true of the mass is also true of some of the molecules, it is by no means true of all of them. Thirdly, the only point of view from which we can regard this phenomenon at present is that from which we can only discern the mass-result; but that it is by no means inconceivable that there may be another point of view is evident to all who are familiar with Clerk Maxwell's demon, a being who is so essential to my argument that I shall make no apology for quoting his creator's description of him in full: "One of the best-established facts in thermodynamics is that it is impossible in a system enclosed in an envelope which permits neither change of volume nor passage of heat, and in which both the temperature and pressure are everywhere the same, to produce any inequality of temperature or pressure without the expenditure of work. This is the second law of thermo-dynamics, and it is undoubtedly true so long as we can deal with bodies only in mass, and have no power of *perceiving or handling the separate*[1] molecules of which they are made up. But if we conceive

[1] My italics.

a being whose faculties are so sharpened that he can follow every molecule in its course, such a being, whose attributes are still as essentially finite as our own, would be able to do what is at present impossible to us. For we have seen that the molecules in a vessel of air at uniform temperature are moving with velocities by no means uniform, though the mean velocity of any great number of them, arbitrarily selected, is almost exactly uniform. Now let us suppose that such a vessel is divided into two portions, A and B, by a division in which there is a small hole, and that a being who can see the individual molecules opens and closes this hole, so as to allow only the swifter molecules to pass from A to B, and only the slower ones to pass from B to A. He will thus, without expenditure of work, raise the temperature of B and lower that of A, in contradiction to the second law of thermo-dynamics."

The point of view of the demon is so different from that of the physicist that one of the truest generalisations of the latter would be declared absolutely false by the former; yet no one remains blind for a moment to the fact that the contradiction of their respective statements is only apparent, and is due to the radical difference in their points of view.

Now I believe that the difference between the point of view of the Mendelian and the biometrician is very like the difference between that of the demon and that of the physicist. The biometrician, with a new weapon of observation, is only concerned with mass phenomena; the individuals which go to swell his correlation tables are, like the atoms of the physicist, units of which no knowledge is required to attain the result at which he aims. But I need not dwell on the exactness of the parallel when we have these words from "the inventor of the term biometry": [1] "Our [2] knowledge of atoms and our application of atomic and molecular hypotheses to problems in heat, elasticity, and cohesion is essen-

[1] *Nature*, Oct. 27, 1904, p. 626.

[2] Karl Pearson, "Grammar of Science," 2nd Ed., pp. 500–1.

tially based on statistics of average conduct. Corpuscles in each other's presence are supposed to obey certain laws of motion, but no explanation has hitherto been given of these laws. So it is with vital units; they vary, why they vary we know not, and we *explain* nothing by attributing it to bathmic influences. As we can predict little or nothing of the individual atom, so we can predict little or nothing of the individual vital unit. We can deal only with statistics of average conduct. We have laws of variation and laws of heredity, in themselves quite as general and as definite as the majority of those we meet with in physics."

I may perhaps take this opportunity to explain that I have used the term biometric theory advisedly, and that the definition of it that I have had before my eyes is not merely "the application of exact statistical methods to the problems of biology,"[1] but the aspect of vital phenomena, just quoted from the "Grammar of Science," which prompts that application.

And I believe that I am justified in including under the term "biometric" both Pearson's and Galton's theories which, though in one respect they are radically different,[2] resemble each other in regarding heredity as a mass-phenomenon and in treating it by the statistical method.

The Mendelian, on the other hand, with a new application of experiment, is a biological demon who, "perceiving" and "handling the separate" units themselves, tries to find out their properties by mating them with other units. But here again I need not expatiate on the closeness of the parallel I have suggested when we have these words from the champion of Mendelism in this country:[3] "In the Mendelian method of experiment the one essential is that the posterity of each *individual* should be traced separately.

[1] *Nature*, Oct. 27, 1904, p. 626.

[2] Karl Pearson, *Biometrika*, Vol. 3, pp. 110–11.

[3] Presidential Address to Section D, Brit. Assoc., 1904, in *Nature*, August 25, p. 409.

If individuals from necessity are treated collectively, it must be proved that their composition is identical. In direct contradiction to the methods of current statistics, Mendel saw by sure penetration that masses must be avoided."

There is one direction in which my parallel may seem at first sight to be incapable of being pushed very far; it may be urged that we never can have any knowledge of the individual, but only of kinds of individuals, because in single cases it is impossible to eliminate the attributes which are due to chance; so that Mendelian methods are more to be compared with chemistry, which tests the property, not of units, but of masses of units which are known to be all the same.[1] But this fact does not in the least lessen the closeness of the parallel, for we have no reason to believe that the demon, if his attributes are as "essentially finite as our own," would have or need any knowledge of the individual molecules, but merely the ability to classify them into, say, ten classes, ranging from very fast to very slow, and to close his door according to their speed and direction. This point does not seem to be of much importance, but I did not wish, by not referring to it, to appear to have overlooked it.

One result, which seems to me to follow naturally from the truth of my comparison, is that it is unreasonable to apply, as has often been done, the criteria of either theory to a set of facts in which the conditions, on which that theory is true, do not obtain; and the manner in which materials for the study of heredity are collected by Mendelians is so different from those employed by biometricians that this is very rarely, if ever, the case.

From this it follows that the naturalist who sets out to attack the problem of heredity will not as in the past collect his facts and then see whether they fit the one theory or

[1] *Cf.* "The breeding-pen is to us what the test-tube is to the chemist" —same Address, p. 409, first column; and *cf.* "Reports to Evol. Com- Royal Soc.," I., p. 159.

the other, but will make it his first duty to decide whether he will attack it from the point of view of the physicist or the demon, from the outside or from the inside. If he decides on the former, he may if he wishes, breed his material, but he will find a great deal ready to hand in the records of matings of, for example, greyhounds, racehorses, and men. If he decides on the latter, it is almost indispensable that he should breed his material for himself. That is why biometricians are concerned with "ancestry," and Mendelians with "posterity." Yet these are not two things, but one thing, looked at from opposite ends. But there is a difference between ancestry and posterity, namely, that the latter only can be dealt with by the method of experiment.

A confirmatory sidelight on the truth of my comparison is thrown by the consideration that of the two men whom I have quoted as representing the rival theories of heredity, the biometer is a mathematician, while the Mendelian is a zoologist; and it is entirely in accord with expectation that the former regards the phenomena of heredity from that point of view which does not presuppose knowledge of the unit, while the latter is concerned with the properties of the individual organism.

If we could imagine the demon and the physicist incapable of appreciating each other's point of view, we could understand the contempt each would have for the clumsy methods and erroneous opinions of the other.

And though we can perhaps understand the Mendelian declaring as he slides the latch of his breeding-pen that "Operating among such phenomena the gross statistical method is a misleading instrument; and, applied to these intricate discriminations, the imposing Correlation Table into which the biometrical Procrustes fits his arrays of unanalysed data is still no substitute for the common sieve of a trained judgment"; and that "nothing but minute analysis of the facts by an observer thoroughly conversant with the particular plant or animal, its habits and properties, checked by the test of crucial experiment, can disentangle the truth"; [1] and appreciate the point of view of the biometer marshalling his vast arrays when he contends that it is "better to use the purely descriptive statements of Galton and Pearson than to invoke the cumbrous and undemonstrable gametic mechanism on which Mendel's hypothesis rests." [2] I do not see that we have any right to remain blind any longer to the fact that the contradiction of their respective theories is only apparent, and is due to the radical difference in their points of view.

[1] Pres. Address to Sect. D. *Nature*, August 25, 1904, p. 408.
[2] *Nature*, Sept. 29, 1904, p. 539.

APPENDIX. (*See* p. 146.)

Facts.	*Mendelian Interpretation*	*Biometric Description.*
"Versuche über Pflanzen - Hybriden," by Gregor Mendel. *Verhandl. naturforsch. Ver. in Brünn*, Vol. 4, 1865. Abhandl. p. 1. Transl. into Engl. in "Mendel's Principles of Heredity," by W. Bateson.	(i.) Mendel, G., p. 72, in Engl. transl. by Bateson. (ii.) Bateson, W. Mendel's Principles, p. 57 *et seq.*	Weldon, W. F. R. *Nature*, Vol. 70, p. 539.
"Versuche mit Kreuzungen von verschiedenen Rassen der Hausmaus," by G. von Guaita. *Ber. d. naturforsch. Gesell. Freiburg*, x., 1898, p. 317. Zweite Mitth., etc., *Ibid.* xi., 1900, p. 131.	(i.) Bateson, W. *Proc. Zool. Soc.*, 1903, Vol. 2, pp. 86-8. (ii.) Castle, W. E. *Proc. Amer. Acad. Arts and Sci.*, Vol. 39, No. 8, p, 231, Table II.	
"L'Hérédité de la Pigmentation chez les Souris" (3me Note), by L. Cuénot. *Arch. de Zool. exp. et gén.*, 1904 (4), Vol. 2. *Notes et Revue*, No. 3, pp. 45–56.	Cuénot, L. *loc. cit.*	Weldon, W. F. R. *Nature*, Vol. 70, p. 539.
"On the Result of Crossing Japanese Waltzing with Albino Mice," by A. D. Darbishire. Four Reports. *Biom.*, Vol. 2, pp. 101, 165, and 282. Vol. 3, p. 1.	(i.) Bateson, W. *Proc. Zool. Soc.*, 1903, Vol. 2, p. 88. (ii.) Castle, W. E., and Allen, G. M. *Proc. Amer. Acad. Arts and Sci.*, Vol. 38, No. 21. (iii.) Bateson, W. *Nature*, Vol. 67, pp. 462, 585; 68, p. 33.	(i.) Darbishire, A. D., *loc. cit.* (ii.) Weldon, W. F. R. (*a*) *Biom.*, Vol. 2, p. 286. (*β*) *Nature*, Vol. 67, pp. 512, 610; 68, p. 34.

IV

The Laying Bare of the Marvel—A Legend[1]

(*The Manchester University Magazine, April,* 1905)

It was in the days when men said, "This is an age of civilisation"; but their words were not true. For it was a time when things were, as they say, "topsy-turvy"; when those who did much work had little goods, and those who did none at all were fat and rich and clothed in the furs of animals; it was a time when the exchange of goods was greatest between those in whom the desire for goodness was least, when those who had great store of goods drave in chariots, noiseless, dragged by horses, and when those who were richer still and more wicked drave in chariots, noisy, driven by spirits. But I speak not of these because I know them not; but of those who do much work, and have no stores; for they busy themselves with many things, and derive pleasure from wondering how those things which they see round about them have come to be. And the greatest pleasure amongst them is to discover how such things come to pass, and the greatest foolishness amongst them is to think they can discover why. Now of such folk there are two kinds—one kind which busies itself with things that are dead and have never been alive; and another whose delight is in things which are alive but which do not remain so for long. The knowledge got by the first is necessary to the labours of the second; but the knowledge got by the second is not

[1] This fable is an account of the Cambridge debate on Heredity at the British Association Meeting, 1904, in which Mendelians and Biometricians disputed. Petúrcha = A. D. D.

necessary to the labours of the first. But those that are busied with dead things and those that are busied with the living agree with one another in this, that they desire that there should be orderliness in the things with which they deal. And behold, this desire is so strong within them that they do not rest until they have discovered, in those things with which they deal, this order, this regularness. But more words must be spoken of the dead things, and more of the living. Now wizards declare that all things both living and dead are made up of things so small that they cannot be cut up, and so small that they cannot be seen, and so small that unbelievers in these matters have said, "They are not."

The things (or as some say, the units) with which those that treat of the dead are busied are these things that cannot be cut up; and the things with which those that treat of the living are busied are not the things so small that they cannot be cut up, but things so large that they can be, and often are, cut up: herbs and beasts. Nevertheless, they are made up of the invisible things which cannot be cut up; and that is why, on the one hand, the knowledge of the wizards of the dead is necessary to the labours of the wizards of the living; and why, on the other hand, the knowledge of the wizards of the living is not necessary to the labours of the wizards of the dead. And when a wizard of the living asks a wizard of the dead, "What are the things which cannot be cut up like; and what do they?" he makes answer, "Wullahy! we know not what any one of them is like; and we know not, neither can we foretell what any one of them will do; but we know very surely what a vast multitude of them is like, and can foretell what it will do." And when a wizard of the dead asks a wizard of the living, "What like are your things which can be cut up, your herbs and beasts? Can it be so that you can see and handle them and know their virtues?" he makes answer, "Very truly this is so; and it is our chief desire to know what like is each one in itself and in its relation to others. It is as if I were a shepherd of much experience and a thousand sheep; I live among them and tend them, and know the face of each one of them; while you are a shepherd of no experience with a thousand thousand sheep. I know the properties of every sheep, while you trouble yourself only with the properties of flocks." Now this was spoken in a friendly manner, but the wizard of the dead went away, saying, "I do not understand your words, and I had rather have a knowledge of flocks than be the companion of sheep." Surely there was no justice in his wrathful taunt; but it is often thus. Now it is established that the greatest difference between the things which cannot be cut up and the things which can be and often are, is that the first are always the same, indestructible, undying because they are never alive, and that the last are destructible, mortal, but continuing through the ages by begetting young after their kind; for beasts do not beget herbs, nor herbs beasts; nor even one kind of beast another kind of beast; but beasts of certain kinds beget beasts of the same kind, or nearly; and herbs of certain kinds beget herbs of the same kind, or nearly. Now this likeness of the generation begotten to the generation which begat it was a marvel for all time; but it was not till the time of which we have spoken, the time when men lied, saying, "There is civilisation," that wizards said to themselves, "What is this marvel, and what are the inward workings of it?" And so great was the desire to discover the secret that both wizards of the dead and wizards of the living were fired with it. Now it must be remembered that in all wizards the passion to discover an orderliness in the things with which they busy themselves is very great; and the wizards of the dead asked, "How can we discover an orderliness in this marvel of the things which can be cut up?" and they answered themselves, "In the same way as we discover it in the things which cannot be cut up." So, forthwith they gathered vast records of the likenesses of sons to fathers and sons to grandfathers, and of daughters to mothers, and daughters to grandmothers; and they busied themselves in this way with men, and with horses and with dogs; and then they said, "Behold we, the life-measurers, have discovered an orderliness in this marvel, and we are satisfied." But the wizard of the living did not hearken. For they were trying to discover an orderliness in the marvel, in the manner in which they were wont, namely, by busying themselves, not with large collections of things, as the life-measurers do, but by finding out the properties of each of the different kinds of herbs and beasts, and how they are passed on from father to son. Now they believed that a key which would unlock the secret of the marvel had been given them by a certain priest who had remained obscure for a long time; [1] so that the wizards of the living who sought to find an orderliness in the marvel were called followers of the priest; and the wizards of the dead who sought the same thing were called life-measurers.

At the time of the year when the sun is hottest and the thunder rolls there was a great gathering together of wizards on the banks of a narrow and sluggish river which flows through a flat and swampy region of a certain island. There were many dwellings near the river; some were old and others were new; and in one of the new ones on a certain day there gathered together the life-measurers and the priest-followers; and the great chief of the life-measurers was there, and the great chief of the priest-followers was there, and their names are too sacred to be breathed. There was also there one Petúrcha, who tarried in the hut of one of the kindest and most jovial of the wizards; Petúrcha was young; he was not a chief, but he marvelled at the marvel. In the dwelling, at the appointed time, words fell from the mouth of the chief of the priest-followers which were not pleasing to the life-measurers; and that which the chief of the life-measurers said found no favour with the priest-followers;

[1] Mendel.

and each party said, " We are right " ; and each party said, " You are wrong." But Petúrcha said, " Both may be right," and he found favour neither with the life-measurers nor with the priest-followers. And the wizards departed from the banks of the sluggish river, without having laid bare the marvel. And Petúrcha came back to the banks of another river, whose waters were black and sluggish, and on whose banks there are many huts ; and though the huts are black the dwellers in them are not sluggish. He came back to the wizard who was his chief, the chief who by the other river sat upon the fence-work and was not sealed a priest-follower, and was not sealed a life-measurer. And Petúrcha, who was likewise not sealed, sent forth word saying, " There are two ways of busying with the marvel : the way of the wizards of the dead, and this is done by life-measurers ; and the way of the wizards of the living, and this is done by the priest-followers ; it is for a man to choose which way he will pursue ; but if he thinks the way which he chooses not is wrong, his thoughts are wrong. There is no strife between the two ways." But save from a few there came no answer.

These things happened in the days when civilisation, rising in the east, had not spread over the face of the earth.

V

On the Difference between Physiological and Statistical Laws of Heredity

(*Manchester Memoirs, Vol. l.*, 1906, *No.* 11.)

§ 1. Introductory.—§ 2. Statistical Laws: (*a*) Pearson's Law; (*b*) Galton's Law; (*c*) The difference between the two.—§ 3. Physiological Laws: (*a*) The Law of Diminishing Individual Contribution; (*b*) Mendel's Law; (*c*) The difference between the two.—§ 4. The difference between statistical and physiological Laws: (*a*) The former "descriptive": the latter "explanatory"; (*b*) Mendel's Law true of units: Pearson's of masses; (*c*) Examples of the confusion between physiological and statistical Laws; (*d*) Description of a method of dealing with the material of a breeding experiment in such a way that the data obtained may be used to test the validity both of Mendel's and of Pearson's Law; (*e*) Why do white sheep eat more than black ones?

§ 1

THERE are those who maintain that it is not the part of the biologist to argue, to discuss, and to explain; and who assert that he is transgressing his proper limits when he ceases to confine himself to the description of observations and experiments, and to drawing from them certain obvious conclusions.

I do not hold this opinion: because I am convinced that if as much (I do not say more) trouble were taken to understand the meaning of a term as is spent in establishing the authenticity of a fact, the progress of our knowledge of fundamental natural processes—heredity, variation, the determination of sex, to name a few—would be more rapid than it is at present. For it seems to me to be evident that nothing short of a firm but unprejudiced grasp, in the mind of the investigator, of the relation between the facts themselves and past, present, and possible attempts to account for them can enable him to advance toward a closer knowledge of these phenomena.

I think that the reader will admit the truth of this contention, if he is not one of those who still cling to the Baconian delusion that all that is necessary for the elucidation of the problems of nature is the bringing to light of as many facts as possible by as many workers as possible; whereas, as a matter of fact, it is obvious that that which hinders the progress of natural knowledge is not the slowness with which facts are brought to light, but the paucity of investigators capable of dealing with them properly.

The incapacitating fault among biologists which is at once the commonest and the most serious is the unconscious ease with which they fall into the error of using a term without having previously ascertained its meaning. And so long as biologists turn a deaf ear to speculation, this disease will flourish. That which is necessary, therefore, to make progress both surer and swifter is a greater aptitude to formulate a clear idea of the meaning of the terms which are employed—a habit of mind which is not likely to be common so long as the consensus of biological opinion regards with less favour the attempt to discover the essence of a newly suggested hypothesis, than the attempt to describe the course of the vas deferens in a newly discovered worm.

In the study of heredity in particular the most extraordinary confusion has resulted from the fact that not only has the same term been used to mean different things by different writers, but very often has had many significations in the writings of a single author. This state of affairs is due, in my opinion, to the absence of any patient and laborious attempt to thresh out the meanings of the terms continually on the lips of those who take part in the discussion of this subject; and this absence is due in its turn to the callousness, if not disfavour, with which such an attempt is likely to be regarded. Nevertheless I propose to make it.

§ 2

Space forbids me to discuss the question of the advisableness of using the term "law" *at all* as summarising vital phenomena, more than to say that the fact that I use it 166 times in this paper demands some apology.

I use it because, besides possessing the advantage of brevity, it is of all terms in biology the vaguest; signifying as occasion demands either a theory, or a résumé, or a hypothesis, or a formula, or a generalisation—to name a few of the more or less legitimate senses in which it is used: and because it shelters, under its wide roof, Laws whose authors aim at explanation, and those whose authors are satisfied with description. And I spell it with a capital L because that is the conventional way of writing terms the discussion of whose meaning is postponed.

(*a*) Pearson's Law

No one has any excuse for not knowing what the Law of Ancestral Inheritance is; the essential features of it are outlined by Pearson in the following words:—

"Taking our stand then on the observed fact that a knowledge neither of parents nor of the whole ancestry will enable us to predict with certainty in a variety of important cases the character of the individual offspring, we ask: What is the correct method of dealing with the problem of heredity in such cases? The causes A, B, C, D, E, . . . which we have as yet succeeded in isolating and defining are not always followed by the effect X, but by any one of the effects U, V, W, X, Y. We are, therefore, not dealing with causation but correlation, and there is therefore only one method of procedure possible; we must collect statistics of the frequency with which U, V, W, X, Y, Z respectively follow on A, B, C, D, E. . . . From these statistics we know the most *probable* result of the causes A, B, C, D, E and the frequency of each deviation from this most probable result. The recognition that in the existing state of our knowledge the true method of approaching the problem of heredity is from the statistical side, and that the most that we can hope at present to do is to give the *probable* character of the offspring of a given ancestry, is one of the great services of Francis Galton to biometry." [1]

(*b*) Galton's Law

Galton formulated his Law as follows: "The two parents contribute between them on the average one-half, or (0·5) of the total heritage of the offspring; the four grandparents, one-quarter, or $(0{\cdot}5)^2$; the eight great-grandparents, one-eighth, or $(0{\cdot}5)^3$, and so on. Thus the sum of the ancestral contributions is expressed by the series $\{(0{\cdot}5) + (0{\cdot}5)^2 + (0{\cdot}5)^3$, etc.$\}$, which, being equal to 1, accounts for the whole heritage."

(*c*) The Difference between Pearson's and Galton's Law

It will be seen how profoundly Galton's differs from Pearson's Law. Yet the belief that the two are much the same is not rare, and the statement that the latter is merely an extension of the former is often made. A clear appreciation of the difference between the two is necessary to anyone who wishes to be conversant with modern theories of heredity.

One feature the two have in common: both of them are true only of masses, and do not pretend to apply to individuals. This is so obvious to the careful thinker that Pearson only refers to it in a footnote; [3] yet it is often forgotten. The difference between the two lies in this: Pearson's Law measures the degree of correlation between a character or characters in a given generation, and some similar (or dis-

[1] Pearson, :03*a*, p. 215. [2] Galton, '97, p. 402. [3] Pearson, :04, p. 161.

similar) character or characters in the preceding generation. Galton's Law states the amount which a given generation *contributes* [1] to the generation which it produces. It definitely states that on the average a half of the filial generation are like the parental, a quarter like the grandparental, and an eighth like the great-grandparental, and so on. From the knowledge that the parents of a given generation of cats are tabbies, and that half of its grandparents are tabbies, a quarter whites, and a quarter blacks, you are enabled to predict by Galton's Law the proportion in which these three kinds of cats will occur in that generation. Pearson's Law does not enable you to do this: it is of an entirely different kind. For Pearson's Law to be true it is not necessary that *any* of the children should be like *any* of the parents; all that is necessary is that a particular kind of parent should be associated with (i.e. should produce) as often as not a particular kind of child. On the next page is an imaginary Correlation Table, in which Pearson's Law is borne out, yet in which none of the children are like any of the parents.

The fact that the relation between a given generation and those that precede it is described by a series of figures which in the case of Galton's Law is ·5, ·25, ·125, ·0625, etc., and which in the case of Pearson's, for eye colour in man, for example, is ·4947, ·3166, ·1879,[2] has led some to believe that the figures mean the same thing (which, of course, they do not) and has thus constituted a trap for the unwary. Castle has done good service to progress in the study of heredity by falling into it.[3]

I hope I have made clear what the difference between the meanings of the two series is; for to understand this is to understand the difference between the two Laws.

This difference is sometimes expressed in the statement that Pearson's Law is more comprehensive and less bio-

[1] *See* Appendix B, p. 197. [2] Pearson, :03*a*, p. 221. [3] Castle, :03, p. 224.

FATHERS.

