I succeeded at once in obtaining it, when the condensed discharge was used. This glow has many interesting properties, of which a preliminary publication seems desirable.

I believe it to be due to pure nitrogen. Lewis states that it cannot be obtained from atmospheric nitrogen, but this does not agree with my experience. I have used atmospheric nitrogen exclusively.

The glowing nitrogen is unaffected by silver gauze, which quenches the ozone glows. It is destroyed by mixing oxygen with it, but merely diluted by hydrogen or ordinary nitrogen. When acetylene is led in, a bright flame is produced at the point of confluence. This flame replaces the original glow. It has a spectrum consisting of the swan and cyanogen bands, along with others not identified. If the nitrogen glow is led over iodine a magnificent blue flame is produced, contrasting sharply with the original orange glow. With sulphur the original orange glow is quenched, but no other replaces it. The sulphur becomes hot, and a metallic-looking sublimate is formed along the tube.

The most remarkable phenomena, however, are with metallic vapours, which give line spectra when the glowing nitrogen is led over them. Sodium, potassium, thallium, mercury, zinc, cadmium, and magnesium have all yielded line spectra in this way.

Investigation is being pushed on as fast as possible, but the facts so far obtained seem to point to the production of a chemically active modification of nitrogen. It is suggested, provisionally, that the spectra are developed by the chemical union of this active nitrogen with the various metals and with iodine and acetylene. The orange glow obtained with nitrogen only would, on this view, be due to the transformation of the hypothetical active nitrogen into ordinary nitrogen. R. J. STRUTT.

Imperial College of Science and Technology,
January 30.

Singularities of Curves.

I HAVE not, at present, access to the books referred to by "T. J. I'A. B." in his letter of January 12; but he is altogether wrong in thinking that the singularity he mentions cannot be investigated by the methods explained in my "Geometry of Surfaces." An arbitrary line through the origin has sextactic contact thereat; but since the axis of x has 12-tactic contact at the origin, the latter cannot be an ordinary sextuple point, because no line through such a point can have a higher contact than septactic. The singularity is either a singular point of the sixth order or one of lower order with coincident branches passing through it, and it illustrates the necessity of drawing a distinction between ordinary multiple points and singular points. The trilinear equation of the curve can be obtained by eliminating t between $\beta=\alpha t^6$, $\alpha\gamma-\beta^2=\beta^2(t^3+t^4)$. The factor $\alpha\gamma-\beta^2$ suggests the existence of tacnodal or other branches of a similar character, and that the singularity might be transformed into a simpler one lying on a curve of lower degree than the sixteenth by using Cremona's transformation,

$$\frac{\alpha}{\alpha'\gamma'+\beta'^2}=\frac{\beta}{\beta'\gamma'}=\frac{\gamma}{\gamma'^2}$$

before applying the methods of chapter iv. of my book.

But it would have been foreign to the plan of my treatise to have introduced parametric methods when discussing singularities; moreover, the method of which the example is an illustration is only applicable to unicursal curves, whereas my own methods are independent of the deficiency. For example, the various singularities the point constituents of which are nine nodes could not be investigated by means of a unicursal curve without complicating the problem by introducing additional nodes isolated or in combination sufficient in number to reduce the deficiency to zero; and this might limit the generality of the investigation, for when the nodes exceed a certain number they are not arbitrarily situated, but lie on one or more dianodal curves. A. B. BASSET.

January 14.

MR. BASSET now admits that he has seen neither Zeuthen's two papers of 1876 nor Jordan's book of 1893, thus practically acknowledging the accuracy of my criticism—that the treatment of singular points in his "Geometry of Surfaces" is incomplete. With this admission from Mr. Basset the matter ends, so far as I am personally concerned.

But I must enter a protest against Mr. Basset's inference that the methods of Zeuthen and Jordan are only applicable to unicursal curves; since Mr. Basset has not read the work in question, his only reason for this statement is the fact that the example in my first letter happens to be a unicursal curve. This example was made up so as to provide a simple illustration of the general methods; but these methods hold good for curves of any deficiency.

It is absurd to suggest that parametric methods cannot be used for any algebraic curve; of course, the coordinates are expressed in the form of infinite series (convergent near a particular point of the curve) instead of terminated series. Mr. Basset's objection to using parametric methods would be quite justified if he had provided us with a satisfactory substitute; but he gives no *systematic* plan for resolving an assigned singularity, and this is the main object of the parametric method as used by earlier writers. T. J. I'A. B.

FRANCIS GALTON.

