Agricultural Science, Plant Breeding and the Emergence of a Mendelian System in Britain, 1880-1930

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Submitted in accordance with the requirements of the degree of PhD

The University of Leeds

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September 2011

The candidate confirms that the work submitted is his own, except where work which has formed part of jointly-authored publications has been included. The contribution of the candidate and the other authors to this work has been explicitly indicated below. The candidate confirms that appropriate credit has been given within the thesis where reference has been made to the work of others.

The collaborative paper, Berris Charnley and Gregory Radick (2010). "Plant Breeding and Intellectual Property Before and After the Rise of Mendelism: The Case of Britain", in D. J. Kevles et al. (eds.), *Living Properties: Making Knowledge and Controlling Ownership in Modern Biology*, pp. 51-55. Berlin: Max Planck Institute for the History of Science, submitted alongside this thesis as loose sheets, was produced in collaboration with Prof. Gregory Radick. Material dealing with W. F. R. Weldon and the ancestral law of heredity was produced by Prof. Radick. The introduction was planned collaboratively and written by Prof. Radick. All other material was produced by Berris Charnley. Material from this paper appears in Chapter 3 of this thesis.

Acknowledgements

First and foremost I would like to thank Gregory Radick, my lead supervisor. Greg has been tireless in keeping my often wayward prose on the straight and narrow, and in leading by example, his intellectual curiosity and enthusiasm have inspired me on too many occasions to count. Secondly, I would like to thank Graeme Gooday, my co-supervisor, and the other members of the Owning and Disowning Invention project; Stathis Arapostathis, Graham Dutfield, Jonathan Hopwood-Lewis and Christine Macleod. On the many happy occasions when I shared my work with this group their feedback was insightful and always helpful. The Centre for History and Philosophy of Science, my institutional home over the last four years, has always been a hospitable place to work. I hope the centre's staff and students see their ideas flatteringly reflected in my thinking in this thesis. These people have also been my friends, so I should thank them twice, along with other friends, and my family, for their caring support along the way. One other group of friends deserve a special mention; Sam Bacha, Dom Berry, Chris Renwick and Paulo Wylis formed a team who proof read this thesis. Their patience and perseverance with this un-enjoyable task is only matched by my luck in having such good friends.

Many of the ideas presented here were born and shaped at conferences and workshops. I would like to take this opportunity to thank several groups which have been unstintingly generous in creating opportunities for me to present my work: Daniel J. Kevles, Hans-Jörg Rheinberger and Jean-Paul Gaudillière who organised the *Living Properties* and *Plants, Animals and Ownership* workshops held at the Max Planck Institute for the History of Science and Yale; Alessandro Nuvolari, Kristine Bruland and Liliane Pérez who organised the *Innovation without Patents* workshops in Geneva, Eindhoven and Utrecht; Staffan Müller-Wille and the other organisers of the Egenis Seminar series; and the organisers of ISHPSSB 2009 in Brisbane; the British Council's Darwin Now conference; WEHC 2009 in Utrecht and SHOT 2008 in Lisbon. The participants of these events have been equally generous with their ideas, feedback and critical engagement with my work.

Collecting the evidence for this thesis involved a great deal of time spent in archives, in some cases, literally on the other side of the world. The members of staff at the archives I visited were unanimously patient, professional and friendly. Kate West at the John Innes and Tricia Cullimore at NIAB were both particularly helpful in bringing new material to my attention. My archive research was funded by the Department of Philosophy at the University of Leeds, the Royal Historical Society and the AHRC. The AHRC also provided financial assistance with maintenance and fees, without which writing this thesis would not have been possible.

Finally, I would like to dedicate the thesis to Marija and Julian. Marija, my wife, has never given up on me, no matter how high the drift of papers has piled up in our front room. I will always be thankful for her loving support over the past few years. Julian's help runs throughout these pages. I often stayed with him when I was working in London or traveling back and forth to conferences and workshops. Julian's curiosity about my work and willingness to engage with, and challenge, what I was telling him always inspired me. Julian's life held the world around him to a higher standard, he will be sorely missed.

Julian Brown (1977-2011) *il miglior fabbro.*

Abstract

Following Thomas P. Hughes's systems approach in the history of technology, and making use of previously unexamined sources, this dissertation seeks to show that the development of British Mendelism may be explained, and the success it enjoyed more accurately gauged, by analysing the emergence of a system whose elements justified the theory, protected it, made it useful, and slowly territorialized the world. Accordingly, the analysis will cover the principle elements of this system: the system builders, institutes, ideas and varieties that were, in one way or another, Mendelian. The first of the Mendelian system builders, William Bateson, is already well known for his introduction of Mendelism to Britain in the years after 1901 and his coinage of a new name for the discipline; Genetics. He was joined by two colleagues, Rowland Biffen and Thomas Wood, both of whom collaborated with Bateson in creating a string of institutes concerned with changing agriculture by using the new Mendelian theory. The proponents of the new theory often talked of their new found ability to transfer characters and build up new varieties of agricultural value. These claims were welcomed by politicians and the popular press and the idea that the new genetics would lead to a beneficial revolution in agriculture became a popular cause of the day. However, the release of the first of these new Mendelian varieties in 1910 in Britain is far less well known than the almost simultaneous development of the chromosome theory at Columbia University by Thomas Hunt Morgan. On one view of the history of genetics, the discipline, which had been born in Moravia, and popularised in Britain, was from 1910 most fruitfully developed in Morgan's fly room. From this perspective it might be thought that the British School, under Bateson, became a disciplinary backwater, at least in part because Bateson refused to accept chromosome theory. This thesis argues that far from being in a genetic backwater, Bateson along with Mendelian allies Biffen and Wood were at the cutting edge of a wide ranging movement to improve agriculture through the introduction of new Mendelian varieties.

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Introduction

> Mendelism stands or falls by the purity of its cultures. No other method of plant breeding is known which can guarantee 100 per cent of purity in its cultures. When buyers once realise that such a cherished dream is practicable there will be no more "rogues," and Mendelian seeds will be the only profitable seeds to grow.[1]

THIS IS HOW ONE of the most fervent early Mendelians, the Leicestershire-based horticulturalist Charles Hurst, ended the night's speeches to the Royal Horticultural Society in London, one evening in 1910. By this time the idea that Mendelism would revolutionise breeding was a trope with which the RHS's members were familiar. Hurst's great friend and fellow Mendelian, William Bateson, formerly a Cambridge University man but now director of the new John Innes Horticultural Institution in Merton, Surrey, is famous for having made exactly this sort of claim on Mendelism's behalf.[2] A generation later, in 1926, Bateson's former student, Professor Sir Rowland Biffen – himself now a Cambridge don, formidable breeder of wheat varieties, and director of his own plant breeding institute – would talk of a pre-1900 breeding regime ruled by "precepts [of heredity] as fantastic in their way

[1] Hurst 1910: 52. Hurst delivered this speech to the Society following in a long line of distinguished botanical speakers interested in horticultural production, including Hugo de Vries 1910: 321-326 and Bateson 1907b: 90-97. Hurst's speech to the RHS was followed three years later by one from Rowland Biffen 1913: 313-23.

[2] For example, in 1902 Bateson claimed that thanks to Mendelism the breeder would no longer be "trudging in the old paths of tradition", Bateson 1902a: 208 and that breeders could now, thanks to Mendelism, "take out greenness and put in yellowness […] take out tallness and put in dwarfness, etc…". Bateson 1902b: 2. For more on Bateson's promises to the breeders see Radick forthcoming. Merton, at the time, was a village on the outskirts of London in the Surrey countryside. It is now part of South London, having been engulfed by the city as it sprawled southwards.

as mediaeval medical prescriptions. Now, thanks to Mendel's work, the main principles of the subject are fairly clear".[3]

Are Hurst, Bateson, Biffen and the many other like-minded breeders and botanists of the period to be believed about Mendelism's impact on breeding? On the one hand it seems as though Mendelism's application to plant breeding was incredibly successful. The agricultural success of Biffen's Mendelian wheat varieties, though rather forgotten now, was an article of faith for a whole generation of geneticists keen to establish the validity and utility of their discipline.[4] Plant breeders of a particular age claim that Mendelian theory allowed Biffen to explain and predict his crosses, in a way unavailable to previous breeders, with seemingly spectacular results. At one point, it was reported by *Nature*, Biffen-wheat occupied "practically one-half of the country's wheat crop".[5] These results in turn shored up the truth of the Mendelian theory.[6] The belief that scientific plant breeding only really began with Mendel lingers on to this day.[7] On the other hand, Bateson radically tempered many of his earlier and most strident claims, as did Biffen.[8] Several of Biffen's breeder contemporaries were equally successful while remaining dubious about the importance of Mendel.[9] Some geneticists, such as Biffen's star

