KARL PEARSON, 1857–1957

Being a Centenary Lecture by J. B. S. Haldane, delivered at University College London on 13 *May* 1957*

We are met here to-day to celebrate the centenary of the birth of Karl Pearson. To me, at least, this means that I am glad that Karl Pearson was born, that I think the world is better because he was born.

A greater man than any of us said

> The evil that men do lives after them
> The good is oft interrèd with their bones.

Let us begin, therefore, with some criticisms. And then let us study not only those of Pearson's contributions to science and culture which are widely known, but perhaps some also which should be disinterred and brought once more into the light of day.

As Pitt first stated, and Acton restated more precisely, all power corrupts. It is impossible to be a professor in charge of an important department, and the editor of an important journal, without being somewhat corrupted. We can now see that in both capacities Pearson made mistakes. He rejected lines of research which later turned out to be fruitful. He used his own energy and that of his subordinates in research which turned out to be much less important than he believed. It is, however, very easy to say what any one ought to have done fifty years ago!

But this criticism can be, and has been, pushed much further. It is said that Pearson espoused a fundamentally false theory of heredity, and therefore of evolution, and that as a consequence his work was not merely useless, but actually retarded progress. Had Pearson become dictator of British research on heredity and evolution, this might have been true. Fortunately he did not. I believe that his theory of heredity was incorrect in some fundamental respects. So was Columbus' theory of geography. He set out for China, and discovered America. But he is not regarded as a failure for this reason. When I turn to Pearson's great series of papers on the mathematical theory of evolution, published in the last years of the nineteenth century, I find that the theories of evolution now most generally accepted are very far from his own. *But* I find that in the search for a self-consistent theory of evolution he devised methods which are not only indispensable in any discussion of evolution. They are essential in every serious application of statistics to any problem whatever. If, for example, I wish to describe the distribution of British incomes, the response of different individuals to a drug, or the results of testing materials used in engineering, I must start off from the foundation laid in his memoir on 'Skew variation in homogeneous material'. After sixty-three years I shall certainly take some short cuts through the jungle of his formulae, some of which he himself made in later years. Very few ships to-day follow Columbus' course across the Atlantic.

Let me put the matter in another way. Anyone reading the controversy between Pearson and Weldon on one side, and Bateson and his colleagues on the other, which reached its

* The author is quite aware that he has repeated himself in a way which would be unjustified had the material been put together for an article, but which is justifiable in an oration. On the other hand, he considers that it is seldom desirable to hack what was designed to be spoken into a form suitable for reading.

culmination about fifty to fifty-five years ago, might have said 'I do not know who is right, but it is certain that at least one side is wrong'. In fact both were right in essentials. The general theory of Mendelism is, I believe, correct in a broad way. But we can now see that if Mendelism were completely correct, natural selection, as Pearson understood it, could not occur. For the frequency of one gene could never increase at the expense of another, except by chance, or as we now put it, sampling errors. It is just the divergence between observed results and theoretical expectations, to which Pearson rightly drew attention, which gives Mendelian genetics their evolutionary importance.

After this preamble I pass to my main task. Pearson's connexion with this College began when he was nine years old, and was sent to University College School, where he remained for seven years. He left at sixteen and obtained a scholarship at King's College, Cambridge, at eighteen. As an undergraduate he studied mathematics and was third wrangler in 1879. He had already shown something of his future mettle by a successful refusal to attend divinity lectures. In spite, or perhaps because, of this independence of spirit he became a fellow of King's in 1880. He spent about a year in the universities of Heidelberg and Berlin, attending lectures on philosophy and Roman law as well as physics and biology. However, the most striking effect of his German year was to interest him in mediaeval and Renaissance German literature, especially the development of ideas on religion and the position of women. At about this time he began to spell his Christian name with a K instead of a C. This may have been a homage to German culture. It may have been a special homage to Karl Marx, for we know that he later lectured on Marx, and his daughter tells me that when in Germany the police once searched his rooms, and he considered that one of Marx's books was the most subversive of the documents which they found there.