FEBRUARY 16, 1822—JANUARY 17, 1911.

THE death of Francis Galton marks, not only the removal of another link with the leaders of the great scientific movement of the nineteenth century—represented by Darwin, Kelvin, Huxley, Clerk-Maxwell, and Galton in this country—but something far more real to those who have been in touch with him up to the last, namely, the cessation of a source of inspiration and suggestion which did not flag even to the day of his death. The keynote to Francis Galton's influence over the science of the last fifty years lies in those words: suggestion and inspiration. He belonged to that small group of inquirers, who do not specialise, but by their wide sympathies and general knowledge demonstrate how science is a real unity, based on the application of a common logic and a common method to the observation and treatment of all phenomena. He broke down the barriers, which the specialist is too apt to erect round his particular field, and introduced novel processes and new ideas into many dark corners of our summary of natural phenomena.

The present writer remembers being asked some years ago to provide a list of Francis Galton's chief scientific achievements for use on a public occasion. It did not seem to him that a list of isolated contributions, such as the establishment of anthropometric laboratories, the introduction of the composite photograph, the transfusion experiments to test pangenesis, the meteorological charts and improved nomenclature, the practical realisation of the possibilities of finger-print identification, the demonstration of the hereditary transmission of the mental characters in man, the law of regression, the idea of stirps, or the foundation of the novel science of Eugenics, fully represented the nature of the man. What is the spirit of the contributions—large and small, almost two hundred in number—which Francis Galton made to the science of the last sixty years? [1] The unity of those contributions lay largely in the idea that exact quantitative methods could be applied, nay, rather must be applied, to many branches of science, which had been held beyond the field of either mathematical or physical treatment. In this manner his inspiration and suggestion tended to give physical and mathematical precision to a large number of outlying sciences, to meteorology, to anthropology, to genetics,

[1] His first contribution dates from 1849 and concerns a method of printing telegraphic messages at the receiving station.

and to sociology. In this idea itself there is nothing novel; many of the world's great minds have realised the same truth. What did Roger Bacon say towards the middle of the thirteenth century?

"He who knows not mathematics cannot know any other science, and what is more, cannot discover his own ignorance or find its proper remedies."

How was it echoed again, full two hundred years later, by Leonardo da Vinci?

"Nessuna humana investigatione si po dimandare vera scientia s'essa non passa per le mattematiche dimonstrationi." *Libro di pittura* i. 1.

We wait another century and hear Lord Bacon's aphorism:—

"The chief cause of failure in operation (especially after natures have been diligently investigated) is the ill-determination and measurement of the forces and actions of bodies. Now the forces and actions of bodies are circumscribed or measured by distances of space, or by moments of time, or by concentration of quantity, or by predominance of virtue; and unless these four things have been well and carefully weighed, we shall have sciences, fair perhaps in theory, but in practice inefficient. The four instances which are useful in this point of view I class under one head as *Mathematical Instances* and *Instances of Measurements*."

The words actually used by Lord Bacon for his third and fourth instances are "per unionem quanti aut per prædominantiam virtutis." They cover very fully the sociological, psychological, and genetic phenomena which Francis Galton kept so closely in view.

Another hundred years, and again a great thinker echoes the same idea:—

"Ich behaupte aber, dass in jeder besonderen Naturlehre nur so viel eigentliche Wissenschaft angetroffen werden könne, als darin Mathematik anzutreffen ist." Kant: *Metaphysische Aufangsgründe der Naturwissenschaft*. Sämmtliche Werke, Bd. iv., S., 360. Leipzig, 1867.

Lastly, coming down to our own age, the great contemporary of Galton, Lord Kelvin, wrote:—

"When you can measure what you are speaking about and express it in numbers, you know something about it, but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind."

Clearly, then, Francis Galton was far from originating the idea that exact quantitative methods are applicable far beyond the range of the physical sciences. Wherein lies then his significance for the science of to-day and, perhaps, more still for the science of the future? Not solely in the fact that he sketched in broad lines the manner in which quantitative methods could be applied to many branches of descriptive science, but that without being a professor or teacher of students, he succeeded in creating a school of enthusiastic disciples who, inspired by him, have carried his work and his suggestions into practice in craniometry, anthropology, sociology, genetics, and medicine. The elements in Galton's character and life which made this achievement possible for him are manifold. Heredity, tradition, education, economic independence, all played their parts, and not least among these stands hereditary temperament. No younger man who knew Francis Galton at all intimately failed to be influenced by his marvellous keenness, his wide but wise generosity of suggestion and practical help, and above all, his equable and lovable personality. His manifest pleasure and gratitude for the simplest little thing done for him; his complete respect for the time and duties of others, whether they were his friends or the servants of his own household, produced a reverence which worked its effect, not only on his immediate environment, but upon the men who carried his inspirations and suggestions into practical science.