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[3] Biffen and Engledow 1926: 13.
[4] See one of Biffen's successors at the Plant Breeding Institute, Bell 1976 or the accounts of noted academic plant breeders turned historians, Lupton 1987; Murphy 2007: 268.
[5] "Notes" 1921: 543.
[6] On the virtuous circle of truth and utility associated with some theories see Dear 2005.
[7] See, Kingsbury 2009: 186. In a recent radio interview about his book the interviewer could not help posing the question, "let's go back to the development of the science of plant breeding, I mean that didn't really become a major factor until Mendel and the acceptance of his laws?", to which Kingsbury replied, "Mendelian genetics allows you to make predictions so it takes a lot of the effort out of plant breeding". BBC Radio 4, The Food Programme: Seeds, Broadcast Sun 7 Feb 2010 12:32: 14m53s available at <http://www.bbc.co.uk/iplayer/console/b00qg0r4> [accessed 17 Feb. 2010]. See also the historical material of the British Society of Plant Breeders, *Plant Breeding: the Business and Science of Crop Improvement*, <http://www.bspb.co.uk/BSPB%20Handbook.pdf> [accessed 10 Oct. 2010]: p. 2.
[8] See Bateson 1912 and Biffen and Engledow 1926: 7.
[9] John Percival and Edwin Sloper Beaven are two examples; there are several others. See Percival 1921 and Beaven 1947 and for detailed analyses of the careers of these Mendelian

pupil, successor to his chair at Cambridge and obiturist, Frank Engledow, also expressed their doubts. In his obituary of Biffen, Engledow, albeit obliquely, suggested that Biffen was more of an old school worker than he let on. Mendelism might have less to account for in Biffen's success than his art, especially his breeders' eye.[10] Moreover, after Biffen's first successes other Mendelian varieties were slow coming; when they did appear it was only after Biffen and his colleagues (including Engledow) had released a string of failures. Beyond the claims made by Hurst, Bateson and Biffen, opinion as to what results might arise from Mendelism was, at the time – and still is – rather divided.[11]

More generally, there are several wider considerations against Mendelian triumphalism. In France, Germany and the Netherlands the reception of Mendelism seems to have been complicated at best, and negative at worst. Worldwide, agricultural yields jumped spectacularly in the 1950s – a period associated with the coming of age of hybrid corn technology, dwarf rice and wheat varieties and the Green Revolution. This has led some historians of economics to suggest that before this period little biological innovation occurred, especially in land rich/labour poor America.[12] The American case is further complicated by the hybrid corn story. Although this is claimed as a great Mendelian success, Marxist scholars have questioned the part Mendel played, if any at all.[13] Finally, historians of science have also given a mixed interpretation of Mendelian success.

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sceptics see Palladino 1993 and 1994. I would like to extend many thanks to Paolo Palladino who was very generous in making time to meet me and discuss ideas as I was planning this thesis.

[10] Engledow 1950: 20. See also Engledow quoted in Roll-Hansen 2000: 1112.

[11] For a brief but clear overview of this dilemma see Roll-Hansen 2000: 1108-1109, see also Allen 2003 on the claims of Mendelism for application.

[12] See Olmstead and Rhode 2008 for a review of the historiography and their thoughts on how it should be revised.

[13] On the success of hybrid corn see "Science: Santa Claus's Corn", *Time Magazine*, Monday, 18th Dec. 1939, <http://www.time.com/time/magazine/article/0,9171,763106,00.html> [accessed 17 Feb.

The analysis employed in this thesis offers a novel approach to an area where theoretical consideration of Mendelism's social context has oftentimes overlooked the day to day activities of many Mendelians. To gain new purchase on the questions of whether Mendelism was successful, and if it was, what that success meant, this thesis treats the history of British genetics as the development of a simultaneously investigative and productive enterprise.[14] British Mendelism cut across the traditionally assumed domains of science and technology and British Mendelians were deeply involved with the production of new organisms. These activities were interrelated with their theoretical and pedagogical work. In order to analyse the circulation and interaction of students, ideas and organisms in the Mendelian school, this thesis borrows an analysis from history of technology: Thomas Hughes's systems analysis of technological change. In the final two chapters the systems analysis is taken beyond Hughes's interpretation to include consumers and the situation of that system in an international context.

The rest of this introduction gives an overview of the key themes extant in historical writing on genetics and its relationship to agriculture. The focus here is largely (though not exclusively) on the case of Britain. As we will see, despite some shared views the authors who have broached these subjects offer different interpretations of how and why Mendelism was or was not successful and the impact it did or did not have on agriculture. The British historiography will then be placed in the wider context of the reception of Mendelism in Europe, America and Mexico. Within this literature there is also little to no consensus and much to be learnt at both

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2010]. For Marxist critique see Kloppenburg 1988 and the wealth of material from Lewontin, Levins and Burlan especially Lewontin 2000b; Lewontin and Burlan 1986c; Levins 1986. Hybrid corn has also caused much agitation amongst sociologists and economic historians, becoming the focus for development of diffusion and uptake models from the 1950s onwards. On this rich vein of literature see Griliches 1957, 1960 and Lissoni and Metcalfe 1994.

[14] For more on the productive agricultural context of Mendel's work see Orel 1973 and Orel and Wood 1983.

national and international levels. Some of these gaps in our knowledge might be filled by employing a systems analysis. In particular this thesis will show the benefits of moving beyond a fragmentary analysis of institutions and individuals to a more coherent and overarching analysis of the Mendelian school as a whole. Having introduced the subject, current knowledge and my methodology the introduction ends with an overview of the thesis's structure detailing what can be expected in each chapter as regards prior knowledge, new evidence and conclusions.

British Genetics and Agriculture in the Twentieth Century

When Cyril Darlington, Bateson's successor as director of the John Innes Institution (after A. D. Hall) and president of the Genetical Society, introduced a special symposium in 1944, the question of how much impact genetics had on agriculture was still, to his mind, open:

> Genetics owes a debt to plant and animal breeding both for its foundation and its development. If the purpose of agriculture in the future is to be the highest production, genetics will have the opportunity of repaying that debt. The object of the symposium [is] to discuss whether genetics has the capacity to do so.[15]

However, some listeners might have been forgiven for thinking this question had long been answered. Surely the spectacularly successful varieties of wheat produced by Rowland Biffen's new Mendelian breeding had long ago repaid the debt that genetics owed to plant breeding? Yet, Darlington's pessimism ran deep. Along with

[15] "Application of Genetics to Plant and Animal Breeding" 1944: 780.

Engledow, he believed Mendelism had enjoyed only a limited success in impacting on plant breeding.[16] This tension has been a staple feature of the historiography of the emergence of genetics.[17] It can be readily discerned in work on the British context from Robert Olby and Paolo Palladino. These authors have done most of the ground work in analysing agricultural themes in the emergence of genetics; outlining along the way the key individuals and organisations which connected Mendelism to agriculture. However, they have formed strongly differing views on the impact of genetics on plant breeding.

Olby's initial work on Mendel focused on the intellectual development of Mendelism. These early works also had a strong revisionist flavour, combating what Olby calls the mythology of the emergence of the discipline. The mythology of Mendelism, for Olby, was manifest in the romantic beliefs, in common currency at the turn of the century and ever since, that Mendel had been forgotten for forty years and rediscovered simultaneously by three European botanists.[18] It turns out that

[16] Darlington attributed this failure to commercial plant breeders. As he put it in a remarkably blunt swan-song article, published in the year of his death, "On one side were a few hundred commercial growers with a united economic interest in their capital investment. They sell fruit; they resist the introduction of better new varieties. On the other side were a thousand times as many private gardeners who grow fruit. They eat it themselves and they want the best. These latter ... collaborated with Bateson... Since the death of Bateson ... the new John Innes fruit varieties have accordingly been suppressed, with economic consequences that are now painfully recognized". Darlington 1981: 403. These thoughts are painted in the broadest strokes, however, the economic and even political dimensions of the heterogeneous plant breeding community that they reveal will be encountered repeatedly over the course of this thesis. For more on Darlington see Harman 2004 and 2006.
[17] The historiography of the emergence of genetics is a rich and complicated one, however, broad trends can still be discerned. Initially, many of the first generation of Mendelians wrote histories of Mendel and their own and each other's work. A second generation of biologists and geneticists also wrote histories of the early period dealt with in this dissertation, see Sturtevant 1965a; 1965b, Dunn 1965, Carlson 1966, Stubbe 1972 and Mayr 1982. These historically internalist accounts were supplanted in the 1980s and 1990s with more broadly critical and contextualising accounts; the recent historiography in focus in this section. See Garland Allen's historiographical essay for the History of Science Society, for an instructive overview of the historiography of genetics in the 1960s-90s available here: <http://www.hssonline.org/teaching/teaching_allen3.html> [accessed 1 Oct. 2010].
[18] For Olby's classic internalist account of Mendel's work see Olby 1966 [2nd ed. 1985]. For Olby's revisions to the mythology see Olby 1968, 1979, 1987 and a special overview of this

Mendel was not deeply forgotten, the rediscovery was not simultaneous and the definition of rediscovery is somewhat stretched to fit what might more accurately be called a re-working.[19] Olby's focus changed somewhat towards the end of the 1980s to alight on the institutional support of Mendelism in Britain.[20] In the first of three papers, published in quick succession, Olby described how Bateson secured a place for Mendelian research at the John Innes Horticultural Research Institution with the aid of a civil service and a government predisposed to state support of science. Indeed, by Olby's reckoning, there was "a close network of influence" between academics and civil servants charged with distributing the funds of the Development Commission.[21] This was a body established under David Lloyd George's chancellorship in 1910 as part of his and Winston Churchill's ambitions for greater state involvement in rural reconstruction. Part of this reconstruction effort was the provision of funds for scientific research to aid agriculture. Biffen's early Mendelian work at Cambridge underwrote, for these politicians, the need for further funding. According to Olby a small elite of civil servants and academics gave form to Lloyd George's unstructured plans. This elite group ensured that research funding was largely spent on Cambridge based projects. As Olby put it in 1990:

> The most noteworthy features of the Commission's work were the importance attributed to longterm research on a coherent plan, the liberal interpretation of research 'of economic value', and the operation of the plan

work, *Mendel, Mendelism and Genetics* available at the online resource MendelWeb: <http://www.mendelweb.org/MWolby.intro.html> [accessed 10 Jan. 2010].

[19] See the now classic paper, "Mendel no Mendelian?" Olby 1979.