CHILDREN.		Black.	Dark Grey.	Grey.	Pale Grey.	White.	Totals.
	Purple.	3	11	2			16
	Bluish Purple.	8	27	19	8		62
	Purplish Blue.	6	30	39	32	3	110
	Blue		12	27	33	18	90
	Pale Blue.			4	10	8	22
	Totals.	17	80	91	83	29	300

The degree of correlation in the above Table is that between the number of dice exhibiting 4 or more than 4 points (or "pips") uppermost in first throws, and the number exhibiting those faces uppermost in second throws, in a series of 300 double throws. The two throws are correlated by leaving half the first throw on the table, so that the second throw has half the dice lying exactly as they fell in the first throw. For details of this process the reader is referred to Weldon :06, p. 100.

The degree of correlation in my imaginary Table is ·54; for calculating which I am indebted to Mr. Udny Yule. The "coefficient of parental heredity," therefore, in this case is identical with that for the inheritance of deafness, of which Pearson's Law is true, recently worked out by Schuster (:06, p. 478). Yet, in my imaginary case, none of the children could be mistaken for any of the parents.

logical[1] than Galton's; and inasmuch as it embraces sets of facts which are not described by Galton's formula, the first of these statements is true; and inasmuch as the relation between successive generations which it measures is the same as the relation between two series of throws of dice (in which reproduction is unknown), of which every throw of the second series consists of half the dice lying exactly as they fell in the corresponding throw of the first series, the second is true also.

§ 3

(*a*) The Law of Diminishing Individual Contribution

In my paper on the supposed antagonism of biometric to Mendelian theories of heredity, I showed that a set of facts (summarised in the Table, *vide* p. 148 *supra*), appearing at first to be a complete refutation of Mendel's Law, could easily be shown to be equally in accord with both Mendelian and Galtonian theories.[2] Mendel's Law describes the individual phenomena in this case *perfectly*: Galton's Law describes the mass result composed of these very individuals mating at random *perfectly*. The latter describes the proportions, the former accounts for them. The Galtonian deals with individuals from the point of view from which the physicist deals with atoms; the Mendelian deals with them from that of Clerk Maxwell's demon.

Now just at the same time that I announced my discovery that the proportions of the albinos in this case were not evidence against the truth of Mendel's Law,[3] Castle made the same discovery.[4] But he argued from his discovery, not (as I did) that the two theories were compatible but that Galton's was wrong; that is to say, he must have thought that the two theories were mutually exclusive; which indeed he did: but not in the same way that I did.

[1] Fruwirth (:05, p. 147) goes so far as to say that " das Ahnenerbengesetz ist kein biologisches Gesetz. . . ."

[2] Darbishire, :05*a*, p. 6. [3] *ibid.*, :05*a*, p. 9. [4] Castle, :05, p. 17 *et seq.*

For whilst the way in which I made that error was by lifting Mendel's Law from the level of a would-be explanatory to that of a purely descriptive Law, he made it by lowering Galton's from the level of a purely descriptive to that of a would-be explanatory one. And the reason that I discovered my mistake before he did was that it is easier to see that Mendel's Law is something more than a purely descriptive one, than it is to see that Galton's is not a would-be explanatory one. And the reason again of this is that Galton's Law is confused with another one which resembles it in one respect, but differs from it in being would-be explanatory. The remarkable thing about this Law is that whilst it is characteristic of most Laws to be enunciated and receive a name first and then become widely believed in afterwards, the reverse is the case with this one ; for it is believed in by all biologists who are not Mendelians, by all breeders of animals or plants, and by all persons not belonging to these classes who think about heredity at all.

But it has not yet received a name. I propose to call it the *Law of Diminishing Individual Contribution.*

According to it : *the germ plasm of an individual contains contributions from all of its progenitors : the amount of the contribution being large in proportion as the progenitor is near*, i.e. large in the case of the parents, smaller in the case of the grandparents, and so forth.

It is a very good type of biological Law : it has the advantage of simplicity : it is also, except in a few cases, untrue.

I will now give three cases to show how widespread belief in this Law is.

The first that I give is that of the result of crossing a yellow and white pink-eyed Japanese waltzing mouse with a pink-eyed white mouse—that is, an albino. The result is, usually, a black-eyed grey mouse.[1] And to anyone not familiar with it, the result is most astounding : it is quite the

[1] Darbishire, :04*a*, p. 7.

opposite of what one would expect. Expect from what? From one's—possibly unconscious—belief in the Law of Diminishing Individual Contribution.

Another case. Now that we know that a blue Andalusian fowl is a heterozygous form produced by mating a black and a white, and that Andalusians when mated together produce 25% Blacks, 50% Andalusians, and 25% Whites, we no longer try to get a pure strain of Andalusians by throwing away the blacks and whites and by continuing to breed from the Andalusians for many generations, because we know that we can always get Andalusians and nothing else by mating blacks and whites.[1] What is the conception of heredity which underlay the old-fashioned attempt to breed pure Andalusians by weeding out the blacks and whites, but the Law of Diminishing Individual Contribution?

Again, Coutagne,[2] in discussing the possibility of the hybrid nature of some dark-lipped individuals of *Helix hortensis,* which occurred in a collection of that species and *Helix nemoralis* living in one locality, concluded from the fact that these supposed hybrids were *unbanded,* whereas the great majority of the *H. nemoralis* in that locality were banded, that they were *not* hybrids. To translate his own words, "If the *H. nemoralis* were the parents of the 113 black-lipped individuals there is every reason to believe (*tout porte à présumer*) that this character of banding would appear at least in some cases in these 113 individuals." Through Lang's [3] work we know now that in a cross between a banded *H. nemoralis* and an unbanded *H. hortensis* the unbandedness is dominant. So that now we should *not* expect "this character of banding" to appear in *any* of the individuals: and Coutagne's argument falls to the ground.

But what is "*tout porte à présumer*" but the expectation

[1] Punnett, :05, p. 28. [2] Coutagne, :95, p. 72.
[3] Lang, :04, p. 497, and :06 (*see also* Darbishire, :05*b*, p. 196).

based on a firm belief in the Law of Diminishing Individual Contribution ?

These three cases show how widespread is belief in this Law ; and they also show that in these three cases at least it is not valid.

The difference between the expectation based on this Law [1] and the accurate knowledge of what actually takes place (which it is the business of Mendelian investigation to supply) is the same as the difference between common sense and science, and the same as the difference between that which stands to reason and that which rests on evidence.

(*b*) Mendel's Law

I do not propose to discuss here the statement that the time has not yet come when we are justified in speaking of Mendel's *Law*, nor to inquire into the meaning of this statement : the question I propose to answer is, " What is the essential feature of that which is called Mendelism by those who believe in it, and Mendelianism by those who do not ? "

I divide definitions of the Law into two primary categories :—

(i.) Suitable for those who desire to establish the invalidity of the Law.

(ii.) Suitable for those who wish to discover whether Mendelism has helped us or is likely to help us to attain to a more intimate knowledge of heredity.

There is no difficulty in finding a definition of the first class : a very satisfactory one is one which binds the Mendelian down to the Law exactly as enunciated, and the description of the phenomena exactly as given by Mendel

[1] Huxley must have been thinking of some such Law as this when he made the remarkable statement that science was organised common sense.

himself. If this is carefully done, no difficulty will be encountered in establishing the invalidity of Mendel's Law as facts accumulate.

To discover a definition of the second class is not so easy. To my mind, there are two perfectly distinct things included under the one term Mendelism. One is belief in the existence of character-units in the germ, and in the thesis that these units are pure in respect of the characters which they represent. The other is the *method* by which the extent, separateness, and transmission of these units is discovered. The first may be called the Mendelian theory, the second the Mendelian method—which is the application of the experimental method to the study of heredity. I think that both these things are implied when the term Mendelism is used; and whether they are or not—and it does not in the least matter—I believe that the Mendelian method will do as great service, in accounting for the phenomena of heredity, as that particular theory which Mendelians happen to be employing at the moment.

For I think that it must be evident to anyone who has followed closely the Mendelian work of the last few years that, while the method which workers of that school have employed has remained the same, the actual theories, by testing the validity of which they have sought to attain their end, have been from time to time considerably modified. This procedure, of course, makes it difficult for those who wish to criticise or base statistical calculations on the theory itself. Pearson comments on it in these words: "The original Mendelian theory has been replaced by what are termed 'Mendelian Principles.' In this aspect of investigation the fundamental principles propounded by Mendel are given up, and for each individual case a pure gamete formula of one kind or another is suggested as describing the facts. This formula is then emphasised, modified, or discarded, according as it fits well, badly, or not at all with the growing mass of experimental data. It is quite clear that it is impossible while this process is going on to term anything whatever Mendelian as far as theory is concerned." [1]

But should we be right in refusing to commend the efforts of a well-digger if, in sinking his well, he alternately used a spade, a pickaxe, and dynamite, according as he had to deal with gravel, sandstone, or granite, provided that he found, or even that he thought he would find, water at last ?

The aims of the Mendelian and the well-sinker are the same—to discover something ; and they each employ a definite method, but the tools they use are continually being changed. That is why I think that the method is at least as essential a part of Mendelism as the theory. And that is why I think that there is no more connection between Pearson's generalised theory of alternative inheritance (with special reference to Mendel's Law), and Mendelism, than there is between the second law of thermo-dynamics and the Maxwellian demon's knowledge of atoms *plus* the method by which he has acquired it.

There *is* a definite relation between a generalised theory of alternative inheritance and that particular doctrine on which it is based : it is the same as the relation between the second law of thermo-dynamics and the theory held, *ex hypothesi*, by the demon as to the nature of the atom.

But there *can* be no relation between any generalised theory of inheritance and Mendelism[2] unless that term signifies the Mendelian theory *only ;* and, even so, this re-

[1] Pearson, :03*b*, p. 53.

[2] I do not, of course, intend to imply that Pearson tries to establish any relation between a generalised theory of inheritance and Mendelism : I *know* his was a generalised theory of alternative inheritance based on the theory of the pure gamete. All I wish to insist on is that the theory which Mendelians happen to be testing at the moment is, to my mind, not the essential thing in Mendelism. If the commonly accepted explanation of the proportion 1DD : 2DR : 1RR were shown to be false, would experiments, called Mendelian, now in progress be prosecuted with less zeal ? By no means. Such a discovery would even be an incentive to more strenuous search.

lation cannot be permanent unless the Mendelian is pledged not to change his theory in the smallest degree. I hold that, in the first place, Mendelism has, as I have shown, a wider signification—namely, that it embraces the method as well; and secondly, that no Mendelian can be expected to take the pledge demanded, if by doing so he believed, as he probably would, that he would be prevented from attaining his end. It is idle to accuse him of inconsistency. What should we think of the consistency of a well-digger who died of thirst because he would stick to his spade although only a few feet of granite separated him from water?

And what right have we to expect that the demon should pledge himself not to alter his theory of the nature of atoms, if he hopes that by being free to alter it he will attain a knowledge of them that will enable him to live in a warm compartment without having to do any work for it? What right has a physicist to expect a demon not to alter his theory, on the ground that such an alteration makes it exceedingly difficult for him (the physicist) to use the theory as a basis for statistical calculation?

(c) The Difference between Mendel's Law and the Law of Contribution

Now that we come to discuss the difference between Mendel's Law and the Law of Diminishing Individual Contribution, we must clearly understand that by Mendel's Law we mean the theory associated with that name, and not the method.

These two Laws resemble each other in being physiological, in that they attempt to picture the way in which characters are represented in the germ-cell. But they differ profoundly in the picture which they draw. The difference is so obvious that it is hardly necessary to speak about it. A remark on the difference between the predictions of the two Laws as to the nature of the offspring of extracted recessives will suffice. Suppose that two hybrid mice with grey coats and black eyes were to produce an (extracted) albino—which, if the Law of Contribution were true, they could not do: the Mendelian prediction about the offspring of a pair of such albinos is that they will be all albinos; the expectation based on the Law of Contribution is that a quarter of the coat of each individual child will be grey—supposing the proportions for individuals in which each progenitor contributes, according to that Law, to be the same as that demanded for populations by Galton's. The Mendelian prediction is right.[1] In fact, the Law of Contribution is so utterly invalid that every case of alternative inheritance is a contradiction of it. It may apply to some cases of blended inheritance. But the reason that I have formulated it and given it a name is not that it may perhaps apply to one or two cases, but because, unless it is definitely enunciated, it will not be reckoned as having any claims to recognition, and because, the sooner it is widely recognised, the easier will it be to put an end to its confusion with Galton's Law.

§ 4

(*a*) Statistical Laws, "descriptive": Physiological Laws, "explanatory"

The remark might be made about the Law of Contribution that it is Galton's Law made applicable to the individual; and this in a sense is true, but there is a profound difference between the two, for whilst the Law of Contribution is an attempt to picture the way in which characters are represented in the germ-cells of individuals, Galton's Law is merely a statement that the characters of the ancestry of a population reappear in certain definite proportions in that population. It is only concerned with that which is above the horizontal line *AB* in the figure on p. 182. Moreover, it is only true of the aggregate of adults, and not of the individuals which compose that aggregate.

[1] Darbishire, :04*a*, p. 23.

The Law of Contribution, on the other hand, deals with that which is below as well as with that which is above the horizontal line, and it is true of the individuals.

Now, although Galton's Law is true of the mass but not of the component individuals, the Law of Contribution is true of the individuals and of the mass as well, because according to it all the individuals are contributed to, in the same degree.

The widespread belief in the existence of a Law which is true both of mass and individual is the result of the interaction of two outstanding characters of the human mind: (i.) an inability to distinguish between truths about masses and truths about individuals; and (ii.) a passion for explaining things; for possessing a formula for familiar phenomena; in the case of heredity, for seeing below the line in the diagram on p. 182.

Man has observed that on the whole Galton's Law is true of masses—though he has not expressed himself in these words. Feature No. (i.) had led him to think that the truth is of individuals, and has thus satisfied No. (ii.) and supplied him with a brief formula, a simple picture of the way in which characters are inherited—a picture of what is below the line *AB* in the diagram.

Even if this is not a true history of the origin of the Law of Contribution, the point about that Law which I wish to enforce is that it is a physiological theory of heredity. It is an attempt to account for the phenomena of heredity by picturing the way in which the characters of an organism are represented in the germ-cell which produces it. And being a physiological Law, it is profoundly different from a statistical one like Galton's, which does not pretend to account for anything, but is a generalisation about the relation between the aggregates of adults of successive generations.

In a word, a physiological Law deals with the individuals of each generation on both sides of the line *AB* in the

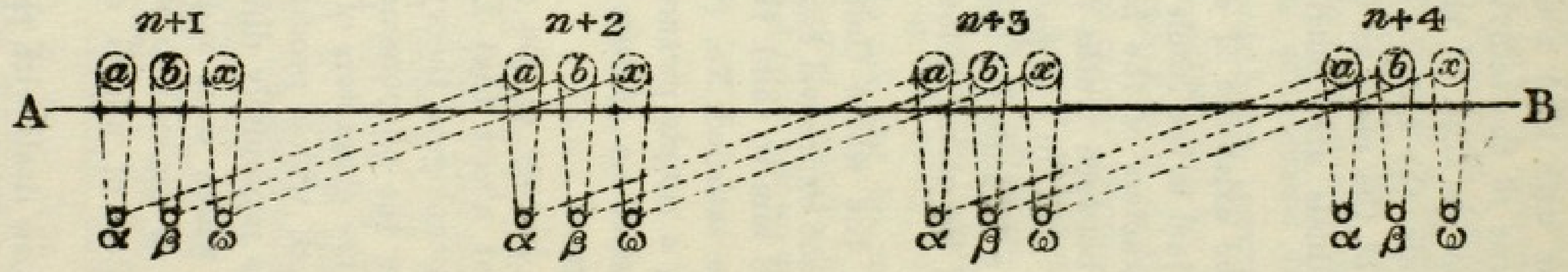

DIAGRAM TO ILLUSTRATE THE DIFFERENCE BETWEEN PHYSIOLOGICAL AND STATISTICAL LAWS OF HEREDITY.

$n + 1$, $n + 2$, . . . etc., represent successive generations. a, b, x the adult individuals of a given generation ; and α, β, ω the germ-cells formed by those individuals.

(The fact that I have not joined the sets of germ-cells α, β, ω by lines may seem to indicate disbelief on my part in Weismann's theory of the continuity of the germ-plasm. It does not.)

diagram; a statistical one, with the generation as a whole on the upper side only. Another way of marking the difference between physiological and statistical Laws of heredity is to say that the former are explanatory while the latter are descriptive. To which it will be immediately objected that no Law ever explains anything. I am perfectly aware of this, and of the fact, moreover, that no theory does; in fact, that we cannot *explain* anything at all in nature; that all that we can do is to describe. But at the same time it cannot be denied that there is all the difference between attempting to account for a phenomenon and contenting oneself with describing it; and one is, I hold, perfectly logical in making this attempt to explain, although one knows that, however intimate a knowledge of causation one has acquired, one has done no more than describe phenomena. One great difference between these two things, the logical attempt to explain, and satisfaction with mere description, is that the method of the former is experiment and that of the latter is observation. Another difference is that it is only by attempting to account for things that we have been enabled to get what knowledge we have of the causation of natural phenomena and so to obtain what control we have of the operation of natural processes. Just because we know that explanation is after all only description, it does not follow that we should abandon the attempt to account for things.

We are now in a position to classify Laws of heredity under these two headings:

Physiological.	*Statistical.*
(*a*) Mendel's Law.	(*a*) Galton's Law.
(*b*) The Law of Contribution.	(*b*) Pearson's Law.

Now inasmuch as of physiological Laws *b* has been shown to be invalid, and of statistical ones *a* has been shown to be less comprehensive than *b*, the discussion of the mutual relation of physiological to statistical Laws of heredity resolves itself into a discussion of the relation of Mendel's Law to Pearson's Law of Ancestral Inheritance.

(b) Mendel's Law True of Units: Pearson's, of Masses

Discussing a little while ago with a friend the relation of Mendelism to biometry, I suggested as the briefest possible statement of the difference between the two that Mendelism treated of units, and biometry of masses of units. My friend replied: "You speak of the animals and plants with which you deal as breeding true to such and such a character. What do you mean by this statement? Are the offspring absolutely identical with their parents in respect of the character under consideration? If they are not, how like are they; and how is the degree of this similarity to be measured, except by biometric methods?"

I saw that there was truth in what he said, but I could not see the relation which the idea in his mind bore to my original idea, to which I still adhered, that Mendelism dealt with the units, while biometry was concerned with masses. Now I see that my inability to do so was due to the fact that biometry meant to my friend that the only way to measure the resemblance between parents and children is the method of the correlation table; in which he was quite right; while biometry called up in my mind the Law of Ancestral Inheritance, and especially the manner in which data designed to establish that Law are collected and dealt with; such, for example, as in the case of one of the last series of data from which such correlation coefficients have been worked out—that of greyhounds. I had in my mind the collective treatment in one correlation table of such different characters as Black, Brindled, Fawn, White, and Red;[1] while he was thinking of the only method of measuring the intensity of inheritance within a single such character —say, Red. Now this, I believe, brings us to the heart

[1] Barrington, etc., :05, p. 264.

of the matter. When I say that the Mendelian deals with units and the biometrician with masses, I mean, not that the former deals with a few and the latter with many, but that the former first settles what character he is going to treat as a unit and then only deals with it in large numbers when he is sure that the component units of this number are identical, their sameness having reference to such properties as can be discovered by mating them with their like and with their unlike. It is just as necessary for the Mendelian to have a large record of matings as the biometrician to establish his generalisations. But though the Mendelian might allow that the only method of measuring the similarity between parents and offspring within a group (such as the unit character Red) was that of the correlation table, he would vehemently maintain that the biometric method overstepped its limits when it included in such a table more than one such category. What Bateson means when he says that Mendel saw by sure penetration that masses must be avoided, is that the biometric method oversteps its limits when it does this.

The answer that I should now give to my friend is this: "I fully admit that the only method of measuring the degree of resemblance between a generation and the one which produced it, within such a unit, is the biometric method; I fully agree with the biometrician when he says that all green peas are not alike in respect of their greenness because all green peas are green, and that the biometric method is the only one to measure their dissimilarity; but I stoutly maintain that when he puts green and yellow peas together into a correlation table he has started on a path which will not lead to a more intimate knowledge of heredity."

Biometry furnishes the only means of actually measuring the intensity of heredity within a unit; Mendelism furnishes the only means by which a fuller knowledge of the properties of these units may be acquired.[1]

[1] Exactly the same idea is expressed by Lotsy, :06, p. 143.

It is sometimes easy to determine the extent of these units, as in the case of discontinuous characters such as purpleness and whiteness of flower in *Pisum* :[1] it is often difficult, as in the case of characters varying continuously such as the weight of beans.[2]

Careful consideration of the table in my paper[3] on the supposed antagonism of Mendelian to biometric theories shows that the accuracy with which Mendelian or Galtonian Law describes the facts of heredity depends on the composition of the unit tested. There is nothing *a priori* illogical in treating the sharply defined category of dark-eye-and-coloured-coat, in mice, as a unit. Suppose this is done : Galton's Law fits the facts beautifully, while Mendel's is triumphantly refuted (by showing that the amount of albino ancestry of a hybrid affects the percentage of albinos produced by such hybrids mated *inter se*) ; to which the Mendelian would make the following answer : "Are you sure that your unit 'dark-eye-and-coloured-coat' is incapable of resolution into still simpler units ? Are you sure that you are not regarding as a simple thing that which is really compound, just as the 'fixed alkalies' were regarded as elements until Davy[4] showed them to be compounds ?[5] I can prove that you are ; for by testing the gametic constitution of the dark-eyed and coloured-coated forms I can show that they are sharply distinguished into heterozygous and homozygous forms. Now I claim to have discovered what should really be treated as a unit, namely,

[1] Fruwirth, :06, p. 141.
[2] Johannsen, :03.
[3] Darbishire, :05*a*, *vide supra*, p. 148.
[4] Davy, '08.
[5] This illustration may at first sight appear not to be strictly parallel. A chemist reading it would think that what was going to be shown was that the unit "dark-eye-and-coloured-coat" was resolvable into "dark-eye" and "coloured-coat," whereas, of course, what is really going to be shown is that the units "dark-eye" and "coloured-coat" are of two quite distinct kinds. The true parallel to my case is the idea of the element before Davy's time. Amongst the things that were classed as elements, some really were undecomposable (*cf.* the homozygous forms), while others —the alkalies—were decomposable (*cf.* the heterozygous).

that character which, like the elements of the chemist, cannot be split up into simpler characters. I do not pretend that my formulæ of heredity describe the numerical results obtained by jumbling a lot of my elemental units together, any more than you pretend that the Law of Ancestral Heredity describes the phenomena exhibited by my units when dealt with separately."

The point that I wish to bring home to the reader is that the statement that Mendelism deals with units and biometry with masses is not merely a brief summarising statement which pleases the mind, but that it has an actual meaning in relation to the facts; that is to say, that the limits of these units are not set by the imagination, but are discovered by experiment.

(c) Examples of the Confusion between Physiological and Statistical Laws

Having spoken this much on the difference between physiological and statistical Laws of heredity, I propose to consider a few cases where failure to perceive this difference has led to confusion. As I have already referred to Castle's case, I will finish with it before I proceed to others. He says:[1] "The foregoing results show very clearly that albinism conforms in the mode of its inheritance to Mendel's law of heredity. The fact, however, must not be overlooked that a somewhat *different explanation of its inheritance* (1) has recently been given, based on Galton's "law of ancestral heredity." I shall not at this time enter into a detailed discussion of Galton's hypothesis, which was an entirely rational one *in the form in which it was originally proposed* (2); and quite in harmony with the phenomena of *gametogenesis* (3) as then interpreted. I have shown elsewhere[2] by a specific test in the case of mice, based on the observations of von Guaita, that Galton's law *fails to account for the observed facts* (4) concerning the inheritance of albinism, but that

[1] Castle, :05, p. 16. [2] *ibid.*, :03, p. 231.

Mendel's law does this perfectly. Nevertheless Darbishire, likewise dealing with albinism in mice, though admitting that certain of his results are not in disagreement with Mendel's law, is inclined rather to interpret the phenomena *on some such hypothesis as that of Galton*" (5).

The numerals refer to the words in italics preceding them.

(1) This shows that Castle followed me in confusing Galton's Law with the Law of Contribution.

