

Alan G. Cock
Donald R. Forsdyke

Treasure Your Exceptions

The Science and Life of
William Bateson

 Springer

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By

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ISBN: 978-0-387-75687-5

e-ISBN: 978-0-387-75688-2

Library of Congress Control Number: 2008931291

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To past, present, and future Christiana Herringhams and
Eliza Savages, who treasure those “the system” will not.



William Bateson, 1905

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Abbreviations

BA. The British Association

CBSS. Conjoint Board of Scientific Societies

Defence. Bateson's *Mendel's Principles of Heredity: A Defence* (1902)

EB. Evolutionary Bioinformatics

FRS. Fellow of the Royal Society

IRC. International Research Council

IUBS. International Union of Biological Societies

Letters. *Letters from the Steppe*, edited by Beatrice Bateson (1928)

Materials. Bateson's *Materials for the Study of Variation* (1894)

MBA. Marine Biological Association

Memoir. *Essays and Addresses* with a Bateson biography (1928)

Principles. Bateson's *Mendel's Principles of Heredity* (1909)

Problems. Bateson's *Problems in Genetics* (1913)

RHS. The Royal Horticultural Society

RS. The Royal Society

RSM. The Royal Society of Medicine

RS-Report. *Report to Evolution Committee of the Royal Society*

SEB. Society for Experimental Biology

Prologue

Nevertheless, if I may throw out a word of council to beginners, it is: Treasure your exceptions! When there are none, the work gets so dull that no one cares to carry it further. Keep them always uncovered and in sight. Exceptions are like the rough brickwork of a growing building which tells that there is more to come and shows where the next construction is to be.

William Bateson (1908)

Part 1 by Donald Forsdyke

To understand evolution we must first understand the historical development of ideas on evolution. But to understand its history, we must first understand evolution. This paradox implies that the study of evolution and of its history must go hand in hand. There can hardly be a better example of this than the life of Gregor Mendel, the founder of Genetics. Mendel died in 1884. Any biography written between 1884 and 1899 would have described his life as a monk in an Augustinian monastery in the city of Brunn in Moravia. There would have been plenty to write about. As Prelate of the monastery he was much involved in community affairs. Under his predecessor the monastery had become a major intellectual and cultural centre, and Mendel encouraged this. He became chairman of the Moravian Mortgage Bank and fought bitterly with the German Liberal Party, which had imposed severe taxes on monasteries. Oh, and yes, he did dabble in research for a few years before becoming Prelate in 1868. He crossed different lines of peas and scored the inheritance of various characters among the offspring – even published a paper or two.

As has been often told, in the 1890s and unaware of Mendel's work, various botanist in continental Europe (de Vries, Correns and Tschermak) began to think along the same lines. Since the publication of Charles Darwin's great book in 1859 [1], Darwinism had dominated the biological sciences. Now, as if scales had dropped from their eyes, researchers began to push beyond Darwin and understand, for the first time, a paper that Mendel had published in 1865 in an obscure journal, the *Verhandlungen des naturforschenden Vereines in Brunn*, copies of which had been distributed to many academic centres [2]. In 1900 Mendel's work was found and confirmed (i.e. "rediscovered"). Studies of evolution between 1865 and that date were defective because studies of the history of evolution in that period had been defective.

And studies of the history of evolution were defective, because studies of evolution in that period had been defective. Evolutionists had not alerted historians to the significance of the new approach Mendel had pioneered, and the few historians who come across the work had not effectively communicated it to evolutionists. It fell to William Bateson, who had also begun researching along these lines, to communicate Mendel's ideas to the English-speaking world, and to fight for their acceptance.

By 1909 the battle was over. Mendelism was accepted and Mendel's paper had been translated and distributed world-wide. Holder of the first Chair in Biology at Cambridge University, Bateson was at the height of his power and influence. In 1906 one of his students had spread the word in a popular textbook – *Variation, Heredity and Evolution* – which ran to several editions. In 1907 Bateson had given the Silliman Lecture at Yale, and in 1909 his advanced treatise, *Mendel's Principles of Heredity*, was published by Cambridge University Press. To commemorate the centenary of the birth of Charles Darwin, a call to contribute essays to a volume entitled *Darwin and Modern Science* had gone to leading figures in the field. Bateson took this as an opportunity, not only to pay tribute to Darwin and Mendel, but also to push the agenda of science ahead and beyond them. His essay – “Heredity and Variation in Modern Lights” – encapsulated in its clearest form, a new view of evolution that he had developed from foundations laid by Francis Galton decades earlier.

It might just as well have been 1865. Unlike Mendel on that date, Bateson in 1909 was not an obscure monk in a distant monastery. He, more than any other in the biological sciences, was centre-stage. This was his hour. Yet, as with Mendel, his words fell on fallow ground. In a way, he was partly responsible for his own demise, as he later came to recognize. He, more than any other, had travelled the land, indeed, the world, trumpeting Mendelism – the idea that the various characters of living organisms were represented by distinct units (later known as genes), half of which came to a child from its mother and half of which came to a child from its father. In essence this was quite a simple message. Then as now, in marketing, simple messages worked. Then as now, the same applied to the marketing of scientific ideas. Accordingly, subtle scientific ideas tended to lose out to simple scientific ideas, and subtle scientists tended to lose out to the unsubtle. The appeal of Darwinism had been so seductive and had suggested so many interesting lines of research, that Mendel's abstractions (after all, no one had ever seen a Mendelian unit) would very likely not have been understood in 1865. Likewise, the appeal of Mendelism was now so seductive that Bateson's abstractions, to be discussed in the chapters ahead, suffered a similar fate in 1909.