[20] Olby 1989a. The bulk of this paper deals with negotiations between Bateson, the executers of the John Innes' behest, the Board of Agriculture. The paper ends with several fascinating insights into Bateson's research programme and his notorious refusal to accept chromosome theory.

[21] Olby1991. Olby's analysis here extends the work of Roy Macleod on public funding of science, see Macleod 1971, Turner 1980 and social historian Samuel Hynes 1968.

> through a professional elite constituted of University professors, directors of institutes and the civil servants in the Board of Agriculture, whose secretaries were former Cambridge professors. Thus there was created a network embracing the Commission, the Board and the Cambridge School of Agriculture.[22]

The operation of such a group, although regrettably nepotistic, was by Olby's lights, a good thing. The network "proved [to be] an effective instrument for the support of scientific research relevant to agriculture and related disciplines".[23] The failure of this plan is attributed by Olby largely to the interruptions caused by the First World War.[24]

Arguing against Olby, historian Paolo Palladino has suggested that research funding was a substitute for what farmers really wanted; trade tariffs and price structuring. Furthermore, the institutes established by the Development Commission were co-opted by local interest groups who were allowed space to manoeuvre by relative government indifference. In Palladino's view, the research conducted using this funding did little to address the concerns and worries of farmers. Accordingly, the research program at Cambridge was orientated towards Mendelian genetics by a negotiation of interests between the University, academics and the seed and milling trades. Farmers had little to no say in these debates. Apparently, academics (Biffen amongst them) stressed the importance of Mendelian theory as a means of re-

[22] Olby 1990: 70. For this translation I am very grateful to Robert Olby for providing me with his un-published manuscript in English.
[23] Olby 1991: 525. On the cosy nature of the funding arrangements made by the Development Commission see also Vernon 1997 and an excellent article from Kraft 2004.
[24] Olby 1991: 524. This is an interesting and difficult claim, on the one hand the War did undoubtedly cause "restriction, if not suspension" of much work, Biffen, for example, was seconded to the war time Food Production Department in 1916 and as Olby notes several Mendelians died young, in this period. But on the other hand, none of them died in service and there was plenty of post war funding motivated by the frightening possibility – glimpsed during the First World War – of mass starvation.

conceptualising plant breeding as an academic activity suitable for the university context. Cambridge University, although initially resistant to what were called “bread studies”, was converted to Biffen’s cause by the early success of his varieties and indeed, came to lay a stronger emphasis on practical research than the State.[25] For their part, Palladino argues, the seed trade happily suffered misrepresentation of their methods as backwards in comparison to the new Mendelism, if it meant that the costs of plant breeding could be transferred nationalised. The lack of patents in this area meant that commercial firms could sell new Mendelian varieties without needing to pay royalties. Finally the millers, whose interest was in the production of new varieties better suited to the mill, maintained strong links to Biffen through one of his former pupils, A. E. Humphries, who went on to become secretary to the National Association of British and Irish Millers. All of this meant that “it was to be expected that governmental support for plant breeding research would have little impact upon agriculture”.[26]

Palladino returned to this subject to focus in on one particular interest group; academic plant breeders, including Biffen. In a paper published in *Technology and Culture,* Palladino described the particular instantiation of the relationship between Mendelian science and plant breeding technology promoted by academics like Biffen as one which “may have been an artefact devised by … the budding community of geneticists to advance the institutionalisation and professionalization of their particular branch of scientific inquiry”.[27] The roots of this artefact were to be found, for Palladino, in institutional location and personal history. The notion that

[25] See Palladino 2002: 56, 95.
[26] Palladino 1990: 468. Palladino’s early work on this subject continued the analyses of professionalization, institutional development and the social construction of science by interest groups started in his prior explorations of the development of US and Canadian entomology and ecology. This work was published after he had switched his attention to British genetics and agriculture. See Palladino 1996d.
[27] Palladino 1993: 322.

science should, and did, function as a stimulus to technological advance was just the sort one should expect from an individual such as Biffen, son of a Cheltenham school master, based at Cambridge University. The contrast Palladino draws to Reading based agricultural botanist, John Percival, is intended to show quite clearly that Mendelism was optional to a successful breeding career; Percival was never entirely convinced of the importance of Mendelism. Instead Percival believed that systematics, understanding and taxonomising the history and relations of wheat varieties, was the way to better wheat varieties. Palladino situates these beliefs in Percival's biographical background as the son of a farmer, who shared some training at Cambridge with Biffen but became more concerned with farmers within the institutional arrangements at the University of Reading. In focusing on Percival's doubts about Mendelism, Palladino is closing the social constructivists' circle and creating symmetrical social explanations for what he takes to be two sides of a bitter dispute between Mendelian and non-Mendelian plant breeders.

The idea that Mendelian theory was related to plant breeding by social interests is one that Palladino developed in a cross national study of the "professionalization of agricultural science".[28] Here Palladino explored the careers of two non-Mendelian plant breeders, Edwin Sloper Beaven in Britain and Luther Burbank in America, who were nonetheless celebrated and successful. The comparison explicitly furthers Palladino's claim that, "Mendelism was … a vehicle designed and developed quite expressly to advance the claims of academic plant breeders … as they vied … for greater social prominence and power".[29] In other words Mendelism had very little to do with plant breeding and far more to do with creating academic and social status.

[28] Palladino 1994.
[29] Palladino 1994: 411.

A radically different view of Mendelism's success has been presented by sociologist Kyung-Man Kim.[30] Responding to the established historiography on the Biometrician-Mendelian debate, Kim has suggested that at least part of the reason for Mendelism's success was due to the superior way in which it explained the results breeders and medics obtained for themselves.[31] These researchers, in Kim's words, "the rank and file", were won over by the efficacy of Mendelism in explaining their own empirical data.[32] As this critical mass was won over Mendelism's authority was cemented. Kim's purely cognitive explanation is useful here in defining the difference between Palladino and Olby's positions. While we might expect Palladino to hold little truck with such claims, for Olby the successful application of Mendelism to explain breeding phenomena was at least part of the picture. However, Olby eventually seems to have incorporated some part of Palladino's thinking into his own analysis. In an overview piece for the *Routledge Companion to the History of Modern Science*, after discussing the intellectual development of Mendelism, Olby cites three factors in the growth of genetics as a discipline:

> Firstly, there was the undoubted success of the Mendelian theory and method as the basis for a research program. Then there was the interest of academic agriculturalists who were striving to make their subject genuinely scientific and who found in Mendelian experimentation a fruitful avenue to pursue.

[30] Kim 1994.
[31] For the established Biometrician-Mendelian debate literature see Olby 1989b; Kevles 1980; Kim 1994 and more recently Buttolph 2008. From these one can work back to prior publications from Provine 1971, Cock 1973 and Barnes and Mackenzie 1974, 1979, see also Olby 1989b: fn.3.
[32] Kim 1994: introduction.

> Last, but not least, there was a strong interest in the possible social applications of genetics.[33]

Palladino's influence is obvious in the second of Olby's factors, the influence of academic agriculturalists.

The success of Mendelism is a clear trope in the work of these authors. However, there is no clear consensus as to what constituted success and how this was or was not achieved. Olby clearly conceives two types of Mendelian success; as a research program and as a method of plant breeding. The success of Mendelism as the basis for a research program was due, on Olby's account, primarily to its experimental nature. There was a certain amount of congruence, he suggests, between Mendelian experiments and a more general move in this direction in the biological community in both Britain and America.[34] Furthermore, as regards the second type of success, as a method of plant breeding, Olby reads Biffen's early work in producing varieties as having been un-problematically successful and Mendelian. Later events, beyond Biffen's control, undermined these successes. In a sense Olby's interest in the success of the new Mendelian varieties ends at the test plot gate, once out in the world they were subject to the usual forces of politics and economics. One could hardly blame Biffen if the world failed to make good use of his varieties. Palladino, in contrast, views success as a far more problematic question. On the one hand he argues that "the agricultural context [was] far less important for the growth of genetics [in Britain than in America]", suggesting that the Mendelian research program owed whatever success it did enjoy in Britain to factors other than

[33] Olby 1996: 533.
[34] On the shift to experimentalism see Olby 1996 and 1989b. For similarities and differences to trends in the US see Kevles 1980 and more recently Kevles and Geison 1995. For critique and defence of the existence of this trend in the US see Allen 1981.

the agricultural context.[35] On the other hand, on the success of Mendelism as applied to plant breeding, Palladino is much more sceptical than Olby. Pointing to the lack of new varieties from Cambridge Mendelians after the First World War, Palladino suggests that Biffen's early successes might have owed more to his skill in negotiating with local interest groups than his deployment of Mendelian theory.[36] This position is underwritten by the three biographical accounts of successful non-Mendelian plant breeders, Edwin Sloper Beaven, John Percival and Luther Burbank, provided by Palladino.[37]

Mendelian Success in Britain and Further Afield

At roughly the same time that Olby and Palladino were writing on these subjects, a growing appreciation of the links between early genetics and agriculture was also developing in America. Historians of science Barbara Kimmelman and Diane Paul have pointed to the support provided for Mendelism in America by land-grant universities and those interested in breeding and agriculture.[38] Garland Allen has

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[35] Palladino 1993: 302.

[36] On Palladino's doubts about the impact of genetics on plant breeding before the 1950s, see Palladino 1996a; 2002: 183, Thirtle et al. 1997; 1998 and the more detailed discussion of wheat economics in chapter 4 of this dissertation. See also Dejeager 1993.