(2) Here we see that Castle has started on a right track: he has perceived that Galton's Law in the sense in which I used it, meaning the Law of Contribution, is not the same as that Law in its original form.

(3) And yet he thinks that in its original form it was in harmony with the phenomena of gametogenesis as then interpreted, whereas it seems to me that the chief characteristic of a statistical Law is that it is independent of any theory of gametogenesis whatsoever.

(4) Of course it does: because it does not attempt to. What he means is that the Law of Contribution attempts *and* fails: and this is quite true.

(5) Here again as in (2) we see light breaking in on the confusion between Galton's Law and the Law of Contribution. Castle sees that the theory of heredity I had in mind is not quite the same as Galton's: I have shown (p. 180) exactly how it differs from it.

I will now refer to a case in which the confusion between Galton's Law and that of Contribution is complete.

In 1904 I wrote:[1] "*I do not propose to discuss here the difference* (1) between Mendelian principles and the *statistical conception of inheritance* (2), but to consider one part of the hypothesis put forward by Mendel, which is at variance with *Galton's theory* (3). I refer to the phenomenon of segregation. We have seen what Mendel says. But this is flatly contradicted by the *Galtonian generalisation* (4),

[1] Darbishire, :04*b*, p. 9.

according to which the greater number of generations a given hybrid is from the first hybrid . . . the fewer pure recessive and dominant forms is it likely to produce when mated with another hybrid of its own generation."

(1) For the very good reason that I did not then understand it: as I do now.

(2), (3,) (4) If "Law of Contribution" is put in the place of the terms 2, 3, and 4, this passage is quite true As it stands, it is nonsense.

Perhaps, after all, the most complete example of this confusion is to be found in Castle's writings; it occurs in his paper on Galton's and Mendel's Laws. He gives a Table[1] to show the difference between the Mendelian and Galtonian prediction of the number of albinos produced by crossing Japanese waltzing mice with albinos, together with the actual numbers that occurred in von Guaita's well-known experiment.[2]

I give the top two lines of the Table.

Generation.	*Total Young.*	*Mendel's Law. Calculated No. of Whites.*	*Observed No. of Whites.*	*Galton's Law. Calculated No. of Whites.*
II.	28	0	0	14

Substitute Law of Contribution for Galton's Law, and the idea conveyed by the Table is sensible and true.

I will refer to one more passage in which Galton's Law is used in the sense of the Law of Contribution: "Nach *Galtons* Theorie muss jede Gamete, welche von einem Individuum produziert wird, imstande sein, *alle* Merkmale der Sippe, welcher es angehört, auf die Nachkommen zu übertragen; es ist unmöglich, dass gewisse Gameten für immer von der Übertragung gewisser Merkmale ausgeschlossen

[1] Castle, :03, p. 231. [2] Guaita, '98 :00.

werden."[1] Here "Galtons Theorie" is made to refer not merely to the individual but to the gamete borne by it, and the expression as here used means nothing more nor less than the Law of Contribution. It is true that Galton himself tentatively suggested,[2] when he formulated his Law, that it might become applicable to the individual.[3] But his Law, as it stands, is a statistical Law true of masses of units; and when a physiological theory of heredity, as in the above quotation, is spoken of as "Galtons Theorie," it is high time that a new term is invented to describe it. I have proposed the "Law of Contribution."

Nothing could be more fatal to profitableness of discussion than that two such profoundly different things as Galton's Law and the Law of Contribution should go by the same name.

So long as physiological are not clearly distinguished from statistical Laws of heredity, biologists will continue to slide from meaning a physiological to meaning a statistical one; and the transition will be unconscious because the term by which they denote these two different things is the same—namely, Galton's Law. Progress in the study of heredity will be slow as long as this confusion prevails. For so long as it prevails we shall continue to hear the insensate statement that ancestry *makes a difference*. Of course it makes a difference—in the mass; which it is the business of the biometrician to measure and of the Mendelian to account for. Anyone who proclaims that his results prove that ancestry makes a difference, without making it clear whether he has in mind a physiological or a statistical theory, is drawing a conclusion which is meaningless. For his conclusion to have a meaning, he must make this clear. If he is referring to the former, he is declaring for the Law of Contribution; if to the latter, for the Law of Ancestral Inheritance.

When the Mendelian says that ancestry does not make

[1] Lotsy, :06, p. 152. [2] *See* Appendix A, p. 196 *infra*. [3] Galton, '97, p. 403.

a difference, he is not denying the validity of the Law of Ancestral Inheritance but the Law of Diminishing Individual Contribution. At least, I think this is the correct attitude.

And I cannot bring myself to agree with Bateson when he says that facts once describable by Mendel's Law are permanently removed from the operation [*sic*] of the Law of Ancestral Inheritance, unless all that he means by this statement is that when we have gained this deeper knowledge of certain hereditary phenomena their further treatment by the method of the correlation table will not increase our knowledge of them. I should like to think that this is all he means; but his writings prevent me, for he imputes to upholders of Pearson's Law belief in the Law of Contribution;[1] yet on the next page he shows that he has not confused the two, by saying that the Law of Ancestral Heredity "*does not directly attempt to give any account of the distribution of the heritage among the gametes* of any one individual." I do not know whether Bateson *still* holds that Mendel's Law is antagonistic to the Law of Ancestral Inheritance as well as to the Law of Contribution. If he does, I do not understand on what grounds. Pearson has investigated the relation between the two, and concludes "that in the theory of the pure gamete there is nothing in essential opposition to the broad features of linear regression, skew distribution, the geometric law of ancestral correlation, etc., of the biometric description of inheritance in populations."[2] And no flaws in the argument of my paper on the supposed antagonism of Mendelian to biometric theories have been pointed out to me; in fact, Correns[3] and Giard[4] have expressed their agreement with it.

I feel most strongly that so long as we confuse physiological with statistical Laws of heredity we are wandering in the dark: we cannot know in what direction our studies are leading us, whether we are establishing correlations

[1] Bateson, :02, p. 21, second half. [2] Pearson, :03*b*, p. 86.
[3] Correns, :05, p. 43. [4] Giard, :05, p. 22.

among the leaves or are digging among the roots. It may or may not be that what we learn by the former method is all that we shall ever know, and that we shall find nothing by our digging; but be this as it may, I hold that it is essential to progress in discovery, no less than to clearness of thought, that we should know which of the two we are doing.

(*d*) Description of a Method of Dealing with the Material of a Breeding Experiment in such a way that the Data obtained may be used to Test the Validity both of Mendel's and Pearson's Law

When I had finished my last paper on my hybridisation experiment with mice[3] I was still of the opinion that Mendelian and biometric Laws of heredity were mutually exclusive, and that if I could discover which of the two was true, I should be making a forward step in our knowledge of heredity. I therefore devised an experiment which was destined to settle this question; and wasted a year in carrying it out. As soon as I discovered the true relation of the two Laws I devised a method of dealing with my experiment, of such a kind that the results could be utilised by the Mendelian or the biometrician to test his own particular Law; for the stringency with which the mice were selected in the previous part of the experiment rendered the results useless for anyone who wished to test the Law of Ancestral Inheritance by them.

What was wanted was some device to ensure the random mating of the mice, and at the same time to ensure the possibility of tracing all the ancestors and all the offspring; in fact, all the relations of every degree of every individual mouse; the second condition had been fulfilled in the previous part of my experiment, but the first had not, because the different kinds of mice had been rigidly selected.

The method by which I mated the mice at random was

[3] Darbishire, :04*a*.

very simple. I wrote the catalogue-name of each mouse on a counter, then I put the counters representing female mice into one hat and those representing males into another: all that remained to be done was to draw out at random a counter from the "female" hat and similarly one from the "male" hat, and to mate the actual mice represented by these counters. As I have said, this method enables one to test the Law of Ancestral Inheritance and Mendel's Law. As far as the first is concerned, it is the most perfect conceivable; but for the second it is clumsy and involves unnecessary labour, because what is aimed at in a Mendelian experiment is the discovery of the properties of character-units, as far as they can be discovered by determining the specific results of their union with similar and dissimilar character-units. Now some particular combination of characters may turn up very seldom by the method of random union; and if one wishes to discover the result of such a combination one has to wait until the drawings from the hat give it. One is in the position of an observer, and if one wishes to attain the knowledge of the result of such a combination quickly and in large numbers, the random mating and the counters must be discarded and one must deal *experimentally* with the material, isolating the individuals the properties of whose character-units one wishes to determine.

I had planned at the beginning of last year (1905) to do the same experiment with peas by mating at random peas with green round seeds (Eclipse) with peas with yellow wrinkled seeds (British Queen), and with each other; and had already sown the seed, when it occurred to me that I need not have done so. We know[1] the result of crossing a yellow wrinkled with a green round pea, and of their mating *inter se*: so that all that is necessary is to start with a hat containing equal numbers of yellow and green counters representing pistil parents, and a hat with similar contents

[1] Hurst, :04.

representing pollen parents, and to mate the contents at random, the result of each of the three possible unions, $g \times g$, $g \times y$, $y \times y$, being known by previous experiment. And the result of the matings of the various kinds of offspring can be predicted from the knowledge which we have of their gametic constitutions. Thus, for example, in F_2, a yellow resulting from the union yellow $\times$ yellow[1] will produce only yellow when mated with green; but a second yellow (indistinguishable by outwardly observable features from the first) produced by the union yellow $\times$ green will produce half yellows and half greens when mated with green; while a green of what ancestry soever will always produce green when mated with green.

Ex hypothesi Mendeliano it is possible to predict the result of these unions for however many generations the experiment is continued, because in its simple form that Law states that the result, for example, of $DR \times DR$ will always be the same whether the mating of hybrids takes place in F_4 or F_{40}. The Mendelian hypothesis in this simple form may or may not be right; and I, for one, think that it is not. But this does not damage my argument. My point is that you *can* determine the properties of the hybrids in different generations—supposing that they are not the same in all; and having acquired this knowledge you can then return to the counters and see whether the result of mating your material at random can be described by the Law of Ancestral Inheritance. What I want to make clear is that the knowledge of heredity acquired by the Mendelian is deeper, is nearer the phenomena themselves, than is that acquired by the biometrician, and is such that the latter, if he is inclined, can use it as material with which to test the Law of Ancestral Inheritance without the labour of conducting a breeding experiment.

Having devised my method, therefore, I discovered that it was unnecessary to use it. So I abandoned it, in the case

[1] These colours refer to the gametes.

both of the mice and of the peas. I am now investigating the properties of the various kinds of individuals in various generations in both cases, accumulating information (of a physiological nature) which will be available to the biometrician for use in testing the Law of Ancestral Inheritance.

(*e*) Why do White Sheep eat more than Black Ones?

I was asked the other day this well-known riddle, and as I had forgotten the answer I was told it: "Because there are more of them."

The supplying of the answer never provokes a laugh, yet the relation between it and the question is full of interest. Let us discuss it. When you ask the riddle you do not say that you are not referring to individual white and black sheep, but the man of whom the riddle is asked *invariably* thinks that you are. In attempting to answer it, the ideas that rush through his mind may either take the form of seeking for some pun on the words, or perhaps for some humorous quotation in which they appear; and so forth: or, as usually happens, he thinks that, as a matter of fact, a white individual *does* eat more than a black, and (if he is a biologist) he may be trying to think of some physiological explanation of the fact, in connection possibly with the well-established relation between pigmentation and the getting rid of waste products.

In the answer he is told that the amount eaten by the sum-total of white sheep as compared with that eaten by the sum-total of black sheep is the subject under discussion; and not any peculiarities of ingestion, digestion, or egestion associated with whiteness as compared with blackness.

If the antithesis between truths about masses and truths about individuals which constitutes the point in this riddle were more widely and more clearly perceived than it is to-day, there would no longer be that confusion in the minds of most biologists which prevents them seeing the profound difference that exists between a physiological Law like Mendel's, which is true of units, and a statistical one like the Law of Ancestral Inheritance, which is true of masses. All intending students of heredity should be asked this riddle ; and if they cannot detect the fallacy in it they should be declared unfit for their intended task.

The similarity between the impression made on the mind by asking the question and Mendelism, and that between the idea conveyed by the answer and the Law of Ancestral Inheritance does not lie only in the fact that while the question and Mendelism deal with individuals, the answer and the Law of Ancestral Inheritance refer to masses. The idea implied in the question is like Mendelism, because it suggests what Mendelism effects, the discovery of a hitherto unsuspected order in familiar phenomena ; while the truth conveyed in the answer is like the biometric treatment of heredity, because it is the accurate statement of a relationship that you already know to exist. Everyone knows that the sum-total of children are more or less like the sum-total of their parents ; the biometrician accurately measures the degree of this resemblance.

The answer you expect is physiological. The answer you get is statistical.

APPENDIX A to p. 190

It is interesting to inquire what Galton himself said, when he formulated his Law, on the subject of its applicability to individual cases. He said ('97, p. 403) : "It should be noted that nothing in this statistical law contradicts the generally accepted view that the chief, if not the sole, line of descent runs from germ to germ and not from person to person. The person may be accepted on the whole as a fair representative of the germ, and, being so, the statistical laws which apply to the persons would apply to the germs also, though with less precision in individual cases. Now this law is strictly consonant with the observed binary subdivisions of the germ-cells, and the concomitant extrusion and loss of one-half of the several contributions from each of the two parents to the germ-cell of the offspring." Mark his words, ". . . though with *less* precision in individual cases"—the italics are mine. If one were referring to Galton's Law (in the form in which it is true of masses only) one would say, ". . . without applying at all to individual cases"; and if to the Law of Contribution, ". . . with *absolute* precision to individual cases." But I may be interpreting this wrongly, for the "less" may refer not to the difference between population and individual, but to the difference between person and germ. And, in fact, I think the following quotation from the previous page ('97, p. 402) justifies us in concluding that Galton conceived his Law as being true solely of masses without being true of the component individuals. "The neglect of individual prepotencies is justified in a law that avowedly relates to average results . . ." At any rate, it simplifies matters very much to consider that Galton's Law as he formulated it is true of masses only, and not of their component units; for if we do not, we have to keep *three* laws distinct in our minds:

1. Galton's Law as he formulated it: true of masses, but also, though with less precision, of individuals. Statistical and Physiological.
2. Galton's Law: true of masses only. Statistical.
3. The Law of Contribution: true of units. Physiological.

APPENDIX B to p. 171

There is nothing, of course, in the word "contribute" to definitely signify that the thing which is contributed is the same as that which contributes: in fact, in the everyday usage of the term this is hardly ever the case. But it is reasonable to hold that Galton's Law is the generalisation that like contributes like and not unlike; and it is certain that Galton himself meant this, as the last words of his illustration of particulate inheritance readily show: ". . . each piece of the new structure is derived from a corresponding piece of some older one, as a lintel derived from a lintel, a column from a column, a piece of wall from a piece of wall." ('89, p. 8.)

APPENDIX C to 4 (b) pp. 184-7

There is an apparent paradox, in the ideas just expressed, about which I think it is necessary to say a few words, in case the reader should detect it himself and think that it had not occurred to me.

I have said that biometry deals with masses and Mendelism with units; but I have also said that the biometrician exceeds his proper limits when he goes beyond the boundary of a unit, while the Mendelian is concerned with the mutual properties of numerous units; in other words, the sphere of the biometrician is within the unit, while that of the Mendelian is outside it.

The fundamental idea on which the Law of Ancestral Inheritance is based is that set forth in the quotation from Pearson[1]; it is that a knowledge of the characters of the parents does not enable us to predict the character of the offspring in individual cases.

The fundamental idea in Mendelian theory is that the ascertainable gametic characters of the parents *do* enable us to predict the character of the offspring in individual cases.

How are these two diametrically opposite ideas about heredity to be reconciled? The answer which most naturally suggests itself is that the biometrician happens to have dealt with cases about which it was impossible to predict in individual cases; while the Mendelian happens to have dealt with cases in which prediction was possible. This answer presupposes the existence of two sets of phenomena in heredity, those about which it is possible to predict, and those about which it is not. Now let us grant for the moment that the Mendelian theory (which I think by no means proven yet) that the characters of an organism consist of a number of separate character-units, is true. What relation, if any, do the two sets of hereditary phenomena—the predicable and the non-predicable—bear to these units? Just this. The non-predicable phenomenon is the incomplete correlation between the degree in which any character x is exhibited by a parent, in a single case, and the degree in which that same character is exhibited in its child. The predicable phenomenon is the result of the union of x with x, or of x with y.

[1] *Vide supra*, p. 169

Chemistry furnishes a parallel. The chemist cannot predict the rate at which any given atom in a litre of oxygen is travelling; he can only deal with "statistics of average conduct"; but he can predict the result of passing an electric spark in a vessel containing oxygen and hydrogen. Yet he who deals with the properties of the elements may be said to deal with units, and he who deals with the component atoms—and one can only deal with them in large numbers—may be said to deal with masses.

It is true that the biometrician possesses the only means of measuring the "intensity of heredity" in a non-predicable case, but it seems to me that to extend the application of these means to predicable cases is fallacious. If the true function of the biometrician is to give us statistics of average conduct where we *cannot* predict individual conduct, it seems to me that to deal by the biometric method with cases where we *can* is not only unprofitable, but likely to lead men to think that where there are two methods dealing with the same material, of which the one can predict while the other cannot, the latter is fallacious. Whereas if the biometrician confined himself to the non-predicable and the Mendelian to the predicable, the general conclusion would be that each had his proper sphere—which indeed, in my belief, he has. I do not set forth these views in any spirit of dogmatic certainty; and nothing could please me less than that they should go unchallenged by anyone who believes me to be mistaken.

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[1] *Vide supra,* p. 139

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VI

Recent Advances in Animal Breeding and their Bearing on our Knowledge of Heredity

(*Reprinted from the Report of the Royal Horticultural Society's Third International Conference on Genetics*, 1906, *by permission of the President and Council*)

Curious results are obtained by crossing albino with the so-called Japanese waltzing mice. It is perhaps not necessary to say that an albino mouse is one with an absolutely white coat and with pink eyes, the pink colour in them being due, not to a special pigment, but to the colour of the blood in the vessels at the back of the eye.

The colour of the waltzing mice used in this experiment is best described by saying that were it not for a patch of fawn on the shoulders, and sometimes on the rump, they would be albinos. Their curious movements, inaccurately denoted by the term "waltzing," are not likely to be forgotten by those who have seen them. The animals appear to have no control of the movements of their heads, nor of the direction in which they themselves proceed; and when they are awake, they spend most of their time in twirling round and round, apparently mad, in a very small circle.

When these two are mated the result is a mouse hardly distinguishable from our common house mouse (when the albino parent is pure bred).

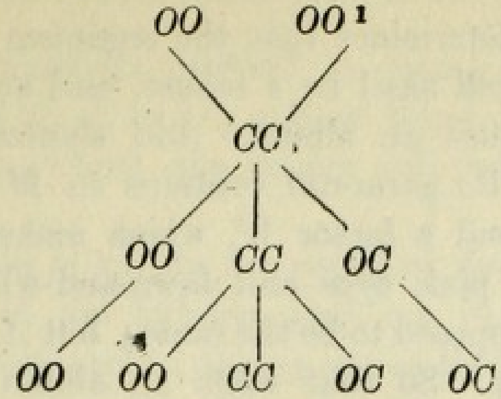
The hybrid, therefore, has a grey-brown coat and coal-black eyes.

We start with a pink-eyed mouse with a colourless coat (which we may denote for brevity's sake by the formula *OO*), the albino, and mate it with a mouse which is also pink-eyed but has a partially-coloured coat (which we may call *OC*), and get as a result a black-eyed mouse with a fully-coloured coat (which we may call *CC*). So much for the nature of the hybrids as far as colour is concerned. Now for their progression. *The hybrids never waltz.* This is true of the hundreds that I have raised.

Let us now consider the result of mating these hybrids together. First with regard to *colour.* The offspring produced by the union of these hybrids fall into the three categories *OO*, *OC*, and *CC*, in the proportions 25, 25, and 50 per cent. respectively. That is to say, *in point of colour*, on the average one mouse in every four is like its albino grandparent; one in every four like its waltzing grandparent; and two in every four like their parents the hybrids. It should be mentioned that all mice falling into the category *OC*, for example, are not exactly like the Japanese waltzer in colour. For example, the fawn colour may extend over the whole body; or, again, a new colour, lilac, may arise, associated with pink eyes, in this generation. So long as a mouse has pink eyes and some colour in its coat it is reckoned as belonging to the category *OC*. But the number of colours that can co-exist in a mouse with pink eyes is limited; for example, neither a dark grey nor a black mouse ever has pink eyes.

Now let us look at the posterity of the three kinds of mice denoted by the formulæ *OO*, *OC*, and *CC*. *OO*'s, i.e. albinos, when mated together produce only albinos; *OC*'s also when paired breed true with very rare exceptions; while the offspring of *CC* × *CC* fall as before into the three categories *OO*, *OC*, and *CC* in proportions which I have not yet determined.

The inheritance of colour in this case is shown at a glance in the following Table:



Now let us consider the generation, produced by mating the hybrids, from the point of view of its progression. Less than a quarter of them waltz. But the deficiency is probably due to the fact that waltzers are constitutionally weak creatures and are more likely to die before they reach the age at which their characters can be recorded than other mice are. What is of interest is that the waltzing habit is not necessarily associated with that colour character *OC* with which it is associated in the pure strain, but is distributed at random over the three categories *OO*, *OC*, and *CC*.

We have so far confined ourselves to the description of phenomena. Now let us consider two theories which have been put forward to account for them; first, one by Von Guaita associated with the name of Weismann, and secondly, one by Bateson associated with the name of Mendel.

The theory suggested by Von Guaita was intended to account for the results of a hybridisation experiment (similar to mine except for the fact that his waltzers had black eyes) carried out by himself.

He suggested that there were two kinds of factors in a germ-cell that gives rise to an albino; one, which we may

[1] It may be objected that I have introduced an element of confusion by representing one homozygote by two similar letters, the other by two different ones, and the heterozygote by two similar ones. But this objection is successfully met by saying that my formulæ can only lead to confusion among those students who have not been taught that such formulæ are nothing more than conceptual descriptions of features of the germ-cells which we have not yet perceived. And such beginners will be confused anyhow.

call *M*, which determines that the organism which develops from the germ-cell shall be a mouse, and another *A*, which makes that mouse an albino. And similarly in the case of the waltzer, its germ-cell contains an *M* similar to that of the albino, and a factor *W*, which makes it what it is, a waltzer with pink eyes and fawn-and-white coat. Now *M* and *M* are supposed to be the same; but *A* and *W* different and antoganistic. So that when an albino and a waltzer are mated it is a question which of the two factors which are antagonistic, *W* or *A*, will be manifested in the offspring. What happens, according to Von Guaita's theory, is that the two factors *W* and *A* struggle, and neutralise each other so that neither of them is manifested in the offspring, leaving the two *M*'s, which are similar and compatible, in sole possession of the field. This theory accounts in a very ingenious way for the character of the hybrid; and doubtless some elaboration of it could be suggested which would account for the reproduction of the three categories *OO*, *OC*, and *CC* in the next generation.

The theory put forward by Bateson two years ago to account for these phenomena is that there is in the germ-cell of an albino a factor determining albinism, which he calls g, and similarly that there is a factor g' in the waltzer, determining its colour characters, which we have already called *OC*.

The result of the union of g and g' is a hybrid $g' g$, whose character we have already denoted by *CC*. The result of the union of g' and g—the production of a form more different from either of them than they are from each other—is said to be a specific result in the sense that the production of water is said to be the specific result of the union of H_2 and O. But the most striking part of this theory is that which refers to the germ-cells of the hybrid. According to it, 50 % of the germ-cells of the hybrid bear the factor g' and 50 % g. Now the result of the union of two hybrids each containing 50 % g' + 50 % g germ-cells is the production of offspring falling into the following categories in the proportions indicated by the numbers prefixed to them—25 $g'g'$ (or g') 25 $g'g$, 25 gg', and 25 gg, or ($g'g$ and gg' being the same) 25 g', 50 $g'g$, and 25 g—which, the reader will remember, is the actual result.

