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EARLY DAYS OF GENETICS *

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MR CHAIRMAN, LADIES AND GENTLEMEN, may I first of all take this opportunity of thanking you for the high honour you did me in recently electing me an honorary member of this Society. Such honours, I regret to say, are always received with a certain regret because they almost invariably happen when one grows old and when one's senses are becoming dimmed, and for this reason, perhaps, I may seem to you considerably out of date. In reading a good deal of literature in the past few weeks I have frequently come across the word neo-Mendelism. Well, I suppose therefore that you are all neo-Mendelists. I do not know, I must confess, speaking as an ur-Mendelian, what it means, because the situation to-day seems to me to be the natural and logical outcome of what happened in 1900. So if I seem to you like a labyrinthodont addressing an assembly of Houyhnhnms you must forgive me.

It is related of Gregor Mendel, aware of his supreme discovery and of its neglect by the biologists of his day, that he uttered the words " Meine Zeit wird schon kommen." And I incline to think that he, who fought so strenuously in the Ultramontane dispute, would have felt no repugnance to the circumstance that his " Zeit " was also born in controversy. " Nur der Streit enthalt die Wahrheit " once wrote a distinguished German morphologist, and I feel that Mendel would have endorsed that saying. For the science of genetics was born in controversy and the manner in which this came about will take us back into the latter years of the past century.

Darwin had conquered in 1859 and for a generation the orthodox regarded the process of evolution as brought about by the action of natural selection on a continuously varying material. Natural selection, variation and heredity were the key words, though what were the limits of the two latter no one bothered much to enquire. It was the age of morphology, and the morphologist drew freely on the blank cheque which Darwin had provided. It was Francis Galton who first attempted to bring precision to these two key words by proposing the " Law of Ancestral Heredity," a " Law," as I have elsewhere pointed out, which had already been adumbrated by William Wollaston in 1722. The " Law " postulated a definite degree of resemblance, statistically measurable, in any familial relationship.

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If these values, as worked out from collected data, agreed closely with those postulated by the "Law of Ancestral Heredity" they would offer strong presumptive evidence in favour of the orthodox Darwinian position that natural selection worked by the accumulation of small continuous variations. So came into being the School of Biometry which, as Karl Pearson remarked, would eventually reduce biology to a branch of mathematics. This new line of enquiry was enthusiastically taken up by Weldon, then Linacre Professor at Oxford and one of the leading biologists at that time. On the mathematical side he had the support of Karl Pearson with whom he subsequently founded *Biometrika*, and with whose help were developed the correlation tables for working out the values of parental and fraternal resemblances. The great assumption made throughout was continuity of variation.

We may now turn to more personal matters. Weldon and Bateson were up at St John's * together and at that time were close friends. Weldon, who was senior to Bateson, was a dominant personality, and in referring to those days Bateson said that he was often made to feel like Weldon's bottle-washer. Both were convinced that morphology had played its part and that for further progress an attack must be made on the problem of species. It was here that their paths diverged. Weldon, as I have said, developed Galton's views with their assumption of continuity in variation. Bateson, however, was not convinced. He was more of a naturalist than Weldon and could not get away from the specificity of species. I fancy that his thoughts turned in the direction they were to take through his friendship with Professor W. K. Brooks when he was working at Woods Hole on the development of *Balanoglossus*. His Wanderjahr on the Russian steppe was an attempt to gain evidence of evolutionary change through a change in the environment. On his return he decided that the study of variation itself offered the best line of attack on evolutionary problems. As he gathered in material from all sources—from stock breeders, fanciers, horticulturists and others, he became more and more convinced of the frequency of discontinuity in variation, and eventually published his views with his collection of facts in the *Materials for the Study of Variation* in 1894.

