such an instrument open to the world. British surveying instruments have been supplied in the last few years for use on the great Sydney, Zambesi, and Benue bridges, Takoradi Harbour, the Congo Boundary Commission, Egyptian dam construction, the 15-mile Lochaber water tunnel, and the preservation of St. Paul's Cathedral. Important developments of a few years ago in microscopes have been made accessible for the study of bacteria and other organisms, and of structures of the smallest sizes. The recent discoveries of the life history of the filter-passers have been chiefly accomplished in this country as a result of British improvements in instruments. Under the stimulus of Sir Robert Hadfield, a recent metallurgical microscope has been developed which, with other British inventions, has placed British metallurgists in a more favorable position than any of their rivals.

Strong as was the British position in 1925 in optical instruments for scientific research, which are the severest test of competence in manufacture and design, it is said to be even stronger to-day. Although some of the instruments which in 1926 were made only here are now made abroad, it is asserted that in quality no other country can equal them; and other instruments developed more recently in Great Britain are not made anywhere else. Britain is also the only country in which X-ray spectrographs are made by an optical firm of reputation. There is a British firm which makes four distinct types. In this achievement it has been directly helped by the remarkable work of Professor Laby, of Melbourne University, in the analysis of minerals by means of X-ray spectrographs, and the work done by the school of Sir William Bragg and Professor W. L. Bragg.—*The London Times.*

SCIENTIFIC BOOKS

The Life, Letters and Labours of Francis Galton. By KARL PEARSON, Galton Professor, University of London. Vol. III^{A} Correlation, Personal Identification and Eugenics. Pp. xii + 438. Frontispiece, extra plate; tail piece, 42 plates, and two loose charts. Vol. III^{B} Characterisation, especially by Letters, Index. Pp. 4 unnumbered + 232 numbered 441–672. 17 plates. Cambridge (University Press), 1930. 69 shillings for the two volumes.

NEARLY six years ago the first two volumes of this great biography were reviewed in SCIENCE (61: 209). Now, sixteen years after the appearance of the first volume, the work is complete. It will stand for all time as a monument to both subject and author. No other man of science ever had such a biography to preserve his memory.

These two final volumes are really one, as is indicated in the numbering, and are only bound in two for convenience in handling. The same infinity of painstaking care over the details of the production, illustration and documentation that marked the first two volumes is apparent here. In spite of advancing years it is still a firm and sure hand that penned these volumes.

This last volume deals, in its later parts, with the portion of Galton's life which was known at first hand by many persons now living. Obviously this has both advantages and disadvantages for a biographer (and in a minor degree for his reviewers). In this case it has led to a result which will be welcomed by Professor Pearson's old students. It is that the particularly full account of the last decade of Galton's life carries along with itself a good deal of autobiography of its writer. Regarding this Pearson says (III^{A}: vii):

> One apology I must make if the reader feels that in the chapter on the last decade of Galton's life the biographer has introduced too much of himself. To me that last decade was essentially bound up with our joint work for a subject we both had closely at heart; and I believe that for Galton himself our common aim—the establishment of Eugenics as an accepted branch of science—was a leading, if not the principal, purpose of those years. My own enthusiasm may possibly have deceived me, but I believed that Galton during that decade lived more in the struggles and difficulties of our infant Laboratory than in any other phase of his wide interests. The sympathy and help he always so readily tendered to his friends may again have misled me, but I think the history of the Laboratory he founded and finally endowed was also the essential history of his own life in those last years. At any rate such is the aspect of Galton's many-sided nature that I then saw most closely, and it is accordingly that which I am best fitted to render account of. To me his final crusade for eugenic principles was the crowning phase of a life whose labours in medicine, evolution, anthropology, psychology, heredity and statistics directly fitted him to be the teacher and prophet of the new faith.