This proportion is simply the result of the random union of the gametes of the hybrids, and can be illustrated by making pairs of counters by taking one of the pair at random from a hat containing red (R) and white (W) counters in equal numbers, and the other of them from another hat with similar contents. The result of a large number of trials will be in percentage 25 RR, 25 RW, 25 WR, and 25 WW, or 25 RR, 50 RW, 25 WW.

The difference between the above-outlined Mendelian and Weismannian theories is that while the former tries to account for the segregation and not for the reversion, the latter tries to account for the reversion and not for the segregation. It is when we fix our attention on that part of the Mendelian theory which refers to the nature of the gametes of the hybrid that we see what the doctrine of gametic purity really means, how profoundly new and definite a thing that theory is, and how widely it differs from any other theory of heredity whatsoever.

Let us imagine that we have one of our hybrids, with its rich brown coat and black bead-like eyes, before us; a mouse that we might easily mistake for a wild one caught in a trap, if we did not know its parentage. According to the particular Mendelian theory we have been discussing, *none of the gametes of this mouse contain an element representing the character of the animal which bears it*—namely, $g'g$. But half of the gametes bear the element, g' and half g. The fact that 50 % of the children of such hybrids are like their parents is not due to the presence in the germ-cells of their parents of any elements representing their own characters, but to the chance union of g' and g, borne by different parents. A hybrid in this generation—F_2—is, therefore, never produced by the union of similar, but always by the union of dissimilar, gametes; and if an F_1 hybrid could multiply parthenogenetically, none of its offspring would be like itself. This theory as to the nature of the character-representing elements borne by the hybrids is so remarkable that one requires very strong evidence for it to believe it. The only evidence so far adduced is that the proportions in which the various kinds of young occur are those demanded by the theory; but this does not prove the theory to be true.

The question we have to ask ourselves in considering the value of the evidence for an hypothesis is not "How many cases are there which are consonant with its truth?" but "Is there a single case which is not?" The list of cases in which the proportion 1 : 2 : 1 obtains in the F_2 generation is lengthening every day, and it is imagined that the value of the evidence for this particular theory becomes greater as the list becomes longer. The simple truth that I have stated in the form of two questions above is often forgotten.

We are too apt to think that it is sufficient to rest content with the many that are with us; and too ready to forget that we ought to be up and seeking out one that may be against us.

Are there any facts which render the above-outlined Mendelian theory untenable? I have at my disposal two, to only one of which will I refer now. I may say, by way of preface, that I do not wish my remarks to be construed as antagonistic to Mendelian theory as a whole, but merely critical of a particular hypothesis bearing that name, which was put forward two years ago—an hypothesis in which Mr. Bateson ceased to believe before I did.

At a time when I still thought that it was a useful subject for investigation to try to find out which of the two theories, Galton's or Mendel's, fitted the result of my experiment best, I obtained a result that was apparently conclusive in favour of the former.

The result, which I have described before, but which I may briefly recapitulate here, was obtained by tabulating the difference between the results of mating two kinds of hybrids differing not in any visible character but in their pedigree. The two kinds of hybrids that were used were (i.) a hybrid produced by the union of two hybrids, and (ii.) a hybrid produced by crossing a hybrid with an albino; we may call the former HH and the latter HA. Three kinds of matings can be made with this material—namely, $HH \times HH$, $HH \times HA$, and $HA \times HA$. In each of these types of union a hybrid is mated with a hybrid. So that I argued that according to the Mendelian theory a quarter of the population produced in each of the three cases should be albino; but that according to the commonly accepted view of heredity, known as the law of contribution, one would expect the proportion of albinos to be greater in proportion as the number of albinos in the pedigree of the hybrid parents was greater. This was found to be the case. But it was pointed out to me that this result was not evidence against Mendel's theory, unless I had established the hybrid nature of every individual used in the experiment. "Have you done this?" I was asked. "Are there any cases of families in unions of type (i.) where no albinos have been produced?" "Yes," was my reply "Then one or both the parents of these families were really dominants," was the answer to me.

I did not think that this theory was true, because it seemed to me that to say that unless a quarter of the family produced by an animal consisted of recessives it was not a hybrid, was a very easy way of establishing the fact that hybrids always produce recessives in the proportion of one in every four. It seemed that an argument of that kind was not likely to be based on anything having any existence in nature, but it is a strong warning not to be led away by appearances that when I tested this theory I found it to be true. The animals falling into the category CC in F_2 are sharply distinguished into two kinds: (i.) hybrids that will produce albinos in the proportion of one in every four; and (ii.) dominants which, when mated *inter se*, produce no albinos at all, and when mated with albinos are dominant over them. I have proved these two kinds to exist. But the existence of the second of them is fatal to the suggested Mendelian theory outlined above. For how could individuals, whose germ-cells appear to bear a new unit $g'g$, arise from a hybrid in which the germ-cells either carried an element representing g' or g, and never both? The suggestion that neighbouring ova, or spermatozoa, fuse is not likely to meet with general approval, yet it is the only one which will account for their appearance if our Mendelian theory is true. We have to choose between the two improbabilities: (i.) that neighbouring germ-cells fuse, and (ii.) that none of the germ-cells of the hybrids bear elements representing animals like them; and two probabilities: (i.) that neighbouring germ-cells do not fuse, and (ii.) that some of the germ-cells of the hybrid contain elements representing animals like them. I choose the probabilities. But, in claiming to have demonstrated the falsity of the Mendelian theory described above, I do not wish to be credited with having "discovered an exception to Mendel's Law" On the contrary, the best measure of the progress which Mendelian inquiry has made in these two years is the fact that while at the beginning of them the existence of hybrids that breed true would have been regarded as a difficulty, to-day a reasonable explanation of their occurrence has been given.

Progress in knowledge is made by the suggestion of hypotheses, and their rejection when found to be false; by this means the Mendelian has been able to account for some very complicated cases of segregation, and for reversion in some cases as well.

It is natural to inquire how much experiments of this kind tell us about heredity. We are told that Mendel's Law only applies to a very limited class of facts, that there is only a certain very limited and definite set of characters to which it applies, or that it only deals with the phenomena of hybridisation.

Let us consider these objections one by one. With regard to the first I would say, what I have said before, that the service which Mendelian theory has done to progress in the study of heredity lies partly in the facts which it has accounted for, and partly in the method which it has introduced; and that even if Mendel's Law has a limited application, his method has a great future.

The second objection is merely a detailed expression of the first; it states in what the limitation lies. Mendel's Law is said to apply to a very few characters, of which colour stands out pre-eminently among the rest. And although it is true that the list of characters whose inheritance can be described in terms of Mendel's Law comprises many other characters than colour, e.g. the shape of the comb in fowls, the waltzing habit in mice, and even, lately, resistance to disease in plants, it is nevertheless true that the number of characters to which Mendel's Law can be said to apply is very small indeed when compared with the number of characters which go to make up an organism. And it can be said with some truth that the characters with which the hybridiser can deal are in a sense superficial. When we cross an albino and a waltzing mouse the result is to our eyes remarkably different from either parent; it is like a wild mouse, but it is a mouse. The feature in which it differs from its parents are its colour, its progression—it never waltzes like one of its parents—and to a certain extent its vigour and temperament, for it is healthier and wilder than either parent: but it is still a mouse. The charge is brought against the hybridiser that he can only stir up the surface, but that he cannot disturb the depths. My answer to this objection is that it is entirely well founded; that there certainly are two sets of characters, one which can be affected by hybridisation, and another, a much larger one, which cannot, and that it is legitimate to regard the former as upper and the latter as lower. By saying this I do not mean to subscribe to the view that recently arisen characters have less tendency to be transmitted than old ones. The case of snails will illustrate my meaning. In *Helix nemoralis* the unbanded condition is dominant over the banded. Now, it is probable that some form of banding is more ancient than colourlessness, and still more probable that some form of colouring, at any rate, is more ancient than colourlessness, yet absence of colour is dominant over colour. But my point is that in crossing these snails the only thing affected is colour; this is almost true when *H. nemoralis* is crossed with another species, *H. hortensis:* it is not quite true, because the cast of the spire of the shell is also altered, but the main thing which is affected is the colour. The animal was a snail, a *Helix*, before it had any definite colour; and, even after it had become stamped as *Helix*, probably underwent many alterations in coloration. I hold that it cannot be denied that the characters with which one deals are in this sense superficial. But I do not think that this need be regarded as a damaging admission by the hybridiser. On the contrary, I hold that the recognition of a limit between the two sets of characters—alterable and unalterable—is desirable, and that the discovery of the difference between the kinds of characters which it separates would be intensely interesting.

The objection that the Mendelian only deals with hybridisation phenomena—doubtless very interesting and important phenomena, it is often perhaps semi-ironically granted—must be met. Those who urge it complain: "It is all very well to tell us about hybridisation—about the result of the union of unlike; we want to know about the union of like. Hybridisation seldom occurs in nature, and when it does the results are more perplexing than in the case of crossing domesticated breeds. What we want to know is, 'What is the mechanism by which the similarity between parent and son is brought about?' The existence of a form *Triton Blasii*, which is a cross between *T. marmoratus* and *T. cristatus*, is undoubtedly interesting, but it is an anomaly. I want to know how it is that the offspring of the crested newt is like its parent. And this you can't tell me. I want to know about normal heredity; you give me nothing but information about abnormal. I ask for bread, and you give me what is to me a stone; interesting and curious, but still a stone."

I should answer objections of this kind by asking, "But why abnormal?" Why should we regard the disintegration of biological units as more "abnormal" than that of chemical ones? It is only by experiment with "abnormal" phenomena that the chemist has progressed. If he had stuck as rigidly to the observation of "normal" water as those who bring this objection against the hybridiser would have him do, he would know as little about the chemistry of water as the biologist did about heredity before he began to experiment with it.

But this answer, though it sounds plausible enough at first hearing, can only be thoroughly satisfactory to those who urge this objection if we can show them that the appearance of abnormality is merely due to the fact that we are dealing with *normal* units in an "abnormal" condition (the result of disturbance by cross-breeding), and if we can show them that we really are not dealing with an abnormal hereditary phenomenon.

Now what are we to understand by abnormal? The most definite formulation of what is meant by abnormality in heredity is that of Dr. Archdall Reid. According to him, alternative inheritance has been evolved as a means of keeping the sexes separate, or, to put it in a teleological way, of ensuring that an individual shall be either a male or a female. When the alternative mode of inheritance first became differentiated it was only sex which was inherited in this way. But just as sex, so to speak, sometimes makes a mistake, and trespasses on forms of heredity which do not belong to it, and *blends* in inheritance, with the result that a hermaphrodite is produced, so sometimes not-sexual characters, albinism for example, trespass on the mode of inheritance reserved for sex and are inherited alternatively.

Mendelians, says Dr. Reid, have lately suggested that the inheritance of sexual characters may be Mendelian. We shall be much nearer the truth, he thinks, if we say that the inheritance of Mendelian characters is sexual.

There is undoubtedly a parallel between the manner in which Mendelian and that in which sexual characters are inherited. The Mendelian view is that Mendel's work has provided us with conceptions which will enable us to account for the mass of hereditary phenomena; the latest extension of the method being an attempt to account for the phenomena of the inheritance even of sex by it. Dr. Reid's view is that Mendelian phenomena are merely anomalies which are the result of the accidental association of certain varietal characters with a mode of inheritance primarily evolved to ensure bisexuality. This view may or may not be right; but it deserves careful consideration because one of the most deep-rooted weaknesses of the mind is the tendency to regard that with which we have been acquainted for the longest time as the starting-point from which we must proceed to other things. For example, the most hopeless confusion characterised the attempts that were made to homologise the body cavities in leeches so long as zoologists persisted in regarding the cœlom of that member of the *Hirudinea* with which they had been longest acquainted—the medicinal leech—as their starting-point, and in interpreting the state of affairs in other leeches in terms of this one. And the question remained in darkness until a few years ago, when Asajiro Oka showed that the cœlom of the medicinal leech, far from being the starting-point, formed the very last term of a series of gradual modifications of the body cavity; and, in fact, that the anatomy of this leech could not be understood without a knowledge of the series of which it was the culmination.

References to recent literature on this subject will be found in a paper by me entitled, "On the Difference between Physiological and Statistical Laws of Heredity" (*vide supra*, p. 167).

VII

Some Tables for Illustrating Statistical Correlation

(*Manchester Memoirs, Vol.* 51, 1907, *No.* 16)

It was not my original intention to preface the description of my new Tables with an account of Weldon's experiment.[1] But I was persuaded that without such an account the meaning of my Tables would not be evident to many. It must be understood, therefore, that except in the matter of presentment the first part of this paper makes no claim to originality.

The second part contains an account of an interesting extension of the experiment described in the first.

I

Let us begin at the beginning, so far as we can. In the case of a very great number of vital phenomena we are unable to predict exactly what the result of certain events will be. We know that they will fall within certain limits, but where within those limits we cannot tell. We believe that a duck will not produce a duckling with a beak as narrow as a snipe's, but the exact breadth of the beak—measured, let us say, in terms of its length—in a given instance, we cannot foretell.

If these words ever happen to lie before the eyes of a

[1] :06. Weldon, W. F. R., "Inheritance in Animals and Plants," pp. 81–109, in "Lectures on the Method of Science." Edited by T. B. Strong. Clarendon Press, Oxford, 1906.

biologist, the chances are that he will be inclined to ask me, " What *does* it matter what the length-by-breadth index of a duck's bill is ? " My answer to this interruption is that so long as we are as much in the dark as we are at present about the circumstances which may affect an animal's or plant's chances of attaining maturity, any statement that such and such a feature matters or does not matter is unwarrantable.

But let us return to our argument. Some living things are variable. We may adopt two attitudes towards this variability. We may either say that it does not matter and ignore it, or we may suspect that it may matter and measure it. In my opinion, evidence does not justify us in adopting the former attitude. Statisticians have provided us with a method for measuring this variability. But we usually want to know more than this; we want, if possible, to measure the closeness of the relation between two such variable things. Statisticians have again provided us with a method which enables us to measure the closeness of that relation in which biologists are most interested, namely, that between parents and children.

The first step in this method is to construct a Correlation Table. How this is done is best explained by giving an account of Weldon's beautiful experiment. The variable phenomenon he dealt with was the number of dice, in a throw of 12, which fell so that faces with 4-or-more pips on them were uppermost. When we throw a single die it is an even chance whether it falls so that a face with 3-or-fewer pips on it lies uppermost, or whether a 4-or-more-bearing face lies uppermost. Therefore the most probable number of dice with faces bearing 4-or-more uppermost in a throw of 12 is 6, but the number may be anything between 0 and 12 inclusive, though these extreme results occur very seldom. Here is a list showing the frequency with which the 13 possibilities occurred in a thousand throws which I have made:

Result of Throw.	*Frequency.*
0	0
1	3
2	15
3	55
4	110
5	208
6	223
7	179
8	129
9	64
10	11
11	2
12	1

Imagine that I am before you and that I have 12 dice in a dice-box. I shake it and throw them. The result happens to be 7 dice with 4-or-more-bearing faces uppermost. I pick up all the dice, put them back into the dice-box, shake it and throw them again; the result happens to be 5 such dice. What I want you to observe is that in this pair of throws the two throws which compose it are absolutely independent of one another; the result of the second is not affected by the result of the first; a knowledge of the result of the first does not help us to predict the result of the second.

Let us think of some way of making the two throws in such a pair dependent, of making the result of the second affected by the result of the first, and of bringing it about that a knowledge of the result of the first shall help us to predict the result of the second.

I suggest to you that a good way of doing this is to leave half the dice, which formed the first throw, lying on the table, and allow them to form half of the second throw. If I do this, the second throw will consist of six dice lying exactly as they did in the first throw and of six dice thrown afresh. Six of the twelve results which determine the total result of each throw will be common to the two throws of a pair.

But you will say, "How will you know which six dice to leave on the table? You will not be able to help leaving the ones showing 4-or-more down and picking up the others, except by making it a rule not to do so. And that would introduce too much complication. It seems to me that it will be very difficult to make the decision as to which dice shall be picked up and which not a matter of chance and not of choice." This objection is quite reasonable, but the difficulty is not insurmountable. All that is necessary is to make six of the dice different from the other six. This is easily effected by leaving them for a few hours in red ink. It does not matter whether we make it our rule to leave the red or the white dice down on the table when we gather up the six dice to make the second throw. Let us decide on the red.

We can now start to make a pair of connected throws, in which the decision as to which dice pass over undisturbed from the first to the second throw is a matter of chance and not of choice. I put all the dice—the 6 red and the 6 white—into the dice box, shake it about, and throw the dice on to the table. The result happens to be 6.[1] Now I gather up the white dice, put them into the dice box, and throw them. In describing the results of the second throw I count the red as well as the white, although only the latter have been thrown a second time. So that half of the results which determine the total result of the first throw are exactly the same as half of those which determine the total result of the second. The two throws are connected together. For instance, let us consider the maximum possible difference between the two connected throws and compare it with the maximum possible difference between two independent

[1] The number describing the result of a throw means the number of dice exhibiting faces with four-or-more pips on them uppermost in that throw.

throws such as we started by making. The maximum possible difference between two independent throws is twelve. A 12 may follow a 0. Or a 0 may follow a 12.

But in the case of two connected throws the maximum possible difference is 6. It may happen by all the red dice showing 4-or-more and all the white ones 3-or-less in the first throw, and by all the white ones showing 4-or-more when thrown again (that is, by a 12 following a 6); or by all the dice showing 3-or-less in the first throw and the white ones all showing 4-or-more when thrown again (that is, by a 6 following a 0). The number of ways in which the maximum difference between the two throws may be attained is given by the number of pairs of figures that follow. The first figure in each pair indicates the result of the first throw in that pair; the second that of the second: 0–6, 1–7, 2–8, 3–9, 4–10, 5–11, 6–12, 7–1, 8–2, 9–3, 10–4, 11–5, and 12–6. The essential point is that 6 is the maximum possible difference.

But you see how seldom it is likely to occur. It depends on *all* the white showing the opposite kind of face uppermost in the second throw to those which they exhibited in the first. The fact that it does not occur often, however, does not concern us now. What concerns us at present is that the maximum possible difference between the result of a pair of throws connected in the way we described above is 6, whilst that between two unconnected throws is 12. The two results in the "connected" pairs are, as it were, chained together. We may compare the two results in unconnected pairs to a couple of dogs, not chained together, in a show stall at a dog show. Let us imagine the stall to be 12 feet long and each foot to be marked on the base of its frontage so that, as you stand looking at it, the left boundary of the stall is over the 0 and the right over the 12. The two dogs could, if they wanted, lie as far away from each other as the size of the stall permitted, namely one over the 0 and the other over the 12. We may compare the two results in "connected" pairs to two dogs in such a stall leashed

I.II.	I.II.	I.II.	I.II.	I.II.	I.II.	I.II.	I.II.
5.3	9.9	9.8	3.2	3.7	7.7	5.8	7.8
6.7	4.6	2.4	8.5	8.7	5.4	3.4	7.7
7.5	7.9	5.3	9.7	6.6	4.2	6.7	5.5
6.7	6.8	4.6	7.7	7.5	6.8	6.7	7.7
4.8	5.7	5.6	5.4	9.5	5.4	6.10	9.7
4.7	4.6	6.4	7.6	4.8	6.7	8.7	8.7
5.6	4.2	3.6	6.6	6.5	6.7	7.7	5.4
7.4	6.7	9.8	8.9	7.5	5.4	5.6	6.9
7.7	6.7	7.8	6.7	2.5	8.4	7.8	5.6
7.9	7.7	5.5	6.5	5.7	3.5	9.5	7.7
5.7	9.10	5.4	9.5	4.4	6.4	7.6	4.5
6.8	6.3	9.9	5.3	5.8	6.10	8.8	6.7
8.7	5.4	8.4	5.4	7.6	3.2	4.6	7.7
3.3	8.8	8.8	3.3	5.7	9.8	3.4	7.3
7.6	5.5	5.9	6.7	5.5	7.10	6.5	7.9
6.5	8.10	5.5	3.5	6.6	8.6	5.6	5.5
3.5	8.6	6.4	6.8	6.3	5.5	4.8	8.8
6.7	3.7	3.4	4.6	6.4	7.4	4.6	4.3
5.7	5.7	6.4	5.6	7.7	4.5	5.5	6.6
6.5	6.9	8.5	3.4	6.6	4.7	8.6	5.7
4.3	8.6	7.6	7.6	4.3	6.5	5.7	4.6
8.8	5.7	8.7	5.6	9.7	6.6	6.4	7.5
5.7	5.7	5.8	5.4	6.7	6.6	4.4	9.10
6.8	6.6	7.5	5.6	6.4	4.5	7.5	9.8
9.6	8.8	5.6	8.8	6.9	8.8	9.8	5.7
7.6	6.6	7.6	6.5	8.6	4.3	6.6	5.6
8.6	6.7	5.6	6.6	6.6	7.7	6.7	6.7
4.2	4.6	7.6	8.7	6.2	8.6	4.5	6.8
3.5	8.7	5.5	8.6	7.7	4.5	5.7	7.7
6.6	7.6	8.6	6.8	2.5	7.5	5.5	4.2
5.8	1.4	8.6	5.6	6.5	9.6	5.7	2.4
4.9	8.7	8.8	9.7	5.5	7.6	6.5	5.7
7.7	5.6	6.6	10.7	4.5	5.4	5.6	6.4

I.II.	I.II.	I.II.	I.II.	I.II.	I.II.	I.II.	I.II.
5.5	4.5	4.3	7.6	7.7	7.7	4.8	8.7
8.7	4.5	5.5	4.2	7.8	5.7	5.5	10.7
9.8	9.8	7.7	5.7	6.8	4.7	9.6	4.6
4.3	7.6	6.6	8.8	5.7	8.9	6.5	7.7
8.8	5.8	5.7	7.7	5.5	7.7	8.8	6.7
10.9	4.8	6.7	8.7	5.4	5.5	4.7	
5.5	7.5	4.6	9.9	4.6	2.2	6.5	
5.5	8.7	6.7	8.7	6.3	6.7	5.6	
8.6	5.5	6.7	9.8	4.7	7.5	10.9	
6.5	5.8	5.4	6.4	7.9	6.5	5.5	
6.5	8.7	6.5	3.3	4.4	6.4	11.8	
6.5	7.6	6.5	4.5	4.6	8.9	4.8	
4.4	5.6	7.8	8.10	9.8	7.5	8.7	
7.7	5.8	7.6	5.5	8.6	6.8	8.8	
8.9	8.7	4.6	3.7	6.8	6.7	6.5	
7.3	7.8	5.2	6.6	5.4	8.5	7.7	
6.3	5.7	9.7	9.6	7.8	7.5	4.5	
5.2	5.4	4.5	6.8	6.9	6.5	6.5	
6.8	8.5	6.8	8.7	5.8	6.6	9.9	
7.7	8.8	6.8	4.3	4.7	7.10	4.6	
7.5	7.7	5.8	6.6	8.10	1.2	6.4	
5.7	2.6	4.6	7.7	10.8	3.7	6.8	
6.6	5.7	7.7	5.4	6.10	4.7	6.5	
7.8	6.6	6.4	5.7	6.4	6.4	3.7	
6.6	6.5	3.5	6.6	7.6	4.4	5.7	
5.8	6.6	5.3	8.5	7.7	9.7	6.7	
8.7	5.7	3.4	4.4	9.9	1.3	8.7	
8.7	3.5	5.6	5.3	4.4	5.4	5.4	
5.6	7.8	5.8	8.7	7.8	5.5	7.8	
6.5	6.5	2.5	8.5	3.6	5.9	7.3	
8.9	5.5	7.5	8.6	4.4	6.3	7.8	
6.8	8.7	6.7	6.6	3.7	4.6	5.6	
6.5	9.6	8.5	6.7	4.3	2.6	5.5	

together by a 6-foot chain. If one of them wishes to sleep over the 0 the other has to lie in the middle of the stall over 6. If after a time the latter insists on moving to 12 the former must put up with 6. And similarly with intermediate positions. This parallel illustrates only the maximum possible difference between first and second throws in "connected" and "unconnected" pairs. To find a parallel for the most usual difference between first and second throws in connected pairs we should have to imagine the leash connecting the dogs to be made of a piece of elastic with a maximum stretch of 6 feet.