Bateson's book was a direct challenge to the orthodox view and Weldon accepted it as such. Thenceforward battle was joined and the first clash came in 1895 over the origin of the cultivated *Cineraria*. Orthodoxy held that these cultivated forms had come from a wild species through the accumulation of small variations in the hands of the horticulturist working towards the ideal he had in view. Bateson protested that the many varieties in cultivation had come through hybridisation between several recognised species, and in support of his view he brought forward evidence from experiments by Mr Lynch, then Curator of the Botanic Garden at Cambridge. But he fought a lone hand for, in the controversy that ensued, Weldon

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was supported by such botanists of note as Botting-Hemsley and Thistleton-Dyer. Convinced, however, of discontinuity in variation, Bateson now decided to devote himself to experimental breeding with a view to discovering how these discontinuous variations behaved in the hereditary process. In this work he was joined by his first colleague, Miss E. R. Saunders, to whom this Society owes much. Crossing experiments were started with sweet peas, poultry and stocks, the last being Miss Saunders' province.

The aim of the new work was outlined in an address given to the International Congress on Hybridisation convened by the Royal Horticultural Society in 1899. It reveals how near Bateson's aims and methods were to those of Mendel. Then came the sudden revelation in 1900, when in the train on the way to deliver a lecture before the Royal Horticultural Society. How he revised his lecture on the journey and brought to this country the first news of Mendel's discovery you may read in Mrs Bateson's life of her husband. Instant and deep as was Bateson's appreciation of what Mendel had done there was another who also understood what it might mean. For Weldon was sufficiently alert to realise that discontinuity in heredity meant an end to that continuity in variation upon which the Biometrical School was founded. So the first step in the Mendelian controversy proper was provided by Weldon's belittlement of Mendel's work in the first volume of *Biometrika*.

Bateson's rejoinder was delayed. For on reading Mendel's paper he at once set to work to repeat the experiments. It was characteristic of him that when the rejoinder came in 1902 it took the form of a small book entitled *A Defence of Mendel's Principles of Heredity*. The gloves were off and Weldon was not spared. He retaliated with a further article in *Biometrika* in 1903. To this Bateson replied in a letter to *Nature* which, however, was never published since the editor stated that he was " not prepared to continue the discussion on Mendel's Principles and therefore returns herewith the papers recently sent him by Mr Bateson." Why, you may ask, did not Bateson send his reply to *Biometrika*, the venue chosen by Weldon for his criticism ? The answer is that the pages of *Biometrika* were also closed to him. In 1901 Pearson published a long paper in the Philosophical Transactions on what he termed " Homotyposis." As with all Royal Society papers it was submitted to referees of whom Bateson was one. His opinion was by no means favourable and he felt it incumbent on himself to justify it in a short paper in the Proceedings. This drew forth an answer from Pearson in *Biometrika* 1902. Bateson did not consider this satisfactory and submitted a further consideration of the matter. This the editors of *Biometrika* refused to accept for publication, so Bateson finally had it privately printed by the Cambridge University Press, using the format of *Biometrika*.

It was a difficult time for struggling geneticists when the leading journals refused to publish their contributions to knowledge, and we

had to get along as best we could with the more friendly aid of the Cambridge Philosophical Society and the Reports to the Evolution Committee of the Royal Society. And since most of the earlier genetical work was published in these Reports, I may digress for a moment to consider how this came about. At the instigation of Francis Galton the Royal Society in 1894 appointed a Committee for conducting statistical enquiries into the measurable characters of plants and animals. In 1897 Bateson was invited to become a member but, recognising its biometrical import, he declined. Later on, when the Committee was reconstituted as the Evolution Committee, he joined and eventually became its secretary. Weldon resigned and with Pearson later founded *Biometrika*. The Evolution Committee proved of great service both by providing small grants towards the experiments, and by affording a means of publication.