Genes explained so much that the temptation to believe that they could explain everything was overwhelming. And the genic paradigm spawned an

eminently marketable research agenda. What were genes? How were they passed from generation to generation? How was the information contained in a gene expressed in an organism carrying that gene? Could “good” genes be transferred to organisms with “bad” genes, so curing genetic defects? With so much to be done there was little incentive to look beyond genes. The genic juggernaut moved off, gaining a momentum that would take it through the twentieth century and into the twenty-first.

Nevertheless, Bateson’s voice was not a lone one. As set out in my book *The Origin of Species Revisited*, Darwin’s research associated, George Romanes, had anticipated Bateson in 1886 [3]. In a subsequent book, *Evolutionary Bioinformatics*, I showed how biologist Richard Goldschmidt had taken a similar position in the 1930s [4]. Paleontologist and biohistorian Stephen Jay Gould struggled for decades to make sense of Goldschmidt, but in 2002 in his final work *The Structure of Evolutionary Theory* he admitted defeat [5]. Gould confessed his “relative ignorance” of the flood of new information on genomes emerging from various genome sequencing projects. It turned out that, just as Bateson and the Continental botanists had to push beyond Darwinism in the 1890s to “rediscover” the work of Mendel and found a new science – Genetics, so those analyzing the vast quantities of DNA sequence information made available in the 1990s had to push beyond genocentrism to “rediscover” the work of Romanes, Bateson and Goldschmidt, and found a new science – Evolutionary Bioinformatics (EB).

Yet, there are remarkable differences between the “rediscoveries” of the 1890s and the 1990s, which makes the task of writing a biography of Bateson particularly fascinating. Mendel quietly wrote his few papers that, although of major import, were narrowly focussed on his experimental work. He then meekly (regarding science) retired to his cloister, was “rediscovered” within two decades of his death, and was celebrated henceforth. Bateson, on the other hand, took the whole of biology (and much more) as his domain, and was far from meek in arguing his case. Both before and long after his death, he was attacked by the genocentrists (labelled by Gould as “ultra-Darwinian fundamentalists”), and he was not “rediscovered” until many decades after his death [3]. Whereas Mendel’s eclipse was for thirty-five years, Bateson’s was for a century.

Up to the present time, while his early contributions to genetics are recognized, his fundamental contributions to evolutionary biology have never been celebrated – indeed, the very opposite – he has been condemned as a conservative who slowed scientific advance. Biohistorians have acknowledged their difficulties in determining his role. John Lesch observed [6] that “The development of evolutionary theory in the two decades from Darwin’s death to the turn of the century remains very largely terra incognita for the historian,” while William Provine lamented [7] that “Evolutionary biology in

the period 1859–1925 is extraordinarily complex.” To understand these years we have to understand Bateson.

Which brings us to this book, and its dual authorship by Alan Cock and myself. The reader will find below part two of this three part Preface, which was written by Alan around 1980. The typescript had a pencilled date indicating that he reread and approved it in 1984. A quarter of a century separates it from my parts. Briefly, the story is as follows. When Bateson died in 1926 he was director of the John Innes Institute at Merton in Surrey. His wife, Caroline Beatrice Durham (hereafter “Beatrice”), removed the bulk of his personal scientific papers and correspondence. To these she added various personal and family papers, and transcripts of letters sent to her by some of his correspondents. She used them when writing and editing *William Bateson, F. R. S. Naturalist. His Essays and Addresses Together with a Short Account of his Life* [8], and when editing *Letters from the Steppe Written in the Years 1886–1887 by William Bateson* [9]. Both were published in 1928. Her goals were limited. She “attempted only to sketch a rare personality” and hoped that a “more competent hand” would later complete her work.

At her death in 1941 the papers were held in storage until 1954 when they were transferred to her son Gregory Bateson, who, with his wife Margaret Mead, had gained a reputation as an anthropologist and was based in the USA. The papers eventually found their way to the loft of an out-house at the summer-home of their daughter, Mary Catherine Kassarjian, in the small New Hampshire town of Hancock, near Boston. Here they were inspected by historian William Coleman in 1964, and he borrowed a selection (perhaps 20% of the whole), which he microfilmed at Johns Hopkins University. The microfilm became part of the collection of the American Philosophical Society in Philadelphia [10], and assisted William Provine when composing his book *The Origins of Theoretical Population Genetics*, 1971 [11]. Despite its daunting title, this was also a mini-biography of Bateson. Coleman wrote a long article entitled “Bateson and Chromosomes: Conservative Thought in Science,” which was published in 1970 [12]. In 1975 some of the papers were examined by David Lipset, who had travelled with Gregory in Asia in the early 1970s. This led to a comprehensive biography *Gregory Bateson. The Legacy of a Scientist*, in 1980 [13]. It was also, in many respects, a splendid biography of William, but Lipset mentioned in the Preface that a more complete biography was being written by Alan Cock.

After graduating in Zoology at Cambridge University in 1947, Alan worked at the Poultry Genetics Unit in the School of Agriculture at Cambridge. He was assistant to Michael Pease, who had himself been an assistant to Reginald Punnett, who had been Bateson’s assistant. So Alan could rightly claim to be a scientific great-grandson of William Bateson. Remarkably, Pease was still using the same system of short-hand notation and record-keeping

that Bateson and Punnett had developed in earlier decades. In 1957 Alan went to the Poultry Research Centre at the University of Edinburgh where he obtained a doctorate in genetics in 1962. In 1964 he was appointed lecturer in zoology at the University of Southampton. In the 1970s his interests turned to biohistory: “My personal interest in Bateson (evoked by beginning my research career in a Unit directly descended from his Cambridge operations) developed in 1972 into a serious intention to write a book about him.” That year Alan obtained a copy of Coleman’s microfilm and in 1973 he examined the remaining materials that Beatrice had not removed from the John Innes Institute – now relocated to Norwich. He also explored the Bateson-Punnett research notebooks held in the Department of Genetics in Cambridge. His first biographical paper was entitled “William Bateson, Mendelism and Biometry” [14]. In December 1974 Alan visited America on a short-term travel grant from the Wellcome Trust to examine the remaining papers. He reported back to the Trust in February:

My original plan was to remove the papers temporarily to Harvard University and there to obtain as full and complete a permanent record of them as was possible within the six weeks available by a combination of note-taking and selective Xerox-copying. The practical side of this programme was radically altered (in an almost wholly welcome sense) during the first few days of my visit by the decision of Bateson’s only surviving son (the anthropologist Gregory Bateson) and granddaughter (Mrs. Mary Kassarjian) to allow the papers to be sent on indefinite loan to the University Library, Cambridge, England. The papers are in fact now on their way across the Atlantic. In view of the rarity with which historical papers travel eastwards across the Atlantic, I feel rather proud of having influenced their fate. Dr. Stephen Gould, of the Agassiz Museum of Comparative Zoology, Harvard, very kindly offered me working space in the museum. Only six days were spent away from Harvard: two in collecting the papers from Mrs. Kassarjian’s house at Hancock, and four in visiting Dr. Gregory Bateson at Santa Cruz, California.

Elsewhere he noted that “Gregory Bateson very generously agreed to my suggestion that the entire body of papers be returned to Britain. Cambridge University Library was agreed upon as their final home, once I had finished with them.” Thus, in 1975, with help from Mary Catherine Kassarjain, he and David Lipset sorted through the Bateson papers in Hancock. Old family papers and ones relating to Gregory Bateson were to stay in the USA (at the University of California, Santa Cruz). The remaining papers were repatriated and the long task of cataloguing and indexing began at the University of Southampton [15].

At the outset Alan began submitting proposals for the book to various publishing houses. For unclear reasons, clues to which may emerge here,

there was no great enthusiasm. Fortunately, Lipset did not have this problem, and his book on Gregory Bateson was shortly to appear. But the shadows were lengthening. Alan had long been prone to periods of depression and in 1984 he was operated on for a tumor. Although he was able to complete the cataloguing, and produced several valuable papers, the prospects for the ultimate book were not good.

Meanwhile in the 1970s there had been great advances in sequencing technology. The genetic information that passes between generations in the form of a sequence of bases in DNA could be directly read. In the 1980s the technology became automated and sequencing machines began to gush forth so much data that only computers could handle them. In the 1990s bioinformatic analyses in my laboratory and elsewhere began to suggest a re-evaluation of William Bateson's work. Like Alan two decades earlier, I was increasingly drawn away from science per se, and towards its history. Yet, in my readings of the historical literature I repeatedly encountered papers, such as that of Coleman [12], that disparaged Bateson. Unfortunately, it was not until *The Origin of Species Revisited* was in press that I encountered the thoughtful, meticulous and sympathetic analyses that Alan has tucked away in predominantly low profile journals.

Having greatly enjoyed Abraham Pais's *The Science and Life of Albert Einstein* [16], I toyed with the idea of something similar about Bateson. A major difference was that, whereas Pais had been obliged to restrict Einstein's science to segments that only those with advanced knowledge would comprehend, I felt certain that I could explain evolution from the new perspective in a way that generally educated readers would follow. Through the biography of a major originator of the new perspective, evolution, with all its subtleties, might be made intelligible to the educated layperson. More than this, the field of evolutionary biology had split into factions, personalized in the forms of Richard Dawkins (advocate of Darwin's natural selection as the supreme agency in evolution), and of Stephen Jay Gould (advocate of hierarchical agencies). Resolving this rift was one of the goals of a text nearing completion [4]. A new, less technical, work might further help.

However, there was a major stumbling block. I was still much engaged in research. The prospect of foraging through dusty archives for materials with which to reconstruct Bateson's life, while appealing, was not something for which I was particularly well trained. So I continued the development of some web-pages on Bateson and let the matter rest. Then I learned from David Lipset's biography of Gregory that Alan Cock was writing a full biography of William. Here was the "more competent hand" that Beatrice had hoped for. Since Lipset's book was published in 1980, Alan's should already be published? But a search revealed no trace.

In 2001 I emailed Lipset asking what he knew of Alan's project. "I doubt that Cock ever finished the project on WB. He was an up and down sort of man." Alan was no longer working at the University of Southampton, but I traced him to London where he was living in an apartment close to his eldest daughter. In the spring of 2004, a year before his death, I met him and two of his daughters, and had an opportunity to examine his files. With his and their approval, his personal collection of Bateson-related materials and many of his own personal papers, were shipped to me in the fall of 2004. It did not take long to decide, as I checked his copies of the Coleman microfilms and rummaged through copies of the Hancock papers, that, although still a major undertaking, a dual-authored work was feasible without unduly trespassing on my other research interests.

In the pages that follow I present my gleanings from the incomplete chapters and other jottings I found among Alan's papers. Sometimes Alan's words are directly attributed to him, particularly when large segments are transposed from his files. Two chapters are his alone (apart from minor editing). Five chapters are mine alone. However, to spell out at every point the relative contributions of Alan and myself would be too distracting. All I can say is that the entire collection of Alan's papers (together with his copy of the Bateson papers) are now deposited in the Archives of Queen's University where they are available for those who might wish to pursue the matter.

A question of much interest was whether Alan's sympathy for William Bateson reflected a deeper understanding of his work than Alan's scientific contemporaries had been capable of. The answer seems to be – as indicated in Alan's own words above and below – that he was interested in Bateson, because of their connection with the same Cambridge laboratory, because of their common interest in fowl genetics, and last and most importantly, because Bateson was a fascinating figure in his own right. The revolution in EB of the 1990s was too late to inform Alan's account, as it did mine. However, we both had long careers as productive "card carrying" scientists, rather than as historians of science. As will become apparent here, this difference in perspective makes our account different from previous biographies.