We must return to the dice. Let us make a number of pairs of such connected throws, and see what the result is. On pages 224–225 are given the results of 500 such pairs. The Roman numerals at the top of each column mean that the left-hand figures give the results of the first throws; the right-hand ones those of second throws.

The list does not show very much in this form. If you look through it you will find that a high number is as a rule followed by a fairly high one, and that a low one is usually followed by a fairly low one. But this is not presented at all vividly to the eye. What we want is some means of finding out, without the labour of counting through the whole series, the number of times a given result in a first throw is followed by a given result in a second. This want is supplied by the so-called Correlation Table. Here is one (*see* next page) on which are exhibited the results of the 500 pairs of throws detailed on pages 224–225:

The Table proper is bounded at the left and top by single lines, and at the right and bottom by double ones.

It is made up of 169 squares. A horizontal series of these squares is spoken of as a row; a vertical series of them as a column. So that we may say that the table consists at once of 13 horizontal rows, each of which is made up of 13 squares; and of 13 vertical columns, each of which is likewise made up of 13 squares.

The numbers at the top of (but outside) the columns stand for the various possible results of second throws, which, as we know, may be anything from 0 to 12. The numbers at the left-hand end of (but beyond) the rows have the same signification, except that they refer to first throws.

At the base of (but below) most of the columns are numbers which signify the number of times the event, indicated

Second Throws.

First Throws.	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1			1	1	1									3
2			1		2	3	2							8
3			2	3	5	6	2	6						24
4			5	9	8	11	16	7	6	1				63
5			2	5	17	24	19	25	11	2				105
6			1	5	14	25	24	24	17	4	3			117
7				2	2	13	16	27	12	4	2			78
8					2	7	13	22	14	5	3			66
9						3	5	6	9	5	2			30
10								2	1	2				5
11									1					1
12														
			12	25	51	92	97	119	71	23	10			500

by the figure at the top of the column, happened in the 500 second throws. For example, a 2 occurred 12 times; a 3, 25 times; a 4, 51 times; and so on. At the right-hand end of (but beyond) most of the rows are numbers which signify the number of times the event indicated by the figure at the left of the row happened in the 500 first throws. For example, a 1 occurred 3 times; a 2, 8 times; a 3, 24 times; and so on.

So far, we have only referred to the figures outside the Table, and I hope I have made clear to you what they mean. Now, we must turn our attention to the table itself. It will be found that the numbers at the bases of the columns are the sums of the numbers in the columns above them; and that the numbers at the right-hand ends of the rows are the sums of the numbers in the rows to the left of them. Each column intersects all the rows, and each row intersects all the columns of the Table. Every square is part both of a column and of a row.

What is the meaning of the numbers in the squares? The figure in any square gives the number of times the result indicated by the number at the left-hand end of the row of which it is a part happened in a first throw, and was followed by the result indicated by the number at the top of the column of which it is a part, in the second throw. For example, starting with the top row, we see that there was not a single case of a 0 thrown at all in these 500 pairs of throws. Coming to the second row, we see that there was one case of a 1 in a first throw followed by a 2 in the second; one of a 1 in a first followed by a 3 in the second; and one of a 1 followed by a 4. Examination of the third row tells us that there was one case of a 2 followed by a 2, two of a 2 followed by a 4, three of a 2 followed by a 5, and two of a 2 followed by a 6. And so on throughout the table.

The best way to familiarise yourself with the construction of such a table is to make one for yourself from the figures on pages 224–225. You draw a correlation table like the one we have been examining, but quite blank; and write the numbers 0 to 12 along the tops of the columns, and at the left-hand ends of the rows just as in that Table. The plan is to indicate the result of a pair of throws by putting a dot in one of the squares of the table. But which square? We shall see in a moment. The first pair of throws on the list is a 5 followed by a 3. The figure 5, denoting the result of the first throw, tells us in what row the dot must be. The figure 3, denoting the result of the second throw, tells us in what column the dot must be. The square, therefore, formed by the intersection of this row by this column is that in which the dot must be placed. The next pair of throws is a 6 followed by a 7. We find the position of the square in which the dot representing this result is to be placed in the same way. We continue this process until all the pairs on the list are entered; then we add up the dots, and write the totals thus obtained, in each square; add up the figures in each square composing a column, and write the total at its base; and add up the figures in each square composing a row, and write the total at its end. The result is the Correlation Table on p. 227.

There is one feature of it which cannot fail to attract your attention immediately. It is that the figure-containing squares lie diagonally across the Table. It is not very difficult to see what this means. It is the expression of a fact we already know, namely, that low numbers are associated in a pair with low numbers, high ones with high ones, and intermediate ones with intermediate ones.

We are now approaching the outskirts of a vast subject. The task I set myself was to show you the way to it; but not the way into it. Having given you an account of Weldon's device for illustrating correlation, I will go no further, but will leave you in the hands of the statistician, who, I may perhaps tell you, will provide you with a means of working out from such a Table a number called the *correlation coefficient*, which is a measure of the degree of connection between the two things you are dealing with. In the case of the dice throws connected in the way we have just been considering, this number will be approximately ·5. In the case of Table 0 (p. 232) it will be approximately 0; in the case of Table XII. (p. 238), approximately 1. In fact, quite generally, if m dice are left down in the 12 the coefficient is $m/12$ths.

II

Weldon's experiment may be varied in the following way. Instead of staining 6 dice red and leaving the six red dice of the first throw on the table to form half of the second throw, we may stain some other number, say 9, and allow 9 dice to pass over from the first to the second throw. In fact, we may stain and leave over from the first to the second throw any number of dice from 0 to 12 inclusive. Table 0 shows the result of 500 pairs of throws, in which, to make the second throw in each pair, all the dice were gathered up from the table and thrown again. In this case there is no correlation between the two throws. Table I. shows the result of 500 pairs of throws, in which to make the second throw, all the dice except one were gathered up from the table and thrown again. In this case there is very slight correlation between the two throws. To make Table II., 2 dice were left down. To make Table III., 3 were left down. And so on.[1] To make Table XII., it did not matter whether the dice were stained red or not, for the second throw was merely the first throw counted over again. And the Table consequently shows any given number in the first throw always followed by the same number in the second.

Each of the thirteen Tables which are seen on the Plate was made by substituting for the Arabic numerals in each square of Tables, 0 to XII., a corresponding number of dots, and then in erasing all the lines inside the four boundary lines of the Table.

The attempt to make the phenomenon of correlation clear to an audience, previously unfamiliar with it, is in my belief less likely to be successful if it is only possible to show one Table such as VI., instead of a series of Tables exhibiting at a glance the gradual increase in correlation as shown by the transition from a circular blur to a diagonal line, as seen in the Plate. The reason for this is the same

[1] I am indebted to Mr. Charles Biddolph for making all the throws, except those which compose Tables 0, VI., and XII.

as that which would make it very difficult for anyone to explain that the angle which the two arms of a "governor" on an engine make to one another, becomes obtuse in proportion as the speed of rotation becomes great, if he lived in a world in which "governors" always travelled at a constant rate such as would keep the two arms at a constant angle of 90 degrees to each other. Table VI. might convey nothing to the mind of anyone regarding it even after he had read the first part of this paper. But a cinematograph, the successive pictures composing the film of which were the successive dot-Tables on the Plate, would show movement resulting from known causes. Cause, "no dice left down" has effect "circular blur." Cause, "12 dice left down" has effect "diagonal line." Cause, "6 dice left down" has effect intermediate between the last two effects.

Imagine that you have a small model of a "governor." If you do not touch it the arms hang down. If you spin the axis as fast as you can, the arms lie in the same straight line. Spin it at a moderate rate; the arms make an angle of 90 degrees to each other.

Directly we can play with machinery we can see how it works. Movement and change enable us to perceive and to understand.

The two squares intersected by diagonal lines in Table I. are squares in which from the conditions of the experiment a throw cannot fall. In the rest of the Tables, the same is true of all the intersected squares and of all the squares to that side of the intersected ones remote from the diagonal of the Table.

TABLE 0.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1						1			1					2
2				1		4				1				6
3				1	4	7	8	5	4	1	1			31
4			4	4	7	9	6	12	5	5				52
5			3	5	13	26	14	14	12	6	1	1		95
6			1	6	15	25	24	28	15	6	2	1		123
7			1	5	7	16	22	15	13	6	1		1	87
8				1	7	15	19	12	6	6				66
9		1		1	2	9	7	6	6		1			33
10					2		1	2						5
11														
12														
		1	9	24	57	112	101	94	62	31	6	2	1	500

TABLE I.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1														
2						2	3	2		1				8
3		1		1	4	4	5	8	4	2				29
4		1		2	3	10	11	6	8	5	2			48
5		1		9	11	13	15	22	11	6				88
6			2	5	8	40	25	32	8	7	1	1		29
7				7	8	13	14	14	14	9	3			82
8			1	2	7	9	12	10	13	2	2	1		59
9					5	10	8	12	7	5				47
10						1	1	3	1	2				8
11					1				1					2
12														
		3	3	26	47	102	94	109	67	39	8	2		500

TABLE II.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1						1								1
2				2		1								3
3			2	2	4	7	7	3						25
4				5	7	7	19	10	4	5				57
5		1	4	3	10	20	26	21	9	3				97
6			1	3	6	30	26	18	11	6	2	3		106
7			1	2	12	13	15	17	30	3	3			96
8		1	1	4	8	7	10	16	10	6		1		64
9				2	6	2	9	8	6	2				35
10						2	1	3	4	2				12
11							1	1		1				3
12						1								1
		2	9	23	53	91	114	97	74	28	5	4		500

TABLE III.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1					1		1							2
2							6	1						7
3		1	1	5	2	2	4	5						20
4			1	8	6	21	16	6	6					64
5			4	3	12	15	23	22	9	3	1			92
6		1		10	16	17	23	28	22	5	1			123
7			1	4	9	17	18	24	16	5	3			97
8				1	5	6	10	14	8	7	2	1		54
9					4	3	9	6	6	2				30
10						1	1	1	4	3				10
11								1						1
12														
		2	7	31	55	82	111	108	71	25	7	1		500

TABLE IV.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1					1	1								2
2			1	1	1	2	1	1						7
3			2	4	3	4	5	2						20
4			1	2	8	18	8	12	6	1	1			57
5			3	8	16	16	19	21	7	5	2			97
6		1	1	5	19	25	25	20	12	2				110
7			2	1	6	22	17	32	17	12	3			112
8					5	6	16	18	14	7	2			68
9						3	2	6	5	2				18
10							3		2	2				7
11								1	1					2
12														
		1	10	21	59	97	96	113	64	31	8			500

TABLE V

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1					1	1								2
2			3	1	5	1	1							11
3			3	3	8	4	4	4						26
4			3	6	9	21	14	10	5	1				69
5				4	11	23	21	15	9					83
6			1	3	9	18	27	29	16	3	2	1		109
7			1	2	5	14	24	28	10	7	4			95
8				1	5	9	10	18	14	4	2			63
9						2	9	13	4	3				31
10					1		2		2	3	1	1		10
11								1						1
12														
			11	20	54	93	112	118	60	21	9	2		500

TABLE VI.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1			1	1	1									3
2			1		2	3	2							8
3			2	3	5	6	2	6						24
4			5	9	8	11	16	7	6	1				63
5			2	5	17	24	19	25	11	2				105
6			1	5	14	25	24	24	17	4	3			117
7				2	2	13	16	27	12	4	2			78
8					2	7	13	22	14	5	3			66
9						3	5	6	9	5	2			30
10								2	1	2				5
11									1					1
12														
			12	25	51	92	97	119	71	23	10			500

TABLE VII.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1				1										1
2			1	2	4	2								9
3			4	2	6	5	4	3						24
4			1	7	10	19	13	8	1					59
5			1	5	16	14	24	14	2	1				77
6				3	13	17	28	22	9	4	1			97
7				1	3	15	26	40	18	8	2			113
8						8	14	16	16	12	3			69
9						2	3	10	10	9	4			38
10							4	2	3	2		1		12
11							1							1
12														
			7	21	52	82	117	115	59	36	10	1		500

Table VIII.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1														
2			4	2	3	2								11
3			2	8	9	7	4							30
4			3	1	12	20	9	5	2					52
5				4	21	30	26	13	4					98
6				4	12	30	36	19	10	7	1			119
7				1	1	15	29	27	13	7	1			94
8						1	8	19	13	10	1			52
9						1	1	4	13	5	5	3	1	33
10								1	3	3	3			10
11									1					1
12														
			9	20	58	106	113	88	59	32	11	3	1	500

Table IX.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1			1	1										2
2			2	5	1	1								9
3				5	7	3	1							16
4			1	8	18	19	5	1						52
5				6	17	30	32	13	1					99
6				1	10	18	34	26	10	1				100
7					4	17	26	30	18	7				102
8							7	28	16	11	5			67
9							3	6	15	9	7	1		41
10								1		4	3	2		10
11											1		1	2
12														
			4	26	57	88	108	105	60	32	16	3	1	500

TABLE X.

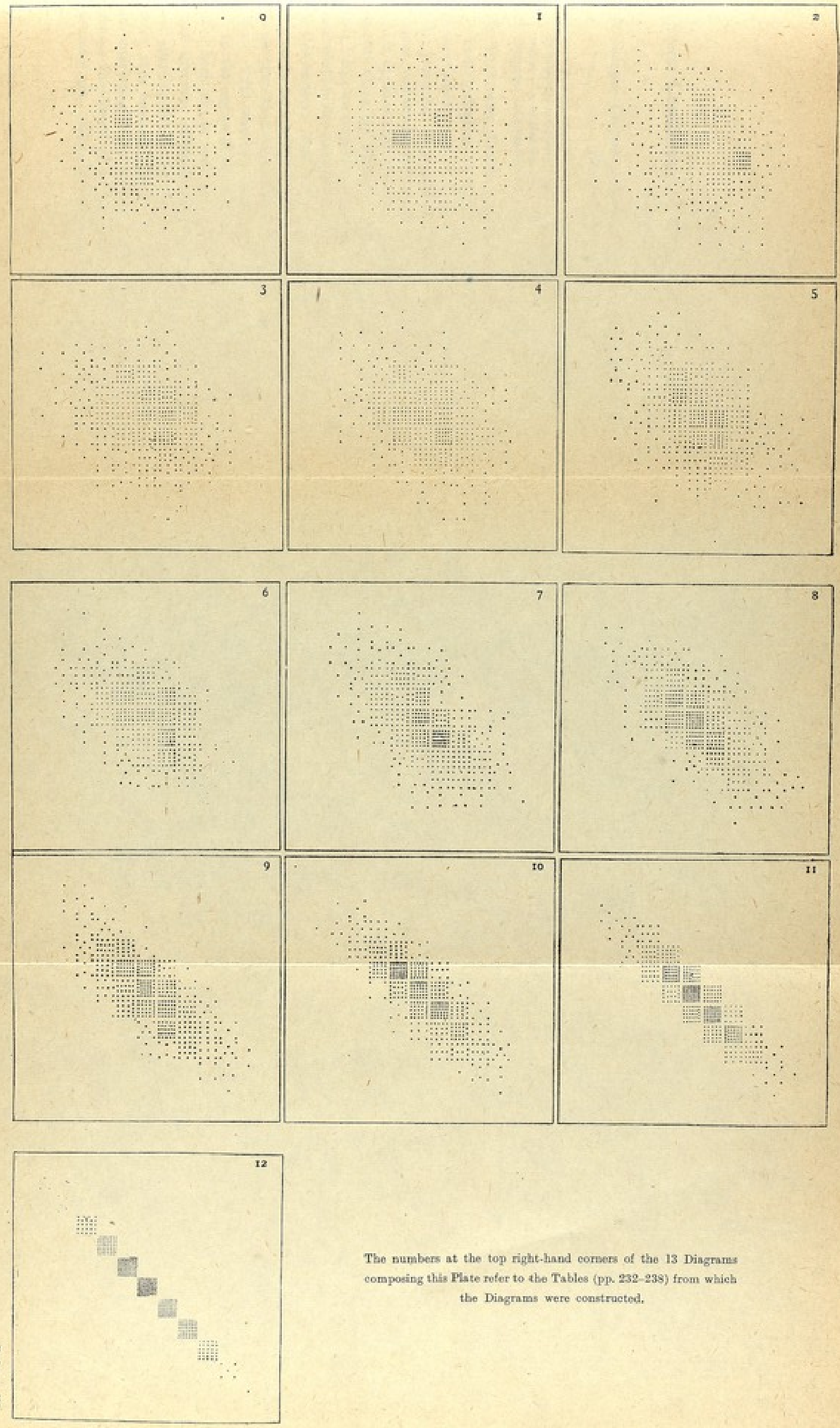
	0	1	2	3	4	5	6	7	8	9	10	11	12	
0	1													1
1		1												1
2			2	5										7
3		1	3	8	9	3								24
4			2	10	18	19	6							55
5				1	24	43	32	10						110
6					4	22	37	24	6					93
7						6	27	39	19	5				96
8							9	17	24	9	1			60
9								10	14	11	7			42
10									1	6	2	1		10
11											1			1
12														
	1	2	7	24	55	93	111	100	64	31	11	1		500

TABLE XI.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1														
2			5	3										8
3			2	7	10									19
4				7	21	23								51
5					19	44	33							96
6						22	56	30						108
7							31	49	16					96
8								25	38	13				76
9									15	18	5			38
10										1	5	1		7
11											1			1
12														
			7	17	50	89	120	104	69	32	11	1		500

TABLE XII.

	0	1	2	3	4	5	6	7	8	9	10	11	12	
0														
1		3												3
2			8											8
3				24										24
4					63									63
5						104								104
6							117							117
7								81						81
8									64					64
9										30				30
10											5			5
11												1		1
12														
		3	8	24	63	104	117	81	64	30	5	1		500



The numbers at the top right-hand corners of the 13 Diagrams composing this Plate refer to the Tables (pp. 232–238) from which the Diagrams were constructed.

Facing p. 238

VIII

Some Conditions of Progress in Biological Inquiry

(*Manchester University Biological Society. Opening Address for the Session* 1906–7)

> " *You who speculate on the nature of things, I praise you not for knowing the processes which Nature ordinarily effects of herself, but rejoice if so be that you know the issue of such things as your mind conceives.*"—Leonardo da Vinci.

Mr. President, Ladies and Gentlemen,

Your invitation to me to read a paper to you at the opening meeting of this session of your society gave me great pleasure. If you had thought me an efficient demonstrator, you would have made me a present of a silver clock; but it appears that you were clever enough to estimate at its real value my ability in that capacity. If you thought that I had on occasion something interesting to say to you, you would invite me to give the opening paper of the session to this society. And let me tell you that the verdict you pronounce on me by having done so is far more gratifying to me than that expressible by the presentation of a silver clock. " He who can, does; he who cannot, teaches."

In the course of settling on a subject for my remarks I have been struck with the fact that, notwithstanding the great diversity of topics of addresses of this kind, and notwithstanding the variety of the reasons alleged for choosing them, they have one feature in common—namely, that they are interesting to the lecturer. This common feature determining the choice of a topic is obviously in many ways a desirable one. But a human mind is such that it often fails to perceive that what is interesting to itself is not necessarily interesting to other minds. And when you come to consider the kind of things in which a man becomes deeply interested, you will realise that the very fact of their being interesting to him is the reason of their being uninteresting to others. For the only things which can beget a real interest in a man are new things, especially things for whose bringing to light he is responsible.

The fact that we are responsible for the bringing to light of these new things—our discoveries, our intellectual offspring —accounts for our own absorbing interest in them and for the belief that others take a similar interest; and makes us try to go further into them and find out everything about them in the most detailed manner possible. The more we do this the more does each new little thing we find out swell in importance in our eyes, until at last we enter a state in which we can see nothing but this new thing; and even if we do not get so far as to believe that nothing else exists, we at any rate very soon come to think that it is more important than anything else. The modern scientific belief that the world is much as we see it, and is not peopled by hosts of spirits, is not without its disadvantages. For if we begin by saying that the world is what we see, we may end by coming to believe that what we see is the world. And if we are not careful we may ultimately lose the power of detaching ourselves from the little world we have created around the thing which we have discovered, and of viewing it with the same degree of magnification as that with which others look at it. So it often comes about that a man chooses as a topic some minute point with which he has himself been engaged, and that he is apt to conclude from the fact that he is absorbed in it that it will be interesting to his hearers.

I shall therefore try to escape from the possibility of tiring you in this way, by not talking about any experiments which I have been making. And I propose to invite your attention to a topic which deeply concerns every student of nature.

An Introduction to a Biology

The aim of the biologist is to discover all about life; and a biologist is successful in proportion as he contributes to our understanding of vital phenomena. For example, we know something, though very little, about the nature of fertilisation; but we know next to nothing about the nature of heredity, and absolutely nothing about the causes which determine the sex of animals. We feel fairly sure, now, that animals and plants have descended from pre-existing ones, and that the diversity of forms which exists at the present day is the result of this descent going on hand in hand with a gradual modification. But we do not know how this modification has taken place. We are accustomed to express our belief in the fact of evolution; but no one pretends to know in detail how it has been brought about.

These, then, are some of the unsolved problems in biology. And a biologist is successful if he proves one of these mysteries less of a mystery. Now inasmuch as none of you here will, I suppose, question the desirableness of improving natural knowledge, you will, I conclude, allow that it is profitable to discuss the question, What is it that makes a successful biologist? in order that we may quicken our progress in biological discovery. Some of you here will form part (as indeed some of you already do) of the body of investigators whose business is the study of life; and it is meet that we should consider what are the things which retard progress in this branch of knowledge, and what are the things which help it. It is my object this evening to suggest lines of thought which may help you to form a definite opinion on this matter.

The process by which the biologist performs his task consists of two stages. The first is the description of phenomena, the second is the formulation of theories to account for them Description is a record of what we perceive; interpretation what we achieve by reasoning. And, broadly speaking, I think it is reasonable to hold that the difference between man and the other animals is that whilst the other animals do not get much further than perception, man goes on to reason about the things he perceives. But you see, of course, that he must perceive the things first, before he can go on to reason about them.

Now you know that man in his attempts to account for things has repeatedly gone astray. Even those people who are stupid enough to think that the ways in which we account for things to-day are right and true and everlasting are not blind to the fact that the theories by which their predecessors tried to account for things were wrong, false and transient. But we are not concerned with that point now. The point is, we may take it as established that in the second part of the process, in trying to account for things, man has gone hopelessly astray. Are we sure that in the sphere of description there is no chink into which error may creep ? Let us make quite sure of this before we see to keeping our machinery of reasoning in order. For unless we are perfectly sure that we see things aright, it is manifestly absurd to try to account for them. We cannot interpret nature until we know what nature is. Our first business is to describe ; when we have done that properly we can go on to explain. We ask ourselves : Has error characterised our description of things to the same extent as it has our attempts to account for them ? And the answer I suggest is : Yes, but in a less degree.

The question naturally suggests itself : How can we go astray if we merely describe things ? Surely the same thing cannot be two different things at the same time. And the answer I should make to this is that, if one can be sure about anything, one can be sure that a thing cannot be two different things at the same time. But we hardly ever do merely describe things. Practically all description of natural phenomena is made with a view to its bearing on some general problem, and the describer is expected to give some pronouncement as to the direction in which his work points, whether it be for or against one or other of current rival theories, or for a theory he himself has put forward. Now, although I have just described the method of investigation of nature as consisting *first* of description and then of interpretation, it is only in quite recent years that the necessity for this chronological sequence has been recognised. The merest glance at the history of human thought suffices to show that the farther we go back in time the more does the proper order of description and interpretation tend to be reversed. We need go no farther back than the Middle Ages for an example of a time when a complete interpretation of the universe was not only attempted but believed in so firmly by men that they killed those who denied it. Yet not the smallest fraction of that universe had been trustworthily described.

