Journal of the History of Biology (2016) 49:425–459
DOI 10.1007/s10739-015-9424-5

Irene Manton, Erwin Schrödinger and the Puzzle of Chromosome Structure

NICOLA WILLIAMS
School of Philosophy, Religion and History of Science
University of Leeds
Leeds LS2 9JT
UK
E-mail: ph11nw@leeds.ac.uk

Abstract. Erwin Schrödinger's 1944 publication *What is Life?* is a classic of twentieth century science writing. In his book, Schrödinger discussed the chromosome fibre as the seat of heredity and variation thanks to a hypothetical aperiodic structure – a suggestion that famously spurred on a generation of scientists in their pursuit of the gene as a physico-chemical entity. While historical attention has been given to physicists who were inspired by the book, little has been written about its biologist readers. This paper examines the case of the English evolutionary botanist and cytologist Irène Manton, who took an interest in *What is Life?* for its relevance to her own research in chromosome structure as a clue to plant phylogeny. Drawing on recently discovered correspondence between Manton and Schrödinger, the paper reconstructs Manton 's path to the book (including the role of the chemist-philosopher Michael Polanyi) and her response to it by way of throwing new light on a pivotal moment in the history of the debate on chromosome structure.

Keywords: Irene Manton, Schrödinger, Polanyi, Chromosome structure, Cytology, Genes

Introduction

"When a great physicist takes the trouble to explain in simple language some of his matured thoughts on topics of general interest outside his own subject, it is an event for which one cannot be too grateful." So wrote the Manchester botanist, Irène Manton (1904–1988), FRS, in 1945 just as the war had begun to subside in Europe. Erwin Schrödinger's recent excursion into biology in his book *What is Life?* (1944) had captured her attention and prompted her to write "Comments on

Chromosome Structure,'' soon to appear in the prestigious journal *Nature* (Manton, 1945a, p. 471).[1] In the meantime she found herself in correspondence with the man himself.

The cross-disciplinary sweep of *What is Life?* was unusual for its time (Penrose, 1991, p. 2), and yet Schrödinger was a particularly successful rhetorician (Ceccarelli, 2001, pp. 82–110). Historians have documented its significant impact with comments such as: ''Everybody read *What is Life?*'' (Judson, 1996, p. 250). Manton's piece was one among many commentaries and reviews concerning a publication read by biologists and physicists alike.[2] The text has received attention for its importance to the emergence of molecular biology (Yoxen, 1979; Symonds, 1986; Kilmister, 1987, pp. 234–251; Dronamraju, 1999; Keller, 1990; Sarkar, 2013), and, more specifically, molecular genetics. Schrödinger's own research has been viewed as pivotal in terms of the post-war developments in biology (Pauling, 1987, p. 228). Francis Crick, a physicist involved in the 1953 discovery of the structure of DNA, described the book as ''peculiarly influential,'' and Maurice Wilkins, likewise a physicist involved in the search for the double helix, noted that ''Schrödinger's book had a very positive effect on me and got me, for the first time, interested in biological problems'' (Moore, 1989, p. 404). Historians have considered the influence of physicists in general upon the discipline of biology as contributing an ''attitude: the conviction which few biologists had at the time, that mysteries can be solved'' (Fleming, 1969, p. 161).[3] Such confidence is perhaps unsurprising after the spectacularly successful creative period physics witnessed since the turn of the twentieth century, of which Schrödinger was a part. The contribution he made to the conversation between biologists and physicists is certainly without question yet it is the story of the book's impact specifically upon *physicists-turned-biologists* in the fledgling field of molecular genetics that has been viewed as the legacy of *What is Life?* Physicist-turned-biologist, Gunther Stent (1966, pp. 3–22), for example, thought that *What is Life?* had no influence upon biologists at all (Dronamraju, 1999, p. 1075). Yet James Watson, who began his career

[1] Manton's given name was Irène, but she dropped the accent and went by Irene, which will be used here.

[2] See Yoxen (1979) and Ceccarelli (2001) for more information on reviews of *What is Life?* For a comparative disciplinary history of biology and physics in the early twentieth century, Schrödinger provides a brief overview (1944, pp. 47–48), as does Smocovitis (1996, p. 107).

[3] Fleming quotes from the Hungarian-American physicist and inventor Leó Szilárd. On the topic, see also Keller (1990, p. 390), whose paper considers the social authority of physicists, including their language and attitude.

as a biologist (he took his degree in zoology) before he ventured into molecular genetics, said "From the moment I read Schrödinger's *What is Life?* I became polarized towards finding out the secret of the gene" (Moore, 1989, p. 403; Fleming, 1969, p. 180). In a recent work of popular science discussing the DNA "discovery," the author noted that it seemed "surprising that [*What is Life?*] had an equally profound impact on biologists," in light of its appeal to both James Watson *and* Francis Crick (Gribbin, 2013, p. 239). It is in this context, then, that the case of a biologist *not* in pursuit of the gene who nevertheless drew inspiration from the classic book is of particular interest. Irene Manton, who was neither a biophysicist nor a geneticist, late in her life reflected on the impact of *What is Life?* and described it as a stimulus on her own thoughts in her chosen field of botany (Yoxen, 1979, pp. 20, 46).[4]

Schrödinger had posed the question in *What is Life?*, "How can the events in space and time which take place within the spatial boundary of a living organism be accounted for by physics and chemistry?" (Schrödinger, 1944, p. 3). The phenomena under the scrutiny of the two sciences are fundamentally different – animate matter being the focus for the biologist and inanimate matter for the physicist. Furthermore, the laws of nature as known to the physical world do not apply so readily to the biological world. Schrödinger set the scene for a discussion of intracellular structures (in particular, the chromosome fibre) as the "most essential part of a living cell" (Schrödinger, 1944, p. 5). The chromosome as the seat of the heredity material was of course an established fact. But in attempting to address the intractable biological problems of heredity and variation, relative to a physical structure, the difficulty encountered was in contemplating "a physical substance that had to be almost perfectly stable and yet express immense variety" (Judson, 1996, p. 250). Schrödinger's solution was to propose an aperiodic crystalline structure. Rather than the ordinary periodic crystal as known to organic chemistry with its dull, repetitive pattern, the aperiodic crystal represented an elaborate, coherent, meaningful design. Rather than the same old ordinary wallpaper, then, the essential structure, the aperiodic crystal, was akin to a masterpiece of embroidery much like a Raphael tapestry (Schrödinger, 1944, p. 5; Kay, 2000, p. 61). The potential for immense variety was stored in a chemical "code-script" that was operationally akin to the "Morse Code."

[4] Edward Yoxen received correspondence from Manton, in which, according to Yoxen, she wrote of the considerable impact of the book upon her thoughts. Further to this, Yoxen also discusses another influential biologist, J. A. V. Butler, who was likewise inspired by the book.

Figure 1. Professor Irene Manton was awarded an honorary degree of Doctor of Science at the University of Lancaster, 1979. Reproduced with the permission of Dr. Peter Evennett

This study presents a fresh approach to previous discussions of the impact of *What is Life?* while at the same time highlighting the work of a biologist who was both influential and pioneering during the period in question and beyond, and yet who has since become historiographically invisible (Figure 1). Manton's journey to *What is Life?* is traced from the time of her training at the University of Cambridge through the sixteen years she worked at the University of Manchester.[5] Here newly discovered correspondence between the biologist Manton and the physicist Schrödinger, written in the aftermath of the appearance of *What is Life?,* are examined. The letters provide a window into early liaisons between the historically disparate sciences of biology and physics and make possible the identification of view some interesting, insightful and sometimes misaligned perspectives. The Manton–Schrödinger letters are of historical interest not only in terms of potential insight into interdisciplinary communication, but also in light of

[5] This paper is not presented in the pretext of a traditional "life and work," for which the reader can find more details in Irene Manton's biographical sketch for the Linnaean Society of London by Leadbeater (2004). Manton was the first female president of the society, serving 1973–1976.

Manton's designation as a botanist and cytologist, and for that matter, as a woman in science.[6] Another intriguing aspect of the story behind the correspondence has been the discovery of a connection between Manton and the renowned physical-chemist and philosopher of science, Michael Polanyi.[7] Polanyi's writings in philosophy of science, particularly his book *Personal Knowledge*, published in the late 1950s, involved much biological discussion. Manton's research, it seems, proved a useful reference from which he drew inspiration. Preparations for this later work were already underway in the 1930s and 1940s, when both Polanyi and Manton worked as scientists in Manchester (Nye, 2011, p. 261). After Manton's encounter with Schrödinger, the story moves on to consider some of the conclusions she drew from her Manchester work and the implications they had for her evolutionary thinking. Written in the aftermath of the Second World War, Manton's views were unorthodox in the context of the Evolutionary Synthesis, and a reference to Schrödinger's work surfaces in the controversy.

A lot of attention has been paid to biophysicists, but it is important to realise that biologists do feature in the story of *What is Life?,* and do so from its inception. Schrödinger referred to the work of biologists and physicists in preparations for his book. Where biologists are concerned, however, Schrödinger's consultations favoured only those of a genetical bent. In his preface he thanked biologists J.B.S Haldane and C. D. Darlington. Haldane was a population geneticist; while Darlington was a chromosome cytologist, he was theoretically poised with a ready interest in genetics and the molecular realm.[8] Inevitably, then, there is an inherent biophysical and genetical bias to the work. Manton was initially drawn to Schrödinger's discussion on chromosome structure and the importance this might have in relation to her own evolutionary work. After reading *What is Life?,* Manton alerted Schrödinger to the existing cytological research on the topic. Notwithstanding a desire to

[6] Concerning Manton and Cambridge, case studies of women scientists presiding there in the generation before her have been well-written: see Richmond (2001, 2007). Also representative of this generation is the "life and work" of Cambridge plant morphologist Agnes Arber, FRS, undertaken by Packer (1997).

[7] Manton to Schrödinger, letter dated 25th February 1945, located at the Dublin Institute for Advanced Studies, Schrödinger Archive: SCH/C/1.