There are three chapters in Volume III^{A}. The first of these deals with correlation and the application of statistics to the problems of heredity. Galton's first experiments on heredity began in 1875 with plants. He first attempted to get the relation between the weight of mother and daughter seeds of some plant like cress. Meeting with no success he turned to sweet pea seeds in the same year. From data in a notebook

of the time Pearson reproduces the first regression ever calculated, that between the diameters of parent and offspring peas. He comments as follows (p. 3):

It is strange that both Galton and Mendel should have started from peas, the former from sweet and the latter from edible peas. Galton tells us distinctly why he chose the former, namely because he would not be troubled to the same extent by variation in size of peas within the same pod. We must leave it to the future to judge whether the correlational calculus, which has sprung from Galton's peas, is or is not likely to be of equal service with the vast system of factorial genetics which has arisen from Mendel's peas—and this even in the theory of heredity. We see now what Galton might have done, he might have provided us with data to check Johansen's later bean-weight experiments, he might have thrown light on the "pure line." He might possibly have reached the correlation coefficient instead of the regression slope in his first attempt to get a measure of correlation. Whatever he might have done, he reached the idea of regression before he reached that of the coefficient of correlation. As long as he was dealing with heredity in the same sex, the approximate equality of variabilities in the two generations preserved him from any great error.

Following the plan used in Vol. II, Pearson proceeds in this first chapter to analyze, digest and criticize Galton's biometric contributions to the problems of genetics. His ideas on correlation and heredity culminated in "Natural Inheritance," published in 1889, when he was 67 years old. Pearson says that this book created Galton's school, and started Weldon, Edgeworth and himself off on the study of correlation.

Galton believed that evolution proceeded chiefly by discontinuous steps, through the occurrence and preservation of what we now call mutations, and he called "sports." He believed that while what he called "variations proper" were inherited, they were of no importance in evolution because of "the constant tendency to regress." Pearson points out that Galton's methodology had led him astray here. He says (p. 83):

In other words there is no "unexpected law of universal regression." Regression in Galton's sense arises solely from the fact that by clubbing into a single array the offspring of all fathers of a given character deviation he has given them not only mothers whose average stature will be mediocre, but also a mediocre ancestry. . . . Shortly, there is no law of "universal regression," and we can deduce from Galton's own theory that his "variations proper," if selected and inbred, would establish a breed with a "new center of regression." It is of course more than probable that our new center of regression, *i.e.*, the type of our new breed, may be unsuited to survive, that is to say in Galton's sense may be unstable. One can not alter one character in an organism without modifying all the correlated characters, and some of these altered are likely to have survival value. But Galton's own data and Galton's own theory rightly interpreted lead to no "universal regression," still less to an argument that "variations proper" can not be the subject of selection and the formation of new breeds.

This does not prove that "variations proper" have been the basis of evolution, but it removes Galton's chief reason for belief in evolution by discontinuity, that is by sports or mutations. The law of "universal regression"—over which Galton undoubtedly stumbled—is only true when we neglect ancestry beyond the parents and suppose mating at random, but these are not the conditions which exist when intense selection is taking place and the selected interbreed.

There is much meat in this first chapter for the biometrician, and also some highly amusing bits, like the "average flush of excitement," and the problem of how to cut a round tea cake, the consumption of which was to extend over three consecutive days, in such manner as to leave a minimum of exposed surface to dry. Galton solved the problem; Pearson is suspicious that Galton and his niece did not eat all the cake according to the solution, because on the third day roughly four sevenths of the residue to be eaten was dry rind. A mere American's experience with English tea cake, extending at odd intervals over more years than he likes to think of, leads him to the view that the whole problem was supererogatory. No known English tea cake ever had sufficient moisture at any stage of its evolution or degustation to make the manner of its cutting of the slightest importance!

The end of the chapter recounts, with much reserve, the story of the Royal Society's "Committee for the Measurement of Plants and Animals." The committee was appointed at Galton's suggestion in 1894. The members were Galton (chairman), Francis Darwin, Macalister, Meldola, Poulton and Weldon (secretary). In 1896 Pearson was added. This group wanted to measure plants and animals; in short to do biometry. But from the start they were pestered by a group of unbelievers, led by Bateson. In 1897 some nine new members, zoologists and breeders, were added, including Bateson. The committee was renamed, "Evolution (Plants and Animals) Committee of the Royal Society." By 1900 Pearson and Weldon had resigned, and later Galton dropped out. The victory, for the other side, was complete. The Reports of the Evolution Committee became the vehicles of publication for the major part of Bateson's early Mendelian experiments.