To-day we profess greater modesty as to the completeness of our interpretation. Yet those of us whose knowledge of nature is culled from textbooks written by people who talk about natural processes being governed by immutable laws, have got no further than handing over the government of the universe from a hierarchy of spirits and demons to one of principles and laws. Even those men of science who see that this notion is nonsense make the most unwarrantable assumptions. We are told that living nature is orderly and not chaotic; that the more we find out about it the more simple will it become and the less complicated; that if a fundamental principle is discovered in the case of animals, it will be true also of plants; and so forth. These things may be so. But it is our business not to assume that they are, but to find out if they are. And we are far from knowing that yet.

As it with our general view of nature, so it is with our investigation of particular problems. At the threshold of a piece of research, Interpretation does not politely wait at the door and say, "Allow me; Description first," but both rush in together without ceremony, hand in hand; with the result that the work of Description is always influenced by the suggestions of Interpretation, and always will be; for they are inseparable companions. The former is tedious without the latter; the latter is not countenanced without the former.

We have seen that description is influenced by interpretation. We have seen that interpretation is not always right. We therefore cannot escape the conclusion that description is sometimes wrong. And even if this argument did not prevent our escape from this conclusion, we could not escape from it unless we shut our eyes to the hosts of instances of it that are afforded by a study of the history of the interpretation of nature. The pages of that history are strewn with the carcasses of descriptions of things that are not. Instances of such will crowd up into the mind of the reader. For who is not familiar with numbers of cases in which a man with a definite interpretation of a phenomenon in his mind has expected to find such and such a thing, and *has* found it, in spite of the fact, incontrovertibly established by general and impartial testimony afterwards, that nothing of the kind existed all the time?

The upholders of the doctrine of *evolution*, as opposed to that of *epigenesis*, asserted that a miniature edition of the adult bird could be seen in the hen's egg. Hartsoeker actually made a drawing of a spermatozoon in which a little man is seen crouching with his knees tucked up to his body and with his hands holding his knees in place.[1]

No sooner had the wonderful cytological phenomenon which bears the name of the quadrille of centres been described in the animal kingdom than it was promptly discovered in the vegetable one. We know now that it exists in neither.

The leech *Pontobdella* is said to have a lateral blood-vessel lying inside a lateral sinus, a part of the cœlom. There is, as a matter of fact, only one vessel, and this is homologous

[1] *See* Delage. "L'Hérédité et les grandes problèmes de la Biologie générale," p. 380.

with the lateral sinus of *Clepsine* and the so-called lateral blood-vessel of *Hirudo :* in fact, all three of them are lateral sinuses (i.e. parts of the cœlom) in gradually advancing stages of taking over the duty of the vascular system, a process which reaches its completion in *Hirudo*, in which the vascular system proper has absolutely disappeared. But if you start from two fixed points : first the point (fixed by observation) that the lateral sinus in *Clepsine* is a part of the cœlom ; and secondly, the point (fixed by tradition) that the lateral blood-vessel in *Hirudo* is morphologically a blood-vessel ; and if you try to interpret the state of affairs in an animal, *Pontobdella*, which is intermediate between the two animals bearing these two fixed points, and if you strive to show how the transition from the one to the other may have been effected, the most natural thing that suggests itself is that they both exist in *Pontobdella*, one inside the other. They do not. But that did not prevent their being described so. The reader can see an actual drawing of the one tube inside the other by turning to page 518 of Sedgwick's "Student's Text-book of Zoology."

Further instances of this type of visual disease will probably recur to the reader's mind if he has devoted himself to the investigation of nature.

Before we turn to the consideration of the question of the most desirable relation between description and interpretation, let us pay some attention to interpretation itself.

When we say that we have interpreted a phenomenon, we think we have succeeded in seeing deeper into it ; that we have succeeded in penetrating below the face of the clock and have made out the works. Now it is, in my opinion, very important to consider what right we have to say that we see deeper into a phenomenon when we interpret it. I hold that we do not.

I can see the face and hands of a large clock—such as the one outside in the Quadrangle—from a very considerable distance. But in order to be able to understand the works, I have to go upstairs behind the clock and examine and touch the various wheels, and minutely trace out how they work together. That is to say, I could describe the face of the clock and the changes in position of the hands, the difference between the speed of the two, and so forth, from a distance of 50 yards. But in order to be able to account for these phenomena, I must come as close as possible. So that we may say that, in this case, all that we need, to be able to interpret the phenomenon, is opportunity of coming closer to it (provided we are not in so low a state of intellectual development that we interpret it as the result of the agency of spirits). We may put it another way: we may say that interpretation involves the coming to closer quarters with a phenomenon; it means a decrease in the distance between the eye of the observer and the thing observed. And all this is perfectly true so long as the constituent parts of the mechanism under investigation are of such bulk that they can be perceived and handled individually by man. Now the constituent parts of a clock are, for the simple reason that the mechanism is the work of man. But the constituent parts of vital mechanism are not; yet we continually act and speak as if they were.

We are nowadays always sighing with relief at our happy deliverance from the old anthropomorphic interpretation of the universe. But are we emancipated? Not a bit. We are still slaves. What right have we to imagine that the mechanism of the things around us should be of such a size that it bears that relation to us which will enable us to understand it if we only look close enough? Why should it not bear that relation to bacteria, or to some other beings as much smaller than bacteria as bacteria are smaller than ourselves? What are we that we should expect this? Nothing but the last term of a series of changes in one direction of mammalian development. We are so incapable of estimating our own place in the universe that we have not yet got out of the habit of referring to all the other animals as the lower animals; nor of marvelling at nature for having evolved such wonderful creatures as ourselves. "What a piece of work is man!" we cry. Yes. What indeed! What sort of a piece of work should we think a clock that stopped eight hours out of the twenty-four? We get some idea of how far we fall short of any satisfactory standard of activity and endurance from the fact that when a mutation that can dispense with sleep (Napoleon) does arise, he nearly conquers the world single-handed.

It is certain that we think we have a right to understand things in nature. How many naturalists, when they have arranged their oil immersion, peer down the microscope and think that they alone are privileged to view the ultimate structure of protoplasm?

The question whether life on this planet was created or not does not matter. The point to insist on is that it was not created by us. Yet we are continually acting as if it had been. We believe that when we interpret a thing we are seeing closer into it; on the analogy of the clock. We forget that we have not created the thing in nature. We look closer into it and at first can see no works; gradually we think out what the works must be like; we retain the image that we invent of them in our minds, and then come away and tell everyone we have discovered the works. Nägeli and Mendel both looked at the phenomenon of hybridisation. Nägeli saw only the face of the clock, and it meant nothing to him. Mendel thought he could discover what the works were like by the various things that happened on the face of the clock, and it is true that his theory of the works corresponds with what happens on the face. Yet he never saw any part of the works. Have they any objective existence, then, outside Mendel's brain? Probably something faintly like them has. The point I want to bring out is this. The interpretation by man of a work of man (e.g. a clock) may be said to consist in the shortening of the distance between the eye of the observer and the thing observed. Is it so in the interpretation of a thing which is not the work of man —a thing in nature? No; emphatically no.

Let us regard the distance *EP* between the eye and the phenomenon, when the latter is just so far away that it can merely be perceived and nothing more, as 10 units of linear measurement, and the distance *EI* between the eye and that part of the brain which imagines (wherever it may be) 2 units. The interpretation of the clock consists in decreasing the length of the line *EP* by dividing it by 1,000, say. But what about the interpretation of natural phenomenon? Does it consist in the decrease of the length of the line *EP*? No. It consists in increasing it by the length of the line *EI*. So that whilst we think that the more we interpret a phenomenon the more we are getting at close quarters with it, as a matter of fact the inverse relation is what really obtains. If we admit that interpretation consists in going beyond the limits of our vision, we have to admit that what we do on the other side of that limit is not seeing, but imagining. And really it is tacitly conceded that this is so. For when a particularly ingenious theory which, we think, enables us to come to close quarters with the inner mechanism of a phenomenon is put forward, our praise is not for the marvellousness of the mechanism discovered, but for the ingenuity of the brain that conceived it. We praise Mendel, not the mechanism of segregation; how could we? We have never seen it. We say, "What intellect!" and not "What works!" Moreover, it is easily proved that this is so, for if interpretation really meant a making out of the works, there should be greater unanimity in the sphere of interpretation than in that of description; because the closer we can look, the more accurately can we see.

I hold, therefore, that the view that interpretation signifies a coming to closer quarters with a thing is baseless.

Another very fertile source of error in interpretation is one which probably results from the nature of the mechanism of thought. For whether we consider it to be a liquid running in tubes, or the result of the vibration of molecules, it is certainly true that the more often a certain thought has been formulated the less difficult is it to forget it. The oftener we read a book the better do we know it. Yet this property of the brain has dire consequences. A thought merely by being formulated a great number of times becomes fixed. I am thinking not so much of thoughts that occupy a prominent place in our mind, or thoughts in which we are particularly interested, but of thoughts which occupy a back seat, which are used as a starting-point for trains of thought.

Sir Harry Johnston had heard about the animal which we know now as the okapi from the natives, who called it a donkey (doubtless because of its large ears), and this, coupled with the fact that a strip of skin from its rump, which the native soldiers used as bandoliers, was beautifully striped, led him to conclude that the animal was a new species of zebra. How long Sir Harry Johnston went on thinking it must be a zebra I do not know. But when he came to explore the forest for this animal, he so little questioned the idea that it could be anything but a zebra, that he abstained from following any artiodactyle spoor he came across because, he thought, it belonged not to the beast he was after, but to some great forest eland. In the light of what we now know of the okapi it is probable that, if the idea that the animal was a zebra had been only just so little less firmly implanted in his mind as to allow him to try one of the spoors of the cloven hoof, he would have been rewarded by a sight of this wonderful animal.

It is probably this feature of the mind which makes us so incapable of recognising that a thing which we have come to regard as a starting-point from which we may proceed to the discovery of new things may not really be the starting-point, after all.

In heredity the theory of unit-characters owes its wide acceptance to-day to the glare of light which has been directed on to Mendelian hereditary phenomena. Some of those who have been engaged in investigating this phenomenon have suggested that the difficult problem of the nature and determination of sex may be elucidated by applying Mendelian conceptions to it. But Dr. Reid says: "You have got hold of the wrong end of the stick. The Mendelian phenomenon is not fundamental at all. It is merely an anomalous hereditary process. The inheritance of sex is the real starting-point. Mendelian phenomena are merely due to the acquisition by non-sexual characters of the mode of inheritance—the alternative—which was primarily evolved to keep the sexes separate. Mendelian phenomena do not form a starting-point, but are the last terms of a series." [1] I do not express an opinion for or against this view. I am merely concerned now in pointing out that the Mendelian has great difficulty in adopting it. To him it is simply unthinkable nonsense. His feelings are like those of a man who, standing on the top of a pyramid which he himself has constructed, is told by a cheeky one down below that the pyramid is the wrong way up, and that he must come off it and put it the other way up. The reason that the man on the pyramid absolutely refuses to do anything of the kind is that he has spent years in building it. The reason that the man down below is able to see that the pyramid is really upside down (supposing that it is) is that he has played no part in its erection. Indeed, in the instance we are discussing, he only had his attention drawn to its existence a year before he saw that it was upside down.

There is seldom any difficulty in convincing people of the existence of a particular form of mental disease; but it is never easy to convince them that they themselves are suffering from it. We are ready enough to point out the frailty of the human intelligence, but are (perhaps unconsciously) apt to exclude our own from the general condemnation. I

[1] A fuller discussion of this point will be found under my name in the proceedings of the International Conference on Plant-breeding, 1906. *Vide supra*, pp. 217-218.

remember a drawing in one of the German humorous papers of a young nincompoop lolling back in an arm-chair and remarking to his elderly host: "Es ist merkwürdig, Jeheimrath, wie viele dumme Leute es in der Welt giebt"; and receiving the answer, "Tja, aber es ist immer einer mehr als man denkt."

So, in case the reader should think that I imagine that I am not as liable to this disease as I think others are, I will now record in considerable detail a case which revealed to me the appalling susceptibility of my own mind to this fearful complaint.

In the spring of this year I had occasion to go to Ischia, an island near Naples, for the purpose of convalescence. While I was there I collected, in the deep mountain gullies worn out by the hot streams in the volcanic tufa of which the island is made, six toads of the species *Bufo viridis*. The first stage of my journey back to England was to Naples, whither I went with the toads. Whilst I was there I gave some of them to a friend of mine who was working in the laboratory there; but when, after I had seen the last of him, I tried to remember how many toads I had given him, I could not do so, though I was sure it was either one or two.

From Naples I started on a North German Lloyd ship for England. The toads were in two tin boxes, which in their turn were in a wicker trunk I had bought in Naples. On the second day out I thought it was time to feed the toads, so I provided them with pieces of ox tongue cut up small. At Gibraltar I was joined in my cabin by an elderly Scottish doctor, who was travelling for rest after a nervous breakdown; I occupied the upper berth, he the lower. The noises heard during the night on a ship bounding in the Bay of Biscay are many and varied. But notwithstanding the number and variety of the sounds, I was almost sure I could detect the croak of the toads, which is rather shrill. I was naturally a little uneasy lest my cabin companion should hear the noise and inquire about its origin, but I comforted myself with the thought that the next night I could remove the cause of my uneasiness by putting the trunk containing the toads out in the passage. For I must mention that out of a general regard for the state of my companion's nerves, and on account of a definite statement by him at dinner that he could not stand toads at any price, I had not informed him that I had any with me, and still less that there was a party of them under his bed.

So the next evening, before dinner, I removed the trunk to the passage. After lying awake for about an hour, I was gradually dozing off when, to my horror, I heard the shrill croak of the night before. What was the meaning of this? The sickening thought suddenly struck me that one of them had escaped on the occasion on which I had fed them, and was now at large in the cabin; but another possible interpretation was that I had only put the trunk just outside in the passage and that between the cabin and the passage there was a window, so that really the toads were not so much farther off after all, and the croak I heard was from one of the captives out in the passage.

To find out whether this comforting theory was true, I opened the tin boxes in which the toads were, in the hope that I might be able to find out in that way whether one had escaped or not. But I was disappointed. There were four toads left; but as I could not remember whether I had left one or two with my friend in Naples, the fact that four remained was no help. Had there been five it would have been all right, for I knew that I had left at least one in Naples. Having failed to discover in this way whether one of the toads had or had not escaped, I set myself to find out whether there was actually a toad at large in the room. This I did by removing the trunk containing the tin boxes from the position it occupied just outside the cabin to the end of the passage, which was so far away that a toad's croak could not reach my cabin from it. It can be imagined that that night, directly my cabin companion had put out the light, I listened as intently as I could for the sound I had heard the night before. I had listened for more than an hour, and had already begun to congratulate myself on the fact that a toad could not have escaped after all, when I distinctly heard the croaking noise. I was all attention; and needed to be, for the creaking of the timbers of the ship and the rattling of the rudder gear was also in my ears. I tried to put myself in the calm, unexcited condition of the scientific observer in order to be quite sure whether I really did hear the sound or whether it was only that I thought I did. And I had not been in this condition of attention for long when I heard the noise so clearly that there was no possibility of doubt that it originated in the cabin. Not only so, but I thought I could hear it under my companion's berth. So that I leant out of my berth to wait for the next sound. And sure enough, when it came there was no question that it came from the quarter of the room where I thought I heard it first. It can readily be imagined that I spent the night in continual fear that the noise would sooner or later awaken my companion; and in continual elaboration of lies to account for it if it did. However, I resolved to have a thorough search on the morrow, and whether or no I went to sleep finally that night, I do not know.

On the next day I thoroughly searched the room, turning out all the boxes from under my companion's berth and from under the sort of settee which occupied the other side of the cabin. I looked in all cupboards and possible places of retirement in which the toad might have been hidden. But all in vain. I could not find him. And I was just abandoning my search when I noticed a place where I had not looked. It was a step, just inside the cabin door; it was hollow and open at both ends, about two feet in length and about two inches in height and two in breadth. I could not get my hand into this. Nor could I force an entry with a stick so as to drive the fugitive out at the other end, because I could not put the stick into the open end except at such an angle that it would not enter more than a few inches, and it was so strongly built that I could not wrench one side away. I could not bring myself to ask my steward to get the ship's carpenter to unscrew it; partly, I suppose, because my knowledge of German, though by this time extensive, did not reach so far as to enable me to tell the cabin-steward what I wanted doing, and still less to enlightening him as to the reason of it; and partly because I could not bear the thought of the merriment that would be engendered by the possible non-discovery of the toad. However, I derived what little comfort I could from the fact that the next was my last night on the ship; and resolved to leave the said instructions for the ship's carpenter in order to save any elderly person who might occupy the cabin after me the agony of terror, or even the possibility of death, that would be quite likely to attend the discovery of a toad in their cabin; and to impart these instructions to my steward at the moment of leaving the ship and of tipping him, so as to escape by the former the bitterness of witnessing the merriment caused by the conceivable non-discovery of the toad, and to drown by the latter any objection he might raise.

That night it was a very long time before I heard any sound of the fugitive; in fact, so long that I was dozing off to sleep when I heard the first one. I was soon alertly attentive to discover whether I had only dreamt I heard it—which would not have been unlikely—or whether it had really begun again. I had not to wait long. The shrill cry soon rang out in the stillness of the night; the night was still because we had reached Southampton Water and were anchored there. I listened to discover if I could trace the croak to the hollow step just inside the door; but the next time I heard it, I was almost sure it was in its old position under my companion's berth. The next time it was so plain that I thought I could be certain that it was *in* his berth. This was a terrifying thought. I shuddered to imagine what might happen if the old man woke up and put his hand on the clammy thing. To make quite sure, I argued to myself in this way. If the toad really is in his bed it is reasonable to suppose that when the human occupant of it moves he will touch or disturb the toad and make him croak. So I leant over the edge of my berth and waited for my man to move. He presently rolled over and grunted in his sleep—an event that was immediately followed by a croak from the toad. And again ; the sound of the moving followed by the croak. This was past bearing. I lay back in helplessness and waited. The two events occurred together again. I was reduced to such a condition of terror that I imagined that the reason for which the toad (whose native haunt was a warm volcanic stream) had left his retreat under the step, which was very dusty and dry, was to obtain access to the only source of moisture, the only oasis in the cabin—my companion's mouth. In fact, it was only on the assumption that the toad's mouth was pressed to my companion's lips that the immediateness of the response by croaking to the movement on the part of my friend could be accounted for. I had only to hear the sequence of grunt and croak again to be convinced that this was the case. When it did happen that time I leapt out of my berth, turned on the light, and looked into my companion's berth. No toad was to be seen. I had no doubt that it had escaped under the clothes when the light was switched on. But I did not dare to lift up the clothes ever so little, or even touch them, for I had sufficient sense left to realise that if my friend woke while I was doing so he would have some difficulty in understanding my action if I refused, as I certainly should have done, to offer an explanation. He did, as a matter of fact, wake while I was looking at him, and remarked in a sleepy voice that I seemed rather restless. I admitted that there was truth in what he said, and clambered up into my berth. The movement followed by the croak happened once or twice again, but there was nothing to be done, and at last the morning came. On my departure from the ship my courage failed me, and I did not leave instructions with the ship's carpenter. But it may be imagined that I lost no time in writing to my friend in Naples, telling him of all this, and asking him whether I had left him one or two toads, for I knew that I had brought six from Ischia, and that his answer would settle the question whether or not I had left the cabin tenanted.

In about a week I heard from him. "Set your mind at rest: you left me two."

I can, of course, only guess what the noise was. But the fact that I did not hear it until my companion joined the ship, a circumstance which I attributed at the time to the absence of any occasion for the kind of anxiety that I felt after he had arrived; the fact that I heard it each night for the first time an hour or so after retiring to rest; the fact that I first definitely located it under my companion's berth and then in it, and, moreover, that it always followed a movement of restlessness on his part, point to the conclusions that it was effected either gutturally by him or by a grinding of his teeth.

But the two points I wish you to notice about this are these. First, that having started with the not unreasonable idea that a toad had escaped, I spent those long nights in weighing the value of every possible piece of evidence that could conceivably bear on it, and in making every possible experimental test of every interpretation of the phenomenon that occurred to me; and that the more I weighed evidence and devised experiment, the less did I question the reality of the very thing that I was trying to find out. I started by trying to find out whether a toad had escaped; I ended by endeavouring to discover what, since it *had* escaped, it was actually doing. And if you will look back over the details of the story, you will see how gradual the transition from one stage to another was.

And the second point is this. Supposing you start by postulating the existence of a phenomenon *A*, and in order to find the cause of it cast about for some other phenomenon with which it is associated. Suppose you find this other phenomenon *B;* if *A* is usually but not invariably associated with *B*, you will be fairly satisfied, and will conclude that *A* probably exists and that *B* is probably the cause of *A*. But if *A* is invariably associated with *B* your satisfaction will be complete; you will conclude that you have not only established the existence of *A* but determined the cause of it *B*. A little reflection about the toad, however, will show you that you are wrong. Complete association, instead of increasing your sureness, should diminish it. For it shows that there may be an alternative interpretation—namely, that there is no such thing as *A*, but that what you thought was *A* was really *B*. So you must make some other test; if possible a crucial one; and then, maybe, you will discover that there really was no such thing as *A*, but that it was *B* all the time—as in my case.

Biologists who aim at interpreting things may be classed under three headings. The classes are arranged in descending order of bulk and in ascending order of merit. Class I. contains those who do not bother about interpretation at all. They do it, but they never stop to wonder what they are doing. Class II. includes those who bother about it, but can get no further than accepting the view of one or other authority as to what they are actually doing. Class III.—the smallest and the best of all—contains those who have the desire and the intelligence to find out for themselves what they really are doing when they are interpreting nature.

You will please conclude, therefore, that what I want you to do is not to accept my interpretation of interpretation, but to think it out for yourselves. By such means alone will you be able to reduce to a minimum the errors which attend the process of interpretation.

We have discussed the sources of error in description and in interpretation. It now remains to turn our attention to the relation between those two processes.

At present things are in a very bad way. Hardly any trustworthy descriptions of things exist. Of course, if we do not know a thing at all we can find out a great deal by reading what has been written about it. But if we are engaged in some investigation, and we desire some definite and final information on a particular point, we usually end by finding it much more satisfactory to investigate the point for ourselves. We usually find that most of the available descriptions are of little value for our purpose. The reason for the valuelessness of most descriptions is that they fall between the devil of the interpreter, whose description is spoiled by his having made it under the intoxicating influence of his own interpretation, and the deep sea of systematists, who are not sufficiently interested in interpreting anything to make their records so thorough that they may be of use to those who are. We are stranded between the former, who describe not wisely but too much, and the latter, who describe not wisely but too little. So we have to determine the point for ourselves.

But I would not have you believe from what I have said that I think it is desirable to eschew interpretation altogether. It is of course true in biology that we are much less likely to go wrong if we confine ourselves to description. But if we do this we do not get any further; we stagnate. If you say, "I will go no further than description, on the ground that it is better to keep on the safe side," do not be misled by the meaning of the word "side"; I mean, do not think that the road leading to the goal of the biologist is like the Palatine Road, with a safe side and an unsafe one, and that you can choose on which side you will go. No. The division is a transverse one; let us imagine it to be opposite the College. Let us imagine your starting-place to be the Continental Restaurant, and the goal to be Northenden; the safe, descriptive side, the town side of the division; and the dangerous, explanatory one, the other. If you choose to keep on the safe side, and confine yourself to description, you cannot make many mistakes, but you cannot make much progress; you cannot go far wrong, but you cannot go far. If you want to get nearer the goal, you must dare to pass the barricade separating the safe from the dangerous. If you pass it you stand to lose all by spending your life in going astray, but you stand to get towards your goal.

If ultimately you do not pass it you will have the company of Mr. Exclusive Systematist, Mr. Pure Morphologist, Mr. Collector, and Mr. Nomenclature Specialist—men who have never gone astray, all conscious of their blameless past.

But if, tempted by the greater prizes, and careless of the greater dangers on the other side of the barrier, you pass it, a very different spectacle will meet your eyes. You will see Selectionist trudging two miles off the track on the one side, and Mutationist two miles off it on the other; and you will hear the former say: "Look at Mutationist; he has gone hopelessly astray; he is four miles off the track. I am on the high road. Follow me, and you will reach the goal." And the latter: "If that isn't Selectionist! wandering about, four miles from the main road. I am on the high road. Follow me, and you will reach the goal."