In 1880 he began the study of law in London, and was called to the bar in 1881. This may have been a tribute to his father, who was a Q.C., or a means of ensuring a livelihood in future, more probably both. He also published his first books, *The New Werther** and *The Trinity, a Nineteenth Century Passion Play*. Both were anonymous, and had they been signed, would certainly have prejudiced their author's chance of appointment in many institutions, perhaps even in the Infidel College, which suffers from occasional outbreaks of respectability. For both attack Christian orthodoxy.

It was at this period of his life that he lectured on Marx to small audiences in London, on the 'Ethic of Freethought' at South Place, and to the Sunday Lecture Society on 'Matter and Soul'.

In 1884, at the age of 27, he was appointed to the Chair of Applied Mathematics and Mechanics in this College. He had only published two small papers on rather academic problems of applied mathematics. His first publication after his appointment was *The Common Sense of the Exact Sciences*, by his illustrious predecessor in this College, W. K. Clifford. His next was even more surprising. It was written in German, entitled *Die Fronica: Ein Beitrag zur Geschichte des Christusbildes in Mittelalter*, and published at Strassburg. So far as I know it was the first contribution made by a professor of this College to the history of art. It is interesting to see that he regarded this as a worthy topic of academic study. May I hope that, now that we have a Chair of this subject, our Professor may comment on Pearson's contribution to it.

He was clearly a very successful and thorough teacher of applied mathematics, mainly

* For the bibliography of Pearson's works, and for much else, I rely on E. S. Pearson's invaluable memoir (Cambridge, 1938). In this lecture I have not even mentioned some of his books.

to students of engineering. He edited de Saint-Venant's work on the theory of elasticity, and wrote the second part of Todhunter's *History of the Theory of Elasticity*. His radical activities continued. In 1885 he joined 'The men's and women's club', a small body devoted to 'the free and unreserved discussion of all matters in any way connected with the mutual position and relation of men and women'. As, in *The Ethic of Freethought*, Pearson defended the view that unmarried women should be allowed sexual freedom, it is not surprising that legends arose, and still exist, as to this club.* In fact Karl Pearson married one of its members, Miss Sharpe. To-day it is quite normal for a couple to discuss human sexual physiology before marriage. Seventy years ago it was regarded as grossly improper, and all kinds of accusations were made against those who did so. I have not the faintest doubt that in fact the male members of the club were far less promiscuous than most of their contemporaries. If to-day association with prostitutes is generally regarded as degrading, while seventy years ago it was generally condoned and not rarely approved, we owe it largely to men like Karl Pearson.

The Ethic of Freethought was published in 1888, and is a collection of lectures and essays, some of which had been reprinted as pamphlets. It is, in essence, a religious book. Pearson defined religion as 'the relation of the finite to the infinite'. 'Hence', he continued, 'all systems of religion are of necessity half truths.' The most scholarly part of the book deals with the history of religious systems, particularly in Germany. He believed that such a study was part of the duty of an educated man or woman. I read a few sentences. 'By studying the past I do not mean reading a popular historical work, but taking a hundred, or better fifty, years in the life of a nation, and studying thoroughly that period. Each one of us is capable of such a study, though it may require the leisure moments, not of weeks, but of years. It means understanding, not only the politics of that nation during those years; not only what its thinkers wrote; not only how the educated classes thought and lived; but in addition how the mass of the folk struggled, and what aroused their feeling and stirred them to action. In this latter respect more may be learnt from folk-songs and broadsheets than from a whole round of foreign campaigns.'