[37] Surprisingly, the debate about Mendelism's relationship to agriculture in Britain seems to have faded at roughly this point. Both Olby and Palladino have returned to the subject of the rise of genetics but from quite different angles. In 2000 Olby published two papers for centennial celebrations of the 1900 "rediscovery". In both of these papers Olby stressed the importance of the Royal Horticultural Society's early support for Bateson, and his allegiance with experimental biology but on the subjects of agriculture and success he remained silent except to say that a polygenic and chromosomal understanding of heredity (missing from most early Mendelians' thinking) was necessary before early claims to success could be substantiated. Palladino's last work on this subject, published in 2002, is a historiographical meditation on the relationships between history, genetics and modernity. As such it did little to push forwards his analyses of the 1990s except as examples of a social constructivist historical approach which he rejects in the later stages of the book Palladino 2002. See also the article length précis of this book, Palladino 2003.

[38] See Barbara Kimmelman's much quoted PhD thesis, Kimmelman 1987 and two published papers on links between agriculture and genetics, Kimmelman 1983; Kimmelman and Paul

also pointed to these connections.[39] These authors have tended to stress the limits of Mendelism to change plant breeding without going on to undermine its utility entirely. Meanwhile, Daniel Kevles, also working in the history of science, has charted the rise of Mendelism by way of a cross national comparison between the United States and Britain. Kevles's analysis stresses the importance of resources. Mendelism was successful in his view, to the extent that its protagonists could secure funding, laboratory space and land.[40] Sociologists and historians of agriculture have also been alive to the connections between genetics and agriculture, if more critical of the utility of Mendelism to farmers. Deborah Fitzgerald and Jack Kloppenburg in particular have suggested that academic breeders and seed businesses co-opted publicly funded research, channelling it into more profitable directions for themselves.[41] As this research advanced, skills and capital barriers were placed between farmers and the breeding programs that were supposed to aid them. For Fitzgerald the more important of these effects was the skills barrier created by the professionalisation of academic plant breeders and their use of ever more complicated breeding methods. For Kloppenburg it was the capitalisation of breeding by seed firms which cut the farmer out. In both cases the effect was the same, farmers were excluded from the process of plant breeding, a process which they had traditionally undertaken for themselves in the field. The American historiography, even if it is not entirely coherent, makes it obvious that early Mendelism was supported by links to breeding and the land-grant universities,

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1988. The second paper is also available at MendelWeb <http://www.mendelweb.org/MWpaul.intro.html> [accessed 10 Jan. 2010].

[39] From Garland Allen see Allen 1990 and 2000. See also the paper from Kimmelman in the same volume, Kimmelman 1990.

[40] See Kevles 1980.

[41] See Fitzgerald 1990, 1991 and Kloppenburg 1988. For comparative reviews of Fitzgerald and Kloppenburg's books see Palladino 1991 and Allen 1991.

which had their own interests in the potential agricultural applications of the new discipline.

Comparisons between British and US agriculture need, however, to be handled with care. The significant difference between these cases was the principle crop grown in each country. In the nineteenth century this was wheat in both countries, in the twentieth century however, wheat remained important in Britain while its production was overtaken in US fields by corn; maize to British farmers. Maize, an out-breeder, was subjected to the F1 double cross hybrid method. Wheat, an in-breeder, in British fields at least, was not amenable to this method of breeding, as it showed little response to the still poorly understood effects of heterosis. As a result, scientific efforts in wheat breeding have focused on hybridisation, rather than the more elaborate F1 double cross hybrid method.[42] The new F1 hybrid maize strains produced by the double cross method notoriously disbarred buyers from producing their own seeds. In Britain the new Mendelian wheat strains were quite different. Biffen’s wheat varieties bred true, being self-fertilising, so their seeds could be reliably and easily reproduced. The upshots of these differences, among other factors, led to a much more rapidly commercialised plant breeding sector in the US. Where hybrid corn in the US gave higher yields at a price; the need to return every year for fresh seed, hybrid wheat in Britain flowed freely.

Recent works have begun to broach the reception of Mendelism by French and Dutch agriculturalists in more detail.[43] In both cases it seems there was considerable resistance to Mendelism from traditional breeders. In France and the Netherlands plant and animal breeders respectively, had developed their own,

[42] For more on the theory of heterosis at the heart of corn breeding strategies, see Shull 1911, Bruce 1910, 1917, Ashby 1937, Crow 1987, Paul 1992 and in wheat, Briggle 1963.

[43] On France see Gayon and Burian 2006; Burian, Gayon and Zallen 1988, Gayon and Zallen 1998 and Bonneuil 2006. On the Netherlands see, Theunissen 2008 and on the Netherlands as colonial agricultural force Maat 2001.

successful ways of visualising and planning crosses. One could hardly say Mendelism sparked a revolution in these countries. Further afield, the Mexican case is also now being explored, especially in view of the political implications of Rockefeller Foundation supported agricultural research and its impact on Mexican agriculture.[44] Surprisingly, the Rockefeller foundation, which is famous for the funding it provided to green revolution projects, also supported many non-Mendelian breeding projects based on selection and identification of locally suitable varieties. It would be fair to say that, as with the American, French and Dutch cases, success or failure are too simplistic as categories with which to gauge early genetics, as a discipline or as a breeding program.

In the German case Jonathan Harwood has suggested that many agricultural colleges and departments provided early support for Mendelism. However for Harwood this support had far more to do with the social status of academics and their institutes than any successful overhaul of plant breeding. This theme is most fully articulated in *Technology's Dilemma.*[45] Here, Harwood discerned in the German case a generalised pattern in the development of agricultural science towards a prioritization of science (including Mendelism) over the agricultural aspects of the discipline. This is described by Harwood as "academic drift" a process whereby agricultural science moved away from contact with the world of farming at large, including plant breeding, and towards academic disciplines; for example, soil chemistry or, indeed, genetics. This move was driven by the desire of agricultural scientists to secure greater academic status for agricultural science, thereby cementing their agricultural institutes' places in the university context. Along the

[44] On Mexican Mendelism, see Barahona, Pinar and Ayala 2005. On Rockefeller aided plant breeding in Mexico at a slightly later period, see Harwood 2009.

[45] Harwood 2005a. See also Harwood 2005b for a journal article length summary of the main arguments in *Technology's Dilemma*. I would also like to thank Jonathan Harwood for giving incredibly generous early feedback on the material that became chapter 1 of this thesis.

way agricultural science lost contact with small farmers and became much more closely associated with larger agricultural organisations endowed with the capital to take advantage of new scientific agricultural products – often expensive fertilisers or hybrid varieties.[46] In Germany, it would seem, what agriculture got from Mendelians and Mendelism was support for the increasing industrialisation of farming.

Looking to the British case, Harwood, drawing on Palladino's work, has suggested that a similar process occurred at Cambridge:

> At Cambridge University's School of Agriculture RH Biffen was convinced that breeding practice could only be improved by applying and further developing the Mendelian theory. And in keeping with the dominant view among his colleagues at the School he felt that utilitarian considerations need play no part in guiding research in agricultural science.[47]

That is to say the agricultural department at the university was rather more Mendelian than it was agricultural and the economic impact of the department's work was of no great importance to those who worked there. It would follow from this analysis that Mendelism did little for plant breeding but much for the disciplinary insecurities of agricultural scientists at the periphery of the University context.

The question of Mendelism's success as a plant breeding method came to the fore for many of the aforementioned scholars at a cross-disciplinary workshop

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[46] For more on the German case see Wieland 2006 in which Thomas Wieland argues that the relationship between science and practice was instantiated in different ways at different levels; at the level of the variety, academic or institute. This paper was part of a special edition of *Journal of the History of Biology* dedicated to Garland Allen's 70th birthday. See the other papers in this issue referenced below. On the German genetics communities at large see Harwood 1993.

[47] Harwood 2005a: 225-226.

organised by the Department of Plant Breeding at the Swedish University of Agricultural Sciences in collaboration with the Swedish Seed Association, held in Lund and Svarlöv in 1996.[48] The origins of the analysis presented in Harwood's *Technology's Dilemma* can be found in his presentation at this workshop which directly questioned the impact of Mendelism on plant breeding.[49] Beliefs about the efficacy of Mendelism to change plant breeding were, Harwood argued, structured by the institutional position of academics rather than any inherent revolutionary power of Mendelism. In a direct challenge to this thinking Norwegian historian of science, Nils Roll-Hansen, presented a paper in which he argued for a partial restoration of the traditional view that Mendelism had a direct impact on plant breeding and agriculture.[50] For Roll-Hansen, theory did impact on practice, albeit in a rationalising, systematising way rather than a direct one. Where Kim has suggested that Mendelism was better able to account for breeders and physician's own results, Roll-Hanson, in an argument that agrees easily with much of Olby's thinking, suggested that Mendelism, as an experimental system, was more rational and systematic, and so better able to *produce* results. Roll-Hansen based this argument on a comparison between agricultural success in Sweden and Britain. In the Swedish case Roll-Hansen argued that a rather tight linkage between the application of science to the practice of plant breeding and agricultural success can be perceived in the work conducted at Svalöf and Swedish agricultural production data from the same period. Roll-Hansen identified relatively constant yields and pessimism about Mendelism's impact from Engledow as evidence of what had already been achieved

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[48] The proceedings of the workshop were edited by Nils Roll-Hansen and published in the following year in the Swedish Seed Association's journal, Sveriges Utsädesförenings Tidskrift. 107:4 (1997); 163-235. I would like to thank Garland Allen for loaning me a copy of this hard to find edition. Although obscure this focus section is well worth digging out for the range of articles focused specifically on genetics and agriculture.
[49] Harwood 1997.
[50] Roll-Hansen 1997.

in Britain before the turn of the century. Most of the gains that were achievable by rationalising and systematising had already been made in Britain by 1900, so in comparison to the Swedish case, there was little for Mendelism to accomplish.[51] In a surprising move, Palladino's comments on Harwood and Roll-Hansen's papers at the workshop aimed to reconcile the two views.[52] According to Palladino's thinking both views could be right. From a retrospective vantage point it seems obvious that the adoption of Mendelism was accompanied by changes in plant breeding and gains in productivity.[53] But from a contemporary perspective this was not at all obvious to breeders. However, this appeal to perspective, one senses, would only have half placated Harwood and Roll-Hanson, searching as they were for the objective truth of Mendelism's success or otherwise.