[8] Darlington had a reputation for controversy in the early 1930s for his theorising in *Recent Advances in Cytology* (Smocovitis, 1996, p. 136). He was hired at the John Innes Horticultural Institute (JIHI) by William Bateson, who was an early supporter of Mendelism and had imported his "genetically oriented" research to the JI on leaving Cambridge in 1910 (Richmond, 2001, p. 83). Bateson maintained non-Darwinian evolutionary views although his underling Darlington did not share them (Harman, 2004).

unite evidence from two otherwise disparate fields to reach an, as yet, too prescient an ideal of comprehensive understanding, Manton set her sights more locally to argue that cytological work on the spiral structure of chromosomes was being neglected in the wider context of her own discipline of biology. The topic of spiral chromosome structure continues to be neglected by historians of biology. There are few case studies available so far that chart the work of cytologists in the 1930s and 1940s specifically, although two well-known cases published are those of American cytogeneticist Barbara McClintock (Keller, 1983) and British cytologist Cyril Dean Darlington (Harman, 2004). Harman noted that Darlington had a ''preoccupation in the 1930s and 1940s with spirals and spindles'' (Harman, 2004, p. 207), a venture that led him to take an interest in the material basis of the heredity material, placing his work firmly on the ''heredity trail.''[9] But not all cytologists interested in the chromosome were necessarily interested in pursuing the gene. The early twentieth century biological literature was replete with genetical success after success but perhaps; too much historical attention has gone to geneticists over cytologists, and zoologists over botanists, and with this trenchant for zoological emphasis came a sense that ''plants don't count'' (Antonovics, 1987, p. 326).

Irene Manton Before Manchester

Irene Manton was born in Kensington, SouthWest London, in 1904. She first discovered she had an interest in chromosomes after reading Edmund Beecher Wilson's (1902) *The Cell in Development and Heredity* while still in school (Preston, 1990 p. 250; Leadbeater, 2004, p. 15).[10] By the time she embarked on her training at the University of Cambridge Manton had decided it was botany rather than zoology she preferred; she knew she would not be happy prodding animals under a microscope and so chose the more ''harmless'' pursuit of working with plants.[11] Manton attended Girton College, Cambridge, from 1923, where she studied for the Natural Sciences Tripos (NST). There she received a broad training across the sciences, including in her favoured field botany, as well as in zoology, chemistry, and also physics, which she

[9] See Magner (1994, p. 432).

[10] Manton (1984). The first edition entitled *The Cell in Development and Inheritance* was published in 1896, the second in 1902, by which time the title changed to ''heredity'' rather than ''inheritance.''

[11] Manton (1984).

insisted upon taking. The decision proved a good one for Manton said she was "thrilled with physics as a subject" and "could hardly believe Newton's laws of motion (*Ibid*.)." Although botany remained her first love, she realised that a good understanding of physics would prove essential if she were "to understand the workings of a microscope (*Ibid*.)."

By the 1920's there was increasing focus upon cytology and chromosomes within biology, if not at Cambridge then at the Innes (Richmond, 2001, p. 87). Pioneers in polyploidy research, such as the American Albert Francis Blakeslee working with *Datura*, paved the way toward "counting chromosomes" and looking for evolutionary patterns between species and genera.[12] At the time Blakeslee said, "To us one of the most interesting features of the *Datura* work is the possibility afforded of analysing the influence of individual chromosomes upon both the morphology and physiology of the plant without waiting for gene mutations" (Blakeslee, 1922, p. 31). The term "mutation" was an unstable one; gross chromosomal alterations, including polyploidy, were called "mutations" alongside small genic ones. The great advantage of cytological investigation was that chromosomal "mutations" like polyploidy could be seen directly under the microscope. Researchers uncovered a triploid mutant in the early 1920s, and with this, an explanation for how new species might originate (Blakeslee et al., 1923). Soon Blakeslee began experimentally inducing mutations in *Datura* in the hope of confirming the Mutation Theory or at least a revised version of it,[13] and such efforts were perhaps the first attempts at "evolutionary engineering" in biology (Campos, 2007, p. 22; Campos, 2015). Later in the decade, in a now classical experiment, Russian biologist Georgii Karpechenko (1928) carried out a successful intergeneric cross between two cruciferous representatives (Manton, 1950a, p. 13), resulting in an artificial new "species" *Raphanobrassica*.

The atmosphere for women at Cambridge was improved by the 1920s compared with the years of struggle in the late nineteenth century (Bradbrook, 1969; Richmond, 1997, p. 423). Women enjoyed greater status, at least in terms of their presence there (Richmond, p. 454), but the university still did not confer degrees upon women. The "woman question" (Richmond, 1997, p. 423) had been raised once again in 1921,

[12] Manton discussed early pioneers in polyploidy research in Manton (1950a, pp. 2, 13–14). See also Mayr and Provine (1980, p. 88), Rosenberg (1930, p. 182), Dobzhansky (1951, p. 290), and Brink (1934, p. 102).

[13] The Morgan school introduced a subtler version of the Mutation Theory, as noted in Berg (1922, p. ix).

not long before Manton arrived at Girton, but the campaign was met with defeat. In this matter Cambridge trailed behind the other British universities, including Oxford, only granting degrees to women in 1948 (Schwartz, 2011, p. 672). Even so, the attraction of Cambridge for women remained strong. The university had an established record of training women in the life sciences and was perceived by many as being "the best" institution in which to train (Richmond, 1997, p. 424).

After an outstanding performance as an undergraduate, Manton became the recipient of two postgraduate fellowships – the Ethel Sargent and the Alfred Yarrow.[14] On the Ethel Sargent award, Manton travelled to Stockholm to work in the laboratory of cytologist Otto Rosenberg, a previous student of botanist Eduard Strasburger (Leadbeater, 2004, p. 19), who was famous for his contribution to the Cell Theory. Researchers at Cambridge had been doing cytological work on the wild rose (*Rosa*), following up the pioneering work in polyploidy undertaken by Rosenberg in earlier in the century (Rosenberg, 1909; Leadbeater, 2004, p. 19).[15] The placement with Rosenberg was an excellent opportunity for Manton and came about as a result of solid links forged between botanists in Cambridge and botanists in Sweden (Leadbeater, 2004, p. 19). It was under the supervision of Rosenberg that Manton began work on an investigation of phylogenetic relationships in the Cruciferae (a family that included important crop species and varieties and the subsequent well-known botanical model organism *Arabidopsis thaliana*) for her PhD thesis.

Back in Cambridge after nine months in Sweden, Manton continued her work under the supervision of F. T. Brookes, whose teaching and research involved microbiology and plant pathology (Grubb et al., 2004, p. 15) and who had taught "life-cycles" to Manton in her second and third year as an undergraduate (Leadbeater, 2004, p. 18). Brookes was nominated supervisor to Manton on account of his teaching in

[14] Manton received a "double first" – first class honors in Part 1 of the Natural Science Tripos in 1925 and in Part 2 (Botany) in 1926. On her awards, see letter from M. E. Bawden, and A. Bishop, to Manton, 9 December 1980, in the Irene Manton Papers, Special Collections, Brotherton Library, University of Leeds, (hereafter "MP"), sent from Girton College, Cambridge and giving a report on the careers of recipients of the Yarrow Studentship. Between 1920 and 1940, there were eleven Yarrow Research Fellowships and twenty four Research Studentships awarded. Manton's sister, Sidnie, a zoologist, claimed one of the Studentships and, like Irene, had also completed the Tripos, though a few years earlier in 1923. Both siblings inevitably made significant contributions to their chosen fields and became Fellows of the Royal Society, an unprecedented feat for two siblings (Leadbeater, 2004; Preston, 1990).

[15] See also the work of Rosenberg's student, Täckholm (1920).

cytology rather than his direct research interests (Leadbeater, 2004, p. 23). An unofficial mentor to Manton during her time at Cambridge was Leonard Darwin, fourth son of Charles Darwin, who was linked to the institution through his interests in eugenics. Darwin had fostered a mentorship role with the eminent Ronald Aylmer Fisher, a Cambridge student in the generation before Manton.[16] Now he encouraged the young Manton to investigate the evolutionary problems that had, so far, prevented a Darwinian consensus from prevailing.[17] Botanists were revelling in polyploidy in the 1920s and the sudden and abrupt appearance of new cellular hereditary material did not bode well for a Darwinian gradualist approach. Leonard Darwin asked the trainee botanist and cytologist to consider the question from a Darwin friendly perspective: What is the evolutionary significance of polyploidy?

Manton at Manchester

Manton counted the chromosomes of 250 crucifers, carefully drawing them out on paper using a Camera Lucida (Manton, 1984). The project remained underway after Manton left Cambridge and commenced her first academic position at Manchester in 1930.[18] Here she joined a small but productive team in the cryptogam section of the Department of Botany led by Professor William H. Lang, for whom she became demonstrator (Salisbury, 1961). A romantic encounter between a male member of the Cambridge botany team and a female member of the Manchester team occurred at the 1927 BAAS meeting, held in Leeds ultimately led to marriage and to the resignation of the Manchester woman. Manton was consequently "invited to Manchester without having applied for the job."[19] It was specifically a cytology graduate that Manchester wanted to recruit (Kraft, 2000, p. 269). Manton found

[16] Darwin extended support and guidance to Fisher, now renowned for his work in population genetics who shared his views on eugenics, even after he left Cambridge. Bennett (1983) presents the fascinating and extensive correspondence between the two men.

[17] MP: Letter from Leonard Darwin to Manton, 8 May 1928.

[18] In 1930, Cambridge hosted the 5th International Botanical Congress, which Manton attended along with Lang and her former supervisor in Sweden, Rosenberg. Rosenberg (1930) gave the address for Section G: Genetics and Cytology. In his introductory survey of modern cytology, Rosenberg described how the science of cytology was developing along new and different lines (Brooks and Chipp, 1931, pp. 182–187).

[19] Manton (1984).

she was in a paradoxically "favourable" situation considering she had not yet finished her PhD. Career opportunities for women had diminished in Cambridge owing to the closing down of key facilities just prior to her time there and by the 1920s women researchers were a rare breed at Cambridge, despite the research funds directed at women (Needham, 1982: in Richmond, 1997, p. 445). At Manchester, botany professors William Lang and Frederick Weiss were Quakers, which meant they had a "vested interest in being nice to women."[20] Manton was among the second generation of women to be enrolled in an academic position within the faculty of science at Manchester. The first woman to hold a teaching position there was Marie Stopes, appointed in 1904 (Charlton and Cutter, 1998, p. 52). In spite of the poor timing the move proved a good one for Manton, who later said she was "deeply disappointed" by Cambridge.[21] Manchester immediately won her heart and it was here that she received some personal encouragement for "the first time in her life."[22]

Professor William H. Lang was a paleo-botanist specialising in cryptogams, particularly ferns and well-known for his description of an important primitive vascular plant deposit, the *Rhynia*. His studies were innovative in providing details on both morphological and phylogenetic aspects and became highly influential in botanical evolutionary thought (Andrews, 1961, p. 32).[23] The cryptogamic botany department at Manchester was the only one of its kind in Britain, and Lang was determined to lead the department to success. Lang did not take a hands on approach to leadership; indeed, his attentions were frequently diverted due to administrative duties. This, as well as a lack of professional leadership from higher up, meant junior staff members were left to their own devices (Kraft, 2000, p. 265). Accordingly, Manton was able to cultivate a certain amount of independence and scientific freedom from early on in her career.[24] Indeed her skills were unique to her and in demand. Even so, interests of the department were influential to her work. Manton later credited Lang with having taught her "all the botany I know" in the sixteen years they spent working together (Pre-

[20] Manton (1984).