The exact biological bearing of religious differentiation upon the creation of human types has, perhaps, never been fully studied. The doctrines of George Fox drew together many men and women of a kindred spirit, and the stringent regulations as to outside marriage led not only to a union of similar natures, but, we venture to think, almost created a biological type. Not only did the Society of Friends unite men religiously, but it produced special temperaments genetically. Even to this day it is strange how men whose families have ceased to be Quakers, yet find that their common sympathies and temperaments arise from Quaker descent. Galton owed the evenness of his temper, his placid acceptance of criticism, but his power of steady persistence in his own work and his own views, very largely to his Quaker ancestry, to the Galton and Barclay blood. The fact that Galton was never in controversy was, of course, partly due to the novelty of many of his methods and ideas; they were beyond his generation, which left them largely on one side. Even his work on the heredity of the mental and moral characters in man was looked upon as merely academic, and its real bearing on social habits is only now being realised and pressed home.

For one man who had read "Hereditary Genius" (1869), "Human Faculty" (1880), and "Natural Inheritance" (1889), there were ten who had studied "The Origin of Species" or "Man's Place in Nature." But the former were the natural sequel to the latter, and Galton realised at once not only, as Darwin and Huxley did, that the new doctrines applied to man, but also that they must eventually be preached as a guide to human conduct in social activities. Looked at from this aspect, his labour to make anthropometry in both its physical and psychical branches an exact science; his discovery that new types of analysis are wanted to replace mathematical function in biological and social studies, and lastly, his advocacy of Eugenics—the science of the right breeding and training of man—are seen to be successive steps in a continuous ascent. The positive conception that science exists to serve man, and that its highest function is not merely to supply his material wants, but to show him how to elevate himself by obedience to biological principles, was the crowning conception of his life. He lived to see the wide appreciation of his teaching in both Germany and America, and, to perhaps a lesser extent, in Great Britain. But he did not live to see the controversies which will inevitably arise, as the world in general more clearly realises that not all its customs, not all its beliefs, not all its supposed morality and charity, are consonant with scientific knowledge.

But if the fact that Galton was never in controversy had partly a basis in the historic evolution of ideas, it was also deeply rooted in his temperament, the temperament of one portion of his stock. He considered criticism, not as it affected the reputation of his own work, but as it affected his own estimate of the validity of his own work, and he adopted it or passed it by accordingly. Only once do I remember on a public occasion a slight severity in his usually gentle tone. A medical man of distinction, speaking obviously without any knowledge of the literature of the subject, had asserted that the supposition that the children of parents with certain mental and moral peculiarities would reproduce these features, arose from a totally false conception of what the laws of heredity are. The mental and moral aptitudes were for the speaker outside the purview of hereditary investigation. Galton's reply was very simple: Much of what his critic had said "might have been appropriately urged forty years ago, before accurate measurement of the statistical effects of heredity had been commenced, but it was quite obsolete now."

That is the extreme limit to which Galton's Quaker temperament ever, in the presence of the present writer, allowed him to reply, and here it was a question of checking a vague assertion which swept away the best part of a man's life work unexamined. That this calmness of mental attitude was very largely innate and not due to environment, is well brought out by a quaint little biography of the first eight years of his life, written by his mother (Violetta Galton—half-sister of Charles Darwin's father) when he went to a boarding-school in 1830.[1] His after-tastes and temperament, his great good nature, his calm temper, his resourcefulness and courage,[2] are sufficiently indicated by a mother who was closely observant, but who could have no knowledge of the future distinction of her youngest child. A further fundamental factor of Galton's mental outfit was his extraordinary mechanical ingenuity. This may also have been a Darwin heritage, for it has been shown by other members of the stock. At the same time his paternal grandfather, Samuel John Galton, was not only a statistician, but a man of mechanical tastes and a friend of Boulton and Watt, and the same form of ability was markedly evidenced in another grandson, Sir Douglas Galton.