For a scientist looking at history there are three overriding double questions: What did he (she) know and when did he know it? What did he think and when did he think it? What did he do and why did he do it? Answers to these questions give us information on *process* – the process of scientific discovery. If the elements of that process have remained essentially unchanged since Bateson's day – an assertion assumed and not argued here – then a better understanding of ways this outstanding contributor to scientific progress operated may help us improve the process of discovery as it now operates [17].

For many reasons Bateson's life is ideal for this purpose. Before the days of email, correspondence was not entered into lightly, and was usually composed with care. Often there was at least one preliminary draft, and neither this nor the final copy could be destroyed by merely pressing the "delete" button. Bateson's letters were treasured and kept. Beatrice was able to recover many from his correspondents after his death. So it is now possible to reconstruct, in blow-by-blow fashion, the genesis of his ideas and discoveries. Finally, whereas the ultimate value of much contemporary work still remains to be determined, a century later relatively clear end-points have emerged so it is now easier to reach a consensus on the merit of his contributions. Historians tend to label this as "Whig history," but we are unapologetic [18].

Part 2 by Alan Cock

Why is Bateson interest-worthy? A distinguished Victorian-Edwardian scientist – but there are many such. He was one of the founders of the modern science of genetics, but the layman is likely to ask: what really important discoveries did he make? The only answer possible is a rather lame one: two near-misses. He came very close to rediscovering the epoch-making work of Mendel, done thirty-five years earlier but buried in neglect. Bateson *was* the first to show that Mendel's laws apply to animals as well as to plants. He discovered the important phenomenon of genetic linkage, though the explanation of it that he offered proved to be widely off the mark. Near misses, it will be said, are rightly soon forgotten, together with their perpetrators. He invented much of the basic terminology of modern genetics (including the term "genetics" itself), but terminology is a matter for the specialist, and a rather pedestrian affair at that. He demonstrated that acorn worms are evolutionary relatives of the vertebrates. But what are acorn-worms, and who cares?

So far Bateson probably sounds eminently worthy but eminently dull: the last kind of person whose biography one would want to read – or write. What I have stated baldly above is true, but put thus it gives a very misleading picture. To his contemporaries he was quite the opposite of this: not dull, but fiery and always the center of controversy, and to many not even worthy, since he had cast doubt on doctrines such as Darwin's theory of evolution by natural selection, and the chromosome theory of heredity, which in many minds had crept gradually from the status of scientific theories to that of sacred dogmas.

The figures of the past tend to acquire a patina which, instead of giving them added luster, makes them appear more solemn, even pompous, than they really were. Late in life, Bateson was offered, and declined, a knighthood. This tells us several things about him, one of which is that he set no

inordinate store by pomp and circumstance. Certainly he took life seriously – in science, in art, in family affairs and, on the rare occasions when he involved himself with such matters, in politics. But he did not ordinarily lapse from seriousness into solemnity. Examples of his dry humor and often deadly wit are scattered through these pages. For sheer light spirits, intermingled with the humdrum routine of scientific work, the story of his bets is as good an illustration as any. When sorting through his young experimental chickens, he used to lay small bets with his colleague (Reginald Punnett) on the sex of dubious individuals. These bets – and their settlement – are recorded in their experimental notebooks, alongside scientific notes about things like plumage color and comb type.

The things about Bateson which most interest and attract me fall into two categories which, although conceptually separable, in practice interlock a good deal. The first category concerns his influence on the development of genetics – and of biology generally. The second devolves around his personality and character. Over a period which can be centred very crudely but conveniently on the year 1900, biology underwent a transition from being mainly an observational and descriptive science to being mainly an experimental and analytic one. Bateson's career nicely spans this divide, and not just in a chronological sense. He began in an entirely observational way, in the field of comparative anatomy and embryology. Though his work there brought him the beginnings of a "sound" reputation – even some fame – he soon became disillusioned with the limited scope and logical weakness of its interpretative aspects. Switching to a new – and far less fashionable – field, the study of variations, he still remained for long at an essentially observational level. Not until 1897–1898 did he take the vital step of using experimental breeding to investigate the heredity of his variants. He still lacked any general analytical scheme for interpreting his results, but all this was changed dramatically in 1900 by the rediscovery of Mendel's work. Almost overnight, genetics became an analytical science, and broad and exciting new horizons were opened.

In the years that followed, Bateson played a prominent part in developing and extending Mendel's work. However, no list of his discoveries could convey the strength and breadth of Bateson's influence. On his colleagues and students he exerted an almost magnetic effect. They felt an intense loyalty and affection. There was a distinctive Batesonian style or approach to genetics, of which one characteristic was his strong concern for the interrelationships between genetics and the rest of biology. So great was the rate of progress in genetics that many other geneticists of the period tended to regard genetics as a self-contained and autonomous empire. His concern for biology-as-a-whole did sometimes help to lead Bateson into trouble, notably his opposition

(which he eventually admitted was erroneous) to the chromosome theory of heredity.

As to his personality and character, those well-worn adjectives, complex and contradictory, are nevertheless singularly apposite. Self-critical, iconoclastic, no glad sufferer of fools, aesthetic, reticent on emotional matters, ... but such lists tend to be unconvincing. A single story can perhaps do more. This atheist son of a Doctor of Divinity used to read passages from the Bible to his three sons at breakfast – lest they grow up to be empty-headed atheists.

Anyone intending to write at length about Bateson faces at once an obstacle and an encouragement in the 160-page biographical *Memoir* (1928) by his widow Beatrice [8]. This is so well done as to be, within its self-imposed limitations, unsurpassable. The most obvious limitation is that she never delves at all deeply into his scientific work, though naturally it is always popping in and out of the narrative. There are other omissions too (perhaps imposed by limitations on length), and one or two places where Beatrice conceals more than she reveals. The laconic single sentence with which she introduces herself (“In June 1896 we were married, and I began to learn what life may be.”) hides a story worthy of a place in any “Selected Readings in Romantic Engagements.”