[21] *Ibid.*

[22] *Ibid.*

[23] Lang and Kidston produced a series of well-known papers on Devonian plants of the Old Red Sandstone between the years 1917 and 1921, which also introduced the now extinct group of early vascular plants the Rhyniaceae (Manton, 1973, p. 287).

[24] This is in contrast to Cambridge women starting out in the generation before Manton, whose research lives were initially dependent upon the research interests and agenda of a (usually) male mentor (Richmond, 2007).

ston, 1990, p. 251). They remained in contact after their respective departures from Manchester in the 1940s. On an occasion when Manton wrote to Lang with news of a recent microscopical success she'd had, she let him know, "I don't know if you have adequately ever taken in how much poor fools like me depend on your reactions. For the whole of my time at Manchester the only opinions that ever mattered about the quality of work were one's own and yours…."[25]

Manton discovered a cytological treasure trove in the Manchester University botanical garden, and as a result, found herself "catapulted into ferns" (Manton, 1973, p. 287). The Pteridophyta (ferns) are a ubiquitous group of plants and betray their primitive ancestry with their reproductive cycle, which involves an alternation of generations between sexual and asexual reproduction. Earlier in the century, Lang had worked on circumventing meiosis and thereby artificially inducing apogamy in the fern *Osmunda regalis,* but his experiments had been neglected as a result of the First World War. Hybridisation had since occurred, supplying the recently trained Manton with her own readily available polyploid series, and owing to the large and elegant chromosomes of this plant, her own "model" organism for her work on chromosome structure. In the mid-1930s, Manton followed the enthusiasms of her department for morphological investigation: within her own area of chromosome cytology there was a growing concern to add details of chromosome shape to the morass of data charting numbers and sizes. The chromosome was thought to be spiral shaped in the late nineteenth century (Manton, 1950b, p. 486). Confirmation of this fact came in the early twentieth century when cytology advanced sufficiently to permit better visualisation. Even then, however, the process was tricky and the spiral form could be seen only at certain stages of the division cycle. The pioneers of this field were Japanese, and most research effort in this nascent arena had, so far, occurred overseas. Closer to home, in Britain Darlington had begun investigating spiral structure. Manton was keen to learn, and pencil notes can be found scribbled in the margins of papers of fellow cytologists Manton kept on the topic.[26] In one of her own early publications, Manton wrote "spiral structure promises to become a cytological phenomenon of very considerable theoretical importance" (Manton, 1936, p. 1058).

Manton's work drew on expertise from the various fields of botany, cytology, evolutionary biology, systematics, taxonomy, genetics, and palaeontology. Further diversity arose as a result of her differing

[25] MP: Letter from Manton to Lang, 10 October 1950.

[26] MP: Box File 302 "Spiral Structure."

institutional experiences. She continued, for example, to pursue the research problems inherited from Cambridge, alongside fresh opportunities that sprang from the work culture at Manchester. The cytological literature was by the 1930s replete with data on chromosome numbers. Advances in cytology, such as improvements in micro-technique, meant researchers could quickly and precisely examine the chromosomes of many species of a given taxonomic group and of many individuals of a given species (Turrill, 1938; Sharp, 1921, pp. 445–447). The abnormalities associated with hybridization, polyploidy and apomixis proved greatly intriguing to botanists. Meanwhile, although genes had been correlated and mapped, they remained undetected and abstract, a situation that for some biologists seemed less than ideal (Manton, 1950a, p. 4; Brink, 1935, p. 97). As a result of her phylogenetic survey, Manton identified two Crucifers of interest (Manton, 1932). One was a case of polyploidy in *Nasturtium* (Manton, 1934a) and the other a Cambridge favourite, *Biscutella laevigata,* known for its capacity to survive in both alpine and non-alpine conditions.[27] Manton carried out a survey of the geographical distribution of this species, known to be possessed of the ''essential cytological qualification'' intraspecific polyploidy, which could now be investigated using the latest cytological techniques (Manton, 1934b, p. 41; Manton, 1937). This work involved the internal investigation of polyploidy together with an external investigation into climatic effects (Turrill, 1938, p. 353). The externalist component to the research sought to address the Darwinian problem of adaptation, which placed her work amidst the rising trend of the Evolutionary or Modern Synthesis[28] – when Darwin's theory of natural selection was reintegrated within the context of an increasingly unified field of biology (Smocovitis, 1996; Mayr and Provine, 1980).

With a new European war looking increasingly likely, Manton's immediate ambitions became urgent. No longer inclined to ''follow-up amusing side-lines'' – she now felt a need to prioritise (Manton, 1974, pp. 1–3). Henceforth, she chose to concentrate most of her efforts on the more cytologically complex and challenging group of plants she had come to know and love – the ferns. These plants house some of the largest numbers of chromosomes known; *Ophioglossum*, for instance,

[27] It was Bateson who originally collected the species *Biscutella laevigata*, which has two distinct forms, while visiting the Italian Alps (Saunders, 1897–1898).

[28] Accordingly, Manton's research is featured in classic works of the Synthetic period; see: Dobzhansky (1937, pp. 198–201) (the chapter featuring Manton's work prominently, however, is not included in later editions of the book); Huxley (1940, pp. 24, 142; 1948[1942], p. 337) and Mayr (1942, p. 122). See especially Stebbins (1950), for extensive references to Manton's work with *Biscutella* and other species.

has a species with races in which 2n = ~1260 (Manton, 1950, pp. 262–280; Stebbins, 1966, pp. 1463–1469). Added to the challenges involved in counting the threads under the microscope were the methods involved; procedures for staining and fixing material prior to observation under the microscope were not in themselves fixed. Reflecting on cytological practice Manton wrote: "In cytology, more perhaps than in any other science, progress depends on manipulative skill and that type of low cunning which is needed to apply old methods to new uses" (Manton, 1950a, p. ix). Fortunately there were some short cuts. By this time cytologists had discovered colchicine could be used to induce chromosome duplication experimentally (Dobzhansky, 1951, p. 290). This proved an exciting prospect for experimental cytologists hoping to "speed up evolution" (Curry, 2014, pp. 551–563). Manton adopted the new technique while working with *Nasturtium* (watercress) in the 1940s, and the research led her to the identification and naming of a new species (Manton and Howard, 1940; 1946). Manton continued to investigate the evolutionary history of the ferns, now alongside some intricate analyses of the morphological structure and behaviour of chromosomes (a research area known as chromosome mechanics).

In the 1930s and 1940s cytologists carried out intensive investigations into the spiral structure of plant chromosomes (Nokkala and Nokkala, 1985, p. 187). In a mid-1930s paper Manton wrote: "Spiral structure promises to become a cytological phenomenon of very considerable theoretical importance, for it may be a clue to a structural explanation of salient features of chromosome behaviour" (Manton, 1936, p. 1058). In the late nineteenth century the chromosome was thought to be a spiral (Manton, 1950b, p. 486). Confirmation came in the early twentieth century when cytology had advanced sufficiently to permit better visualisation. Now cytologists were observing changes in chromosome shape during the spiralisation cycle. The majority of the research effort in this nascent field had so far occurred overseas (Manton, 1945a). In Britain, Darlington had been investigating spiral structure at the JIHI. In the mid-1930s Manton also began to work on chromosome mechanics. She surveyed the works of various investigators, including a mid-1930s paper of Darlington's.[29] Manton quibbled over observations made, for instance when Darlington wrote "We may now therefore at first approximation, define the resting stage as the part of the nuclear

[29] MP: Box File 302 "Spiral Structure." Manton wrote notes in a reprint of Darlington (1935), "The Internal Mechanics of the Chromosomes, II-Prophase Pairing at Meiosis in Fritillaria." She underlined a passage in Darlington's work on minor spirals in meiosis and wrote "minor spirals largest visible in mitosis. This surely a size relation."

cycle in which the spirals of the chromosomes are relaxed'' Manton disagreed with him (Darlington, 1935, p. 47). She underlined the word ''relaxed'' and she wrote ''except that they are not c.f. previous pan.''

The subject was an intricate one (Nokkala and Nokkala, 1985, p. 194). A lack of microscopical clarity for structures below 0.2 μm, about the width of an ''average'' chromosome fibre (and the same order as the wavelength of visible light), meant sub-microscopic appearances close to this limit, and just about discernible, were open to interpretation (Nebel, 1939, pp. 564–565). Consequently there was more than one school of thought as to the details of the structural arrangement of the chromosome (Nebel, 1939, p. 566; Manton 1945a, pp. 471–472. Darlington's school advocated a single strand view of the chromosome, while others thought it comprised more than one strand (Nebel, 1939, p. 566; Darlington, 1937, pp. 31–33; Kaufmann, 1948, p. 91).

Fortunately for Manton, technical innovations were on the way. In the Easter holiday of 1935 Manton went to Egypt to visit the Cotton Research Station at Giza near Cairo, along with Frederick Weiss and Barbara Colson. The visit proved crucial to the discovery of a brand new technique (Manton, 1973, p. 289).[30] James Philp and F. W. Sansome, authors of *Recent Advances in Plant Genetics* (Sansome and Philp, 1932), were in charge at the station.[31] They showed Manton a series of microscope slides in which the cellular contents had been flattened out using a modified version of the ''squash method'' recently devised by Barbara McClintock.[32] The chromosomes were all in one plane, meaning there was no need to re-focus the microscope continually. What is more, the technique was ideal for the purposes of photography.[33]

Manton was among the delegates at the 7th International Congress of Genetics, which took place in Edinburgh in 1939, and was brought to an abrupt end on 29 August after Germany invaded Poland.[34] There Manton presented a paper on her recent work. She had isolated the spiral structure in *Osmunda* (Leedale, 1988, p. 28), adding this organism to a growing list of organisms for which the structure had been observed. Manton hoped to highlight fresh work on chromosome

[30] Manton (1984).