The book is largely a record of its author's search for truth among religious systems. One chapter is devoted to the mystic Eckehart, and was the first introduction of that remarkable thinker to the British public. Of all the systems examined there can be no doubt that that of Spinoza appealed most deeply to Pearson; and he devoted another chapter to demonstrating Spinoza's debt to Maimonides. If I may be allowed to express a regret which is in no sense a criticism, it is that Pearson's acquaintance with Indian philosophy was confined to translations of Hinayana Buddhist scriptures. I think that he would have recognized more kindred spirits in such ancient Hindu thinkers as Yajnavalkya and the great anonymous humanist whose words are preserved in the first section of the Bṛhadaranyaka Upanishad.

It is a little surprising that the title page does not mention the author's professorship at University College. Perhaps his senior colleagues thought that such a mention would have got him into trouble.

If, in 1890, one had had to pass judgement on Pearson, it might have run as follows,

* [As might be expected, the critics tended to fix on only one side of the picture of the ideal relationship between the sexes in a socialist state which Pearson elaborated in his lectures on "The Woman's Question" (1885) and "Socialism and Sex" (1886), afterwards published in *The Ethic of Freethought*. Ed.]

'He is a first-rate teacher of applied mathematics, and a scholarly compiler of the work of more original men. He has a knowledge of literature and art most unusual in a professor of mathematics. He is somewhat of a radical, but he is only thirty-three years old. He will settle down as a respectable and useful member of society, and may expect a knighthood if he survives to sixty. He will never produce work of great originality, but the College need not be sorry to have appointed him.' Had this judgement been correct, we should not be here to-day.

In 1890 two events occurred which, in my opinion, shaped the course of Pearson's future life. He applied for, and received, the lectureship in Geometry at Gresham College; and W. F. R. Weldon succeeded Lankester in the chair of zoology at University College. At Gresham College he could lecture on what he pleased. His first set of lectures developed into *The Grammar of Science*, his main contribution to philosophy. Later series dealt with 'The Geometry of Statistics', and 'The Laws of Chance'. But since the discussion of probability and statistical method in the first edition of *The Grammar of Science* is superficial, we may take it that in 1891 he had not considered the subject seriously. He certainly did so in later years. I have little doubt that the stimulus to do so came largely from Weldon.

The Grammar of Science is a very remarkable book. Pearson claimed that material objects were merely a conceptual shorthand used to describe regularities in our sense-impressions. This idea is hard to develop, if only because our language is in terms of material objects such as eyes and brains. He did not in fact develop it without some self-contradiction, at least on the verbal level. But he did so, in my opinion, with much less self-contradiction than contemporaries such as Mach and Avenarius. He must be regarded as one of the founders of the important school of logical positivism.

I can well remember the impression which his book made on me when I first read it about 1909. If it is less impressive to modern readers, this is probably because physical theories have changed profoundly, a fact which would in no way have surprised or distressed its author. I do not personally think that Pearson's philosophical views are correct. Nevertheless, a man who first states an important doctrine clearly, even if it is subsequently rejected, is a moment in the thought process of humanity. We can best see whether Pearson did this by listening to the judgement of one of his adversaries. In 1908 Vladimir Ilyitch Lenin wrote *Materialism and Empirio-criticism*. This was an attack on people who, in his words, or rather those of his translator, 'under the guise of Marxism were offering something incredibly muddled, confused and reactionary'.

Now Lenin disagreed strongly with Pearson, and claimed, in my opinion correctly, to have found self-contradictions in his arguments. Nevertheless, he found him vastly clearer than other Machians. Let me read a few of Lenin's sentences. 'The philosophy of Pearson, as we shall repeatedly find, excels that of Mach in integrity and consistency' (p. 119).* 'The Englishman, Karl Pearson, expresses himself with characteristic precision, "Man is the creator of natural law".' (p. 221). And finally (p. 243) Lenin described him as 'This conscientious and scrupulous foe of materialism'. Unfortunately, I do not know how precise is this translation from the original Russian. But praise of this kind from an opponent is in my opinion worth a great deal more than either the assent of uncritical disciples or the patronizing acknowledgements of successors who claim to have improved on Pearson's

* The references are to the pagination in Vol. 11 of Lenin's *Selected Works*. London, Lawrence and Wishart, 1939.

treatment of the subject. The only other contemporary British opponent of materialism to whom Lenin was equally polite was James Ward. I cannot help thinking of Dante's treatment of Saladin, who was, of course, in hell, but so far from suffering from heat, cold, or other torments, was housed in a noble castle. Whatever may be the fate of Pearson's philosophy in his own country, *The Grammar of Science* is assured of attentive reading in those states where Leninism is orthodox.