The foregoing discussion suggests there are no easy resolutions to the question of Mendelism's success either in Britain or abroad. Given the numerous recent calls to pay greater attention to the relationship between genetics and agriculture, a return to the British case is now due.[54] One way forward is hinted at in Roll-Hansen's analysis, just what sorts of rationalising and systematising were happening in British Mendelism?

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[51] For more on Roll-Hansen and his realist approach to Mendelism see the debates between him and SSK philosopher of science Barry Barnes summarised by Kyung-Man Kim in Kim 1991. See also Roll-Hansen 1978.
[52] Palladino 1997.
[53] Note the use of the word accompanied, which allows Palladino to remain ambiguous about any causal relationship. See also papers from Marga Vicedo 1997 and Barbara Kimmelman 1997 who both attended the workshop.
[54] See especially the introduction to the *Journal of the History of Biology* focus section referenced above, Harwood 2006.

A New Historiographical Approach

In the following section I suggest a way forward in conceptualising and answering questions about the relationship between genetics and agriculture. This approach has often been implicit in the extant historiography but has never before been explicitly applied.[55] The historiography as it stands has tended towards a rather atomised analysis of competing interests. But what of the connections between individuals and institutions? As Barbara Kimmelman has recently suggested:

> As we learn more about research and practice at agricultural institutions I suspect we will need to embrace a very different conception of institutional relationships than the rather hierarchical one that currently prevails. The diversity of kinds of scientific institutions during this period is striking, as is the complexity of their relations. ... [E]ach had their own distinctive constituencies and social goals. But the close contacts maintained among researchers as they moved from place to place held the diverse elements together.[56]

Thomas P. Hughes originally developed the "system metaphor" as a means of analysing large interconnected technological structures, comprising numerous elements: states, institutions, education and theory as well as artefacts and the complex relations between them.[57] The focus of Hughes's key works in this area has

[55] A brief outline of this new approach was made at a Max Planck workshop in 2009, see Charnley and Radick 2010 and Charnley and Radick forthcoming. On the appropriateness of a system analysis see Sapp 1983: 313 & 317 and Palladino 2002: 215.
[56] Kimmelman 2006: 277. Unsurprisingly the acknowledgements of Kimmelman's PhD thesis cite the influence of Hughes on her historical outlook, Kimmelman 1987: v.
[57] Hughes uses the term "systems metaphor" in his 1987 publication on systems but refers to them as literal objects elsewhere. On the ontological status of firms see Ginds 2009. In what

been on the development and growth of systems, and how technological change occurred in society over the last two centuries. The analysis given here is drawn from Hughes's most significant book on systems, *Networks of Power*, a chapter, contained in the edited volume, the *Social Construction of Technological Systems*, and to a lesser extent his most recent book *Human-Built World*.[58]

Hughes employed systems thinking as a way to describe various technological structures which solved problems or fulfilled goals. The archetypal systems for Hughes were the emergent electrification systems of the late nineteenth and early twentieth century.[59] As Hughes describes it, an electric system can contain not only the hardware of power-stations, cables and transformers but also financiers, government institutes, consumers, and any other groups that interact with the system. Viewing electrification as a process of systematisation in this way helps Hughes to analyse the "messy complexity" of technological change.[60] For Hughes, technological changes were directed by system builders, people who were problem solvers, who invented technologies, and who worked across disciplinary and institutional boundaries with diverse organisations. On this view, Thomas Edison was the archetypal system builder; a problem solver who invented technologies, organised finance and political support, and established various companies in creating the means for his technologies to be used in the world. System builders such as Edison and their contemporaries brought together disparate resources to build and then maintain a system. In doing so, they overcame what Hughes calls "reverse salients", components of the system which become problematic because

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follows the system referred to is a literal one, but small enough to avoid problems surrounding collective motivation.

[58] Hughes 1983, 1987 and 2004.

[59] This focus reflects Hughes's own academic background as an engineer.

[60] Hughes 1987: 51, fn.1.

they lag behind the advance of the rest of the system.[61] When this happens, wayward components need to be brought back into line.[62] Systems have a number of features including inputs, outputs, and components. Some have subsystems and some are subsystems. They acquire momentum and they can decay and languish. Hughes admits his work is less elegant than systems theories deployed in engineering, science or social science, but he claims it is more useful to the historian of technology because it helps to capture the diversity of resources drawn upon by system builders as their technologies interact with the world. This way of analysing technological change is a loosely social constructivist account. Systems are "socially shaped and society shaping".[63]

The central question addressed in this thesis is how can we best assess the emergence of Mendelism? Conceptualising Mendelism as part of a system for solving problems in heredity – for fulfilling the goal of a better world through either knowledge or modified organisms and recruiting funding and support for those ends – gives powerful insights into the discipline's development. Bateson, Biffen, Wood and several other key Mendelians were truly system builders, in the Hughesian sense. One of their goals was, after all, to produce new agricultural and horticultural organisms and the means to distribute them. On this view Mendelians brought together Mendelism and agriculture as parts of a system, a collection of components which interacted towards a common goal. The speed with which those interactions were organised, and the diversity of participants involved in them, undoubtedly

[61] Originally a military metaphor, used to describe the areas in an advancing front which lag behind the advance. See David Edgerton's critique of reverse salients, and his appraisal of the differences between Hughes and other social construction of technology writers, in Edgerton 1993.

[62] For more on these problems see Hughes 1983: Ch. IV "Reverse Salients and Critical Problems".

[63] Hughes 1987: 51, although Hughes is not really a social constructivist as Edgerton's review makes obvious, see also Edgerton's thoughts on Donald Mackenzie on reverse salients, Edgerton 1993: 75.

helped to stabilise Mendelism's place in the world. To put it another way, Mendelian thinking became institutionally stable at the same time as the Mendelian system became very widely dispersed. The system became widespread as the system builders secured funding and support from a broad spectrum of organisations. The relationships between Mendelism, plant breeding and agriculture that arose in the period reflect the aims of the system in which they were located. If we want to ask what impact these areas had on each other, Mendelism's impact on agriculture was successful to the extent that it realised the plans of Mendelian system builders to change agriculture. Conversely, agriculture impacted on Mendelism in many ways, but the key moments were those occasions when agricultural realities hampered Mendelian system builders' plans.

Thinking about a Mendelian system also helps in identifying and analysing features of Mendelian thinking, of which there were many different types.[64] For example, reading Mendelism as a system helps to explain the predominance of reductionism and determinism in Mendelian thinking. In the decades after the turn of the century possibly millions of people came into contact with an apparently simple theory very quickly. Two of the key features of the theory for early Mendelians were the theory's apparent simplicity and its power to allow the construction of new organisms. These were ideas that funders in government and supporters in other organisations could easily grasp. Simplicity promised easy application; if only the correct unit factors could be discovered, organisms with desirable combinations of characters could be bred. By the time it was realised that the theory was in fact much more complicated and its application less straightforward than had at first been thought, support and funding had already been

[64] See Olby 1979 on the differences between Mendel's thinking and that of the early Mendelians.

promised. The organisations responsible for both were already more or less committed to the system and, crucially, the “unit-factors-direct-organismal-characters” description of the theory had by this time become stabilised, largely because it had been promoted to so many groups. Thanks to the wide angle view provided by a systems analysis we can begin to see how that description came to promote the notions of genetic determinism identified by Richard Lewontin and Evelyn Fox Keller in their critiques of gene fetishization later in the century.[65]

Thinking about a Mendelian system undoubtedly has its challenges, limits and flaws. Defining the correct level of analysis at a global, national, regional or local scale is an immediate challenge, as is defining the boundaries of a system, both structurally and temporally. As a metaphor it is in places anachronistic – not all early Mendelians saw themselves as involved in system-building – and in others inaccurate – thinking in this way tends to exaggerate the coherence of change. Finally, the terms of my question coupled with this type of analysis tend to overemphasise the stability and prominence Mendelism actually attained in a world which has never been wholly Mendelian. Hughes’s analysis is perhaps at its best when describing emergent systems, it is for this reason that I concentrate on the emergence of a Mendelian system, where scope is fairly well defined by the influence of system building Mendelians.

The systems approach has at least one major advantage over more straight forward social construction of technology approaches, its potential for political sensitivity.[66] A systems analysis allows us to recognise the importance of political relationships in a way that is often missing from analyses which emphasise the

[65] Again, Lewontin comes to the rescue, this time to counter determinist accounts of genetics, Lewontin 2000a; 2000b, see also Keller 2002.
[66] For critical analysis of the a-political nature of some social constructivist approaches see Winner 1993.

social. To put it another way, not all elements in the social milieu have the same power to sculpt and change new technologies. System builders, for example, have a very different kind of influence on a system compared with consumers. Very often, such differences emerge because of differential access to political elites. There are, however, two distinct changes I would like to make to the analysis provided by Hughes. The first speaks to the point above. While it is true that we need to recognise unevenness in the distribution of power within a system, I would also like to avoid making the mistake of downgrading the status of consumers to an entirely passive role. While consumers might have had a diminished level of power in comparison to system builders, their effects on the system were certainly tangible. In this case, recognising the diversity of consumers helps to make this point, breeders, farmers, millers and brewers all acted as heterogeneous groups of consumers of new Mendelian products but there are obvious differences in the levels of pressure these groups could bring to bear. Furthermore their positions changed over time, for example farmers were in a very weak position at the turn of the century, but thanks to various factors, they were in a much stronger one by the end of the Great War. This is the first modification I want to make to a strictly Hughesian analysis; a greater appreciation of the role of consumers, framed within an understanding of the power differentials present within and between different groups of consumers, and between consumers and other components of the system.[67] The second modification I would like to make is to provide a more sensitive analysis of intellectual property and its place within the system.