[31] Manton (1984).

[32] Manton and McClintock were acquainted with one another; see Manton (1984) and Leadbeater (2004).

[33] See Manton (1936), for photographs of her ''little alpine chap'' *Biscutella,* in which the spiral structure is readily discernible.

[34] The congress was scheduled to take place from 23 to 30 August. Manton (1974, p. 1).

structure and she was confident there was no comparable study for "any other cytologically worked organism."[35] Cytologists at the time assumed the spiral shape was the result of external stresses and that the thread was, by nature, straight. She wanted to demonstrate that the chromosome was a fundamentally contorted structure and that special circumstances during the division process were required to enable unfolding. The model organism *Osmunda* proved invaluable due to its cytological favourability and propensity toward polyploidy (thus providing further possibilities for analysis of homologous chromosome pairing and the likelihood of aberrations occurring). At the time Manton said, "The realisation that the apparent shape of the chromosome is merely the external form of a spiral coil must obviously entail a very considerable reorientation of old ideas" (Manton, 1939). Unfortunately however, as a result of the mounting hostilities leading to war her paper "Evidence on Spiral Structure and Chromosome Pairing in *Osmunda regalis*" was effectively "stillborn" in 1939.[36]

As the data on chromosome structure mounted, cytologists suspected the spiral to be a universal feature across the plant and animal kingdoms.[37] But the intricacies of chromosome mechanics continued to confound working cytologists. A disputed question was the time of chromosome duplication.[38] The older and simpler view, frequently expressed by British writers, was that of the "prophase split," which retained a strongly theoretical bias. More recent observations, particularly in Japan and America, proposed a "theoretically more complex telophase split" (Manton, 1942, p. 548). Manton had pushed spiral structure to the optical limits with the ordinary light microscope. A shift in resolution was crucial if she were to take this work to the next level. A second technical innovation presented itself to the opportunistic Manton, who made visits once a week during the war to the National Institute of Medical Research (NIMR), in Hamstead, to use the ultraviolet.[39] At two times the resolving power of the ordinary light microscope, this powerful instrument proved a crucial aid to her work on chromosome structure (even if an expensive and complex one; Bradbury, 1967). With it there was no need to "squash" cellular material or

[35] Manton (1984).

[36] Manton (1984).

[37] Spirals have a long history in botany. Long before chromosomes, Johann Wolfgang von Goethe pointed to the "spiral tendency of nature" see, for instance, Sachs (1906, p. 160) and Mainzer (1996, p. 521).

[38] Sax (1936, p. 324) and Manton (1942).

[39] Manton (1984).

for staining (Manton, 1943). While working at the NIMR, Manton asked a colleague about the possibly of using an electron microscope, invented in 1931, to look at biological cells and to her dismay was told it would be "another hundred years" before this would happen.[40] A year later an electron micrograph appeared in *Nature* showing a bacterial cell, which exhibited fine structural details that were "impossible to see in optical pictures."[41]

The internal structure of the chromosome could only be realised if the direction of coiling were known, and for this the UV microscope (or an instrument of higher resolution) was essential (Manton, 1945b, 1950a, b; Nokkala and Nokkala, 1985, p. 187).[42] Manton worked with a colleague, optical physicist John Smiles, and together they broke new ground with the UV microscope (Manton, 1943). They could see precisely the number of chromosome coils and their direction, and they could also determine that there were more coils in mitosis than meiosis. Manton and Smiles were able to estimate the extent of supercontraction, a phenomenon first recognised in 1939, although its nature remained mysterious. The use of photography was essential, notwithstanding the increased microscopical precision, for it was still "more sensitive and more trustworthy than the eye (*Ibid.*)."[43]

In 1942, Manton published a paper in *Nature* in which she stated she had found "incontrovertible evidence" of a "split" in the chromosome of the fern *Todea Barbara* (closely related to *Osmunda*) observed during telophase in a mitotic cycle (Manton, 1942). Despite her best efforts to achieve "objective" photographic evidence and therefore "proof" of her findings, the results were swiftly disputed by a researcher whose perception of the picture indicated an altogether different appearance. Hungarian émigré and mathematical geneticist Pius Charles (Pio) Koller, who was a close friend of Darlington's at the JIHI and well-known for his work on sex chromosomes, suggested an artefact of the preparatory procedure had been responsible for the result, thus supporting Darlington's single stranded view (Koller, 1942, pp. 736–737; Harman, 2004, p. 103).

[40] Manton (1984). Also discussed in Ruiz-Castell (2013, pp. 227, 244).

[41] Manton (1984) and Martin (1938, p. 1063).

[42] Manton (1984).

[43] There were nevertheless problems to contend with that were not encountered with ordinary microscopy. Later Manton would come to use her own custom-made UV microscope at the University of Leeds. Kenneth Oates, her technical assistant from 1953 onwards, recounted the "toxic fumes" the instrument gave off in the laboratory, and with no means of extraction, Manton and Oates had no choice but to rely on a wall fan (Oates, Personal Communication, 2013).

By the 1940s, however, suspicions were growing among cytologists, including Manton, that the chromosome comprised many strands (Nebel, 1939, p. 566; Darlington, 1937, pp. 31–33; Kaufmann, 1948, p. 91). In 1945, the year the war ended, in, Manton revisited ''splitting,'' having become adept with the UV microscope, and she confirmed the existence of a multi-stranded chromosome. Now the question of splitting was no longer relevant, for, as she noted, ''If a chromosome is at all times multiple, 'splitting' in any literal sense may never occur at all'' (Manton, 1945b). Manton thus became known as a proponent of complex chromosome mechanics.

Michael Polanyi as Intellectual and Personal Intermediary

Among Manton's colleagues at Manchester was the Hungarian-born polymath Michael Polanyi. Although he is best remembered today as a philosopher of science whose book *Personal Knowledge* introduced the expression ''tacit knowledge,'' Polanyi in the 1930s and 1940s was best known as a brilliant physical chemist (Nye, 2011). In *What is Life?,* Schrödinger drew attention to the implications of Polanyi's scientific work in terms of enhanced understanding of genic mutations and their rarity in nature. These he likened to the ''quantum jumps'' of the physical world (Schrödinger, 1944, p. 41). This research had evolutionary implications in supporting a Darwinian gradualistic picture, for which small, infrequent mutations are a necessary ingredient. Although these notions fit snugly with Schrödinger's own views (Schrödinger, 1960, p. 174), for Polanyi, ironically, the contribution he made in this context favoured a paradigm he would later reject (Polanyi, 1958, pp. 390–402).

It is not clear when Manton got to know Polanyi, who went to Manchester in 1933, but what is clear is that he suggested she write to Schrödinger, and indeed may well have drawn her attention to *What is Life?*. Several months before the appearance of Manton's *Nature* piece, in December 1944, Polanyi published a review of *What is Life?* in the *Manchester Guardian*. He gave a positive appraisal, although he wrote that cytologists might find the more physical aspects of the book ''difficult.'' That Polanyi chose to use the term ''cytologist'' in his piece rather than use a more generic ''biologist'' reflects his connection with Manton and her fellow cytologists at Manchester. In the late, 1950s, in *Personal Knowledge*, Polanyi referred directly to Manton and her fern studies (Polanyi, 1958, p. 353). As a critique of objectivity in science in which Polanyi reflects historically upon the subject before placing his focus on the twentieth century, the work argued that seeking knowledge that is uncontaminated by (and thus independent of the human observer) in impossible. There is always some degree of intervention, be it an implicit, cognitive bias through the deployment of "tacit knowledge," or the more conscious connoisseurship associated with skilled practice. This, he claimed, is especially prevalent in the descriptive sciences of botany, zoology and medicine, where specialist knowledge is conveyed between master and apprentice (*Ibid*., p. 53), thereby continuously replenishing a reservoir of expertise.[44] Polanyi held that the "personal knowledge" and "passions" of the scientist are to be embraced – for such provide is the driving force of science. Anathema to such creative passion is the doctrine of logical positivism, typically held as a rationale behind the successes of twentieth century physics, a notion Polanyi denied (*Ibid*., p. 6).

The field of biology, in seeking to emulate the success of physics and gain status, became increasingly reductionist and mechanistic in orientation in the 1940s (Brush, 2009 p. 34; Falk, 2000 p. 340; and Smocovitis, 1996). In line with this, the field of cytology became subject to the pursuit of objectivity, but as an unattainable ideal. Cytologists as a result were especially afflicted by "methodological anxiety" due to technical difficulties and the problem of artefacts, resulting from preparatory procedures or microscopical aberrations.[45] These problems, together with limits upon microscopical resolution, warranted the necessity for "a high burden of proof" (Harman, 2004, p. 89). Polanyi acknowledged Manton's extensive cyto-taxonomic work with the ferns (1958, p. 353) in a discussion related to these issues, although more specifically in the context of discussion over tensions between the newer "experimental" and the older "naturalistic" research traditions in biology.[46] The ideal of objectivity and the pressures of simplification and reductionism, coming from the experimental sciences, sat in stark contrast with the reality of the diverse and complex subject matter of biology, commonly respected by the naturalistic tradition. Polanyi noted an irony, in that the traditional data provided by systematists and taxonomists were the foundations for the work of the geneticists (*Ibid*., p. 352). As he wrote, "It all comes down to this. If you want to bring

[44] Daston and Galison (2007) have recently similarly referred to the implicit and personal contribution of the scientist in the quest for scientific knowledge using the notion "trained judgement."

[45] The phrase "methodological anxiety" was used by Súarez-Diaz (2008, p. 463).

[46] Known to historians as the Experimentalist-Naturalist dichotomy or divide, the conflicts arose at the close of the nineteenth century.

order into the multitude of animals and plants on earth, you must first of all look at them" (*Ibid*., p. 353).[47] Hagen has argued that the naturalist–experimentalist dichotomy, as portrayed by historians of science, is an oversimplification tending to obscure the broad research interests of twentieth century botanists (1984, p. 250). Manton's research certainly draws upon a range of expertise. The tensions did, of course, ease toward the mid-century with increasing cohesion between the various sub-disciplines. Even so, the conflicts can be detected in Manton's work during the first half of her career (Manton, 1935, pp. 522–523; Manton, 1950a, p. 2), and the juxtaposition of influence inherited from two different research departments, alongside pressures from the wider currents of her field, make for an interesting situation that left a mark on her career.