To go back to Pearson's own views, I quote three sentences from the *Grammar*, which I think illustrate the strength and the weakness of Pearson's approach to science. 'The unity of all science consists alone in its method not in its material.' 'No physicist ever saw or felt an individual atom. Atom and molecule are intellectual conceptions by aid of which physicists classify phenomena and formulate the relationship between their sequences.' The strength is shown by the fact that the distributions, which Pearson worked out to describe Weldon's measurements of populations of crabs, will equally well serve to describe populations of stars, manufactured goods, durations of life, incomes, barometer readings, and so on. The weakness is shown by the fact that physicists have, during this century, seen individual atoms, or rather atomic nuclei, by the tracks which they make when moving rapidly. Pearson's philosophy discouraged him from looking too far behind phenomena. It was, I think, for this reason, that he never accepted Mendelian genetics, although the *Treasury of Human Inheritance* and his own monograph on albinism contain plenty of evidence in its favour.

His later series of Gresham Lectures dealt with statistics and probability, particularly with graphical methods of representing distributions. I have no doubt that they were written partly as a result of the questions which Weldon began putting to him soon after his arrival in University College. But his full answer to these questions is to be found in the great series of memoirs on the Mathematical Theory of Evolution which were published in the *Philosophical Transactions* of the Royal Society between 1893 and 1900. It is not too much to say that the subsequent developments of mathematical statistics are largely based on Pearson's work between 1893 and 1903. Perhaps we shall be helped to estimate its importance by an exercise in hypothetics. What would have been the effect on Pearson had Bateson obtained the Jodrell Chair of Zoology in place of Weldon? And what would have been the effect had our College contained an economist or engineer interested in what is now called Quality Control? Although Bateson was as interested as Weldon in animal variation, he was more concerned with exceptions, and with discontinuous, or as Pearson and Lee called it in 1899, exclusive inheritance. I doubt if Bateson would have put his questions in a form which would have aroused Pearson's interest. If he had done so, they would probably have discovered what is now called Mendelism. For Bateson, before reading Mendel's paper, did not realize the necessity of dealing with large samples, which Pearson certainly did.

If an economist or technologist had interested him in the variation of manufactured goods, he would have had to deal, as he did, with skew variation. He would presumably have used correlation to measure the likeness between the products of the same craftsman or machine as he in fact used it to measure the likeness between the children of the same parents. Perhaps in 1901 he might have founded a journal *Technometrika* not wholly unlike *Biometrika*. He would almost certainly have invented some, at least, of the statistical methods now used in industrial quality control. He might perhaps have added 1% or so to the industrial productivity of Britain in the early years of this century.

The papers to which I refer are hard to read because Pearson reached his conclusions by algebraical and arithmetical methods which are now seen to be needlessly laborious. Many of them have since been simplified. As a humble tribute to Pearson I have, as I believe, simplified the first of them, which deals with the dissection of a skew frequency distribution into two normal distributions. By an elementary transformation I have thrown his rather formidable nonic equation into a form which allows numerical tabulation, and this tabulation is now under weigh in the electronic laboratory of the Indian Statistical Institute. I hope that as a result, the method will be available to statisticians less pertinacious than Karl Pearson.