Francis Galton had the mechanical ingenuity which makes a great engineer or experimentalist; his suggestions were always of the simplest kind, and he used the simplest constructions and the simplest materials. Most of his friends will remember his delight in some almost primitive solution of a mechanical difficulty, that possibly they had themselves pondered over and brought to him in despair. Nothing worries the secretary of a scientific society or the editor of a journal more than the vagaries of an author who provides diagrams wholly unsuited to the page-size of their publications; Galton would be ready with a photographic method of modifying the linear scales in different ratios in two directions. Nothing is more trying at lecture or theatre than the tall person or hat; Galton had his "hyperscope"—a simple tube with two reflecting mirrors at 45° by which he saw over or round them, and he would use it in a crowd when he wished to see what was going on beyond it. Or he would carry a wooden brick in a parcel with a long string attached to it; slowly lowering it in a crowd, he would stand on his block of vantage, and raise it again by its string afterwards without attracting observation. Elsewhere it has been said that, if one wanted to put a saddle on a camel's back without chafing it, to manage the women of a treacherous African tribe, to measure a snail's shell, or to work a theodolite in the midst of London traffic, Galton would tell you how it might be done.

Beyond mechanical ingenuity[3] he had great wealth of illustration; what he could possibly represent to the eye, he would do, for he had a firm belief that graphic representation is more impressive than mere numbers. Within a fortnight almost of his death, seated outdoors in a shelter, he was discussing with the present writer as eagerly and keenly as he would have done twenty years ago, the best method of graphically representing and comparing typical racial crania.

Through the last years of life, apart from his eugenic work, he was very busy in trying to deduce quantitative measures of general likeness; evidences of this were given in his letters on portraiture to this *Journal*, and in his attempts to make a graduated scale of "blurrers," which like a photometric wedge would equalise divergence until differentiation of the two compared portraits became impossible. Photographs of members of the same family—"similar and similarly situated," as the mathematicians have it—"blurred" more readily than those of strangers in blood. These things amount, not to complete fulfilments, but to suggestions and inspirations. But Francis Galton realised among the earliest that a comparison of the individual organs and characters of local races needs supplementing by a comparison in some manner of two "index" numbers, which by their deviation shall measure the similarity or diversity of these races, each as a unit complex of many individual characters.

Judged from the modern specialist standard, Galton was, perhaps, not a "mathematician," but he had enough mathematics for most of the purposes of scientific observation, and he knew how to enlist mathematical aid when he required it. Few of those who have really studied his work or come in contact with his singularly clear and logical mind, would have wished his education other than it was. The training in observation provided by hospital clerking under a good clinical teacher, could never have been replaced with profit by years spent over symbolic analysis; the man who would patiently watch the workman in a foreign country plying his chisel or trowel in order to learn differentiation of method in craftsmanship, and then take a lesson himself in handling the tool in the native way, was a born observer, whose talents lay in other fields than the higher abstract analytic. Yet the essential feature of his work was, and his reputation with the future will largely depend on, his extension of analytical methods to the descriptive sciences. Without Gauss the work of Quetelet would have been impossible. Without Quetelet we should perhaps have missed Francis Galton, and from Galton and his school the new methods have spread, and are spreading into the most varied branches of science; in medicine both treatment and diagnosis will be influenced by them, in physiology and psychology their advantages are being admitted, in biology, anthropology, sociology and its latest offspring—eugenics—their importance has been fully recognised. And wherein does the validity of this new treatment consist? It lies very simply in this, that Galton following Quetelet recognised that causation expressible in terms of mathematical function was not the only, or even the chief category, under which men of science can work; that exact methods were applicable to that looser relation or association, which now passes by the name of correlation. To Galton is due the honour of having reached the first simple measure of this relationship, and in the earlier writings of his keen disciple Weldon, we find it called "Galton's Function," a name which had to be dropped as the conception became more general and its types differentiated and classified. It ceased to be possible to call after its discoverer a philosophical category wider than that of causation, and embracing causation as a subclass.

The history—at least, the formal history—of his discovery is very suggestive of the man and his method. He had been studying the size of organs in parents and their offspring, and he formed what is now termed a correlation table; that numerical table he sought to

1 Would it be safe to suggest that Galton inherited from his Darwin mother his views on family history? Is "The Life History Album" (Macmillan, 1884 and 1903) with its spaces for observations and photographs of the child, a lineal descendant of this biography with silhouette illustration?