In the years since Hughes published *Networks of Power* a much more fine grained analysis of intellectual property has become available. In terms of Hughes's own subject matter, electro-technical innovation, patents in this area have been

[67] For the limitations of Hughes' analysis regarding consumers see Gooday 2008: 16-17.

shown to be far more contested and less straightforward instruments than Hughes represents them to be.[68] In general terms the analysis of intellectual property has now moved beyond the field of what was patented to include the un-patentable and the non-patented. Plant material has traditionally been thought of as belonging to an un-patentable category of innovations, with the notable exception of asexually reproducing plants in America after the 1930 Plant Patent Act became law. However, if the anachronism can be excused, it seems that many British plant breeders sought to exploit their intellectual property in plants, they were, as Daniel J. Kevles has put it, "alive to the concept".[69] One of the key features of such activity was its operation in a sphere I call the moral economy of plant breeding, adapting the phrase from social historian E. P. Thompson's moral economy of the English crowd.[70] In their turn, Mendelian system builders and their supporters learned how to operate skilfully in the moral economy of plant breeding, adapting existing tropes to argue for the morality of their work. Biffen was praised as an "angel", no less, by one farmer, for giving over his intellectual property in the form of freely available wheat varieties. These sorts of public glorifications no doubt helped Biffen in his system building activity.[71] Intellectual property, construed in a much broader sense than Hughes would, perhaps, recognise, was an important part of Mendelian plans.[72]

Given these two modifications to Hughes systems analysis, the following chapters show that when used sensitively this perspective has great benefits. Initially,

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[68] On electro-technical intellectual property see Gooday and Arapostathis (forthcoming).
[69] Kevles 2008b: 207.
[70] Thompson 1971, see also Charnley (forthcoming). Different concepts of moral economy and the type of moral economy explored here are discussed in more detail in chapter 3.
[71] "Bedfordshire Chamber of Agriculture", in *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University*, p. 2. Rowland Biffen Papers.
[72] For a broader construal of intellectual property than patents, copyright and trademarks see Lewontin and Santos 1997, and Kevles and Bugos 1992, Kevles 2001, 2007 and 2008b, Lissoni and Metcalfe 1994, Biagioli and Galison 2003 and the innovation without patents literature, Macleod and Nuvolari 2006, Nuvolari 2004, Macleod 1991 and Robert Allen 1983.

it reveals the huge amount of work involved in system building, which helps us undermine the notion that Mendelism was passively accepted in virtue of its truth. The move to systems also helps in revealing the interrelations and connections between individuals and institutions, largely treated until now in competitive isolation. In a system there is also a place for theory and a specific understanding of the connections between theory and practice, these now being viewed as relational terms to be defined within a system of connections. As Hughes would argue, system builders see no fundamental division between practice and theory. Biffen sometimes talked about the economic and research sides of his work but in the end it all added up to a somewhat larger plan. This can perhaps be best seen in coherent responses to problems, especially those presented by variability, mounted by early Mendelians. Finally this approach allows us to make an analysis of Mendelian success in the field by providing a better yardstick of what success entailed. Using systems thinking it becomes possible to measure Mendelian success against the early Mendelian system builders' plans.

The Thesis Plan

The structure of this thesis is intended to reflect the system it describes. To accomplish this, each chapter adopts a slightly different approach. When the focus is on institutes, institutional history is used. As the thesis turns from institutions to ideas, history of ideas has been adopted to the systems analysis. Recent developments in innovations studies back up the analysis of production in chapter 3, history of economics and technology the analysis of the use of Mendelian varieties in chapter 4 and global history the analysis of British Mendelism in an international context in chapter 5. This sort of intellectual nomadism is unusual but entirely appropriate in this thesis. The Mendelian system it seeks to capture was a many faceted enterprise.

In the first chapter the considerable efforts of a small band of Mendelians to bring together resources in establishing new institutions and running them, are brought into focus. This work required large amounts of land and funding. Accordingly, the somewhat cottage-industry nature of early Mendelism was replaced by a moderately funded system of research institutes operating in coordination.[73] Furthermore, by the second decade of the twentieth century several early Mendelians had moved into positions in the government's Department of Agriculture, from which they aided their former colleagues at Cambridge. This chapter responds most directly to the histories of Olby and Palladino, discussed above, recasting their protagonists not as local interest groups or an elite technocracy but as system builders, involved from the outset with the creation of an institutional network aimed at aiding agriculture.

In the second chapter the focus moves on to an analysis of the central tenets of Mendelian theory, demonstrating how they developed and changed in view of their agricultural context. Mendelian theory can be thought of as encompassing two dispositions, one to reduction and another to construction.[74] The Mendelian laws defined by early Mendelians reflected these dispositions. As a result they were challenged and changed by feedback from empirical anomalies, in particular the rogues (out-of-type plants) so obviously reviled by Hurst in the epigraph to this introduction. Rogues, as we will see, became a central problem for Mendelian theory; following their story gives a fascinating insight into the lesser studied question of how agriculture impacted on genetics.

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[73] On the cottage-industry nature of early Mendelism see Richmond 2006a.

[74] My discussion of Mendelian theory in this chapter is strongly informed by the line of thinking about Mendelism exemplified in Müller-Wille 2007b.

The analysis then turns from the theoretical development of Mendelism to focus squarely on Mendelian efforts to produce new varieties of agricultural plants. In particular, the wheat breeding program of Rowland Biffen which ran from 1903 until roughly 1926 comes under scrutiny. Over the course of his career Biffen released several new varieties, aimed at different ends. The chapter opens with a discussion of the moral economy of plant breeding that emerged in the nineteenth century. Debates in the 1880s around two varieties of pea are used to illustrate the ways in which breeders came to associate value with their new varieties. The chapter then turns to the new Mendelian breeding, the creation of new Mendelian wheat varieties and their release to the farming community through distribution channels established and maintained by Biffen and his peers. Finally we will see that the out-of-type plants discussed in the second chapter were not just a theoretical problem for Mendelism, they were also a problem for Biffen's productive efforts; one that was deeply bounded to Biffen's intellectual property claims over his varieties. This chapter reveals that despite a lack of patents in plants, Mendelian breeders (as much as their forebears) were concerned to protect the value of their new varieties.

The varieties Rowland Biffen created were intended to solve certain problems. Each was aligned to a strategy, or set of strategies, that were intended to change the way farming worked. The fourth chapter focuses on the extent to which the strategies envisaged by Biffen were or were not incorporated into farming practice. At least two of Biffen's varieties were incredibly successful but new evidence from the farming literature reveals this success came when they were used in ways which were not directly or principally intended. Recovering the ways in which varieties were used demonstrates that the path from test plot to successful variety was not entirely under the system builder's control. The active role farmers played in deciding how to deploy new varieties points to a far more intricate relationship between the test plot and the farm than has previously been recognised.[75]

The fifth and final chapter has the widest scope in the thesis. Britain, in the period, was an entity which extended far beyond the shores of the British Isles. Connections with research institutes across the globe were an important feature of the stabilisation of Mendelian theory. The wider world also provided the raw materials for Mendelian breeding, varieties taken from places as diverse as Canada and Thailand formed the basic building blocks from which Mendelians “made” their varieties. Furthermore, Mendelians argued that the rest of the world would benefit from Mendelism and its products as these were deployed to help colonial agriculture. The international context could, however, also create threats; New World wheat imported into Britain undermined the domestic market. The ambiguity of the world’s position as threat, resource or beneficiary of the British Mendelian system is explored through close attention to both the international aspects of Mendelian work in Britain and the Mendelian aspects of work in the colonies, or what Biffen began to call the, “agricultural Empire”.[76] This was an empire which began in Britain with a series of new institutions founded shortly after Bateson began introducing British audiences to the new theory of Mendelism.[77]

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[75] A new way of thinking about the impact and integration of varieties is imported here from agricultural economic historians Olmstead and Rhodes 2008 and historians of technology Oudshoorn and Pinch 2003.

[76] Harold Begbie, “Professor Biffen: The Idea of a Rural Civilisation”, in *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University*, Rowland Biffen Papers, p. 1. The analysis of global connections and contrasts given in this chapter owes much of its form to the programmatic statement of global history given by O’Brien 2006.

[77] The first English translation of Mendel’s famous 1866 paper was printed in the *Journal of the Royal Horticultural Society*, by Bateson 1901.

Chapter 1

A New Institutional Map: Creating Space for Mendelism

THE ARRIVAL OF MENDELISM in Britain coincided with a vast expansion in the institutional life of agricultural science. In the first quarter of the twentieth century a range of new agricultural organisations were established across Britain, many of them sponsored by government funding. Their appearance fell between two periods, roughly 1875-1895 and 1930-1940, identified with agricultural depression. These organisations were concerned with rescuing agriculture by applying science to agriculture's problems. Several institutes were established in the south east of England around Cambridge and London and these in particular formed a tightly-knit network. This network was held together by geographical location, shared funding and the circulation of individuals, ideas and materials, including, of course, plant varieties. There was even, by the mid-1920s, a burgeoning division of labour.