Commenting on a particular passage in (I think) one of Beethoven's works, a German musical critic remarked 'Hier ist Titanenthum Pflicht'. (Here titanicity is a duty). Karl Pearson attacked Olympus by piling Ossa on Pelion rather than by seeking an easy path. If we, his successors, have made statistical theory relatively easy, and much of Pearson's mathematics are no longer used, we should remember that we are treading in the footsteps of an intellectual titan.

The germs of many later developments in mathematical statistics are to be found in these papers. Thus, in Contribution III to the theory of evolution, Pearson discussed 'the best value of the correlation coefficient' based on a given sample. He decided on the value which maximized the chance of obtaining the observed sample. This method was developed by Edgeworth as 'the method of maximum credibility', and by Fisher as 'the method of maximum livelihood'. In the succeeding paper, with Filon, Pearson developed it further. Critics have asked why he did not generalize it. I think one possible answer is as follows. The expression 'the best' is unfortunately seldom applicable to statistical estimates. The best for one purpose is not usually the best for another. I think Pearson realized this. Some of his successors have not.

In 1900 Pearson attacked the problem of curve fitting. Having fitted the best available curve to a series of data, for example, the numbers of human beings whose heights were in intervals such as 70–71 in., he asked what was the probability that a sample from a population truly represented by his curve should fit it as badly as, or worse than, the sample in question. If the chance was 1 in 3, there is no good reason to doubt the validity of the theory on which the curve is based. If the chance was 1 in 300, the theory is almost certainly wrong, though it may be a useful approximation for some purposes. But the question arises 'what is a bad fit?' Is 38 a worse fit to an expected number of 30 than 4 to an expected number of 10? (It is not!) And how are we to combine these in an overall estimate of badness of fit? Pearson solved this problem by the invention of the function of observations called χ^2, which increases as the fit becomes worse.

This has turned out to be an immensely powerful tool, and is used on a huge scale. To take one example, in the last number of the *Journal of Genetics*, at least fifty-three values of χ^2 were calculated by three different authors. But now comes the curious and characteristic fact. None of these authors used χ^2 as a test of curve-fitting, and it is very rarely so used. It is used as a test of agreement with hypothesis wherever the hypothesis is tested by counting individuals. And it is used, as Pearson pointed out that it might be used, to discover whether a number of sets of data agree with the same unknown hypothesis. For example, if the total of a number of families contains about 17 % of a particular type we may have had no reason beforehand to expect 17 % rather than any other frequency. But we can use χ^2 to determine whether some of the families have a proportion which diverges more from 17 % than could reasonably be accounted for by chance. If not, but only if not, we can justifiably pool the data. In this case χ^2 is said to be used as a test of homogeneity.

This is, of course, a commonplace with great human achievements. The wheel was invented for use in chariots, carts, and so on. But to-day most wheels are used, not for the support of vehicles, but for power transmission. Perhaps the majority of wheels in England are inside watches. It was absolutely characteristic of Karl Pearson that his intellectual inventions were often extremely general. He obtained a solution of a problem which was of such generality that it had entirely unexpected applications.

But for this very reason it often had a limited applicability to the problem for which it was originally designed. In the last few years many experiments have been done on artificial selection of quantitative characters, particularly in *Drosophila* and mice. Their results during the first few generations are often much as Pearson would have expected. But after this they diverge very greatly. In spite of this they are best described by the use of the mathematical tools which Pearson first applied to such problems, that is to say by describing changes in the moments of character distributions, and simple functions of them such as standard deviations and correlations. One can only defeat Pearson intellectually with the weapons which he himself forged. If I may be allowed to quote William Blake,* Pearson's main service to humanity was

> In all his ancient strength to form the golden armour of science
> For intellectual war.

About the same time he began not only to use data collected by Galton and others, on man and other animals, but to collect his own. Among the important biological results of this period were the demonstration that fertility is inherited both in our own species and in race-horses. As an example of his thoroughness I mention his measurements of the same human bones after various periods of wetting and drying, which never changed their length by as much as 1 %, though they did change it.