2 This was of much value to him in his later travels. When five years old his mother took him into a field where the servants were trying to catch some geese. Francis immediately ran among them and seizing the old gander by the neck brought him to his mother muttering at the same time to himself the lines from "Chevy Chase":

"Thou art the most courageous knight,
That ever I did see —— ——"

3 Many of the contrivances devised for his first Anthropometric Laboratory are still in current use.

represent graphically, and to his delight and surprise the rough contour lines, which he drew on the table itself, had the appearance of a family of similar and similarly situated ellipses. The line which joined the means of the organs of the offspring for a given organ in the parent was seen to be straight, and to be the locus of the points of contact of a system of parallel tangents to the ellipses. Galton had reached from his graph the fundamental idea of the simplest type of correlation surface—the generalised Gaussian with linear "regression," and he was not slow to realise its great importance and its wide application to the inter-relationship of contemporaneously varying or associated phenomena. He summoned mathematical aid, and with the help of Mr. Dickson determined the form of the Gaussian frequency surface. Years afterwards it was discovered that the mathematics of that surface had been worked out by Bravais, in considering the distribution of shots over a target. Nowadays we know that there are frequency surfaces which are not Gaussian. Wherein then does the transcendant importance of Galton's work lie? Why, in the fact that he was *not* considering shots at a target, but that he was seeking for a key to open a door for exact quantitative methods into the whole wide range of vital phenomena. From Bravais' mathematical treatment of the Gaussian surface nothing followed, until Galton independently rediscovered it with no idea of shots at a target in his mind, but with the idea of investigating problems in genetics, in evolution, and in sociology.

His work first pointed out to us how the whole field of nature lay open to exact numerical treatment, if we dropped the category of causation and adopted that of correlation.[1] Not from Bravais' mathematics, but from the suggestion and inspiration of Galton's contour lines on his table of observations, has sprung the whole body of modern statistical theory. The problem of evolution, and the study of heredity, were for Galton actuarial problems. Needless to say, he did not place on one side the study of individuals, he was ever in sympathy with individual observation and experiment. But, as the late Prof. Weldon expressed it in a sentence which had Galton's hearty assent, "the actuarial method must be an essential part of the equipment of any man who would make and understand such experiments." It was in this very sense that Galton initiated the Royal Society "Committee for conducting Statistical Inquiries into the Measurable Characteristics of Plants and Animals." And for a long time he had in mind the eventual foundation and endowment of an experimental station for variation, heredity, and selection, treated by statistical methods. If his gift to posterity be now found to have taken another form from his original idea, the change is not unassociated with his views on the need for adequate statistical treatment, or with the change of purpose and method which led to his withdrawal from the Evolution Committee.

If we turn from the inspiration and suggestion provided by Galton in many varied forms of inquiry to his actual contributions to our knowledge, two will occur to the minds of most readers, not necessarily because they are the most important, but because some statement of them has crept into elementary text-books and popular works on science. The first of these is the oft-quoted "Law of Regression"; it was not originally a theoretical deduction but deduced by Galton from his own measurements and observations on individuals. It amounts to the statement that if in a stable population—*i.e.* one in which no selection is taking place, and which is mating at random—a group of all the parents be selected which have a character of a given intensity, then the average of the same character in their offspring will be nearer to the mean of the whole population than the parental value. As Galton stated this statistical result, it has been over and over again verified by mass-investigations. But it has been singularly often misinterpreted by commentators. One group of them extended it into a general law that all populations tend to regress to mediocrity, if we suspend natural selection; they quite overlooked Galton's statement that the population was *stable*. No such general regression to mediocrity was involved in Galton's law of regression; it was a statistical law of distribution of offspring resulting from the *stability* of the population. Another group of critics selected certain special parents, overlooking Galton's word "all," and endeavoured to show that the law did not apply to their offspring, and must therefore be erroneous. The fact is that the very law itself, when applied to the offspring of somatically selected ancestry and not to all parents of the class, shows the cessation of regression, and it is upon this very cessation of regression for selected sub-classes that the general stability of the Galtonian population depends.

The second contribution to the theory of heredity with which Galton's name has been generally associated is that termed the "Ancestral Law of Heredity." The conception Galton had in mind was the following one: in a population mating at random and stable in character, what would be the *average* relation of each class of individuals in the new generation to each grade of their ancestry? Naturally, he measured the relation by his new method of correlation, practically by aid of the steepness of his regression lines. The degree of resemblance to successive grades of the ancestry was found to diminish in a geometrical progression. The exact numbers reached by Galton from his data ($\frac{1}{3}$, $\frac{1}{9}$, $\frac{1}{27}$, &c.) have not been verified by further observation. But the fundamental features of his method, the idea of applying multiple regression and the diminution of the degree of resemblance in a geometric series, have been found correct. Indeed, we now realise that almost any determinantal theory—including that of Mendel—leads directly to Galton's Law of Ancestral Heredity as stated above. No direct test of adequate[1] character has yet been made on Galton's Law, as it is commonly cited—a form which he originally stated himself with great hesitation ("Natural Inheritance," p. 136), and which does not appear wholly in accord with other parts of his observational or theoretical treatment. Strange as it may seem, no one has yet worked out the relationship corresponding to the usually stated form of Galton's Law for a simple Mendelian population breeding at random; the theoretical investigation of it is beset with many analytical difficulties and not a few logical pitfalls. All the criticisms of this law have turned on results deduced from selected gametic ancestors.