Manton's 1945 "Comments on Chromosome Structure"

Manton's *Nature* piece, "Comments on Chromosome Structure," was written in response to *What is Life?* and Schrödinger's topical discussion of the chromosome (Manton, 1945a). Schrödinger had written that the "most essential part" of a living cell – the chromosome fibre – may suitably be called an aperiodic crystal (Schrödinger, 1944, p. 5). A chromosome cytologist could but sit up and take note, as did Manton. Polanyi had written that the book might prove difficult for cytologists, and in "Comments on Chromosome Structure" Manton concurred. In her own critique, she wanted to highlight aspects of the book that might prove confusing to fellow cytologists and, for that matter, biologists.[48] Manton realised that despite the attention given to the chromosome appreciated by cytologists, what Schrödinger really wanted to talk about was molecular structure. In his introduction he wrote of chromosomes, or rather the axial skeleton of the fibre as seen under the microscope, as "containing" the code-script (*Ibid*., p. 21). But on the very same page, he says that it is "chromosome structures" that are instrumental in bringing about the development they foreshadow (*Ibid*., p. 21). To a keen observer whose interests lay in chromosome structure, Schrödinger had apparently committed a crime of conflation. A contemporary reader can infer his meaning in the context of later developments in biology but at the time in

[47] For further discussion on this topic see also Vernon (1993). The issues discussed by Polanyi are useful in understanding how the situation perpetuated approaching the mid-twentieth century.

[48] Manton to Schrödinger, 25 February 1945, Dublin Institute for Advanced Studies, Schrödinger Archive: SCH/C/1 [hereafter SA].

question, precision on levels of biological integration did not exist (Dobzhansky, 1966, p. 546). Only later did this happen with the advancement of molecular biology. But during the 1940s the subject was, at first sight, out of focus even to the trained eye.

When X-ray mutagenesis was used as an experimental tool to explore the physical gene in the late 1920s (Muller, 1927; Stadler, 1928), the notion of a material gene became more widely accepted (Campos, 2009, pp. 14–15; Campos, 2015). In the 1930s, a collaborative effort, atypical for its time, between a physicist, a biophysicist, and a geneticist was undertaken to investigate the physical gene (Sloan and Fogel, 2011). Schrödinger based much of his book upon this "three-man work," also known as the "green paper." Using X-ray mutagenesis to probe the putative gene Nikolai Timoféeff-Ressovsky, Karl Zimmer, and Max Delbrück built a model of the "target area." The team achieved a much greater level of control and sophistication over that of previous experiments because the roentgen as a unit of dose had just recently been standardised (*Ibid.*, p. 54). There was an unsettled matter between artificial and natural mutations (Jepson, 1949, p. 485). The green paper addressed the controversy by confirming that a mutation produced in the lab was exactly the same as that which occurred in nature.

As we saw in the discussion of Polanyi and his work, X-ray induced genetic or "point" mutations were small and known to occur only infrequently in nature. In terms of evolutionary biology, they were therefore amenable to a microevolutionary explanation through the mechanism of natural selection. In the 1940s and 1950s, the importance of cytologically observed chromosomal recombination thus became increasingly "neglected in the genetic literature" in favour of (point) mutations and selection (Mayr, 1982, p. 538). However, as pioneering botanists like A. F. Blakeslee had shown, a mutation did not have to be "*genic* in order to be *genetic*" (Campos, 2008, p. 248).

The green paper was ten years old by the time *What is Life?* was published, but the gene remained a putative entity. Manton spent her time trying to enhance the clarity of photographic evidence to provide demonstrable proof on phenomena that she could actually *see* under the microscope. Therefore she did not wish to get carried away with talk about X-ray mutagenesis experiments, for they really indicated more about the nature of a "mutation" and less about the nature of a "gene" (Manton, 1945a, p. 471). Furthermore, the word *gene*, like that of *mutation*, had been used in a variety of contexts and the meaning of the term was imprecise. In fact, Manton suggested, the word *gene* ought to be removed from the vocabulary altogether (*Ibid.*). Schrödinger was transparent in his book about the putative status of the gene (1944, p. 28), but as a theoretical physicist he was entirely comfortable with speculating about hypothetical entities.

As a result of these considerations, from Manton's perspective *What is Life?* had fallen short of highlighting exciting cytological research on the all-important topic of chromosome structure. She decided she would have to fulfil this task by herself, and so in her *Nature* piece she used Schrödinger's introduction to the topic as a convenient springboard with which to highlight research on spiral structure. According to Manton, this subject was not as widely known as it deserved to be (1945a, p. 472). In light of this, Manton had a further objection to make about the implicit assumption of a single, unitary chromosome fibre that recurred throughout Schrödinger's text. Any reference to "singleness" in chromosomal discussion could potentially be misconstrued on two levels. First, it might refer to the chromosome complement of the organism, or, second, within the context of current research into chromosome structure, it might refer to the chromosome fibre itself. The first point was an established feature of biological analysis that might easily be cleared up. The second was a little more complicated and needed to be reconciled with the cytological facts. As we have seen, while some researchers maintained the chromosome was a unitary structure, others thought it could be a many-stranded structure. In 1945, Manton had indeed followed up on the chromosome split in *Todea*, once again reaffirming the sighting. Now she confirmed that in her view, the chromosome was a many-stranded structure (Manton, 1945b, p. 343).

In spite of reservations, and leaving specific technical and terminological difficulties aside, Manton took an interest in the topic of quantitative measurements of the gene found in *What is Life?* Schrödinger had discussed the total number of genes a chromosome might be expected to house, according to genetical and cytological research, and also the estimated size of a gene (Schrödinger, 1944, pp. 28–30). Later on he drew on the results of X-ray mutagenesis, which revealed a much reduced estimate for the upper size limit of a gene. In suggesting even fewer atoms than could reasonably be expected to exhibit orderly behaviour, these findings contravened the laws of nature yet further (*Ibid.*, pp. 30, 43–44).[49] Manton seized upon the availability of increased

[49] In terms of her own quantitative analyses: as part of her research into the spiralisation cycle Manton had been measuring chromosome lengths to assess the number of gyres – or coils – present during the various stages of meiosis (Manton, 1945c). Also, when Manton first began to focus on the structural details of the cell cycle in the mid-1930s, she had noted differing chromatin amounts between plant species (Manton, 1935).

precision in quantitative measurements for the pertinence it had for her own evolutionary investigations. Through her work with spiral structure Manton came to realise that a helical structure might entail the presence of further fibres, which in turn led her to contemplate the possible presence of *duplicate* copies of the genetical material (Manton, 1945a, p. 473). Paying close attention to Schrödinger's figures in relation to her own knowledge regarding the dimensions of chromosomes, she wrote that there might be room for perhaps 300–1200 duplicate copies of the genetic material, in the genome of *Todea* (*Ibid.*). Manton offered a modification of Schrödinger's structural view, suggesting that chromosome structure should be visualised as an "aperiodic solid in its longitudinal dimension but as periodic in its transverse dimension" a point that was minor in terms of the "philosophic view of a chromosome," but of "immediate importance" for cytologists (*Ibid.*).

The Manton–Schrödinger Correspondence

Prior to the April publication of "Comments on Chromosome Structure," Manton had engaged in correspondence with Schrödinger, then at the Dublin Institute for Advanced Studies. She made contact by introducing herself as a friend of Michael Polanyi's and that he had advised her that Schrödinger would not mind her writing to him. Her letter sparked an exchange of correspondence between the two scientists.[50] Both parties were congenial to the discipline of the other, and the result was an extended conversation and an airing of respective viewpoints.[51] Manton was keen to inform Schrödinger, and thereafter others through her planned publication in *Nature*, about cytological advances in knowledge on chromosome structure:

> If you would have no objection, I would like to send the enclosed for publication in Nature, the object being in no sense to criticise you, but to present a point of view about chromosome structure which is familiar enough outside this country but over here has been curiously slow to penetrate into cytology.[52]

[50] SA: Manton to Schrödinger, 25 February 1945: SCH/C/1.

[51] In Schrödinger's case this is self-evident in his writing of *What is Life?* and he also professed of an admiration for Darwin (Schrödinger, 1960). In addition to this his father had been a botany enthusiast who owned his own microscope; moreover a close university friend of his had been a botanist (*Ibid.*).

[52] SA: Manton to Schrödinger, 25 February 1945: SCH/C/1.

Manton sent Schrödinger a draft typescript, explaining she had necessarily been economical in her writing and that perhaps he might find some elements confusing, as indeed had her friend Polanyi. Schrödinger wrote back, first of all informing Manton that the mathematical, and thereby technically correct term for the spiral pattern, would be a "screw-line."[53] Perhaps Manton's pre-occupation with confusing terminology, as evident in the draft typescript, caused Schrödinger to raise this issue, for he knew it would appeal. Manton took the bait, though she converted "screw-line" to a succinct "helix," first writing it on the back of his letter in pencil before using it in the published version.[54]

Following his correction, and unacquainted as he was with spiral structure, Schrödinger was indeed puzzled by what he read. The confessed lack of clarity is evident in the following sentence in her published piece, in which she objects to the assumed single stranded emphasis of chromosome structure (the guise in which it is commonly visualised under the lens): "It is not always realized that this singleness could be conferred by spiral structure and is not necessarily based on singleness of the genetical material" (Manton, 1945a, p. 471). A perplexed Schrödinger wondered how all of this related to his own discussion, and he wrote her on this point: "But why then should this spiral structure not justify deductions with regard to the genetical material? Do you only mean: you could not deduce the 'size of a gene molecule' from the apparent diameter of the chromosome?" In fact, Manton's intentions were to introduce spiral structure and the possibility of a more intricate internal structure; this, of course, had implications for genomic spatial capacity. Schrödinger considered bundles of fibres in relation to his own hypothesis regarding the structure of the genetic material. He wrote: "The 'pattern' would as might be 'aperiodic' in one dimension, viz. along the chromosome, but periodic across the chromosome," which was paraphrased in Manton's piece. How could these new facts be reconciled with Schrödinger's pre-existing knowledge? How might they

[53] Schrödinger to Manton dated 11 Feb 1945. The date is likely 11 March 1945: it opens with "Thank you very much for your letter of 25.2. with the typescript and your photos, which was all extremely interesting to me." There exists a subsequent letter to Manton with a later date in March.