It was probably through Weldon that he came to know Galton. This very remarkable man had, among other things, invented the recognition of criminals by finger prints, and psychoanalysis (as may be seen from pages 185–207 of *Inquiries into Human Faculty*). Much of Pearson's work in the '90's was a development of the notions used by Galton in his *Natural Inheritance* in 1889. However, there is no reason to think that Pearson's one serious excursion into practical psychology owed anything to Galton. This is described in a pair of papers published in 1899 and 1902 alleged to be on the mathematical theory of errors of judgement, but in fact incorporating a series of measurements made on the same material by Dr Alice Lee, Dr Udny Yule, and Pearson, and by Lee, Dr Macdonell, and Pearson. Each observer had, of course, a characteristic bias and a characteristic spread round the mean. But what was utterly unexpected was the discovery that the errors made by two observers varied in Pearson's words, sympathetically. In fact in one series, Lee and Macdonell showed a high correlation, Pearson being independent. He attributed this to 'the influence of the immediate atmosphere'. Others might have attributed it to telepathy.

Some of his finest work at this time was with Lee, Bramley-Moore and Beeton on the inheritance of human fertility and longevity. I cannot say more for the value of this work than that I could find no better data on which to base a theory which I published in 1949,

* *Vala, or The Four Zoas* (End of last Night).

and which I venture to think explains some results which Pearson found surprising at the time, though, of course, he published them, and did his best to explain them.

In 1901 the first volume of *Biometrika* was published, partly no doubt because the Royal Society, although it had awarded him its fellowship in 1896 and its Darwin medal in 1898, objected to publishing advances in mathematical and biological knowledge in the same paper! *Biometrika* has not fulfilled what Galton, in its first number (p. 9) stated to be the primary object of biometry, namely 'the discovery of incipient changes in evolution which are too small to be otherwise apparent'. The reason for this failure is simple. The mean rate of increase in tooth length during the evolution of the horse since the Eocene is now known to have been about 4 % per million years. Such evolution could not be detected in a human lifetime. But the aims stated in the editorial introduction, presumably the joint work of Pearson, Weldon and Davenport, were fulfilled. In particular the first number contained a paper by Weldon on variation in snails whose importance he did not live to realize. He found that natural selection in a snail species weeded out extremes, reducing the standard deviation of a metrical character without affecting the mean. We now know that this centripetal selection is very common. Had Weldon lived longer he would presumably have discovered this, and the whole history of biometry would have been very different.

In 1903 Pearson's Department received a grant of £500 from the Drapers' Company, and these grants, at the rate of £500 per year, continued till 1932. In 1903 this sum was worth about £3000 or more to-day, and went partly in the payment of Dr Lee and other computers, partly for instruments, and partly for printing.

I have no idea how Pearson obtained this money. We may be sure that he did not either flatter rich men or promise to improve the national health and intelligence in their lifetimes. Perhaps Galton had the ear of some rich acquaintances. Perhaps too, at that time our ruling classes were less permeated than now with the ferocious contempt for the pursuit of knowledge for its own sake, which is voiced in the Archbishop of Canterbury's sermon of March 24, 1957. To-day it is not hard to get money for research which may have economic, military or hygienic advantages. It is extremely hard to do so for the mere search for truth.

About this time Pearson began the series of papers on human biology for which he is best known in some quarters, and the majority of which, I think, were joint work. Even where his name did not appear on papers, I think our chairman will agree that nothing was published from his laboratory without his *imprimatur*, and some of such work must at least briefly be considered here.