Mendelians dominated many of these organisations, creating intellectual and institutional homes for themselves. William Bateson, for example, set the research agenda at the John Innes Horticultural Research Institution with an apparently dictatorial grip, from his appointment as its first director in 1910 until his death in 1926. Rowland Biffen was also tremendously busy creating and running organisations. In the thirty years after the turn of the twentieth century he advanced a Mendelian approach to agricultural science at Cambridge University. Biffen also helped Bateson running the John Innes, co-founded a British Seed Corn Association and a National Institute of Agricultural Botany, and worked with the Home Grown Wheat Committee (an organisation supported by the National Association of British and Irish Millers). Mendelism swiftly became a key part of the agricultural science these new organisations sought to apply to the supposedly ailing agricultural sector.

What follows is an overview of an institutional environment, one created largely by Bateson, Biffen and Thomas Barlow Wood – the third Cambridge Professor of Agriculture after the chair's creation and, along with Bateson, a long-time ally of Biffen.[1] To be sure, the organisations touched by the hand of these men were not the only ones involved in agricultural improvement. The Rothamsted Experimental Station, The Agricultural College at Wye and The Royal College of Agriculture at Cirencester all pre-dated Mendelism and remained largely free of government influence because of their independent funding. There were also government funded organisations which did not come under Biffen, Bateson and Wood's sway; the Scottish and Welsh Plant Breeding Stations for example. However, both of these organisations were established later than their English counterparts and received much less government attention.[2] In private, Biffen was rather rude about the Welsh station and considered these regional outposts to be of little importance.[3] In terms of funding and connections to government, the organisations dominated by Mendelians stood apart from, and often looked down on, their independent or regional equivalents.[4]

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[1] This is a perspective inspired by the panoramic view of German agricultural institutions given by Harwood 2005: 34-35. For biographical details on Wood see his FRS obituary notice,
F. G. H. 1931, and notices in *Nature* H. E. W. 1929, Prothero 1929 and *Biochem. J.,* Russell 1930.

[2] See Olby 1991 on the deal Scotland got from the Development Commission. For an overview of the Scottish and Welsh stations see Palladino 2002: 43-54.

[3] See Biffen to Weaver, 11th October 1918, NIAB Archives, folder marked 1917-1919. The institute's archives are currently being moved into new rooms at NIAB's headquarters, which are still in the same buildings opened by Weaver in 1921. I've tried to identify all of the sources used from here as accurately as possible. I also hold copies and will happily provide them on request.

[4] Indeed, the first conference that involved all three of the British plant breeding stations did not occur until 1947 when it was held in Cambridge, see "News and Views: Dr G. D. H. Bell" 1948: 715. The second occurred in the following year, this time at the John Innes

This chapter, and the entire thesis for that matter, focuses on plants. In some senses this is an artificially divisive focus; plant and animal breeding were often conducted by the same researchers and not infrequently shared institutional space.[5] However, the analysis given here is restricted to plants to reflect the focus of the work of our chief protagonist, Rowland Biffen. Biffen's enduring ambition was to change the face of British agriculture through the use of new crop varieties. Bateson and Wood shared this vision with him even though they were each involved in their own animal breeding programs. Wood's most noted work was on agricultural statistics, he was also well known for his studies on animal physiology and specifically rates of growth.[6] Bateson's work with Reginald Punnett, on linkage using poultry, is amongst his most famous. But as well as these projects, Bateson and Wood were Biffen's chief allies in the institutional expansion of plant breeding that forms the focus of this chapter.

With these restrictions of scope in mind, we begin the story of Mendelian institutional expansion with Cambridge University's Department of Agriculture. During the 1890s, as the department slowly developed under the wing of the experimental science movement at Cambridge, it became just the sort of place where one would expect a new experimental science to develop. From around 1902 the department, which had only formally opened in 1899, was a hotbed of Mendelian experimental activity. Accordingly, the chapter initially focuses on the foundation of

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Horticultural Institution, "Policy and Problems in Plant Breeding" 1949: 51. For detailed historical accounts of the Welsh and Scottish Stations see Palladino 1990. On the Rothamsted experimental station see Vernon 1997 and for a detailed case study that adds to the themes considered here see Donald Opitz's account of the foundation of Whittingehame Lodge, at the University of Cambridge, Opitz 2011.

[5] Institutes of Animal Nutrition and Animal Physiology were established alongside the Plant Breeding Institute at Cambridge. For more on Punnett, animal breeding and genetics see Marie 2004 and 2008.

[6] On agricultural statistics see Wood 1910, Wood and Yule 1910, Wood and Stratton 1910, N. Hall 2007: 301 and Hacking 1988. On physiology and growth see Wood and Marshall 1920. For Wood's thoughts on rationing for humans, developed during the First World War, see Wood 1915 and 1917.

the Department of Agriculture at Cambridge, Mendelism's arrival, the department's consolidation of its position at the University, and its subsequent expansion into a school. During this process the department fostered relationships with the Home Grown Wheat Committee – a small but powerful group which was prominent in furthering Mendelian research. From 1910 the fortunes of agricultural Mendelians began a dramatic upwards turn. With Biffen's help, Bateson took directorial control of the John Innes. Simultaneously, Biffen and Wood petitioned the newly established Development Commission for further funding and in 1912 successfully negotiated funds to establish two new research institutes at Cambridge; one for plant breeding and one for animal nutrition. At the same time Biffen and Wood began working on establishing the British Seed Corn Association to distribute certified seeds. Shortly after the First World War, Biffen and Wood, at the height of their powers, collaborated in the foundation of the National Institute of Agricultural Botany, another organisation deeply involved with the distribution of Biffen's seeds.[7] Finally, in closing, we will see how the creation of this network was represented in three of the most important forums of the period for aspiring Mendelians, the British Association for the Advancement of Science, *Nature* and *The Times*.

The reconstruction offered here builds on the existing historiography in a couple of significant ways.[8] Some of the details are new; much of the material on the National Institute of Agricultural Botany has literally emerged from the institute's basement in the last few years. Among the treasures are several letters from Biffen and Wood to Lawrence Weaver and other key officers of the institute, revealing the

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[7] In contrast see *Nature's* report of E. J. Russell, director of Rothamsted, at the British Association for the Advancement of Science giving a "despondent" account of what was happening at his home institute in 1924, Cantab. 1924: 465.
[8] For an historical overview of agricultural science see Russell 1966.

influence of Biffen and Wood in steering the institute's initial course. There are also new materials that have come from archives which have already been mined, but which have revealed more, as building on previous work the net has been cast wider. The form of the discussion here is also new, rather than looking at fragmentary groups of individuals and their interests, in what follows the focus is on relations between groups and the coherence fostered by key individuals, Hughesian system builders. Among these organisations there was a tendency to work in concert, and Biffen, Bateson and Wood acted as conductors.

Later chapters of this thesis will examine the intellectual problems faced by Biffen et al., the distribution of the products of their efforts, the use of those products and finally the significance of these events on an international level. However, this chapter focuses squarely on the institutional bare-bones of a Hughesian system for the production of new knowledge and biological organisms. The organisations that we will see being created and directed by Biffen, Bateson and Wood are the systematic equivalents of the new power companies established by Edison, Westinghouse and their contemporaries. After the same fashion as their electrical equivalents, and just a generation later, Biffen, Bateson and Wood set about crafting a new system of institutions, with much of the same novelty and ingenuity.

1.1 Agricultural Science and Mendelism at Cambridge

1.1.1 The Cambridge Department of Agriculture before 1901

During the 1890s British agriculture was in the doldrums of a depression. The actual extent of the depression and its putative causes are still open to debate, but contemporary perceptions acted as a prompt for action aimed at relief.[9] One popular strategy was to increase agricultural instruction provided by county councils.[10] Such efforts needed educators, and in 1890 Henry Chaplin (later Viscount Chaplin) wrote to the Chancellor of the University of Cambridge – the Duke of Devonshire – to suggest the University should provide teachers for agricultural education.[11] The syndicate established to investigate the possibility recommended the creation of a department of agriculture offering a degree in the subject, but the idea met with resistance. Academics such as James Mayo were reluctant to allow what they saw as technical instruction, bread studies and farming, a place at the University.[12]

Despite this resistance, in 1891 one of the members of the syndicate, Professor George Liveing, together with Henry Robinson, from Downing College, organised the Cambridge and Counties Agricultural Education Committee and began to give classes for senior students interested in agricultural teaching. With the financial aid of the local county councils and the support of Mr. A. E. Brooke-Hunt (then Educational Inspector of the Board of Agriculture), Robinson, Liveing and

[9] As with most depressions the thing that mattered was people's beliefs, and judging by the slew of surveys of agricultural depression published around this period some people certainly believed the depression to be the worst ever seen. For a contemporary if populist account instigated by the Morning Post see G. Anderson [1899]. For historical analysis of the Great Depression in British agriculture see chapter 4 of this thesis and Collins and Thirsk 2000: VII.1.
[10] On the development of county council agricultural education see Stuart Richards 1988.
[11] *Agricultural Education 1890-1916*, University Registry Guard Books, Cambridge University Archives, C. U. R. 108, section 59-60.
[12] James Mayo was probably a theologian, although little biographical evidence exists. His resistance seems to have carried some weight within the University see *Agricultural Education 1890-1916*, section 21.