The second chapter deals with Galton's finger-print work. Finger prints were a major interest and occupied much of his time between 1887 and 1895. Pear-

son gives first an account of the history of the use of finger prints for personal identification and its official adoption in England. Following this comes the story of Galton's attempts to popularize the method, and finally there is a résumé of his scientific contributions to the subject. This chapter is very fine. It lays down and documents a record which will make it forever impossible to take from Galton's memory the credit for building the foundation upon which all subsequent work on finger prints has been based, and particularly that part of it involving the official use of finger prints in criminal identification. The history of the matter is interesting. In the popular mind there is a wide-spread tendency to think of Bertillon as the originator of the use of finger prints for identification. As a matter of fact Galton introduced Bertillon to the method, as the latter himself acknowledged in a letter written in 1891. Actually the first *use* of finger prints for administrative purposes had been by Sir William Herschel in India as early as 1858. The merits of the claims of a Dr. Faulds to have been the pioneer in the field, put forward in definitive form in 1905, belittling the work of Herschel and Galton, are dealt with by Pearson as they deserve to be. The chapter ends with the following words (p. 215):

> The reader who has had the courage to follow Galton's biographer through the intricacies of this chapter will, I am sure, be convinced not only of the labor Galton devoted to his finger-print studies but also of the amazing energy he exhibited in acquainting not only administrative bodies but the public at large with the possibilities which then lay hidden in finger-printing, and this not solely for scientific but also for practical purposes. If the reader can find anyone who before 1895 had published a tithe of what Galton had issued on this topic, then I will admit him also to be a pioneer; if he can find anyone who has since 1895 done more than amplify in minor, often in very minor points Galton's work, then I will admit him a worthy successor to Galton.
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> Finger-printing as a science and finger-printing as an art are both alike the product of Galton's insight, ingenuity and tireless activity; the attempts to belittle the credit due to him can only spring from those who for their own purposes choose to ignore the literature of the subject.

The third and last chapter, the longest in the whole work, is entitled "Eugenics as a Creed and the last Decade of Galton's Life." It will form the prime source authority for future students of the eugenics movement which flourished in the first half of the twentieth century. Galton started it. Pearson has lived through the flow of the tide. To this chapter all future workers will turn, who may be curious to know what happened. Furthermore, to those now living who are interested in and at all acquainted with the politics of science in England since 1900, this chapter will furnish mildly spiced reading. The battle royal between the Bateson and the Pearson schools, in which, as usual, neither side got all the spoils, but each got some, is handled with extreme discretion, but still with a sufficiently forthright clarity.

As a piece of historical writing this chapter is of great value. But one is reminded of a remark which has been attributed—I believe correctly, though I have not succeeded in running down the exact *locus* of the quotation—to one of England's greatest sons. It is this: "Science commits suicide when it adopts a creed." The story of the last ten years of Galton's life emphasizes the fact that these eight words quoted contain much wisdom. "Eugenics as a creed" makes its appeal to crusaders. Its idols tend to be either those of the forum or those of the cathedral. The record plainly shows that Galton became successively annoyed, harassed and finally appalled by some of the consequences of the thing he had started. Towards the end of his life he withdrew himself just as completely as possible from any connection with organized eugenics, with the exception of the Eugenics Laboratory under Pearson's direction. If these four great volumes before us demonstrate anything about Galton it is that he was enormously more a pioneer than a crusader.

The time has not yet come when an entirely realistic discussion of the origin and development of the eugenics movement will be possible. It is much too near us. Galton, in 1907, gave the money necessary to start the Eugenics Laboratory at University College. Later, in a codicil to his will, he permanently endowed it. That laboratory has made contributions of great value to the science of human genetics, under the guidance of Professor Pearson. This is what Galton primarily wished to accomplish by his gift. In his forty-second year he said: "I shall treat of men and see what the theory of heredity of variation and the principle of natural selection mean when applied to man," and, as Pearson remarks, this treatment only ended with his life. A scant three months before his death he wrote of the work of Eugenics Laboratory: "I hold it to be thoroughly scientific and most valuable, and I rejoice that I was its founder."

Volume III[B] includes one long chapter composed chiefly of letters, and a very detailed and thorough index of the whole work. The letters are, for the most part, family letters, and are delightful. The index is a model.

And so comes to an end a remarkable, indeed a unique, piece of biographical work, a fitting and adequate record of a great man.

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