Many of these papers are as fresh to-day as when they were written. To take an example, in my opinion nothing since written on human craniometry has in anyway superseded Pearson's and Davin's great memoir of 1924. Some of this work was, at the time, of inestimable value. I think particularly of the *Treasury of Human Inheritance*. This is still indispensable. Nevertheless, we now know that it is possible to distinguish between conditions (for example, haemophilia, Christmas' disease, Owren's disease, and so on) which were inevitably classed under a single category by the writers of the *Treasury*. The more polemical writings of this period are of less value to-day, as Pearson doubtless realized when he called a series 'Questions of the Day and of the Fray'. The Fray in question was a many-sided contest. On the one hand, Pearson and his colleagues attacked those who underestimated the importance of heredity, including those who exaggerated the harm done by parental alcoholism. But they also attacked those who oversimplified it, including many Mendelians and many eugenists. Other attacks were on statistical data alleged to prove the value of immunization to diseases. These attacks were fully justified. Mental defect is certainly not a Mendelian character. As Pearson and Jaederholm showed, the distribution of intelligence quotients in defectives is the tail of a nearly normal frequency distribution. Forty-three years later we can say a great deal more about it. We can say, for example, that phenylketonuria is a chemically definable character inherited as a Mendelian recessive, and accounting for perhaps 1% of certifiable mental defect. But the mental defect of phenylketonurics is graded, and a few of them are stupid, but not sufficiently so to be classed as feeble-minded. In fact the diagnosis of phenylketonuria enabled Penrose to dissect the distribution of human intelligence quotients into two very different but still overlapping distributions. The notion that such a dissection is possible was Pearson's first contribution to biometry.

Again, one series of memoirs was entitled *Studies in national deterioration*. This is a polemical title. And it is a fact that as regards most measurable characters the nation has not deteriorated. It may have done so as regards its 'nature' or inborn capacities. I think that if Weldon had lived Pearson would have realized the ubiquity of centripetal selection, and that in fact both the most successful and the least successful members of society were breeding more slowly than those a little below the median. It is, however, easy to be wise after the event. Moreover, Pearson and his colleagues were completely right in one respect. Even if, in spite of his predictions, the nation has improved in some measurable directions, it would have improved more if, say, a million children who were born to unskilled labourers had been born to skilled workers, teachers, and the like.

No such criticism is possible of the mathematical tables which he edited, and in whose computation he played a large part. Their utility was well shown by the fact that 'pirated' editions of them were soon published in America. The subsequent development of statistics is largely based on them. Even the advent of the electronic computer has not yet superseded them. They were published from 1914 to 1934, and the *Tables of the Incomplete Beta-function*, published in Pearson's seventy-eighth year, were his last, but not his least, contribution to science. It appears that no one has yet discovered how to use an electronic computer as efficiently as Pearson used his teams of devoted, painstaking, and remarkably accurate, lady assistants.

In 1911 Galton died, and left funds for the endowment of a Chair of Eugenics, of which Pearson became the first occupant. At last he was able to give up the teaching of applied mathematics to engineers and physicists, and in the next year the present laboratory was begun. Fortunately it was completed by 1914 though it was commandeered as an annexe to the hospital, and he did not get into it till after the war. From 1914 till 1919 he did very little but war work, first for the Board of Trade and later on calculation of trajectories of artillery. When in 1920 the Department of Applied Statistics was formally opened, he was sixty-three years old, and he had, among other things, to develop a new course of lectures and practical work. In 1923 he began a series of papers which combined biometrical and historical research. He was able to measure the skulls of a number of distinguished men and compare them with contemporary portraits. In the course of this work he played the detective, and reconstructed the murder of Lord Darnley, second husband of Queen Mary of Scotland; and his comments on the history of the Reformation in Scotland are well worth reading. To the same period belongs his great life of Galton, which involved much historical research.

I have devoted this lecture entirely to Pearson's published works. If he could hear it I believe his main criticism would be that I have said too little about his fellow workers; for much of his work was in collaboration. He had a wonderful gift for inspiring loyalty in his colleagues, of which more will be said to-day by others. I myself only met him frequently in the last years of his life, and can merely say that he was most gracious to me, though my outlook on many biological questions was very different from his own. He resigned his Chair in 1933, but published one book and at least three scientific papers before his death in 1936.