Meanwhile Weldon dropped the pea and started a new line of attack with mice, instigating a young pupil of his, A. D. Darbishire, to do the work under his direction. Darbishire was a pleasant young man but rather woolly and very much under Weldon's thumb. One of Weldon's complaints against Mendelism was what he called "Neglect of Ancestry," and the mouse work was intended to bring this out. A cross was made between albino and agouti and the F_2 generation gave of course 25 per cent. of albinos. Now one kept on breeding from the agoutis of the F_2 and subsequent generations *en masse*. At each generation the proportion of albinos diminished until eventually there were none at all. Clearly a proof of the influence of ancestry so shamefully neglected by the Mendelians. The fallacy of course was that no distinction was made between homozygous and heterozygous agoutis, and since the former class inevitably came more and more to preponderate owing to the method used the proportion of albinos would inevitably diminish. The experimental results were not presented in a form such as to satisfy a critical mind like Bateson's and a controversy ensued which was fought out in the pages of *Biometrika* and *Nature* until the latter journal refused to publish anything more from Bateson.

But before the mouse controversy reached its end a dramatic change took place. And at this point I may perhaps be excused if I indicate how I came into the story. I had become interested in sex determination and was proposing to start some experimental breeding of mice to find out whether it could be influenced by nutritional or other factors. So I asked Bateson whether, in the designing of my experiments, I could help to provide data for colour inheritance. The outcome was the charming letter of December 1903, reproduced in Mrs Bateson's *Life*, and thenceforward I became a privileged member of the Grantchester household. At the time I joined him Bateson's disciples were few. Besides his old colleague, Miss Saunders, his sister-in-law, Florence Durham, had begun to breed mice and canaries, while C. C. Hurst was engaged near Leicester

with poultry and rabbits. Biffen also had started those experiments with wheat that were to bring rust resistance into the category of Mendelian characters.

You will note that the little band round Bateson in 1904 were all seniors, that is of M.A. standing, and that they gained their livelihood from pursuits other than genetics. Miss Saunders and Miss Durham lectured to Newnham on botany and physiology respectively, Biffen gave lectures on mycology, and I myself was a demonstrator of zoology ; Hurst was a professional horticulturist, while Bateson kept himself by looking after the kitchens of St John's. But Bateson knew very well that if this new branch of enquiry was to live and expand it must capture young men and women, and the opportunity came with his appointment as deputy to the Professor of zoology in 1899. The post was not as important as it sounds for at that time the position in the department was a curious one. Professor Newton, a great ornithologist and a great gentleman, gave a few sparsely-attended lectures, while the large department, with a couple of hundred students or more, was run by Adam Sedgwick, Reader in morphology, assisted by a staff of lecturers and demonstrators. The teaching was entirely morphological and no one felt any interest in the issues which Bateson had raised. Newton's failing health gave Bateson his opportunity of reaching the younger students, with the result that Lock, Doncaster, Gregory, Miss Wheldale, Miss Sollas, Miss Marryat and others were soon brought into the fold.

However, let us hark back to 1904, when my personal recollections start. The set-up was primitive for money was scarce. The poultry occupied a small paddock split up into about two dozen little pens. There were several incubators in a bedroom upstairs though this had soon to be given up since it was requisitioned for the little boys' governess. The chicks were reared in movable brooders along the garden paths. It was not a very satisfactory arrangement for, in a wind, one of them occasionally caught fire, and there was an end to *that* hatch. We had only oil, which meant much work in filling the various receptacles and cleaning the wicks. Every afternoon one of us went out collecting and marking the eggs from the various pens. We had no trap nests but soon became adepts at spotting the egg laid by each particular hen. All birds of course were numbered, and Bateson had devised an ingenious little brass label which clipped round the leg of the newly-hatched chick. These were labelled consecutively with the year on the other side, and each number corresponded to a page in the record book. When the copper ring became too tight for the swelling leg the label was cut off and attached to the wing, an operation we called "wiring." Though rather a tedious operation it was always interesting since it provided speculation on various points. Evidence that we did not always agree could be found by the curious in the form of bets recorded throughout the pages of the notebooks. Moreover, in deciding what to keep, and what to

reject, we had to be all the time planning the programme for the coming year. Birds rejected were destined for the higgler, for even the small sums so received helped to defray the cost of the work.