They met in the winter of 1898–1899 in Dresden on an opera-going expedition. There was a whirlwind courtship and an engagement party at St. John’s College, Cambridge, where William got somewhat inebriated. Mrs. Durham thought William might have alcoholic tendencies and decided that one alcoholic in the family (her husband) was quite enough. William was told, with no shred of explanation, that he was not to communicate with Beatrice again. Beatrice herself was carefully chaperoned by her mother and an elder sister. They were thus kept apart for six years. In 1896, Beatrice published, as a “come-on” signal to William, a short story in the *English Illustrated Magazine* which was her own story thinly disguised. William made contact *via* an older married woman (who played fairy godmother to him again ten years later). They were promptly married and lived happily ever after (they really did, so far as I can tell).

The encouragement to be gleaned from Beatrice’s memoir is in her preface, where she writes; “Later a more competent hand may, I hope, undertake a full biography and account of William Bateson’s work.” This was not just a pious expression. She did something practical about it, by preserving his papers, together with a fairly substantial collection of his letters, borrowed and copied from friends. Both of these are now with me.

Part 3 by Donald Forsdyke

Beatrice placed advertisements for letters in *The Times*, and in the international journal *Nature*. Soon they came from far afield, some of them unfortunately too late to be included in her *Memoir*. But, despite her efforts, many letters could not be located. Writing in 1927 to Charles Hurst, whom we shall meet in Chapter 10, she noted [19]:

I am very glad you have your letters, the loss of the others was a dreadful blow to me. I am of course collecting for a qualified biographer and historian later on. ... I am not intending to make any immediate use of the material I am now collecting. I am simply putting together all that I can collect with a view to a future biography and full history. The Press has in hand, with [a selection of] my husband's [scientific] papers, a short personal memoir.

Commenting on her *Memoir* she noted: "It gives a fair guide to any future biographer should we all be underground, and it is quite uncontentious. At least that is what it is aimed at." Her decisions on what to add or omit were not made alone. For example, her journalist sister-in-law, Margaret, wrote (June 22, 1926):

There are many little allusions to persons which they were never meant to see and which would or might give pain to them or their friends. Also there are passages relating to my own affairs which I should not wish to be published; and similarly allusions to the affairs of other members of the family. But, when all omissions are allowed for, there remains much which is entertaining, humorous and interesting.

When Alan Cock and David Lipset examined the papers in January 1976, they were essentially as Beatrice had left them, done up in bundles, sometimes with an added note, but otherwise uncatalogued. Alan ruefully noted that the price of the "coup" of bringing the papers West-to-East across the Atlantic, would be some fairly basic, and time-consuming, archival work, carried out with shoe-string support from UK funding organizations. Furthermore, many people who had known Bateson were, at that time, still alive. To really fulfill Beatrice's mandate, people such as Cyril Darlington, who had been a cytologist at the John Innes Institute, and Darwin's granddaughter, Nora Barlow, a volunteer worker at the Institute, would have to be interviewed. Again, this was all expensive and time-consuming.

However, now we are in the twenty-first century and William Bateson and those who knew him are, indeed, "underground." There are few people to interview, and the underlying science is better understood. The time would seem propitious for the completion of Beatrice's (and Alan's) work. In Chapter 1, "A Cambridge Childhood," some key characters are introduced, and early family connections between the Darwins, Butlers and Batesons are

noted. There is description of William's birth to a world of great privilege – one of six diversely gifted siblings. After aimless, “self-satisfied and desultory” years at Rugby school, he flowered at Cambridge in animal morphology, rather than in physiology. Among earlier graduates of the school of physiology, were George Romanes (animals) and Francis Darwin (plants), who were both research associates of Charles Darwin in the years before his death. In various ways they were to greatly influence Bateson. However, it was the enthusiasm of his fellow St. John's College student, Raphael Weldon, that first led him to morphology.

Chapter 2, “From Virginia to the Aral Sea,” describes Bateson's successful studies on the embryology of acorn-worms – which won him a college fellowship – but this was followed by disillusionment with the embryological approach. Searching for a new theme there followed solitary expeditions to the Aral Sea and the Nile delta. Noting a frequent lack of correlation between an organism's appearance and the pressures of its environment, Bateson began increasingly to question the potency of Darwinian natural selection. Having lost faith in Darwin, he turned to Darwin's cousin, Francis Galton, whose theoretical insights had moved far beyond Darwin, and who was now pressing for statistical studies of biological variation. Because of the importance of Galton's influence, he is accorded an entire chapter at an early stage (Chapter 3). Others accorded chapters at appropriate points in our narrative are Romanes (Chapter 5), and Samuel Butler (Chapter 19).

Returning empty handed from foreign travels, and with ideas not in accord with the conventional wisdom, Bateson's position was precarious. While Weldon went from strength to strength – to a Chair at University College in London, and to a Fellowship of the Royal Society (FRS) – Bateson became Steward of the St. John's College kitchens. His wooing of Beatrice having failed, he buried himself in a heroic attempt to collate reports of biological variants in the medico-scientific literature. A massive book, *Materials for the Study of Variation* resulted (Chapter 4). Here he placed particular emphasis on the fact that, whereas under Darwin's natural selection progressive changes over many generations (i.e. many steps) were required to generate new and perfect types, such perfect types (e.g. a type with one perfect extra finger), could actually appear in only one generation (one step). This appearance in one generation he described as the “discontinuity of variation,” to contrast it with the “continuity of variation” needed for successive steps.