It has been asserted with some plausibility that Galton's deductions would cease to be of any value

[1] "The conclusions . . . . depend on ideas that must first be well comprehended, and which are now novel to the large majority of readers and unfamiliar to all. But those who care to brace themselves for a sustained effort, need not feel much regret that the road to be travelled over is indirect and does not admit of being mapped beforehand in a way they can clearly understand. It is full of interest of its own. It familiarizes us with the measurement of variability and with curious laws of chance that apply to a vast diversity of social subjects. This part of the inquiry may be said to run along a road on a high level, that affords wide views in unexpected directions, and from which easy descents may be made to totally different goals to those we have now to reach. I have a great subject to write upon, but I feel keenly my literary incapacity to make it easily intelligible without sacrificing accuracy and thoroughness."—(Francis Galton, "Natural Inheritance," 1889, p. 2). It is those "easy descents" to "totally different goals" which have proved very arduous, not because they were not obvious and easy so soon as the "high level road" had been made, but because hey turned out to lead into strictly preserved but largely untilled "strays."

[1] Certain investigations have been made, but in every case they will be found not to fulfil the conditions as to average relations, which Galton laid down. Galton's own material for "Basset Hounds" is really inadmissible, for there is scarcely any doubt about the fictitious character of many of the putative sires.

if we could discover the physiological causes of heredity. To this, we think, answer may be made that Nature does not work like the breeder by testing gametic qualities. She proceeds by selecting with stringency certain grades of somatic qualities, and the intensity of quality, not the gametic value of the individual is her index to survival. Without some degree of correlation between somatic character and gametic value, the Darwinian theory must collapse. This point Francis Galton had ever in mind, and his views on heredity, and his treatment of the subject, always turned on the effect of somatic selection of the ancestry in modifying the somatic characters of the offspring. Hence the establishment of a definite theory of physiological heredity would at once have to be followed by a theoretical deduction from that theory of the degree of resemblance between somatic characters in ancestry and offspring in a population living under natural conditions. The questions of fertility and death-rate in such a population are actuarial studies. No physiological inquiry as to heredity can supersede those studies, but such an inquiry may well confirm, or it may modify, the laws originally stated by Francis Galton for populations mating at random. So far as it is possible to judge at present, current physiological theories of heredity tend rather to confirm than refute Galton's conclusions.

Of the work of the last decade of Galton's life, it is possibly too early yet to speak with any decisive judgment. Darwin, writing to Wallace in 1857, uses the following words :—

"You ask me whether I shall discuss 'man.' I think I shall avoid the subject as so surrounded with prejudices, though I fully admit it is the highest and most interesting problem for the naturalist."

Darwin's later writings testify that he did not avoid the subject, but probably the existence of the prejudices to which he refers prevented him from accentuating the direct practical bearing of the doctrine of evolution on human conduct. The result of this attitude of the earlier evolutionists was that their strength was opposed to one wing only of the army of intellectual inertia. Their critics were theologians and metaphysicians; there was no question raised of the bearing of evolution on social habit. Evolution appeared merely as a problem of a man's intellectual attitude towards the universe, it was a philosophical belief, not a practical code of conduct. Francis Galton's Huxley lecture of 1901 "On the possible Improvement of the Human Breed under existing conditions of Law and Sentiment," slender as it seemed at the time, was really the clarion call which told us that the time was ripe for the recognition that the doctrines of evolution and heredity were more than intellectual belief, they were destined to control the conduct of men in the future and determine the relative efficiency of nations. Others may have thought, some may have said, the same thing before;[1] but to Francis Galton belongs the credit of having said it at the psychological moment, and said it with the emphasis that made many earnest men and women understand its gravity. Later, in his paper of 1904, "Eugenics: its Definitions, Scope, and Aims," Galton more closely defined the lines of development he had in view for the new science :—