[54] Manton used the description "helix" again in a comprehensive discussion on spiral structure (Manton, 1950b, p. 489). Manton had emphasised the supposed universality and thus fundamental importance of the spiral or helical chromosome structure in her research papers, although rather than continue to use "helix" thereafter, she reverted back to the use of "spiral," which was firmly established in the cytological literature. Unbeknown to Manton, the term "helix" would be used again by the physicist Linus Pauling, in his 1951 discovery of the alpha helical structure in proteins.

relate to the experimental evidence from genetics and biophysics? Based on this data, Schrödinger had emphasised the notion of one single quantum event altering one single copy of the "code" (Schrödinger, 1944, pp. 43, 77). In response to Manton's more detailed structural proposals, he wrote:

> Now, if that were proved as assumed as very likely by the biologist, I feel the physicist would have to reconsider the whole question of mutations, both natural and X-ray induced ones. A single "quantum event" could only change one of the single copies of the code and if there are several thousands of them, then one could hardly have a genotypic effect, could it? On the other hand, the experiments attended to on page 43 of my booklet plead strongly for regarding mutation as a single event. That means a difficult dilemma. The only–perhaps very silly-solution I can think of at the moment is: the [mutation] caused by ionisation in one of the copies could spread over the whole cross-section, like a "ladder' unfortunately does along a stocking. But against this stands that it could certainly only happen whilst the chromosomes are assembled. Gosh – I really see no way out. – Excuse all these very cursory remarks. But they show you at least what questions would, in this connection, really trouble the physicist – trouble him much more than the questions of "size of a gene" and "range of action of a disturbance," which are based on very uncertain estimates, while the proportionality between mutation rate and irradiation is a well-established fact, which cannot be evaded.[55]

The additional consideration of multiple strands had complicated the picture of the mutation process, leading to the visualisation of a diluted effect. If multiple duplicate copies were now considered, and indeed, if chromosomes disbanded in the nucleus during the resting phase,[56] wouldn't there be a much reduced window of opportunity for a mutation to happen? And if mutations were indeed diluted, might it take a number of successive generations for the phenotypic effect to emerge? Manton had a more fluid approach to the constitution of a "mutation" having regularly encountered gross chromosomal mutations throughout the course of her work. She freely speculated on how many strands might be affected and on how many future generations it might take for

[55] MP: Schrödinger to Manton, 11 March 1945.

[56] The actual mechanism of mutation remained elusive; see Sloan and Fogel (2011, pp. 49–55).

the phenotypic effects to be exhibited.[57] With her evolutionary hat on, the implications of such speculation were exciting indeed. Schrödinger, however, was less than convinced and reminded Manton she would have to assume mutated strands would segregate together in nuclear division in order to reach her estimates, something he deemed, "possible – but a very special assumption."[58] Indeed, the situation was complicated enough when just two strands were considered, but what of more strands? Surely this might give rise to gametes with, "say, 10%, 40%, 70% of mutated strands," a situation which, Schrödinger objected, would produce behaviour far too complicated to be reconciled with simple Mendelian laws. Manton, though, had a broad enough perspective to know there was much more going on, and so for her there was fascinating possibility in the speculation at hand. Schrödinger, perhaps unaware of such wider implications, objected further on the matter, bringing up the example of dominant mutations whose phenotypic effects were not delayed, but instead appeared in the first generation. How could the newly conceived structural arrangement account for this? Their respective views were apparently irreconcilable but even so, the intellectual exchange no doubt was instructive and enjoyable. Schrödinger thus ended his letter by adding, "I hope all that is not produced by some thorough misunderstanding of your meaning!"

The Influence of *What is Life?* on Manton: Planting Doubts about the Evolutionary Synthesis

In order to assess the impact of Manton's encounter with Schrödinger, through his book and their mutual correspondence, it is necessary to fast forward five years to the publication of Manton's own book *Problems of Cytology and Evolution in the Pteridophyta* (1950a). The book was in many ways the pinnacle of her career as a plant cytologist and phylogeny builder. It was commended for its presentation (in particular the clarity of the text) its unusual accuracy, and the beautiful illustrations provided throughout the text (Stebbins, 1957; Babcock, 1951; Swanson, 1951). The work was designed to be easily accessible and Manton presented a clear and thorough description of her methodology so that others might easily follow her techniques in their own work. Leonard Darwin's question on polyploidy was revisited when Manton presented a hypothesis based upon her *Biscutella* results.

57 This is apparent from the responses given by Schrödinger in his subsequent letter.

58 MP: Schrödinger to Manton, 21 March 1945.

The incidence of unpredictable, mass extinction as evidenced by the fossil record, led her to conclude that huge, dramatic, natural events, such as volcanic eruptions, were responsible for changes in evolutionary patterns, but environmental factors, in general, were of little consequence. Where flora remained undisturbed, the incidence of polyploidy remained low; but as a result of upheaval and changing topography, the chances for hybridisation were much increased. Polyploidy, alongside enhancing opportunity to regain nuclear stability through reversion to asexuality, could reasonably account for the success of the Pteridophyta as a primitive group of plants that make up a huge proportion of the Earth's vegetation. In such cases of massive disruption to Earth's flora, the mechanism of natural selection is far too slow a process to be effective. By all accounts, Manton focussed upon internal explanations, suggesting that it was "fruitless" to look toward external mechanisms such as adaptation. This explanation, which became "well-known and widely accepted," was later confirmed by researchers, who verified the genetic feasibility of the species *Biscutella laevigata*. They found that it did display the remarkable plasticity Manton had envisaged (Tremetsberger et al., 2002). There are parallels between these findings and Manton's work on spiral structure. Both instances afford an opportunity for concealed evolutionary potential: the multi-stranded chromosome at the sub-light microscopic level and the occurrence of easily visualised multiple chromosomes at the microscopic.

Problems of Cytology and Evolution in the Pteridophyta stands alone as a comprehensive and authoritative work on fern phylogeny, but those who consult its pages are treated not only to the carefully detailed practicalities involved in this technically challenging pursuit, but also to the extraordinary manifestos Manton set forth before her readers. At the beginning and end of the book appear Manton's thoughts on theoretical matters in evolutionary biology, which did not readily fit with those of her contemporaries. The rising supremacy of natural selection (Gould, 1983, pp. 72–93) came at the expense not only of alternative evolutionary mechanisms, but also of that central theme in biology: variation (Ulett, 2013, p. 131). To begin with, Manton brought the topic of variation to the fore. As she stated, "now we know enough to be certain that variation is not one process but many, different types of variation have widely different causes and consequences, and all follow their own laws of behaviour which must first be elucidated before they can safely be built into any theoretical scheme" (Manton, 1950a, p. 3). She also addressed other evolutionary concerns.

Manton's survey of the Pteridophyta revealed successive cycles of polyploidy, with ever increasing chromosomes numbers and hence cytological complexity, yet these processes were not always correlated with greater evolutionary success or progression as might have been expected. On the contrary, Manton concluded as a result of her findings, that evolution in the Pteridophyta seemed instead to be slowing down (Manton, 1950a, p. 290). For this, in her opinion, "no cause need be assigned other than the fundamental instability of living matter" (1950a, p. 291). As one reviewer noted, this presented, "a forceful philosophical suggestion" (Babcock, 1951, pp. 416–417). Given enough time, Manton continued, this apparent instability will express itself as parallel evolution or an orthogenetic trend. Puzzling evolutionary problems like parallel evolution and the frequent occurrence of homologous characters could be given either a Darwinian or a non-Darwinian explanation according to the perspective of the observer. Manton's view on this matter was clear to the reader once she used the phrase "orthogenetic trend," for this implied an internally directed mechanism with no recourse to external factors. Parallel trends were an established feature of evolution in the Pteridophyta, although botanists had previously remained divided in opinion over whether or not the group was monophyletic or polyphyletic (Wardlaw, 1952, pp. 89–97). W. H. Lang supported the monophyletic view, whereas Arthur George Tansley, along with the Cambridge botanist, Agnes Arber, held polyphyletic views (Wardlaw, 1952, pp. 89, 97). Those biologists holding non-Darwinian views were often purported to be botanists regularly exposed to saltation-like processes that were, by and large, confined to the plant kingdom (Brush, 2009, p. 33). Not all detractors were botanists, however. Manton's sister, the zoologist Sidnie Manton argued that in arthropod evolution the same structure could evolve independently in different lines (Bowler, 1996, pp. 66, 102). Such views were controversial under the weight of the Modern Evolutionary Synthesis. The author of the botanical contribution to the Synthesis, G. L. Stebbins, whose book *Variation and Evolution in Plants* appeared in the same year as Manton's (Stebbins, 1950), gave a largely complementary review of her book, but disagreed with her views on parallel evolution. Stebbins rather put forward his own Darwin-friendly version, by all accounts correcting Manton (Stebbins, 1951). Another reviewer passed comment on Manton's decidedly non-Darwinian perspectives on evolutionary processes, noting: "Miss Manton would appear to disagree with those who consider microevolution to be but a step in the direction of macroevolution" (Swanson, 1951, p. 282). Manton's stance was, in essence, a

Figure 2. Professor Irene Manton, FRS, pictured using the electron microscope. Photograph taken 27th March 1987, reproduced with the permission of Dr. Peter Evennett

counterweight to a strengthening consensus of popular opinion on evolution, at a time when more evidence was yet needed (Manton, 1950a, p. 3). By 1950, there was promise on the horizon for those biologists fascinated by the intracellular universe. The invention of the electron microscope materialised before the Second World War, although dissemination of the new technology was inevitably marred because of it (Pease and Porter, 1981, p. 288). Manton had already witnessed a doubling of resolution with the UV microscope while working at Manchester (Bradbury, 1967, p. 307), and this allowed her to make progress in her work with spiral structure. Now, with another shift in resolution pending, Manton poised herself, ready to meet with the electron microscope (EM) (Figure 2).[59] In the final pages of her book, Manton mentioned her encounters with Schrödinger and *What is Life?*, stating:

[59] See Ruiz-Castell (2013) for a discussion of the introduction of the EM in Britain. Ruiz-Castell provides a quote from Manton, who became a pioneer in the field (p. 227).

> to understand evolution in general terms we need to look not outside but inside the organism and in particular to study, with all the new tools […], not merely the external attributes of chromosomes (their numbers, shapes and homologies) as in this book, but rather their intimate molecular structure. We have here the usual atomic components of the atomic world harnessed together in a manner which strikes the physicist as unfamiliar. This has been eloquently expressed by Schroedinger in a little book called *What is Life?* with which all biologists should be acquainted. (Manton, 1950a, pp. 291–292)

As far as Manton is concerned, it is precisely because we cannot yet observe the intimate molecular details of the cell that we cannot rule out the internally focussed evolutionary mechanism of orthogenesis. Manton remained open to the possibility of a law-like, physically based internal mechanism of evolution, at a time when many of her biological colleagues had left such notions behind.