However, he went further, holding that “discontinuity of variation” was of high importance for understanding the “discontinuity of species” – namely, that organisms divide into discrete groups (species). The members of a species share common characters and vary *within limits* about a mean type. But how do they vary *beyond* the limits to create a new mean type (i.e. a new species)? Weldon and others correctly scorned Bateson's claim that his

circus of oddities was somehow of relevance to the question of the origin of species. Yet his catalogue of deformities greatly interested physicians and horticulturalists, many of whom were to aid him in the years ahead. At this time Bateson helped Mrs. Herringham, the wealthy wife of a St. Bartholomew's Hospital physician, through a personal crisis. She was to become the "fairy godmother" mentioned above by Alan. His growing estrangement from Weldon was later to broaden into a major dispute between "Mendelians" and "Biometricians."

Despite its generally poor reception, *Materials* won high praise from Galton, and Bateson joined Weldon as a FRS. However, it was to Weldon that the Royal Society (RS) gave financial support in the form of grants for his "biometric" studies carried out with mathematical advice from Karl Pearson. Chapter 6, "Reorientation," outlines Bateson's indirect attack on Weldon through the RS Committee responsible for Weldon's funding, which was chaired by Galton. There was also a direct attack. Week after week throughout the 1890s the Victorians scurried to read the latest scientific controversy in the pages of *Nature*, to which Bateson was often a prominent contributor with Weldon his predominant target. This led to a major coup in 1897, the reorientation of the RS Committee to include Bateson and some like-minded biologists, its renaming as the Evolution Committee, and the extension of funding to Bateson and his research colleague Rebecca Saunders. There was even a glimmer of professional advancement – he was appointed Deputy to the Professor of Animal Morphology in 1899 – the year he and Beatrice moved to Grantchester with their family of two (Chapter 7).

The early breeding studies of Bateson and Saunders, and the joyous "rediscovery" of Mendel's work are the subject of Chapter 8. Bateson lamented the overzealous focus on Darwin that had left Mendel in the shadows. As "Mendel's Bulldog" (Chapter 9), and with the staunch support of Hurst (Chapter 10), Bateson met the Biometricians (Weldon and Pearson) head on. Soon there collected around him an eager, albeit poorly financed, band – the Mendelians – many of whom were brilliant women for whom prevailing attitudes made academic advancement difficult. By 1906 the battle was over. Bateson was "On Course" (Chapter 11) and a Cambridge professorship in Biology soon followed. 1909 was his triumphal year, the year of the Darwin Centenary celebrations, the year when he articulated more clearly than ever before bold concepts that, as with Mendel decades earlier, were to create not the slightest ripple on the intellectual waters of his time (Chapter 12).

Around the time of the Mendelian "rediscovery" it had been observed that the movement of chromosomes during cell division was precisely what would be predicted if they were the carriers of Mendel's units. Although acknowledging the probable correctness of this (indeed he was one of the first to note it), to the end of his life Bateson was highly critical of the supporting

evidence (Chapter 13). While many commentators have construed this as pig-headedness, later chapters suggest it was Bateson's deep reading of Galton, together with his apparent subconscious adoption of Romanes, that had led him to demand more of chromosomes than the mere transfer of character units (genes).

But the term of his Professorship was for only five years, and facilities for extensive breeding studies still escaped him. His appointment as the first Director of the John Innes Institute in 1910 changed all this (Chapter 14). He was no longer fighting the establishment, he *was* the establishment. Moving on from Mendelism he now turned to the study of the many "exceptions" that he had long "treasured" – rogue peas, variegation and chimaeras – in the hope that they might somehow lead to his goal, an understanding of the origin of species. Despite many distractions – the escalating demands of the Eugenics movement (Chapter 15) and growing militarism among German scientists (Chapter 16) – he was able to complete another major book, *Problems of Genetics* that was published in 1913. The outbreak of the First World War found him presiding at the British Association (BA) in Australia. His two eldest sons were soon to die, one on the battlefield, one a suicide.

Chapter 17 ("My Respectful Homage") describes Bateson's struggles in a bizarre post-war world that, despite his Victorian-Edwardian past, he largely understood. Unfortunately, it did not understand him. In many quarters his salutation of the work of the school of Thomas Morgan in America and his recognition that the chromosomes were carriers of Mendel's units, were seen as capitulations. However, his speeches were skilfully framed to reveal his abiding dissatisfaction. Still unresolved was the problem of how the limits of a species were maintained yet could be transgressed for the production of a new species. In these final years, his life-long interest in art – William Blake, Old Master prints, Japanese drawings – flourished, and his expertise was recognised by election to the British Museum's Board of Trustees (Chapter 18).

The remaining chapters deal with issues that do not fit neatly into the earlier chronology. Lipset's biography of Bateson's son, Gregory, began with an entire chapter on Samuel Butler [13]: "Butler's ghost will haunt this book – not only in [Gregory] Bateson's thought, but in the family culture from which it grew." The same applies to the present biography, except that, in this case, Butler comes late (Chapter 19) because, while he was always there, William Bateson unfortunately failed to note his writings until late in life. Chapter 20 describes the various "Pilgrimages" that Bateson made to the Brunn monastery where Mendel had worked, while Chapter 21 ("Kammerer") describes Bateson's long-standing opposition to the view that characters acquired in a single generation could be transmitted to offspring (Lamarckism). Finally, there were his forays into politics, both of post-war reconstruction

(Chapter 22) and of his university – the long battle for women’s rights (Chapter 23). After his death in 1926 there was a continuing failure to understand, and hence to correctly represent, Bateson and his work (Chapter 24). In the Epilogue the conclusion is drawn that to Darwin’s tall shadow had been added that of Mendel. Just as Darwin had once enshrouded Mendel, they now collectively enshrouded the work of many others, including that of Bateson himself. Indeed, it is argued that the history of the biological sciences in the twentieth century would have been transformed had the works of several pre- and post-Mendelian “Mendels” been better understood.