"Persistence in setting forth the national importance of eugenics. There are three stages to be passed through: *firstly*, it must be made familiar as an academic question, until its exact importance has been understood and accepted as a fact; *secondly*, it must be recognised as a subject the practical development of which deserves serious consideration; and *thirdly*, it must be introduced into the national conscience, like a new religion. It has, indeed, strong claims to become an orthodox religious tenet of the future, for eugenics cooperate with the workings of Nature by securing that humanity shall be represented by the fittest races. What Nature does blindly, slowly, and ruthlessly, man may do providently, quickly and kindly. As it lies within his power, so it becomes his duty to work in that direction; just as it is his duty to succour neighbours who suffer misfortune. The improvement of our stock seems to me one of the highest objects that we can reasonably attempt. We are ignorant of the ultimate destinies of humanity, but feel perfectly sure that it is as noble a work to raise its level in the sense already explained, as it would be disgraceful to abase it. I see no impossibility in eugenics becoming a religious dogma among mankind, but its details must first be worked out sedulously in the study. Over zeal leading to hasty action would do harm, by holding out expectations of a near golden age, which will certainly be falsified and cause the science to be discredited. The first and main point is to secure the general intellectual acceptance of eugenics as a hopeful and most important study. Then let its principles work into the heart of the nation, who will gradually give practical effect to them in ways that we may not wholly foresee."

We have cited the whole paragraph, for it is essentially typical of the man, and some word of his message to his nation may fitly appear here. Conspicuously moderate in tone, the study at each point placed before the market-place, it was, indeed, a wonderful appeal for a man more than eighty-two years of age to make from the public platform. It signified that the time was ripe for the labours of the biologist to be turned to the breeding of man. Galton called upon the biologist, the medical man, and the sociologist to grasp what evolution and heredity mean for man, to make out of their science an art, and work thereby for the future of their nation. Nor has that appeal miscarried; its effect may be traced even amid the din of controversy and clash of diverse opinions in almost every recent book, or discussion of heredity or evolution. Those of us, who initially doubted the wisdom of propagandism beyond the academic field, have lived to see a very wide public impression made, not only in this country, but notably in Germany, America, and some of our colonies. If that movement remains within the lines Galton assigned to it—"no over-zeal leading to hasty action" which will "cause the science to be discredited"—then we firmly believe that to the future Galton's life will appear as a rounded whole—the youth of experience and observation, the manhood of development and discovery of method, the old age of practical application.

His school and disciples have lost a leader, but not before he had lived to put the final touches to his work. Of his generosity and helpfulness, his personal modesty and simplicity of nature, many of those who came in touch with him can bear evidence by remembered talk, by letter, and by act. Someday, perhaps, these things may be put together as a memento of

[1] For example, Sir W. Lawrence wrote in 1819 :—"The hereditary transmission of physical and moral qualities, so well understood and familiarly acted on in the domestic animals, is equally true of man. A superior breed of human beings could only be produced by selections and exclusions similiar to those so successfully employed in breeding our more valuable animals. Yet, in the human species, where the object is of such consequence, the principle is almost entirely overlooked. Hence all the native deformities of mind and body, which spring up so plentifully in our artificial mode of life, are handed down to posterity and tend by their multiplication and extension to degrade the race Consequently the mass of the population in our large cities will not bear a comparison with that of savage nations, in which, if imperfect or deformed individuals should survive the hardships of their first rearing, they are prevented by the kind of aversion they inspire from propagating their deformities." What finer text for the eugenist? But Lawrence spoke to a nation still flushed with Waterloo, while Galton, eighty-five years later, appealed to its grandchildren still smarting from South African defeats, and dimly conscious that all was not well with either its physical or mental vigour.

the man whose teaching has just ended, but whose life-work has only begun to run its course. Rewards came to Francis Galton—medals, honorary degrees, corresponding memberships of many learned societies—they came unsought, but not unappreciated. His very modesty made him take an almost childlike joy in these recognitions of his worth, and the present writer remembers with what pleasure, but a few weeks ago, Galton showed him his recently received Copley medal. But these things were not of the essence of his life. Few men have worked so little for reputation and so much for the mere joy of discovering the truth. His three chief pleasures in life were first to discover a problem, secondly to solve it by a simple but adequate process, and thirdly to tell a congenial friend of the problem and its solution. What he cared chiefly for was the sympathy of men who appreciated his special type of work and understood its relation to human progress. Had he spoken of himself and his feelings, which he rarely did, he would, we think, have described his purpose in life much in the words of Huxley:—

"To promote the increase of natural knowledge, and to further the application of scientific methods of investigation to all the problems of life to the best of my ability, in the conviction which has grown with my growth and strengthened with my strength, that there is no alleviation for the sufferings of mankind except veracity of thought and action, and the resolute facing of the world as it is when the garment of make-belief, by which pious hands have hidden its uglier features, is stripped off."