Conclusions

At the outset I suggested that a closer look at the Manton–Schrödinger case was merited on two grounds: first for what it might teach us about *What is Life?*, and second, for what it might teach us about the rather forgotten Manton and her world. Even though the book was broad in its appeal to both biologists and physicists, a close analysis of the Manton–Schrödinger letters along with Manton's note on *What is Life?,* reveals difficulties in cross-disciplinary conversation. Despite the congeniality of each toward the subject of the other, such difficulties were an unavoidable and telling feature of their differing perspectives. Manton's background and research interests, as both botanist and cytologist, caused her to assume a sceptical position upon first encountering Schrödinger's work and vice versa. The grounded, practically oriented science of cytology was in tension, methodologically, with the disciplines of physics, biophysics, and genetics, in which abstract, unobservable, entities are commonly found (Gayon, 2000, p. 86). The dominant school of thought surrounding the Evolutionary Synthesis was closely associated with developing research in genetics and quantum mutation. Manton's views were broad in comparison, but that breadth led her to insights it would take another twenty or thirty years for the biological world, in general, to comprehend. In light of the 1953 discovery of the physico-chemical nature of the heredity material, much historical attention has been given over to physicists-turned-biologists; biologists, when they do gain a mention, are more often than not, geneticists. In retrospect, the attention given over to genetically oriented problems in biology came at the expense of the wider perspective that might be gleaned from the inclusion of cytologically based (and other) findings. Notwithstanding the interdisciplinary communication difficulties, however, the blending of knowledge and ideas from their respective backgrounds led both Manton and Schrödinger toward enhanced insights on the wider topic of the genome. Manton's ideas on extended spatial capacity and the potential for repetition (pre-emptive of redundancy) in the genome sit complementary to those of Schrödinger's "Morse Code." Taken together they make for a more sophisticated picture of the genome than the one typically associated with mid-twentieth century biology.

Acknowledgments

I would like to thank Professor Greg Radick, University of Leeds, for his advice and support during the compilation of this paper. Thanks also to Dominic Berry, who recently brought Irene Manton to life with his work for the University of Leeds Museum of the History of Science, Technology and Medicine. I am especially grateful to Peter Evennett who (along with others) kindly gave his time to discuss his knowledge of Irene Manton and who gave access to materials, in particular a recorded interview, that have been invaluable to this project. Thank you to two anonymous reviewers whose insights have been assimilated into revisions made to this paper. Thanks are due to Richard Davies at the Brotherton Library, University of Leeds, for his assistance and to Michelle Williams, Librarian at the School of Theoretical Physics, Dublin Institute for Advanced Studies, for her help in obtaining valuable correspondence for this research.

References

Andrews, Henry N. 1961. *Studies in Paleobotany*. New York: Wiley.

Antonovics, J. 1987. "The Evolutionary Dys-Synthesis: Which Bottles for Which Wine?' *The American Naturalist* 129(3): 321–331.

Babcock, Ernest B. 1951. "Cytology and the Evolution of Ferns." *Nature* 167(4246): 416–417.

Bennett, John H. (ed.). 1983. *Natural Selection, Heredity, and Eugenics: Including Selected Correspondence of R. A. Fisher with Leonard Darwin and Others*. Oxford:Clarendon Press.

Berg, Leo S. 1922. *Nomogenesis or Evolution Determined by Law*. Cambridge, MA: MIT Press.

Blakeslee, Albert F. 1922. "Variations in Datura Due to Changes in Chromosome Number." *The American Naturalist* 56(642): 16–31.

Blakeslee, Albert F., Belling, John, and Farnam, M.E. 1923. "Inheritance in Tetraploid Daturas." *The Botanical Gazette* 10(4): 329–372.

Bowler, Peter J. 1996. *Life's Splendid Drama*. Chicago, IL: University of Chicago Press.

Bradbrook, Muriel C. 1969. *"That Infidel Place": A Short History of Girton College 1869–1969*. London: Chatto and Windus.

Bradbury, Savile. 1967. *The Evolution of the Microscope*. Oxford and New York: Pergamon Press.

Brink, Royal A. 1935. "Cytogenetic Evolutionary Processes in Plants." *The American Naturalist* 69(721): 97–124.

Brush, Stephen. 2009. *Choosing Selection: The Revival of Natural Selection in Anglo-American Evolutionary Biology 1930–1970*. Philadelphia: American Philosphical Society.

Campos, Luis. 2007. "The Birth of Living Radium." *Representations* 97(1): 1–27.

—— 2008. "Genetics Without Genes: Blakeslee, Datura, and 'Chromosomal Mutations'." Staffan Müller-Wille, Hans-Jörg Rheinberger (eds.), *A Cultural History of Heredity IV: Heredity in the Century of the Gene, Preprint 343*. Berlin: Max Planck Institute for the History of Science, pp. 243–253.

—— 2009. "That Was the Synthetic Biology that was." Schmidt Marcus (ed.), *Synthetic Biology: The Technoscience and Its Societal Consequences*. Dordrecht and London:Springer.

Ceccarelli, Leah. 2001. *Shaping Science with Rhetoric: The Cases of Dobzhansky, Schrödinger and Wilson*. Chicago, IL:University of Chicago Press.

Campos, Luis. 2015. *Radium and the Secret of Life*. Chicago, IL: University of Chicago Press.

Curry, Helen A. 2014. "From Garden Biotech to Garage Biotech: Amateur Experimental Biology in Historical Perspective." *The British Journal for the History of Science* 47: 539–565.

Darlington, Cyril D. 1935. "The Internal Mechanics of the Chromosomes. II.–Prophase Pairing at Meiosis in Fritillaria." *Proceedings of the Royal Society of London B* 118(807): 33–59.

—— 1937. *Recent Advances in Cytology*, 2nd ed. London: J. & A. Churchill.

Daston, Lorraine and Galison, Peter. 2007. *Objectivity*. New York: Zone Books.

Dobzhansky, Theodosius. 1937. *Genetics and the Origin of Species*. New York: Columbia University Press.

—— 1951. *Genetics and the Origin of Species*, 2nd ed. New York: Columbia University Press.

—— 1966. "Are Naturalists Old Fashioned?' *The American Naturalist* 100(915): 541–550.

Dronamraju, Krishna R. 1999. "Erwin Schrödinger and the Origins of Molecular Biology." *Genetics* 153(3): 1071–1076.

Falk, Raphael. 2000. "The Gene – A Concept in Tension." Peter J. Beurton, Raphael Falk, and Hans-Jörg Rheinberger (eds.), *The Concept of the Gene in Development and Evolution*. Cambridge: Cambridge University Press.

Fleming, Donald. 1969. "Émigré Physicists and the Biological Revolution." Donald Fleming and Bernard Bailyn (eds.), *The Intellectual Migration*. Cambridge, Mass: Belknap Press of Harvard University Press.

Gayon, Jean. 2000. "From Measurement to Organization: A Philosophical Scheme for the History of the Concept of Heredity." Peter J. Beurton, Raphael Falk, and Hans-Jörg Rheinberger (eds.), *The Concept of the Gene in Development and Evolution*. Cambridge: Cambridge University Press.

Grubb, Peter J., Stow, Anne, and Walters, Max S. 2004. *100 Years of Plant Sciences in Cambridge: 1904–2004*. Cambridge: Cambridge University Press.

Gould, Stephen J. 1983. "The Hardening of the Modern Synthesis." Marjorie Grene (ed.), *Dimensions of Darwinism*. Cambridge: Cambridge University Press.

Gribbin, John. 2013. *Erwin Schrödinger and the Quantum Revolution*. London: Black Swan.

Hagen, Joel B. 1984. "Experimentalists and Naturalists in Twentieth-Century Botany: Experimental Taxonomy, 1920–1950." *Journal of the History of Biology* 17(2): 249–270.

Harman, Oren S. 2004. *The Man Who Invented the Chromosome*. Cambridge, MA: Harvard University Press.

Huxley, Julian. 1940. *The New Systematics*. Oxford: Oxford University Press.

—— 1948 [1942]. *Evolution the Modern Synthesis*. London: George Allen and Unwin.

Jepson, Glenn L. 1949. "'Orthogenesis' and the Fossil Record." *Proceedings of the American Philosophical Society* 93(6): 479–500.

Judson, Horace F. 1996. *The Eighth Day of Creation: Makers of the Revolution in Biology*. Cold Spring Harbor, NY: Cold Spring Harbor Laboratory Press.

Karpechenko, Georgii D. 1928. "Polyploid hybrids of *Raphanus sativus* × *Brassica oleracea*." *Zeitschrift für Induktive Abstammungs- und Vererbungslehre* 48: 1–85.

Kaufmann, Berwind P. 1948. "Chromosome Structure in Relation to the Chromosome Cycle. II." *Botanical Review* 14(2): 57–126.

Kay, Lily E. 2000. *Who Wrote the Book of Life?*. Stanford, CA: Stanford University Press.

Keller, Evelyn F. 1983. *A Feeling for the Organism: The Life and Work of Barbara McClintock*. San Francisco: W. H. Freeman.

—— 1990. "Physics and the Emergence of Molecular Biology – A History of Cognitive and Political Synergy." *Journal of the History of Biology* 23(3): 389–409.

Kilmister, Cilve W. 1987. *Schrödinger: Centenary Celebration of a Polymath*. Cambridge: Cambridge University Press.

Koller, Charles Pio. 1942. "The Telophase Split in Todea." *Nature* 150: 736–737.

Kraft, Alison. 2000. Building Manchester Biology 1851–1963: National Agendas, Provincial Strategies. Unpublished Ph.D. Dissertation, University of Manchester.

Leadbeater, Barry. 2004. Irene Manton: A Biography (1904–1988). *The Linneaen*, Special Issue No. 5.

Leedale, Gordon F. 1988. "Obituaries: Professor Irene Manton." *The Independent*, p. 28.

Magner, Lois N. 1994. *A History of the Life Sciences*, 2nd ed. New York: M. Dekker.

Mainzer, Klaus. 1996. *Symmetries of Nature: A Handbook for Philosophy of Nature and Science*. Berlin, New York: Walter de Gruyter.

Manton, Irene. 1932. "Introduction to the General Cytology of the Cruciferae." *Annals of Botany* 46: 509–556.

—— 1934a. "The Cytological History of Watercress (Nasturtium officinale R. Br.)." *Zeitschrift für Induktive Abstammungs- und Vererbungslehre* 69(1): 132–157.