There are many quotations. We have not hesitated to let Bateson speak for himself. His rich prose conveys the story far better than we can. Unless otherwise stated, all italicized emphasis is Bateson’s. However, the meaning of language has and is changing. What Bateson meant by a word may not coincide with what we now understand it to mean. Wherever we foresee ambiguity, explanatory remarks have been inserted within square parentheses. A few spelling differences – he wrote “shew” rather than “show” – have been altered without comment.

Many inviting avenues emerge in the course of a work of this nature. But the exigencies of time have dictated that many stones be left unturned. We have been led, partly by intuition and partly by serendipity, to explore some avenues rather than others, and to turn some relatively inconspicuous stones while ignoring some prominent ones that others might consider more worthy. Where we have erred it has been in favor of nineteenth century authors. Where we have omitted to mention, or have unwittingly duplicated, relevant ideas of modern authors, we offer our apologies. But there is a certain logic in this. The writings of nineteenth century authors provide the raw materials for later historians; we saw an error in the former as more likely to impair our story than an error in the latter. For example, John van Wyhe has recently exposed as “entirely absent from the primary evidence” the long held notion that Darwin deliberately delayed publication to keep his ideas on natural selection a secret [20]:

The myth of Darwin’s delay has remained unquestioned for far too long. It generates a cascade of subtle errors that ultimately accumulate to a distorted picture of the man and his science and indeed to early Victorian scientific communities. A varied and overwhelming array of evidence demonstrates that Darwin did not avoid publishing his theory for 20 years. ... And this is the ultimate lesson to be drawn from the myth of Darwin’s delay: the danger of confirmation bias lurks over historians just as much as scientists, if not more so. Once we, as historians, come to believe a story, it is easy to find apparent confirmations and, when the evidence contradicts it, difficult to let it go.

It appears that much concerning Bateson has been “difficult to let go.” As far as we can determine, so different from those of others are the interpretations of Bateson that we offer, so central was Bateson in the post-Darwinian period, and so central was this period to biohistory in general, that our book has come to represent not only Bateson’s life and science, but also a revised history of the biosciences.

Part I Genesis of a Geneticist

Chapter 1

A Cambridge Childhood (1861–1882)

In the beginning was the Word, ... And the Word was made flesh
Gospel according to St. John

There are stories within stories. To the extent that the story of William Bateson can be demarcated it begins in 1809 in the city of Shrewsbury in England. It ends, perhaps, in 2009, in no specific location, but in the minds of people throughout the world when celebrating the two hundredth anniversary of Darwin's birth. By that time computer analyses of DNA sequences (bioinformatics) had revolutionized evolutionary biology. The informational basis of heredity ("the word") was explicit. As will be related here, this seems to have been an essential precondition for the full recognition of Bateson and his work. Our story begins with a general overview and an introduction of the main characters.

1809–2009

The two hundred years 1809–2009 divides conveniently into four equal parts with three major chronological milestones – 1859, 1909 and 1959. In 1809 Jean-Baptiste Lamarck published *Philosophie Zoologique* in France, and Charles Darwin was born in Shrewsbury, England. For several years Darwin was a boarder at Shrewsbury School, where Dr. Samuel Butler was the headmaster. The father of William Bateson, William Henry Bateson (1812–1881), also attended Shrewsbury School, and was accepted by St. John's College, Cambridge, in 1829, where he was a contemporary of Butler's son Thomas. Both were contemporaries of Darwin who was at Christ's College from 1828 to 1831. Thomas Butler and Darwin were acquainted [1]. All three were intended for the church so it is possible that they met around 1831, and may even have discussed, but almost certainly not understood as we do now, the above line from the gospel according to St. John.

Thomas Butler became Canon at Langar Rectory in Nottinghamshire, where his son Samuel was born in 1835. Neglecting his religious studies Darwin turned to extracurricular biology before graduating and setting off on his famous voyage on *The Beagle*. William Henry Bateson continued his religious studies and graduated in 1836. After serving in various parishes, he

returned to St. John's with a fellowship that required that he not marry (a rule not revoked until 1882). Among his pupils (1838–1841) was Charles Kingsley who in 1860 became Professor of Modern History at Cambridge. In 1846 William Henry was elected Senior Bursar, and in 1857, Master. He was acquainted with the younger Samuel Butler who, in response to paternal pressures, began studies at St. John's in 1854 with the intention of entering the church.

1859 saw the publication of Darwin's *The Origin of Species by Natural Selection, or The Preservation of Favoured Races in the Struggle for Life*. Here Darwin proposed natural selection as a major force in evolution, but with the caveat: "I am convinced that natural selection has been the main but not the exclusive means of modification." Since man was deemed to have derived from lesser forms, the book was widely perceived as challenging religious orthodoxy. Thomas Huxley, who taught natural history in London at the School of Mines (now the Imperial College of Science and Medicine), soon became known as "Darwin's bulldog." He traveled the length of the land spreading the Darwinian message. In 1859 the younger Samuel Butler emigrated to New Zealand, omitting for the first time in his life to say his prayers at bedtime. He soon acquired Darwin's book and was to become, as later described by William Bateson, "the most brilliant, and by far the most interesting of Darwin's opponents."