Though we reared some hundreds of chicks each year the great majority of the eggs incubated were never allowed to hatch, for some of the characters on which we were working were sufficiently developed for determination at about the eighteenth day. So we had periodical " openings " which were recorded in a separate notebook known as the " Dead Book." On the day of an " opening " we adjourned to the outhouse in which the incubators were later kept, having previously collected Mrs Bateson to clerk for us, a function which she performed with the greatest efficiency and devotion. Having settled her in a chair at the trestle table with the " Dead Book " and a large bowl, Bateson took off his coat and produced his knife with the big, blunt blade, while I stood by with a pair of scissors. He then took up an egg, read off the numbers of the pen, the hen and date of laying, and after "Have you got that, Beatrice?" proceeded to stab and peel off the shell into the aforesaid bowl, and to call out the peculiarities of that particular embryo such as lt., nts., r.c., n.e., f.l., which was to be interpreted as " light down, no coloured ticks seen, rose comb, no extra toe, feathering on leg." After which the chick was handed to me, who slit it so as to expose the sex glands and give Mrs Bateson the sex to complete the entry. Altogether it was a messy job and " openings " were not much looked forward to.

Sweat peas were the other main line of enquiry. We grew some thousands each year and of course the garden at Merton House could not nearly accommodate such numbers. Mrs Bateson protested that vegetables were necessary to keep the household alive. But we managed to borrow a small plot from a neighbour, and the Agricultural Department very kindly allowed us the use of about an acre of rough arable on the University Farm. Only the precious first crosses and the few families we knew we should want for further breeding were privileged to occupy a garden site and to be cleaned and sticked by us. The rest were sown in drills, mainly on the University Farm which was then at Impington, beyond Histon. We arranged with a farm labourer to eradicate the worst weeds, otherwise they received little attention until the time came for " pulling." Then we set out on the four-mile ride for a long afternoon, Bateson with his wife in the trailer carrying the " Farm Book " and a microscope. Having made Mrs Bateson reasonably comfortable we proceeded to pull, family by family. One of us pulled the plant and sung out its characters (*e.g.* ppw., dk., ax.) and handed the plant to the other, who, with the microscope perched on some odd box picked up at the farm, determined the shape of the pollen. All duly logged by Mrs Bateson. In the garden, of course, we were busy, during the flowering period, emasculating, crossing and recording. This at times meant a lot of work especially after we had hit upon the 9 : 7 ratio and the interaction of factors, for we felt it

necessary to verify this by hundreds of crosses among F_2 whites, and this meant getting up at 6 a.m. and putting in a couple of hours before breakfast, before the anthers had a chance of bursting.

My initiation into these studies was a strenuous one and I was so busy picking up the threads of a new pattern of research that I never fully realised, until it was past, how important 1904 was to be. I had, of course, obtained some insight into the nature of the antagonism between Weldon and Bateson as we chatted in the course of our work. And I knew that Bateson was to be President of the zoology section at the coming meeting of the British Association. I had never attended a meeting of this body and had not thought of it as of any importance to the work we had in hand. Bateson, however, had realised that this was a great opportunity. It was constantly in his thoughts and he neglected nothing in order to make his own part a success. When the sweet pea season came on in June he used to take a chair and a little table out into a small copse at the back of the chicken pens, leaving the strictest orders that he was on no account to be disturbed by anyone. So each day when I arrived with the *Morning Post*—a paper of which Bateson approved since it contained the best account of art sales, and I don't think he read much else—I set to work cleaning, recording and labelling sweet peas while Bateson, in the seclusion of his copse, struggled with his address. But he knew that I was there and after a time would saunter out and invite discussion on some particular point. It was, of course, to ventilate his mind, and the only point wherein I was of any help was to draw his attention to Archbishop Parker's forbidden degrees of marriage at the end of the *Book of Common Prayer*.