—— 1934b. "The Problem of *Biscutella laevigata* L." *Zeitschrift für Induktive Abstammungs- und Vererbungslehre* 67(1): 41–57.

—— 1935. "Some New Evidence on the Physical Nature of Plant Nuclei from Intra-Specific Polyploids." *Proceedings of the Royal Society of London* 118(811): 522–547.
—— 1936. "Spiral Structure of Chromosomes in *Osmunda*." *Nature* 138(3503): 1058.
—— 1937. "The Problem of *Biscutella laevigata* L. II. The Evidence from Meiosis." *Annals of Botany* 1(3): 439–467.
—— 1939. "Evidence on Spiral Structure and Chromosome Pairing in *Osmunda regalis* L." *Philosophical Transactions of the Royal Society of London, Series B* 230: 179–215.
—— 1942. "Demonstration of the Telophase Split in Todea." *Nature* 150(3810): 547–548.
—— 1943. "Observations on the Spiral Structure of Somatic Chromosomes in *Osmunda* with the Aid of Ultraviolet Light." *Annals of Botany* 7(27): 195–212.
—— 1945a. "Comments on Chromosome Structure." *Nature* 155(3938): 471–473.
—— 1945b. "New Evidence on the Telophase Split in Todea Barbara." *American Journal of Botany* 32(6): 342–348.
—— 1945c. "Chromosome Length at the Early Meiotic Prophases in *Osmunda*." *Annals of Botany* 9(34): 156–178.
—— 1950a. *Problems of Cytology and Evolution in the Pteridophyta*. Cambridge: Cambridge University Press.
—— 1950b. "The Spiral Structure of Chromosomes." *Biological Reviews of the Cambridge Philosophical Society* 25(4): 486–508.
—— 1973. "Explanations: How It All Began." *British Fern Gazette* 10(6): 285–292.
—— 1974. "Explanations II: How the Book Came About." *British Fern Gazette* 11(1): 1–6.
—— 1984. "Irene Manton as Microscopist," Interview by Dr. Peter Evennett (Audio recording), for the Royal Microscopical Society.
Manton, Irene, Howard, HW. 1940. "Allopolyploid Nature of the Wild Watercress." *Nature* 146(3696): 303–304.
—— 1946. "Autopolyploid and Allopolyploid Watercress with the Description of a New Species." *Annals of Botany* 10(37): 1–17.
Martin, L.C. 1938. "The Electron Microscope." *Nature* 142(3607): 1062–1065.
Mayr, Ernst. 1982. *The Growth of Biological Thought: Diversity, Evolution, and Inheritance*. Cambridge, Mass.: Belknap Press of Harvard University Press.
—— 1942. *Systematics and the Origin of Species*. New York: Columbia University Press.
Mayr, Ernst and Provine, William B. 1980. *The Evolutionary Synthesis: Perspectives on the Unification of Biology*. Cambrdige, MA: Harvard University Press.
Moore, Walter. 1989. *Schrödinger: Life and Thought*. Cambridge: Cambridge University Press.
Muller, Hermann J. 1927. "Artificial Transmutation of the Gene." *Science* 46: 84–87.
Nebel, B.R. 1939. "Chromosome Structure." *Botanical Review* 5(11): 563–626.
Needham, Dorthothy. 1982. "Women in Cambridge Biochemistry." Derek Richter (ed.), *Women Scientists the Road to Liberation*. London: Macmillan.
Nokkala, Seppo and Nokkala, Christina. 1985. "Spiral Structures of Meiotic Chromosomes in Plants." *Hereditas* 103: 187–194.
Nye, Mary J. 2011. *Michael Polanyi and His Generation : Origins of the Social Construction of Science*. Chicago, IL: University of Chicago Press.
Packer, Kathryn. 1997. "A Laboratory of One's Own: The Life and Works of Agnes Arber, F.R.S. (1879–1960)." *Notes and Records of the Royal Society in London* 51(1): 87–104.

Pauling, Linus. 1987. "Schrödinger's Contribution to Chemistry and Biology." Clive W. Kilmister (ed.), *Schrödinger: Centenary Celebration of a Polymath*. Cambridge: Cambridge University Press.

Pease, Daniel C. and Porter, Keith R. 1981. "Electron Microscopy and Ultramicrotomy." *The Journal of Cell Biology* 91(3): 287–292.

Penrose, Roger. 1991. "Foreword." Erwin Schrödinger. 1944. *What is Life? The Physical Aspect of the Living Cell*. Cambridge: Cambridge University Press.

Polanyi, Michael. 1958. *Personal Knowledge: Towards a Post-Critical Philosophy*. Chicago, IL: University of Chicago Press.

Preston, R.D. 1990. "Irene Manton 17 April 1904–13 May 1988." *Biographical Memoirs of Fellows of the Royal Society* 35: 248–261.

Richmond, Marsha L. 1997. "'A Lab of One's Own': The Balfour Biological Laboratory for Women at Cambridge University, 1884–1914." *Isis* 88(3): 422–455.

—— 2001. "Women in the Early History of Genetics: William Bateson and the Newnham College Mendelians, 1900–1910." *Isis* 92(1): 55–90.

—— 2007. "Muriel Wheldale Onslow and Early Biochemical Genetics." *Journal of the History of Biology* 40(3): 389–426.

Rosenberg, Otto. 1909. "Über die Chromosomenzahlen bei Taraxacum und Rosa." *Svensk Botanisk Tidskrift* 3: 163–173.

—— 1930. "Section G: Genetics and Cytology." F.T. Brooks and T.F. Chipp (eds.). 1931, *Fifth International Botanical Congress, Cambridge, 16–23 August 1930: Report of Proceedings*. Cambridge: Cambridge University Press.

Ruiz-Castell, Pedro. 2013. "Seeing the Invisible: The Introduction and Development of Electron Microscopy in Britain, 1935–1945." *History of Science* 51(2): 221–249.

Sachs, Julius von. 1906. *History of Botany (1530–1860)*. Oxford: Clarendon Press.

Salisbury, Edward J. 1961. "William Henry Lang. 1874–1960." *Biographical Memoirs of Fellows of the Royal Society* 7: 146–160.

Sansome, Frederick W. and Philp J. 1932. *Recent Advances in Plant Genetics*. London: J. & A. Churchill..

Sarkar, Sahotra. 2013. "Erwin Schrödinger's Excursus on Genetics." Micheal R. Dietrich and Oren Harman (eds.), *Outsider Scientists: Routes to Innovation in Biology*. Chicago, IL:University of Chicago Press.

Saunders, Edith R. 1897–1898. "On a Discontinuous Variation Occurring in *Biscutella laevigata*." *Proceedings of the Royal Society of London* 62: 11–26.

Sax, Karl. 1936. "Chromosome Coiling in Relation to Meiosis and Crossing Over." *Genetics* 21(4): 324–338.

Schrödinger, Erwin. 1944. *What is Life? The Physical Aspects of the Living Cell*. Cambridge: Cambridge University Press.

—— 1960. "Autobiographical Sketches."" Erwin Schrödinger (ed.) 2013 [1992] (14th ed.), *What is Life? The Physcial Aspect of the Living Cell with Mind and Matter and Autobiographical Sketches*. Cambridge: Cambridge University Press.

Schwartz, Laura. 2011. "Feminist Thinking on Education in Victorian England." *Oxford Review of Education* 37(5): 669–682.

Sharp, Lester. 1921. *An Introduction to Cytology*. New York and London: McGraw-Hill.

Sloan, Phillip R. and Fogel, Brandon (eds.). 2011. *Creating a Physical Biology: The Three Man Paper and Early Molecular Biology*. Chicago, IL: University of Chicago Press.

Smocovitis, Vassiliki B. 1996. *Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology*. Princeton, NJ: Princeton University Press.

Stadler, Lewis J. 1928. "Genetic Effects of X-Rays in Maize." *Proceedings of National Academy of Sciences* 14: 69–75.

Stebbins, George L. 1951. "Cytology and Evolution in the Pteridophyta." *Science* 113(2940): 533–535.

—— 1957 [1950]. *Variation and Evolution in Plants*. New York: Columbia University Press.

—— 1966. "Chromosomal Variation and Evolution." *Science* 152(3728): 1463–1469.

Stent, Gunther S. 1966. "Introduction: Waiting for the Paradox." John Cairns, Gunther S. Stent, and James D. Watson (eds.), *Phage and the Origins of Molecular Biology*. Cold Spring Harbor, NY: Cold Spring Harbor Laboratory Press.

Súarez-Diaz, Edna M. 2008. "History, Objectivity and the Construction of Molecular Phylogenies." *Studies in History and Philosophy of Biological and Biomedical Sciences* 39(4): 451–468.

Swanson, Carl P. 1951. "Polyploidy in the Pteridophytes." *Quarterly Review of Biology* 26(3): 281–282.

Symonds, Neville. 1986. "What is Life?: Schrödingers Influence on Biology." *Quarterly Review of Biology* 61(2): 221–226.

Täckholm, Gunnar. 1920. "On the Cytology of the Genus Rosa. A Preliminary Note." *Svensk Botanisk Tidskrift* 14: 300–311.

Tremetsberger, Karin, König, Christiane, Samuel, Rosabelle, Pinsker, Wilhelm, and Stuessy, Tod F. 2002. "Infraspecific Genetic Variation in *Biscutella Laevigata* (Brassicaceae): New Focus on Irene Manton's Hypothesis." *Plant Systematics and Evolution* 233: 163–181.

Turrill, William B. 1938. "The Expansion of Taxonomy with Special Reference to the Spermatophyta." *Biological Reviews* 13: 342–373.

Ulett, Mark A. 2013. "Making the Case for Orthogenesis: The Popularisation of Definitely Directed Evolution (1890–1926)." *Studies in the History and Philosophy of Biological and Biomedical Sciences* 45: 124–132.

Vernon, Keith. 1993. "Desperately Seeking Status: Evolutionary Systematics and the Taxonomists' Search for Respectability 1940–60." *British Journal for the History of Science* 26(2): 207–227.

Wardlaw, Claude W. 1952. *Phylogeny and Morphogenesis: Contemporary Aspects of Botanical Science*. London: Macmillan.

Wilson, Edmund B. 1902. *The Cell in Heredity and Development*. New York: Macmillan.

Yoxen, Edward J. 1979. "Where Does Schroedinger's 'What is Life?" Belong in the History of Molecular Biology?' *History of Science* 17: 17–52.