Jumping ahead to the next milestone, 1909, Darwin and the younger Butler have passed away. Darwinism is triumphant and the Centenary of Darwin's birth is celebrated with the volume *Darwin and Modern Science* to which many evolutionists contribute. We can, with hindsight, now assign to Bateson, Professor of Biology at Cambridge, the role of the most brilliant and by far the most interesting of Darwin's opponents, and his contribution to the volume reflects this. Not surprisingly, many of his contemporaries consider him by far the most annoying of Darwin's opponents. He has converted the world to Mendelism and Wilhelm Johannsen now suggests that the factors responsible for the Mendelian character units be referred to as "genes." Across the Atlantic, Edmund Wilson and Thomas Morgan are enthralling freshman classes at Columbia University, which include Calvin Bridges and Alfred Sturtevant. A popular textbook is *Variation, Heredity and Evolution*, authored by one of Bateson's students. Over the next decade Bridges and Sturtevant, together with Herman Muller, will flesh out Mendel's genic abstractions under Morgan's patient tutelage.

Bateson is at the height of his power and influence. Yet, his essay in the commemorative volume is neither self-congratulatory, nor filled with heady optimism. Darwin failed to show, as the title of his great book suggests, that natural selection can originate a species. For this we must look beyond genes. This is not to say that genes are not involved in the critical events that

differentiate one species from another. But for the spark that originates we must look elsewhere. Bateson's message is lost on his contemporaries and most who come after. The more he enunciates it, the more his estrangement. Acknowledging some bewilderment, the German evolutionist August Weismann diffidently declares that the task of estimating "the influence of Darwin's theories on his time and on the future" might be "better accomplished on the 200th than on the 100th anniversary of his birth" [2].

Now it is 1959, and a new cast of actors are on the evolutionary stage. We are in the television era. More importantly, we are in the era of extensive public support for biomedical research. The evolutionists meet at Chicago to celebrate the 150th anniversary of Darwin's birth and a three volume text emerges. The politician Adlai Stevenson joins a television debate. Who better to start the proceedings than Julian Huxley, no less laudatory of Darwin than his famous grandfather [3]:

The emergence of Darwinism ... covered the fourteen-year period from 1858 to 1872; and it was in full flower until the 1890s, when Bateson initiated the anti-Darwinian reaction. This in turn lasted for about a quarter of a century, to be succeeded by the present phase of Neo-Darwinism, in which the central Darwinian concept of natural selection has been successfully related to the facts and principles of modern genetics, evolution and palaeontology.

Among the evolutionists present there is much complacency. A "modern synthesis" of Darwin and Mendel has been achieved. Had Bateson been there he would have referred to Voltaire's hero Dr. Pangloss for whom "everything is for the best in the best of all possible worlds." Now, with the help of the molecular biologists, who have recently announced the structure of DNA, it seems just a matter of working out the details. Most of the distinguished contributors – including Ledyard Stebbins, Alfred Emerson, Theodosius Dobzhansky, and Sewall Wright – do not mention Bateson. If there is a hero it is Bernard Kettlewell of Oxford, whose studies with the peppered moth Edmund Ford declares to have shown "evolution in action." The Agassiz Professor of Zoology at Harvard, Ernst Mayr, refers to what he calls the "mutationism of De Vries and Bateson," which was "wrong," and "meaningless in the light of our new genetic insight" [4].

Early Years

A narrower demarcation of Bateson's story begins with his birth in 1861 and ends with his death in 1926. We cover here his life up to 1882 when he graduated. In 1857 it became clear to William Henry Bateson that he would be elected Master of St. John's and so would no longer be bound by the restrictions of his fellowship. He wrote to his sister Margaret (Feb. 26):

I have an important announcement to make to you, no less than that I am going to be married, and, as I hope, without many weeks delay. You will remember that I wrote to you the other day saying that I might shortly visit Liverpool. The fact is that, so soon as I had it clear before me that I should be made Master of the College, I inquired of Honora [their sister] whether Annie Aiken, her great friend, upon whom I had long secretly set my heart, was free and at all likely to accept any advances from me. I had never made it known to anyone before, much less to the Lady herself, that such was the state of my affection. Honora could make out nothing except that she was free – so as faint heart, etc. down I came last Tuesday week and by incessant dedication of myself to the object of my mission I am delighted to tell you that I am now an accepted suitor. I have thought the matter over with all the prudence and coolness that I could command and I can say *deliberately* that such another wife could not be found. There is literally nothing wanting to complete the group of female virtues and fascination and I am as happy as possible. It was only yesterday afternoon that the final Yes was said – so you have the earliest intelligence. I think you hardly know her, but I am persuaded that you will see all her excellence at first sight and in no long time that you will regard each other with all the love and affection of sisters.

So on June 11th 1857 William Henry Bateson was married to Anna Aiken (1829–1918) in St. Bride’s Church, Liverpool, by the Reverend Thompson the Regius Professor of Greek at Cambridge. Their first child died shortly after birth in January 1859. Margaret was born in 1860. This was followed a year later by William, then three daughters (Anna, 1863; Mary, 1865; Edith, 1867), and finally in 1868 another son, Edward. Unlike some others who enter this story – Grant Allen, Arthur Balfour, William Brooks, Henry Martin, George Romanes and Hugo de Vries – who were all born in 1848, William Bateson’s date of birth allowed time for those who might influence his education to digest their Darwin and loosen the ties of religious orthodoxy.

If one had to prescribe the ancestors, the time of birth, and the location, of someone who might best carry the Darwinian torch forward and into the twentieth century, Bateson’s would be hard to match. The parents of William Henry Bateson and of Anna Aiken were vigorous and wealthy. Richard Bateson (1770–1863) was a Liverpool cotton merchant and in 1806 he married a soldier’s daughter, Lucy Wheler Gordon (1781–1866), who gave birth to twelve children. James Aiken (1792–1878) was a Liverpool shipping merchant and a Justice of the Peace, described by Beatrice as a “staunch liberal.” In the course of his voyages he met Anna Elizabeth Harrison of Charleston, South Carolina, and they were married in Liverpool in 1815.

William Henry and Anna were intent on bringing about reform in their domain, the Cambridge colleges. In 1850 William Henry had been secretary