Eventually, all was set for the great day—somewhere in August if I recollect right. To section D was allotted the large lecture room in the newly erected geological building, and the place was full. Bateson had his say, and how striking a say it was can be judged by anyone with access to a copy of Mrs Bateson's memoir. Hitherto he had been on the defence ; now he attacked, and such statements as " the imposing Correlation Table into which the biometrical Procrustes fits his arrays of unanalysed data is still no substitute for the common sieve of a trained judgment," admitted no doubt of the direct challenge. After the Presidential Address the first meeting of the section was devoted to papers and discussion on Mendelian matters. Bateson deployed his forces. The morning was given up to facts. Miss Saunders authoritatively announced her findings in the stocks. Hurst assured the audience of Mendelian phenomena in poultry, while I chimed in with the story of the fowl's combs.

We adjourned for lunch and on resuming found the room packed as tight as it could hold. Even the window sills were requisitioned. For word had gone round that there was going to be a fight. Probably other meetings were depleted—but after all the Association is British. Weldon spoke with voluminous and impassioned eloquence, beads of

sweat dripping from his face, and I cannot help recalling the admiring remark made by one young Oxford man to another as they sat just in front of me, " Clever beggar that—he hasn't got to stop and think." Bateson replied and there may have been other speakers, I have forgotten. But towards the end Pearson got up and the gist of his remarks was to propose a truce to controversy for three years, after which the protagonists might meet again for further discussion. On Pearson resuming his seat, the Chairman, the Rev. T. R. Stebbing, a mild and benevolent looking little figure for a great carcinologist, rose to conclude the discussion. In a preamble he deplored the feelings that had been aroused, and assured us that as a man of peace such controversy was little to his taste. We all began fidgeting at what promised to become a tame conclusion to so spirited a meeting, especially when he came to deal with Pearson's suggestion of a truce. But we need not have been anxious, for the Rev. Mr Stebbing had in him the makings of a first-rate impresario. " You have all heard," said he, " what Professor Pearson has suggested " (pause), and then with a sudden rise of voice, " But what I say is let them fight it out." And on that note the meeting ended. Bateson's generalship had won all along the line and thenceforth there was no danger of Mendelism being squelched out through apathy or ignorance.

But in spite of the success of the Cambridge meeting in getting Mendelism a hearing the older generation of biologists endorsed Weldon's hostility and the pens of Alfred Russell Wallace, Professor Poulton and Professor J. Arthur Thomson were soon engaged in attempting its belittlement. In this they were supported by *Nature*, though by now such hostility was of less account ; Mendelism had become news and the columns of secular periodicals were opened to us.

Weldon's next attack was directed at Hurst. Now Hurst was a tireless worker and full of ideas, but over-apt to find the 3 : 1 ratio in everything he touched. While valuing Hurst's enthusiasm for the cause Bateson was nevertheless mistrustful of his slickness, for he knew that his critical ability had not been sharpened by passing through the scientific mill. Hurst's acquaintance with hunting folk had suggested to him that chestnut was recessive to bay and brown. He collected data from the *Stud Book* and got Bateson to let him read his paper before the Royal Society. Knowing what was afoot Weldon had also consulted the *Stud Book*, and at the meeting he criticised Hurst severely by bringing forward cases in which chestnuts mated together gave bay or brown offspring. Bateson felt that Hurst had let him down, and he was intensely irritated when Hurst blandly assured Professor Weldon that he was mistaken and that these alleged exceptions were mere errors of entry. At the close of the meeting Bateson withdrew the paper, and together with Miss Durham we set out on foot for somewhere north of Oxford Street where we were due to dine with the Herringhams. It had happened that at this same meeting Bateson had read our communication on the 9 : 7 ratio in

the sweet pea and had illustrated it with a large hanging diagram. So through the length of Bond Street he marched, grim and silent, shouldering a 6 feet roll of paper with Miss Durham and myself trotting behind him in a state of mixed apprehension and amusement. Dinner that evening was a glum affair.