It remains to say a few words about what Samuel Butler would have called his life after death, the results which are still accruing from his original thought. To begin with, all subsequent statistical work is based on the foundations which he laid. If we sometimes find it more convenient to speak of a variance ratio where he would have used a coefficient of correlation this does not mean that his work on homotyposis is obsolete. If his system of frequency distributions is less used for biological data than he might have hoped, yet Gosset, Fisher and others have found that they describe exactly the distributions of many statistical estimates based on finite samples.

At University College his work is being carried on by three professors.* Under his son the Department of Statistics has become the leading teaching department in that subject in Britain, and the new Biometrika Tables, to take only one example of its work, continue his father's great tradition. Prof. Fisher, who succeeded him as Professor of Eugenics, did very great services to statistics in simplifying and rendering more accurate a number of statistical procedures; and, by the application of methods which owed much to Pearson's great memoir on homotyposis, made agricultural experiment an exact art. To mention only one of Fisher's contributions to eugenics, he established a laboratory for human serology, and his interpretation of the human *Rh* antigens has had a considerable influence on the prevention and cure of a very serious human congenital disease. His controversies with his predecessor were perhaps inevitable between two men each so determined to defend the truth as he saw it. Under Prof. Penrose the Department has swung back towards Pearsonian methods. If I may mention two researches which I believe would have delighted Pearson particularly they are the work of Karn and Penrose on infantile mortality as a function of birth weight, which measured natural selection in man with an accuracy which he would have envied, and that of Penrose on abnormality in the offspring as a function of parental age and maternal parity, a subject which Pearson had broached in his 1914 memoir on the handicapping of the first-born.

My own department of biometry has not been so fortunate. I was Professor of Genetics till 1937. I should not have accepted the Weldon Chair had I not been promised accommodation for Biometrical work. Owing to the war, and for other reasons, this promise was not kept. I have been unable to carry out the duties of this chair adequately. In my opinion the best biometric work of the last twenty years has been carried out by Teissier and Schreider in France, and by Mahalanobis and his colleagues in India. In a few months in India I have been able to start new lines of biometrical research. I think particularly of the work of S. K. Roy, now I hope in press, in which he took up the problem of homotyposis where Pearson left it in 1903. A study of some 60,000 flowers from three different plants (to be compared with Pearson's 4443 capsules from 176 poppy plants) has shown that individual plants not only have their characteristic means, but their characteristic standard devia-

* Perhaps I should also include the Professors of Astronomy and Art History, and those of Engineering.

tions, and that both of these alter in a characteristic manner during a season. I believe that the opportunities for Biometric research are now better in India than in Britain, and for this reason among others I have thought it my duty to migrate there. To quote Karl Pearson's most loved poet

In Vishnu-land what avatar?

Whatever the fate of Pearsonian biometry in Britain, I believe that it will live and flower in India.

To me at least there seems to be an element of hypocrisy about the present celebrations. I believe that we should be honouring Pearson more effectively if, to take one example out of many possible, we ensured that the College Library possessed copies of all his works, placed where students could consult them, than by making speeches and eating food. I mention this particular example as I have been trying, without the faintest success, to secure such accessibility for at least ten years.

Pearson's work for free thought and the emancipation of women has been successful in this country, if not always as quickly so as he hoped seventy years ago. The history of art is now taught in this College. His work for socialism has not been as successful here as he hoped. Nor would he have approved of many features of the socialistic systems of the Soviet Union and China. Here again I believe his real heirs are to be found in India, where the editor of *Sankhyā*, the Indian Journal of Statistics, is also the principal planner of the approach to socialism under the second Five-Year Plan.

I fully realize that I have not done justice to my subject. The task set me was an impossible one. No one man now alive could do justice to the breadth of Karl Pearson's interests and achievements. But I thank you for joining with me in celebrating the memory of this great man.