But if we ascend to the top of the tower of the College we can see much more plainly than from down below that each is the same distance from the road. And I think it is a good thing sometimes to detach ourselves from our investigations, and, climbing up into the tower, to try to see ourselves as objects, looking, from that distance, like ants on a gravel path; to try to find out what the relation between ourselves and the things we investigate really is; and to ask ourselves: "Is the orderliness which we discover in things really in them; or is it not rather that we classify things in an orderly way in our mind, and then say, 'Lo! We find order everywhere in Nature!'"

Yet, although it is pleasant and necessary to retire to the tower and reflect, if we are to make progress in biology we must descend to the road. But before we return to our investigations, let us make certain rules which will keep us from going astray. Many valuable rules could be made. But I will suggest only two, which deal with what we have been discussing this evening. One is: To keep description absolutely independent of interpretation. The other is: To remember that what you arrive at by interpreting the thing you have described is not what you see underneath it, but what you read into it.

VIII

Mendelism

(*From the* "*Transactions of the Highland and Agricultural Society of Scotland*," 1913)

MENDELISM is the name given to the method and science of breeding which has come into existence as the result of the work of one Gregor Mendel. The history of Mendel's work and of its reception is a very curious one. About the middle of last century he was Abbot of a monastery in Brünn in Austria ; and in his cloister garden he carried out some hybridisation experiments with the culinary pea, crossing different varieties with one another and keeping detailed records of the results. He published his results in the journal of a local scientific society in 1865. But nobody paid any attention to his paper, although the facts which he described were of the greatest interest, and the explanation of these facts which he put forward was more interesting still. Mendel's paper lay hidden and forgotten in the volumes of the Scientific Society of Brünn from 1865 until 1900, sixteen years after his death, when it was discovered independently by three botanists—one in Germany, one in Austria, and one in Holland. As soon as the attention of the scientific world was drawn to Mendel's paper by these three botanists, the great importance of the facts which Mendel had discovered, and of the theories which he put forward to explain them, was at once seen. And many investigators set to work to find out whether the rules which Mendel said applied to his peas applied to other plants and to animals as well. It is natural that these investigators should choose as material for their breeding experiments plants and animals which multiplied as rapidly as possible, and could be purchased and bred as cheaply as possible. And it was reasonable to test the truth of Mendel's theory by breeding mice and sweet-peas before applying it to the improvement of live stock: because it was possible that Mendel's rules might only have applied to the material with which he worked—the culinary pea—and not have been of general applicability at all. If breeders had started to apply Mendel's rules to the improvement of their stock, the loss of much time and money might have been the result Thirteen years have now elapsed since Mendel's papers were rediscovered, and since the testing of his rules began. And already it has been found that his rules do apply pretty accurately to the breeding of a very great number of different kinds of animals and plants. But these animals and plants are naturally those which breed rapidly and can be bred on a large scale at small expense. So it comes about that our knowledge of the application of Mendel's rules to the breeding of horses, cattle, sheep, and pigs is still in its infancy. The question whether Mendel's rules do or do not apply to a particular case can only be decided after a series of crosses and interbreedings have been carried out in a particular way, and on a considerable scale: the practical breeder scarcely ever—in fact, never—carries out his matings in this way, so that the applicability of Mendel's rules to a particular case can never be decided by the examination of the results of matings not carried out with the express object of answering this question; though the results of such matings sometimes give a rough—very rough—indication of the answer.

Now, Mendel's rules are so simple that if it could be shown that those characters of live stock to which especial attention is paid were inherited in accordance with these rules, the breeder would be in a position to bring about great improvements in these characters in a comparatively short period of time. To bring about these improvements he must know two things: he must know what is the particular improvement he wants to effect; and this, I assume, he does know. He must also be familiar with Mendel's rules; and this, I assume, he does not know, or he would not be reading this article, which is written for those who are not familiar with Mendel's work or with the rules which have been based on that work.

First of all, we will become familiar with the essential facts discovered by Mendel.

The simplest case, and one that tells us all we want to know about Mendel's results, is that of a cross between a tall and a dwarf pea. The practical difference between a tall and a dwarf pea is that tall peas have to be staked and grown in rows not closer than 6 feet apart, whilst the dwarf peas do not require staking and can be sown in rows 2 feet apart. The stumpiness of the dwarf pea is due to the shortness of the divisions of the stem between the joints where the leaves are given off. These divisions in the tall pea are more than twice as long as they are in the dwarf.

Mendel crossed a tall with a dwarf pea, and found that the resulting hybrid was in every case tall. It made no difference whether the tall was used as the male parent and the dwarf as the female, or the dwarf as the male and the tall as the female. The resulting hybrids constitute the first hybrid generation, as shown in Fig. 1.[1] Seed is saved from these hybrids of the first hybrid generation, which have been allowed to self-fertilise. This seed, when sown, produces a generation, called the second hybrid generation, which consists of three talls and one dwarf amongst every four plants, on the average; or 75 per cent. tall and 25 per cent. dwarf plants. These plants of the second hybrid generation are again allowed to self-fertilise, and, when they are ripe, seed is saved from them, This seed, when sown, produces the third hybrid generation. A look at Fig. 1 shows that the dwarfs of the second hybrid generation breed true

[1] *Vide infra*, p. 265.

at once; they produce dwarfs only. Of the three talls in the second hybrid generation, one, at the left of the diagram, breeds true; it produces talls only, so it may be called a pure tall. The two remaining talls—the two in the middle of the second hybrid generation—each produce talls and dwarfs in the proportion of 3 to 1—that is to say, they behave, in their breeding, exactly as do the hybrid talls of the first hybrid generation, and we may therefore call them hybrid talls.

The facts shown in Fig. 1 may therefore be condensed into the following diagram:—

Pure Tall × Pure Dwarf

Hybrid Tall

1 Pure Tall. 2 Hybrid Talls. 1 Pure Dwarf.

or—

Pure Tall × Pure Dwarf

Hybrid Tall

25% Pure Tall. 50% Hybrid Tall. 25% Pure Dwarf.

We will deal with points to be noticed in Fig. 1 in order of their importance, beginning with the least important.

It will be seen that the dwarf character entirely disappears in the first hybrid generation. The tall character completely dominates over it; the tall character is therefore called the dominant character, and the dwarf character is called the recessive character. And the two characters, tallness and dwarfness, constitute a Mendelian pair of characters. I have repeated the word "character" in the preceding lines often, in order to lay stress on the fact that dominance and recessiveness belong to a particular character, and not to the plant as a whole.

The second point to be noted in Fig. 1 is that the

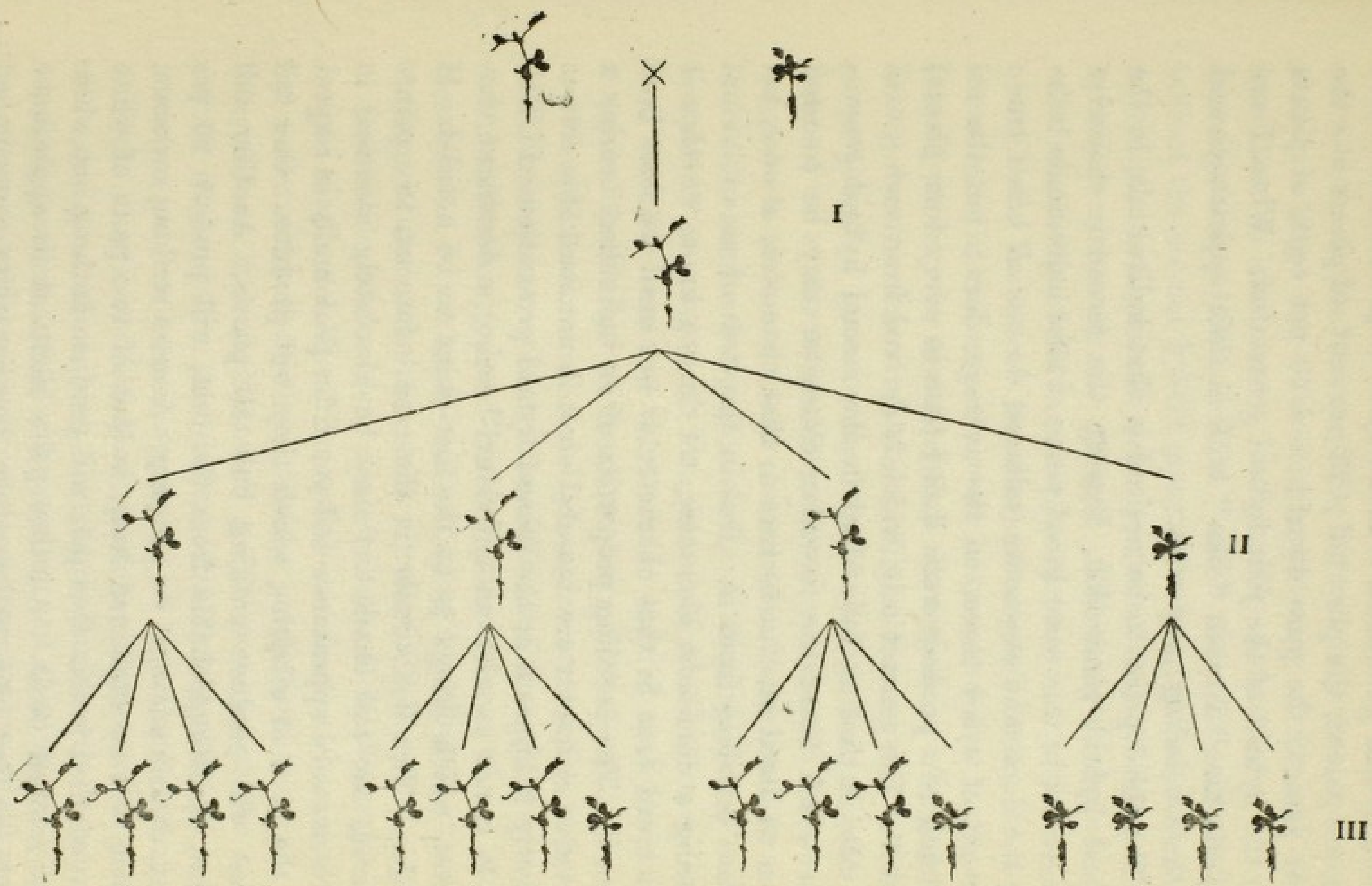


FIG. 1.—RESULT OF CROSSING TALL WITH DWARF PEA.

I., II., and III. refer to the first, second, and third hybrid generations respectively.

(*Reproduced from the Author's "Breeding and the Mendelian Discovery."* CASSELL AND COMPANY.)

second hybrid generation is made up of 25 per cent. of plants like one parent, the pure tall; 25 per cent. of plants like the other parent, the pure dwarf; and 50 per cent. of plants like the hybrid of the first hybrid generation. When I say "plants like," I mean "like" both in their appearance and in their breeding properties.

The third point to be noticed is that individuals, in the second hybrid generation, bearing the recessive character (dwarfness in this case) breed true; whilst individuals bearing the dominant character (tallness) do not *all* breed true: one out of every three, on the average, does; but the remaining two produce some dwarf (one in every four plants) as well. The general rule, which is derived from such a case as this, is that an individual in the second hybrid generation which bears the recessive character may be counted upon to breed absolutely true to that character, *at once*, i.e. before breeding from it. But in the case of an individual bearing a dominant character, we do not know whether it will breed true to that character or not until we *have* bred from it. The breeding properties of an individual bearing a recessive character are branded on it in unmistakable letters directly it appears in the second hybrid generation—PURE.

It is not so with an individual bearing a dominant character, which brings us to the last point to be noticed. It is this. Two individuals (in the case before us, two plants bearing the tall character) may be absolutely identical in their outward appearance and yet differ profoundly in respect of the kind of offspring which they will produce. One tall plant will produce nothing but tall plants. Another tall plant, indistinguishable from the first, will produce 25 per cent. dwarfs amongst its progeny. A more striking instance, which will be explained later, is that of two pairs of white rose-combed fowls. One pair will produce nothing but white rose-combed birds. Another pair, identical in appearance with the first pair, will produce, besides white rose-combed birds, white single-combed, black single-combed, and black rose-combed birds. The general conclusion derived from cases such as this is one of the utmost importance in practical breeding. It is that the appearance of the individual furnishes no clue as to its breeding properties; and its ancestry helps just as little. Both the pure and hybrid talls of the second hybrid generation had precisely the same ancestry; and yet the pure produces only talls, and the hybrid produces dwarfs as well. The individual itself can tell us nothing of its breeding properties; nor can its ancestry. To know how a given animal will breed, we must breed from it and see. Once we have found that its "get" is good we may be confident that the rest of its offspring will be up to the same standard. To estimate the breeding properties of a given animal we must not look straight in front of us at the animal himself; we must not look back, through its pedigree, at its ancestors; all we need to do is to look forward a little way, one generation in fact, at a fair sample of its progeny. This is indeed the lesson which I wish to drive home in this article. And I shall return to it again when more of the facts have been considered.

We will now take a brief glance at one of the first and most striking extensions of Mendel's rules from the vegetable to the animal kingdom. This is the classical case of the Andalusian fowl. It is a useful case, because it enables us to see in striking contrast the difference between the Mendelian rules of breeding and the older rules which the Mendelian rules bid fair to supplant. The blue Andalusian fowl is characterised by a beautiful slaty-blue plumage, each feather edged, or "laced" as it is called, with a darker tinge of the same colour. If you buy a pen of these blue Andalusians and hatch the eggs, you will be disappointed to find that only about half the chicks are like their parents; the remaining half will consist of black birds and white birds with occasional blackish feathers. You will probably put this impure breeding down to some recent cross amongst the ancestors of the birds you bought, and proceed to breed from the blue birds which you yourself have bred. With regard to these you will say, "I, at any rate, know that the parents of these birds were blue, as I bred them myself." But you will have no better luck; the blacks and whites will appear again in about the same numbers. Still you will say to yourself, "This black and white impurity looks as if it must have been introduced very shortly before I bought my birds, so that perhaps I have no right to expect that all traces of it will disappear in a paltry two generations. I will continue to breed from the blues only, always throwing away the blacks and whites, and ultimately, and I hope before very long, I am bound to obtain a race of pure-breeding blues: it stands to reason that if I go on breeding in this way long enough, I shall at last obtain a pure strain." Well, it may stand to reason that it will happen. It *won't* happen. For however many generations the blue Andalusians are bred together they will produce blacks, blues, and whites in the following proportions:—

25% Blacks. 50% Blue Andalusians. 25% Whites.

Are not those percentages familiar to us? Do they not suggest that the blue Andalusian is a hybrid like the hybrid tall pea, and that the blacks and the whites are the two pure forms corresponding to the pure tall and the pure dwarf? There is a very easy way of finding out if this supposition is true. If it is true, one of the 25 per cent. blacks crossed with one of the 25 per cent. whites should give blue Andalusians only. This is indeed what actually occurs, as shown in Fig. 2. If this diagram is compared with the corresponding one for the peas (Fig. 1), the following differences will be seen. There is no question of dominance here; there is no exclusion of one character from the first generation by the other character. Black and white meet and compromise in a blue; there is no victory for one of them, as in the case where tall and dwarf met; so that we

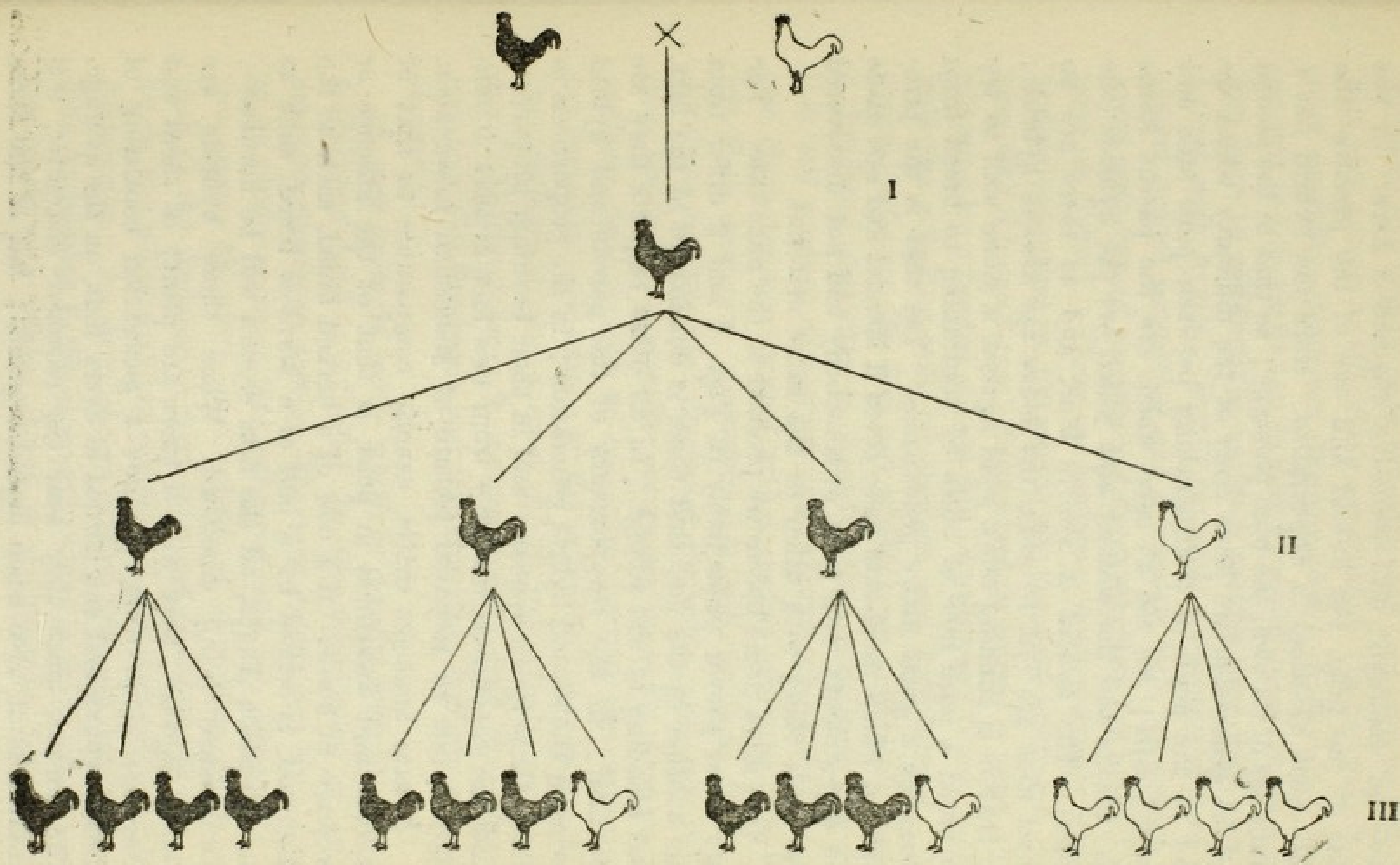


FIG. 2.—INHERITANCE OF COLOUR IN THE ANDALUSIAN FOWL.

I., II., and III. refer to the first, second, and third hybrid generations respectively.

(*Reproduced from the Author's "Breeding and the Mendelian Discovery."* CASSELL AND COMPANY.)

have no dominant and recessive characters here. In the case of the peas, the hybrid and one of the parents—the tall—were identical in appearance. Here the hybrid has a character of its own, the blue plumage ; so that in the second hybrid generation we have none of the difficulty which we had in the peas, of distinguishing between pure talls and hybrid talls ; we see at once which are the parent forms (the blacks and the whites) and which are the hybrids (the blues). The hybrid is always blue, and as there are no blues that are not hybrids, the blues are always hybrids ; so that it is running one's head against a stone wall to try to breed a pure race of blues by continuing to breed from blues for a great many generations. Yet that is the principle on which we should have acted if Mendel had not made his experiments with peas, or somebody had not discovered the same thing with other or the same material.

One more point before we proceed to the next case. The two cases shown respectively in Figs. 1 and 2 differ from one another in the fact that there is dominance in the first, and blending in the second. In all other respects they are identical ; in the reappearance of the parent and hybrid forms in the second hybrid generation ; in the proportions in which these forms appear ; and in their breeding properties. And the conclusion we draw from this fact is that dominance is not an essential feature of Mendelian inheritance.

A case amongst cattle, exactly comparable to that of tallness and dwarfness in peas, is that of the absence or presence of horns. If a cow of a horned breed, such as the Highland, is served by a bull of a hornless breed, such as the Aberdeen Angus, all the first crosses will be hornless—hornlessness being dominant. When these hybrids are mated together, they will, if there are plenty of them and they are given time, produce a generation consisting of three hornless and one horned in every four, on the average. The horned, since they bear the recessive character, will breed true at once when mated together ; but of the three hornless, one will be pure breeding and two will be hybrids. But how are we to tell which are which ? In the peas it was easy enough ; peas are hermaphrodite, both sexes being present in a single flower, so that we could know that in allowing them to self-fertilise we were mating pure with pure or hybrid with hybrid. But if we took a hornless cow of the second hybrid generation and a hornless bull, we should not know with regard to either of them whether the animal was pure or hybrid. There is, however, a very simple way of finding out—that is, by mating the hornless animal in question with a horned one. If the hornless animal is pure, all its offspring by a horned animal will be hornless ; if it is hybrid, half the offspring will be hornless and half horned. The general rule is : To find out whether an individual bearing a dominant character is pure for that dominant character (i.e. will produce exclusively individuals bearing that dominant character when mated with a similar pure), or is hybrid for that dominant character (i.e. will produce three individuals bearing that dominant character, and one individual bearing the recessive character in every four on the average), mate it with an individual bearing the corresponding recessive character. If the individual was pure, all of the offspring of such a cross will be individuals bearing the dominant character ; if it was hybrid, half the offspring will bear the dominant and the remaining half the recessive character.

A case amongst cattle which is parallel with that of the Andalusian fowl is afforded by the inheritance of colour amongst shorthorn cattle. The roan colour has been shown to be produced by crossing the red with the white. In the two cases, therefore—the colour of shorthorn cattle and that of the Andalusian fowl—the red corresponds with the black, the white with the white, and the roan with the blue Andalusian. An even closer parallel amongst cattle to the blue Andalusian is the blue roan, which is produced by crossing black with white. There is a difference, however, between the two cases—that of the Andalusian fowl and that of the shorthorn cattle. There is never, so far as my observations go, any difficulty in distinguishing a black bird from a blue one; they do not merge into one another by transitional stages; but it is sometimes difficult to say whether a beast is a red or a roan. There is a curious case referred to by Prof. James Wilson[1] to explain how some of the records in the Herd-Book, which are not in accordance with the Mendelian interpretation outlined above, may be accounted for. Most of these are cases of mistaken identification of colour. "A bull now standing (1908) at the Albert Agricultural College, Glasnevin, was registered in the Herd-Book by his breeder, who is the most distinguished breeder in England, as being red. This bull's sire was white; his dam was red; and he ought therefore to be roan. He has bred several white calves from roan cows; and on this ground also he ought to be roan. On close inspection *he is a roan*, but such a roan as might easily be mistaken for a red." I mention this case because it affords a striking illustration of the change which has been brought about by Mendel's work. The old pre-Mendelian system was to estimate how an animal would breed by looking at it (and its ancestors); if we were breeding for colour we should look at the colour of the animal in question in order to foretell what its offspring would be. But Mendel has completely turned the tables; and in the case of this bull we had to look at what his offspring had been in order to tell what colour he was! The Mendelian doctrine is that the characters of the body of the animal in question afford a very meagre clue to the value of that animal as a breeder; the proof of the animal is in its breeding—that is to say, in the offspring which it has proved it can get.