The Christmas vacation was devoted to an intensive study of the *Stud Book*. Most of Weldon's alleged exceptions turned out to be of little importance. A stillborn foal requires a return and it is of little matter to the equestrian world whether the groom chooses to call it a bay or a chestnut. Though most of the exceptions were of this nature there was a more serious one with which he had made great play. "Ben Battle" was the horse's name and though registered as a chestnut he had sired bays from chestnut mares. There seemed no getting over this until suddenly Hurst came to the rescue. He consulted a work called *Form at a Glance*, a work unknown to us but with a title expressive of its contents. And one day there came a welcome telegram, "Ben Battle never *ran* as a Chestnut." Though he could not have known it at the time, Hurst's bland assurance to Professor Weldon turned out to be right, and a month later Bateson recommunicated the paper which was then published in the Proceedings of the Royal Society.

At this period we knew that any flaw in our work would at once be pounced upon and subjected to bitter criticism. Not a bad thing really for it kept us on our toes and added a spice to the work. Then suddenly Weldon died ; the controversial stage fizzled out and genetics entered into its own.

Here I may mention an interesting advance in genetical theory which came about in an unusual way. In 1908 I gave an address to the Royal Society of Medicine on "Mendelian Heredity in Man." In the subsequent discussion I was asked why it was that, if brown eye were dominant to blue, the population was not becoming increasingly brown-eyed : yet there was no reason for supposing such to be the case. I could only answer that the heterozygous browns also contributed their quota of blues, and that somehow this must lead to equilibrium. On my return to Cambridge I at once sought out G. H. Hardy with whom I was then very friendly. For we had acted as joint secretaries to the Committee for the retention of Greek in the Previous Examination and we used to play cricket together. Knowing that Hardy had not the slightest interest in genetics I put my problem to him as a mathematical one. He replied that it was quite simple and soon handed to me the now well-known formula $pr = q^2$.* Naturally pleased at getting so neat and prompt an answer I promised him that it should be known as "Hardy's Law"—a promise fulfilled in the next edition of my *Mendelism*. Whether the battle of Waterloo was won on the playing fields of Eton is still, I

* Where p, $2q$ and r are the proportions of AA, Aa, and aa individuals in the population varying for the A-a difference.

gather, a matter for conjecture : certain it is, however, that " Hardy's Law " owed its genesis to a mutual interest in cricket.

For a few more years I worked with Bateson at Grantchester until, in 1910, he moved to Merton to take up the Directorship of the John Innes Institution, no longer to be frustrated by inadequate means. I look back upon those years as among the most pleasant in my life. No one more delightful to work with than Bateson could be imagined. Dominant personality as he was, he was never domineering. Only once can I recall his having lost his temper with me, and then he had every justification because I had imported a trio of Silky fowls without his knowledge. I was getting a little bored with the everlasting single, pea, rose and walnut combs and I had a hunch that the queer little Silky with its unusual comb might bring in something new. That it certainly did, and out of its crosses with the Brown Leghorn came the data which enabled us to establish the doctrine of sex-linkage. So I was forgiven. Another lucky venture was the preservation of the cretin sweet pea, a single plant of which turned up in a long row. It did not appeal to Bateson, who said, " Do throw away that ' monstrous thing '." But it intrigued me and I insisted on making some crosses with it. It turned up trumps for it led to our establishing that repulsion and coupling, hitherto regarded as distinct features, were all part of the same phenomenon, and by its means we were led to establish the principle of linkage.

I have sometimes been asked how it was that having got so far we managed to miss the tie-up of linkage phenomena with the chromosomes. The answer is Boveri. We were deeply impressed by his paper " On the Individuality of the Chromosomes " and felt that any tampering with them by way of breakage and recombination was forbidden. For to break the chromosome would be to break the rules. So it was left for Morgan and his colleagues to make use of Janssen's observations and by their brilliant work to link up genetics and cytology, thereby opening up a new era in these studies.