But in the fulfilment of his purpose Francis Galton was an optimist. He believed that man can not only physically control his environment, but with fuller biological knowledge his future development. Not on this or that contribution to the records of science, but on the justification of this belief, will depend his fame in the roll of the ages. There are some of us who believe that among the great names cited at the commencement of this paper, Galton's will not be the last, for he has given an inspiration which will grow to full fruition. Our country has been the land of dominant scientific ideas rather than of massive contributions to the records of science—gravitation, the survival of the fitter, the electromagnetic theory—may we yet add—the biological control of human development? If so, the name of Francis Galton will be closely associated with the coping-stone of the edifice, which had its foundations first securely laid by his half-cousin, Charles Darwin.

MEGALITHIC MONUMENTS AND PREHISTORIC CULTURE IN THE WESTERN MEDITERRANEAN.[1]

AMONG the many questions to which the attention of the British School at Rome is now directed none is of more interest and importance than the exploration of the megalithic remains and primitive culture of the western Mediterranean which is now in progress.

Sardinia, much the most promising field of study, is in the hands of Dr. Mackenzie, the value of whose report is greatly increased by the admirable plans prepared by Mr. F. G. Newton. First among these remains come the Nuraghe or fortified towers, of which more than one type has been identified. The most primitive form is perhaps the simple strong tower of circular shape, to which succeeded the type represented by that of Voes, a massive triangular building, having four circular chambers on the ground floor and a central unroofed courtyard opening into a massively constructed corridor leading to smaller inner rooms. Above this was a second storey, now ruined, which may have formed the living part of the house and the abode of the women, while the lower floor was occupied partly by guards and attendants and partly used as storehouses. These forms soon develop into more complex types, until we reach an elaborately fortified enciente with massive corner towers, like that of Nossia. Dr. Mackenzie reasonably suggests that in the Bronze age the lords of these Nuraghe may have possessed only limited sovereignty, and that these elaborate fortifications were designed in the event of incursions by the neighbouring local chiefs.

The chief interest of the report lies in the fact that for the first time a seriation of the dolmens is attempted, and that these are now brought into relation with the Nuraghe. First comes the dolmen in its primitive form, familiar in western Europe—a massive slab resting on upright supports and forming a rude chamber. The next stage is illustrated by the monument at Maone, which, instead of being a mere cella with vertical supports, is partly hewn into the sloping rock, partly built up with rough coursed masonry, on the top of which rests the cover-slab. Then comes the form, represented by the dolmen of Su Covecco, which is on the point of being elongated and becoming a so-called "Giants' Tomb." In the latter the apse-like arrangement persists, but the cella and well of the enclosure are much extended, and exhibit a whole series of cover-slabs instead of the single massive stone in the primitive dolmen type. The structure thus often simulates the form of an inverted boat, like the Naveta tombs of the Balearic Islands, which gained their name from this fact. They were perhaps designed to symbolise the boat which conveyed the souls of the people, immigrants from beyond the sea, to a place of rest across the ocean. But the original dolmen type seems to have survived into this later period, and in one case the tomb is provided with a secret entrance, which may have been intended for subsequent interments, while the smaller portal hole in the front was reserved for the periodical rites in honour of the ancestral spirits.

Mr. Peet's report on the prehistoric period in Malta is mainly devoted to a criticism of the views of Albert Mayr, who regarded the culture of prehistoric Malta as mainly Ægean. Mr. Peet, dealing in succession with the arguments based on the use of overlapping or splayed masonry, the occurrence of the spiral form of ornament, and the baetylic or pillar worship, points out that none of these have special Ægean or Mycenæan provenience, and while not denying the existence of Ægean culture in Malta, he regards it impossible to attribute all that appears in the island to this source.

It may be hoped that the establishment of a new society for the promotion of Roman studies will give a fresh impetus and supply increased resources for the survey which has been so well started by Dr. Mackenzie and Mr. Peet.

[1] "Papers of the British School at Rome." Vol. v. Pp. xiv+471+47 plates. (London: Macmillan and Co., Ltd., 1910.) Price 42s. net.

THE FLIGHT OF BIRDS.

TO *La Nature* for December 11, 1910, M. Lucien Fournier contributes a well illustrated article on the flight of birds. One of the pictures, showing various positions taken by the wings of gulls in flight, is here reproduced. Three other of the illustrations, namely