We have so far considered the results which follow when

[1] "Mendelian Characters among Shorthorn Cattle." "Scientific Proceedings of the Royal Dublin Society," Vol. 11. (N.S.), No. 28, p. 322. (June, 1908).

two individuals differing from one another in respect of a single pair of characters are mated. We have now to consider the results of mating two individuals which differ from one another in respect of *two* pairs of characters. This is a very important case, because on it depends the efficient carrying out of one of the most important processes of the breeder—namely, the combination in one strain of characters already existing in separate strains. The behaviour, on crossing, of two very well-known characters of the fowl may be used to illustrate this point. The two pairs of characters will first be considered separately. One pair relates to the colour of the plumage; and the pair consists of white and black, white being dominant over black. The other pair relates to the shape of the comb, and consists of the so-called "rose"-comb (of Wyandottes and Hamburghs) and the single comb (of Minorcas and Leghorns) of which the "rose" is dominant over single. Let us now describe an actual case in which these two pairs of characters are concerned. Such a case is a cross between the Black Hamburgh, which, as its name implies, is black, and like all the Hamburghs has a rose-comb, and the White Leghorn, which is white, and has a single comb as shown, very diagrammatically, in Fig. 3. White being dominant over black and "rose" over single, the hybrid produced by crossing the parent forms described above should have white plumage and a rose-comb, and it actually has. I bred many such hybrids last year. In passing, it may be remarked that this result shows very clearly the profound difference between dominance and a thing to which the name "prepotency" has been given. A prepotent animal is one which is supposed to have the power of impressing its characteristics on all its offspring, whatever it is mated with. It is usually a particular stallion or bull which acquires the reputation of prepotency. This prepotency is supposed to be due to the exceptionally great constitutional vigour of the prepotent animal, a vigour which has no difficulty in overcoming the feeble opposition offered by the

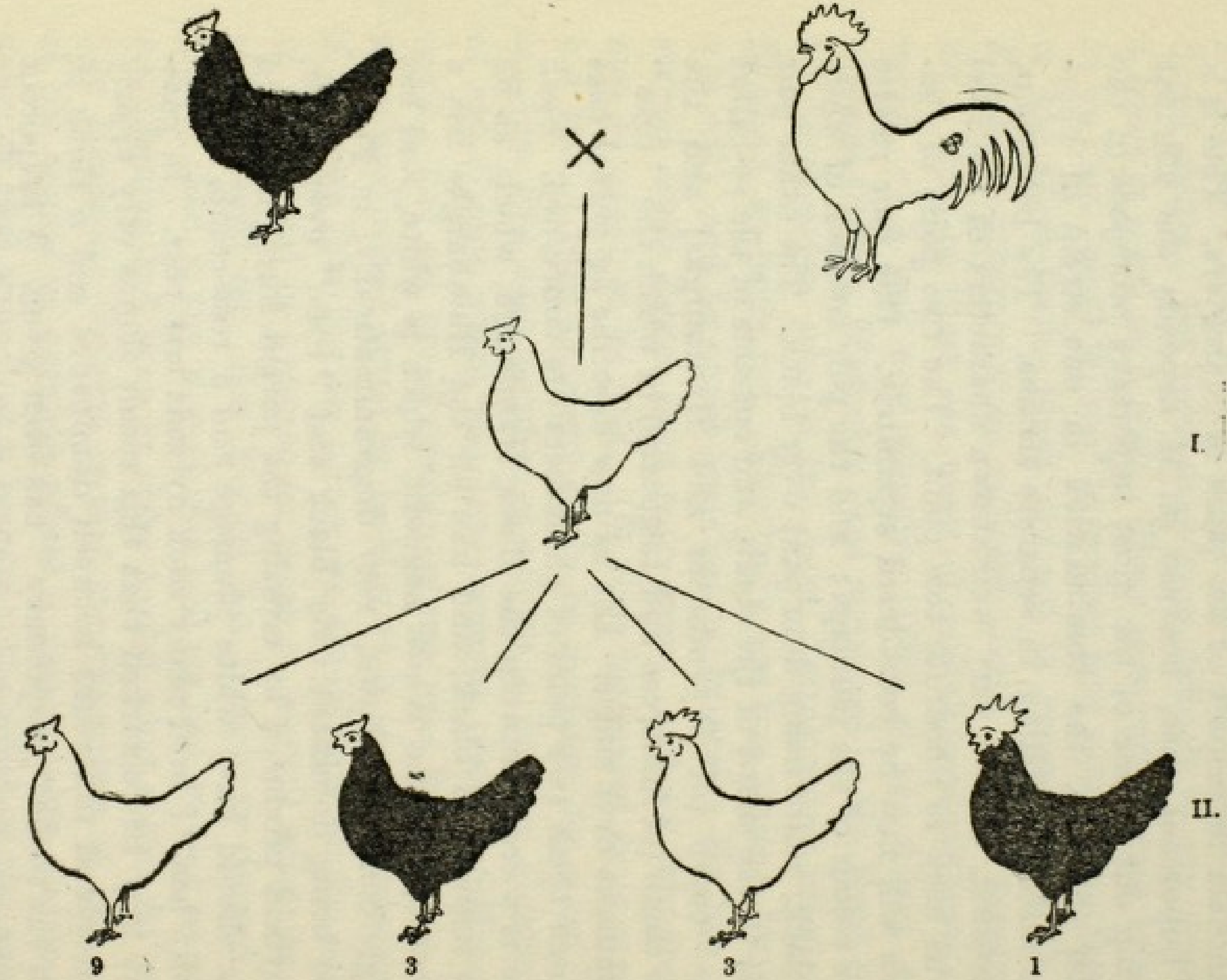


FIG. 3.—RESULT OF CROSSING A BLACK ROSE-COMBED FOWL (BLACK HAMBURGH) WITH A WHITE SINGLE-COMBED ONE (WHITE LEGHORN).

I. and II. refer to the first and second hybrid generations respectively.

inferior vigour of its mate, and in impressing its characteristics in their entirety on their offspring. It might be thought that a tall pea dominates over a dwarf pea because the tall has greater constitutional vigour. But, as I have already pointed out, this is not the case. Dominance and recessiveness belong not to the individual as a whole, but to the distinct characters of the individual. We could not prove this to be so in the case of crosses between individuals differing in respect of a single pair of characters only; but we can prove it up to the hilt in the present case of a cross between two individuals differing from one another in respect of two pairs of characters—the shape of the comb and the colour of the plumage. Of the two dominant characters shown by the hybrid, rose-comb and white plumage, one comes from the Black Hamburgh and the other comes from the White Leghorn. So there is no question here of one of the parents impressing its characters as a whole on its offspring. The reason that the Black Hamburgh transmits its rose-comb to the hybrid cannot be because it has greater constitutional vigour than the White Leghorn, because the Black Hamburgh fails to transmit its black plumage. Similarly, the ability of the White Leghorn to transmit its white plumage to the hybrid cannot be due to its greater constitutional vigour, because it cannot transmit its single comb.

You may say, if you like, that the Black Hamburgh was prepotent with regard to its comb, and the White Leghorn with regard to its colour; but if you do you are merely expressing the Mendelian idea of dominance in terms of prepotency. The truth is that dominance has nothing to do with the individual at all; it relates solely to the separate characters which we can deal with in breeding—colour of plumage, shape of comb, etc.; and this brings us to the very essence of the Mendelian doctrine, which is, that in a scientific principle of breeding, attention must be paid not to the individual as a whole, but to its various characters, which can be shown to be inherited independently of one another.

We shall see what this means if we observe what happens when the white, rose-combed hybrids, between the Black Hamburgh and White Leghorn, are mated together. If a sufficiently large number of chicks is hatched out we shall find all the four possible combinations of the two pairs of characters, white and black plumage, and rose and single comb, in the order shown in the diagram, Fig. 3; and in the proportions indicated—9 white, rose; 3 black, rose; 3 white, single; and 1 black, single. The numbers indicate the average number of each combination in every 16 birds; but, of course, there would have to be many more than 16 birds to get anything like a close approximation to these actual proportions. But the proportions are not the essential fact, though they are very important. The essential fact is that all possible combinations of the characters borne by the parents do occur in the second hybrid generation. This reveals an important fact and teaches an important practical lesson. The fact which it reveals is that the shape of the comb and the colour of the plumage of, for instance, the Black Hamburgh, are not transmitted in conjunction, but can be separated from one another by cross-breeding, so that, for instance, the black plumage of the Black Hamburgh can be taken up as an independent element and united with the single comb of the White Leghorn, and be permanently combined as in the last bird on the right at the bottom of Fig. 3. The principle of recombining characters in breeding is of course not a new one—it *must* have been done unconsciously, and has *probably* been done deliberately; but a familiarity with the simple scheme given in Fig. 3 will greatly facilitate the process of the recombination of characters.

The important lesson to be learnt from the fact that all possible combinations of the parental characters occur in the second hybrid generation we are considering, is that this total possible number of combinations occurs when the first crosses are mated together *and in no other way*. Suppose the cross between the Black Hamburgh and the White Leghorn had been made with a view to get a black fowl with a single comb, nothing could have been more disappointing than the first cross—a white fowl with a rose-comb, a fowl which exhibits neither of the characters it was wished to combine. What the breeder would probably do under such circumstances would be to mate the hybrid back with one of the parents " to put in some more of the required blood." The last thing he would have dreamt of doing would be to mate together his two first crosses, which showed *neither* of the two characters he wished to combine. Yet this is the only mating which would give him the desired combination—black plumage with single comb. The reason that he would not think of mating together the two first crosses is that he was under the influence of the old error of regarding the visible bodily characters of an individual as an indication of its breeding properties. And the reason why he would mate the first cross back with one of the parent forms would be the same. But the fact is that, especially in the case of hybrids, the appearance of the animal gives no clue whatsoever as to what it is likely to produce. And matters are often much worse than in the case of poultry which we have described. The hybrid very often, as is well known, does not, as in the case just described, exhibit the characters of either of the parents, but of some supposed remote wild ancestor. Nothing could be more disappointing than the production of such a scarecrow by crossing two valuable animals of two different sorts; and nothing is more certain than that the breeder would not breed more of them in order to obtain a male and a female to mate together. Nevertheless, this is what, according to the Mendelian doctrine, he ought most unquestionably to do. Indeed, I would even go so far as to suggest this possibility, that the less the first cross approximates to the result which the breeder expected to ensue from the cross, the more worth while will it be to mate these first crosses together. But I do not press this point. All I am concerned to insist on now is that if the immediate result of a cross is far from what was expected, this should not be cause for disappointment. If any valuable new combination of characters is to come out of a particular cross, it is not to be expected till the second hybrid generation.

For instance, I have made some crosses between Jersey and Ayrshire cattle with the view of finding out whether a cow can be raised which has as high a percentage of butter-fat in its milk as the Jersey, and gives as great a quantity of milk as the Ayrshire, or at any rate approaches to this ideal. I am prepared to find that the cows of the first hybrid generation are even inferior to both the parent breeds in the quality and quantity of their milk. But this would not make me less sanguine in the hope that some cow " of noble note may yet be " bred in the second hybrid generation. If the two characters which I wished to combine—high yield and high butter-fat percentage—were recessive ones, the desired combination would only occur once in every sixteen individuals. But I do not anticipate that either yield or butter-fat percentage will exhibit simple dominance. Indeed, in other crosses of this kind, as between the Jersey and the Danish for example, it is known that, so far as butter-fat percentage is concerned, the first cross between these two breeds is a blend between the two parental percentages. In the second hybrid generation, therefore, we must not expect four sharply circumscribed classes as in the case of Fig. 3, but a very wide range of classes, all of which merge into one another—a very wide range of classes (merging into one another) with regard to butter-fat percentage, ranging from the high to the low, and a similar range of classes with regard to quality. It simply remains to be seen whether there can coexist in one cow the high yield of the Ayrshire and the high butter-fat percentage of the Jersey. And it will take many years to find out.

It may have occurred to the reader to object that the immediate result of a cross is not by any means always disappointing. Of this fact I am well aware. But the value of such crosses is usually due to the fact that first crosses commonly possess great strength, health, and vigour, and is not due to the combination of deliberately chosen desirable characteristics, which is the subject under discussion. The best bacon, I believe, is produced by first-cross swine: and the same general truth obtains in the case of other stock as well. But the exceptional value of these first crosses must be distinguished carefully from the value belonging to new combinations of characters. The exceptional value of first crosses is due to the fact that they are first crosses; they have this exceptional vigour and robustness, in virtue solely of the fact that they are first crosses; and they would not hand it on to their offspring if they were mated together. The value of such first crosses is a flash in the pan. But the value of a new combination of characters brought about in the second hybrid generation has no more to do with the animals being hybrid than the fact that it was necessary to make a cross to bring the characters together. But once these characters have been re-combined in a few individuals and made the foundation of a breed, this particular combination has a value so long as the need which called it into existence persists.

In conclusion, let me repeat the central idea in this article: that neither the characters of the individual itself nor of its ancestors gives any indication of the value of that individual as a stock-getter. A few records of the milk-production of the daughters of an Ayrshire bull are of much greater use as an indication of his value than all the available information with regard to his dam and granddams. You cannot tell what the breeding properties of a beast are until you have tested them. You cannot say whether a goose will lay golden eggs or not until it has begun to lay.

This article is intended merely as an introduction to the subject. The most elementary treatise on it is "Breeding and the Mendelian Discovery" (Cassell and Co.), by the present writer. An excellent account of the whole subject of Heredity is given in J. A. S. Watson's "Heredity" (the People's Books series). For fuller information on the subject of Mendelian Heredity the reader is referred to R. C. Punnett's "Mendelism" (Macmillan and Co.); and for an exhaustive treatment of the whole subject to Bateson's classical work, "Mendel's Principles of Heredity" (Cambridge University Press).

Francis Galton[1]

A GREAT personality has passed from us in the death of Francis Galton. To few has it been given to live so full and so valuable a life. The Goddess Fortune, it is true, offered him opportunity with a generous hand; rarely, however, has an offer been more amply justified than that she made to Francis Galton, for the treasure has been invested by its trustee in safe things, which will never cease to pay. Galton never flew too high nor attempted to probe too deep; he was conscious both of the extent and of the limitation of his powers; his work is of lasting value to mankind because he possessed in a high degree that sense which tells its owner which tasks lie within and which beyond his power.

But we should be disloyal to Galton if we regarded him as an isolated personality detached from his natural setting —his ancestry. Indeed, we are not truly loyal to what he believed unless we regard the individual as a product (in the strictly literal sense of a continuation without cessation of individuality) of his ancestors, and intelligible only in the light of a knowledge of those ancestors. Turgenev and Samuel Butler owe their supreme position as novelists to the fact that they perceived this.

Perhaps the most remarkable instance of a monopoly possessed by a single family over a particular business is the limitation to the Darwin family of the business of bringing home the truth of evolution to the understanding of mankind. We make no apology for the term business. It is no longer necessary to point out that Charles Darwin's achievement was not to discover evolution, but the much

[1] Obituary notice in *Science Progress*, 1911.

heavier task of forcing mankind to believe in it. To satisfy oneself of the truth of evolution is the work of a philosopher; to convince other people of this truth is a labour of Hercules.

Erasmus Darwin married twice. By his first wife he was grandfather to Charles Darwin, by his second to Francis Galton. "His hereditary influence," says Galton ("Memories of My Life," p. 7), "seems to have been very strong. His son Charles, who died at the early age of twenty of a dissection wound, was a medical student of extraordinary promise." Erasmus may not have been—probably was not—the first to perceive the fact of evolution; there can be little doubt that Buffon was before him. But with Erasmus Darwin the business of making evolution credible passed over once and for all to the "nation of shopkeepers" and the Darwin family. Not only was Erasmus Darwin's enunciation of the doctrine of evolution less equivocal than Buffon's (through no fault of the latter, however), but the sudden conversion of Lamarck, late in life, to a belief in evolution followed so closely after the appearance of a French translation of "The Loves of the Plants" by E. Darwin, as to leave little room for doubt that the two events were causally connected. Of Charles Darwin's participation in the task of making evolution credible no more need be said.

We must now turn to the part played by his cousin. Galton, by laying the foundation of an exact science of heredity, not only took steps to fill in the most serious gap in the evolutionary hypothesis, but sowed the seed of a tree which will furnish the best, if not the only, antidote to that outbreak of *a priori* speculation which followed the publication of the "Origin." Galton's vivid perception of the necessity of having things surely and certainly described before any attempt was made to explain them, found its expression in the application of statistical methods to the study of biological problems. It was not only by that part of his work which laid the foundations of Biometry that Galton supplemented the work of Darwin; he also drove home the applicability of our knowledge (such as it is) of evolution and heredity to the furtherance of human welfare, and thus laid the foundations of the science of Eugenics.

Galton's greatest work in the purely scientific sphere must be regarded as the foundation of the Biometric method and philosophy rather than the promulgation of the law of heredity which is associated with his name. This law, which was deduced from a study of Basset-hound pedigrees, will in the future be remembered, not so much as a true summary of a vital process, but as the expression of a first valiant attempt to detect some order in the chaos of hereditary phenomena as they appeared at the time. Its purely provisional nature is foreshadowed by the fact that its author did not commit himself to a dogmatic statement on the question whether it was applicable to the individual or to the mass, and demonstrated by the fact that only in rare, probably accidental, cases does it apply to masses, and that in no case does it apply to individuals. Galton's work on Basset-hounds will always be remembered for the same reason as will de Vries's work on "The Evening Primrose," estimating both of these at their lowest possible valuation. Both were the first attempts to break the ground in a new field of evolutionary inquiry. The pioneer nature of Galton's work in heredity is gracefully and fittingly acknowledged by Johannsen, whose "Erblichkeit in Populationen und in reinen Linien," which represents the most recent and ingenious attempt to interpret a certain class of hereditary phenomena, is dedicated to him.

No account of Galton would be complete without reference to his work on Finger Prints. The source of his interest in this subject was delight in the observation and systematisation of the phenomena themselves; their application was a subsequent matter. His interest in them arose through a request to deliver a Friday evening lecture on the system "devised by M. Alphonse Bertillon for identifying persons by the measurements of their bodily dimensions." Galton tried to persuade M. Bertillon to incorporate finger prints into his system of identification, but without success. He found, however, that they had already been employed by Sir William Herschel in his district in India, who succeeded in introducing them into Bengal and subsequently throughout the whole of India. At the present day there are few civilised countries in which they are not employed for the purposes of the identification of criminals. Galton's hopes that finger prints would prove to be of high anthropological significance were not fulfilled. He was unable to find that any particular type is characteristic of members of widely divergent human races; or of such widely different types within a single race as "students of science, students of art, Quakers, notabilities of various kinds and a considerable number of idiots at Earlswood Asylum." Only one result of positive theoretical significance emerged from this study: this was the demonstration that the variation of the pattern was of the discontinuous kind, and the conclusion that the various types had been evolved without the aid of natural selection.

It is a curious coincidence—if indeed it be a coincidence—that the respective founders of two great schools of heredity, the Biometric and the Mendelian, were born in the same year. Galton and Mendel were both born in 1822. Galton ("Memories of My Life," p. 308), with characteristic courtesy, refers to Mendel's work immediately before he refers to his own law. His estimate of the significance of the work done and inspired by Mendel seems to us to be so true and concise that we make no apology for quoting it together with the rest of the paragraph in which it occurs: "I must stop for a moment to pay a tribute to the memory of Mendel, with whom I sentimentally feel myself connected, owing to our having been born the same year, 1822. His careful and long-continued experiments show how much can be performed by those who, like him and Charles Darwin, never or hardly ever leave their homes, and again how much might be done in a fixed laboratory after a uniform tradition of work has been established. Mendel clearly showed that there were such things as alternative atomic characters of equal potency in descent. How far characters generally may be due to simple, or to molecular characters more or less correlated together, has yet to be discovered." The grace and simplicity of this, the delicate manner in which Mendel is associated with Charles Darwin and the soundness of the critical estimation are very characteristic of Galton. It is often said that the course of Charles Darwin's work and thought would have been very different if Mendel's work had come under his notice. It is curious to speculate as to what might have been the course of hereditary inquiry if Galton himself had made the experiments actually carried out by Mendel.

Galton's claim to fame, however, does not rest only on his pioneer work in the investigation of heredity. He will be perhaps longer known, and he is at present more widely known, as the champion of the application of such knowledge of heredity as we possess to the improvement of the human race; in other words, as the founder of Eugenics.

His interest in this question first found expression in two articles on "Hereditary Talent and Character," published in *Macmillan's Magazine* in 1865, which, curiously enough, is the year in which Mendel published the results of his own classical researches. The width of Galton's interests at this time may be gathered from the fact that the publication which preceded this was one on "Spectacles for Divers, and the Vision of Amphibious Animals," the one which succeeded it being that on the "Conversion of Wind-charts into Passage-charts." But the actual investigation of the problem of race improvement was laid aside for many years because he felt that "popular feeling was not then ripe to accept even the elementary truths of hereditary talent and character upon which the possibility of Race Improvement depends." Moreover, he himself was "too much disposed to think of marriage under some regulation and not enough of the effects of self-interest and of social and religious sentiment." The term Eugenics was first applied by Galton to the scientific attempt to ameliorate the human race in his "Human Faculty," which appeared in 1883; and his interest and inquiries up to date were gathered up into his "Huxley Lecture" before the Anthropological Institute in 1901 on the "Possible Improvement of the Human Breed under the existing conditions of Law and Sentiment."

The active prosecution of eugenic inquiry has been handed over to the professed representatives of the Biometric school. How this has been done may best be told in Galton's own simple words. After referring to the foundation by Professor Karl Pearson of a Biometric laboratory in University College and the institution of *Biometrika* and his connection with it as Consulting Editor, he says ("Memories of My Life," p. 320), "The ground had thus become more or less prepared for further advance; so after talking over matters with the authorities of the University of London and obtaining their ready concurrence, I supplied sufficient funds to allow of a small establishment for the furtherance of Eugenics. The University provided rooms and gave the sanction of their name and various facilities, and I provided for a Research Fellow and a Research Scholar. The Eugenics laboratory of the University of London is now situated in University College, in connection with Professor Karl Pearson's Biometric laboratory, and I am glad to say he has consented to take it for the present at least under his very able superintendence; as I am too old and infirm to look properly after it." Eugenics, it may be well to remind the reader at this point, is officially defined in the minutes of the University of London as "The study of agencies under social control that may improve or impair the racial qualities of future generations either physically or morally."

The actual investigation of eugenic problems is thus cared for. But there has also sprung into existence the

Eugenics Education Society, which acts the part of a middle man whose function is to exhibit the results of those investigations so that they shall be intelligible and palatable to the lay mind.

The institution of the eugenic movement was evidently regarded by Galton as his life-work, for he concludes his autobiography with a restatement of his views concerning it. "I take Eugenics very seriously, feeling that its principles ought to become one of the dominant motives in a civilised nation, much as if they were one of its religious tenets." Here is a life-work of which any man might well be proud.

Galton practised the principles which he preached in his marriage with the daughter of Dr. Butler, for many years Headmaster of Harrow and then Dean of Peterborough. Dr. Butler was not merely an able classicist and mathematician himself, but transmitted his qualities in full measure to his children and grandchildren. The remarks which follow the reference to his marriage and to his wife's family are perhaps the most interesting, because the most intimate, of his eugenic pronouncements.

". . . The Butler family well deserve study as an instance of hereditary gifts, but this is hardly the place for it.

"Neither can I enlarge as I could have done on the far greater importance of being married into a family that is good in character, in health and in ability, than into one that is either very wealthy or very noble but lacks these primary qualifications. . . .

"I protest against the opinions of those sentimental people who think that marriage concerns only the two principals; it has in reality the wider effect of an alliance between each of them and a new family."

It is a happy circumstance that Galton lived long enough to complete that vivid and detailed picture of himself, "Memories of My Life." This is by no means a piece of elaborate self-analysis. The picture presented to us of the author is conveyed by his own simple unaffected style and seen in the mirror of his varied environment. The dominant note in Galton's personality was his simplicity; to the last he preserved a childlike delight in trivialities which is an attribute of only very great men. His delight at having a *genus* of plants related to the Hyacinths named after him was unbounded, and we cannot but be deeply moved by the little drawing of *Galtonia candicans* which concludes his autobiography.

Perhaps the most beneficent symptom of Galton's perennial youth was the sympathy which he felt and delighted to express with those who were beginning. There are, to our own personal knowledge, many men who have not yet attained to half the age at which Galton died, and who have done and will do work of the highest scientific value (in the most literal sense of the word scientific), whose greatest and in some cases only encouragement has been a kind word or line from the great man whose death has deprived them of a true friend and science of a noble servant.

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