Thomas McKenny Hughes, the Chair of Geology, organised a course which was delivered in Liveing's rooms in the Chemical Laboratory's basement.[13] Henry Marshall Ward, the Chair of Botany, and Biffen's teacher from 1897-1899, also lent a small room for teaching in the Botany School. Ward and several other professors, such as Liveing and Cecil Warburton, gave lectures to agricultural students on their own subjects; botany, chemistry and zoology respectively.[14] Thomas Wood and John Percival (the future Professor of Botany at Reading University) attended these classes before leaving Cambridge to give lectures in regional farming communities. Although the University may not have liked it, during the 1890s the department was supported by a small group of Cambridge dons who were interested in promoting experimental science.[15]

Towards the end of the century these arrangements became more formalised. In 1896 Sir Walter Gilbey created an endowment to support a lectureship in the history and economics of agriculture for 21 years.[16] Furthermore, in 1898, the assistance given by the nine county councils neighbouring the university was fixed at a yearly contribution of £750. This was supplemented by government money organised by Brook-Hunt. Gilbey swiftly offered a second endowment for another lectureship and shortly afterwards the Drapers' Company of London also offered the university funds to support a Chair in Agriculture and an entire department for the

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[13] Liveing was involved in the introduction of experimental science at Cambridge University, see W. C. D. Dampier, "Liveing, George Downing (1827–1924)", rev. Frank A. J. L. James, *Oxford Dictionary of National Biography*, Oxford University Press, 2004 [accessed 29 Jan 2009].

[14] See Board of Agriculture, *Annual Report on the Distribution of Grants for Agricultural Education and Research in the Year 1900-1901*, Cd. 814 (1902), 17.

[15] On the development of experimental science at Cambridge see Fox and Guagnini 1999: esp. p.107.

[16] Gilbey by contrast was a renowned horse breeder who made his money in the wine trade. In 1896 he also lost his wife and was elected to the presidency of the Royal Agricultural Society. R. J. Moore-Colyer, "Gilbey, Sir Walter, first baronet (1831–1914)", *Oxford Dictionary of National Biography*, Oxford University Press, 2004 [accessed 4 Feb 2009].

next ten years.[17] In 1899 the Chair was established with the Drapers' funds rather than Gilbey's and William Somerville was inaugurated as the first Drapers' Chair of Agriculture.[18] It is unclear why the Drapers' Company chose to sponsor the chair, and little documentary evidence of the event exists at Cambridge, however their sponsorship is instructive. In this early period, funding was raised for agricultural education and research at Cambridge through infrequent endowments and opportunistic alliances.[19] This network of patronage provided initial support for Biffen and Wood who swiftly harnessed it to Mendelian ends.

The order of departures and successions to the Chair of Agriculture at Cambridge followed a seemingly fixed pattern for the next 30 years. When Somerville left Cambridge in 1902 he moved to a position as Assistant Secretary at the Board of Agriculture. From this high ranking civil service position, Somerville continued to collaborate on agricultural research with his former colleagues. His chair at Cambridge was filled by Thomas Middleton. Middleton, like Somerville before him, was recruited from the Chair of Agriculture at the Durham College of Science, Newcastle. In 1906 Middleton also left Cambridge to take the position as Assistant Secretary at the Board of Agriculture (Somerville had by that time left the civil service and returned to academia and the Sibthorpian Chair of Rural Economy at Oxford). In 1919, Middleton moved up in the civil service to act as Commissioner under the Development and Road Improvement Funds Act to the Development

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[17] By this time the Company, (or to use their full title, The Master and Wardens and Brethren and Sisters of the Guild or Fraternity of the Blessed Mary the Virgin of the Mystery of Drapers of the City of London) which had been at one point the third most powerful in the City of London was largely disassociated with the cloth trade and instead acted as the trustee to various charities and funds entrusted to it by its members. See the Drapers Company's own website <http://www.thedrapers.co.uk/> [accessed 4 Feb. 2009].

[18] On Somerville see J. A. S. Watson, "Somerville, Sir William (1860–1932)", rev. Peter Osborne, *Oxford Dictionary of National Biography*, Oxford University Press, 2004 [accessed 29 Aug 2011].

[19] Drapers' money was not exclusively spent at Cambridge; in 1908/9 Karl Pearson, arch enemy of all things Mendelian, received funding from the company for his work at University College of London. See Kevles 1980; and more specifically on Drapers' Company funding, Gooday 1998. See also Opitz 2011.

Commission. After a stint on the Royal Commission on Indian agriculture, Middleton eventually became the vice chairman of the Development Commission.[20] In 1906 when Middleton left the Cambridge Chair of Agriculture, it was filled by Thomas Wood. In the period 1917-1919 Wood, following a now established pattern, also served with the civil service, again as a Development Commissioner. Links between Cambridge and government were strong and enduring. Indeed, Somerville, Middleton and then Wood, when acting for the civil service, were often responsible for apportioning funding for their former colleagues at Cambridge.

There was a great deal of congruence between the department's science-orientated curriculum and Mendelian thinking. In the department's first academic year, 1899, students were offered courses over two years leading to a Diploma in Agricultural Science.[21] The first year's examination for the Diploma (part I) could also be entered as a special subject towards a BA Degree. The first year "comprising the purely scientific subjects" contained only 24 lectures on agriculture, however the second year focused on "the more advanced and also the more practical or technical side of the subject" with a much heavier emphasis on "agriculture only".[22] Students were also offered the opportunity to "reside a third year for the purpose of more advanced work or research".[23] Most students came to the department from the university seeking a module towards their BA degree, the rest came from local colleges on county council scholarships, very few went on to take a third year. Although more technical subjects were on offer, most students passed through the department taking only the more scientific first year of studies.

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[20] Russell 1944: 565.

[21] This arrangement changed several times and the curriculum was the subject of much debate by the governing boards of the department, however this was roughly the structure in place for the first decade of the department's life.

[22] Engledow 1957: chapter 11, p.1.

[23] *Annual Report on the Distribution of Grants for Agricultural Education and Research in the Year 1900-1901*, 17-19.

In June 1900 a former student of Clare College, W. A. McFarlane-Grieve gifted the department the lease of his farm, Burgoyne's Farm at Impington (about three miles from Cambridge), rent free for 10 years.[24] Farming capital of £1,500 was raised by public subscription. At 142 acres the farm provided considerable space for both research and teaching but experimentation was the chief purpose of the site. The good land on the farm was, apparently, particularly well-suited to crop breeding. Accordingly, a large area was given over to growing wheat and barley strains, for comparative purposes, hybridisation experiments and the maintenance of stocks.[25] The farm was a central part of departmental life; the department had a farm for nearly ten years before it had its own set of buildings at the university. The space provided by the department's farms was an essential prerequisite for doing Mendelian experiments of the day.[26] These factors, the availability of money and land from government and private benefactors, support for experimentation and a scientifically orientated curriculum, made the department a fertile place for Mendelism when it arrived, in the form of William Bateson.

[24] *Agricultural Education, 1890-1916*, section 60-62. See also Wood 1922: 228.
[25] See Mansfield 1957 on the division of labour on the farm.
[26] A point well made by Olby 1991: 499, "[G]enetic research, unlike the traditional study of evolution through descriptive embryology and systemics [*sic*], needed long term funding and adequate land".

1.1.2 Mendelism at the Department

When Bateson, the "chief priest worshipper", brought Mendel's work to Britain he did so from a precarious position at Cambridge University.[27] Without a chair or a departmental home, Bateson (see figure 1.1) was an outsider to university life, or to put it more accurately, an insider on the outside. Although he was the son of a college master he had little luck in making his own way at the university.[28] As a result Bateson came to make alliances with other marginalised groups at the university.[29] From this peripheral position he published, researched and also began giving classes on Mendel and his work. Wood and Biffen (see figures 1.2 and 1.3) were among Bateson's first students at these classes.[30] Wood had recently returned from outreach teaching in Devon while Biffen had been employed as a demonstrator at the department having completed a scholarship at Marshall Ward's Botany Department. Bateson and Biffen seem to have become friends, the older man sent a copy of his 1902 *Mendel's Principles of Heredity, a Defence* to Biffen when it was published.[31] When they returned to the Department of Agriculture's farm, Biffen and Wood began working on showing that Mendelian inheritance applied not just to Mendel's peas but also to other agriculturally important organisms; Biffen started working on inheritance in wheat and Wood, inheritance in sheep.

[27] On Bateson's early career at Cambridge see Olby 1989b and Cock and Forsdyke 2009. For the most comprehensive biography of Bateson available see Harvey 2000. More recently see Radick (forthcoming). The reference to Bateson as chief priest worshipper was made by A. D. Darbishire in 1905, the poem from which it is taken is reproduced in full by Richmond 2001: 55. For more on the homely nature of these early days see Punnett 1950 and Richmond 2006a. For more on Darbishire see, Ankeny 2000.
[28] Bateson's father was the master at St. John's College.
[29] Bateson's early work with the women of Newnham College, as discussed by Richmond 2001, displays his willingness to work with groups outside the academic elite. Bateson was also deeply involved in the world of plant breeding and forged strong early links with the RHS, as discussed by Olby 2000b. For a view of the relationship from the RHS's perspective, see Hurst 1949.
[30] Richmond 2006a.
[31] Biffen to Bateson, 4th June 1902, Bateson Letters Collection, G5n-6.

Figure 1.1 William Bateson (1861-1926). Photograph from Alan Cock's copy of the Bateson materials, now in the Archives of Queen's University, Canada.

In 1902, Wood began lecturing on agricultural chemistry, while Biffen started lecturing on agricultural botany and Mendelism. Rising quickly through the department, Biffen and Wood were soon members of the department’s own Board of

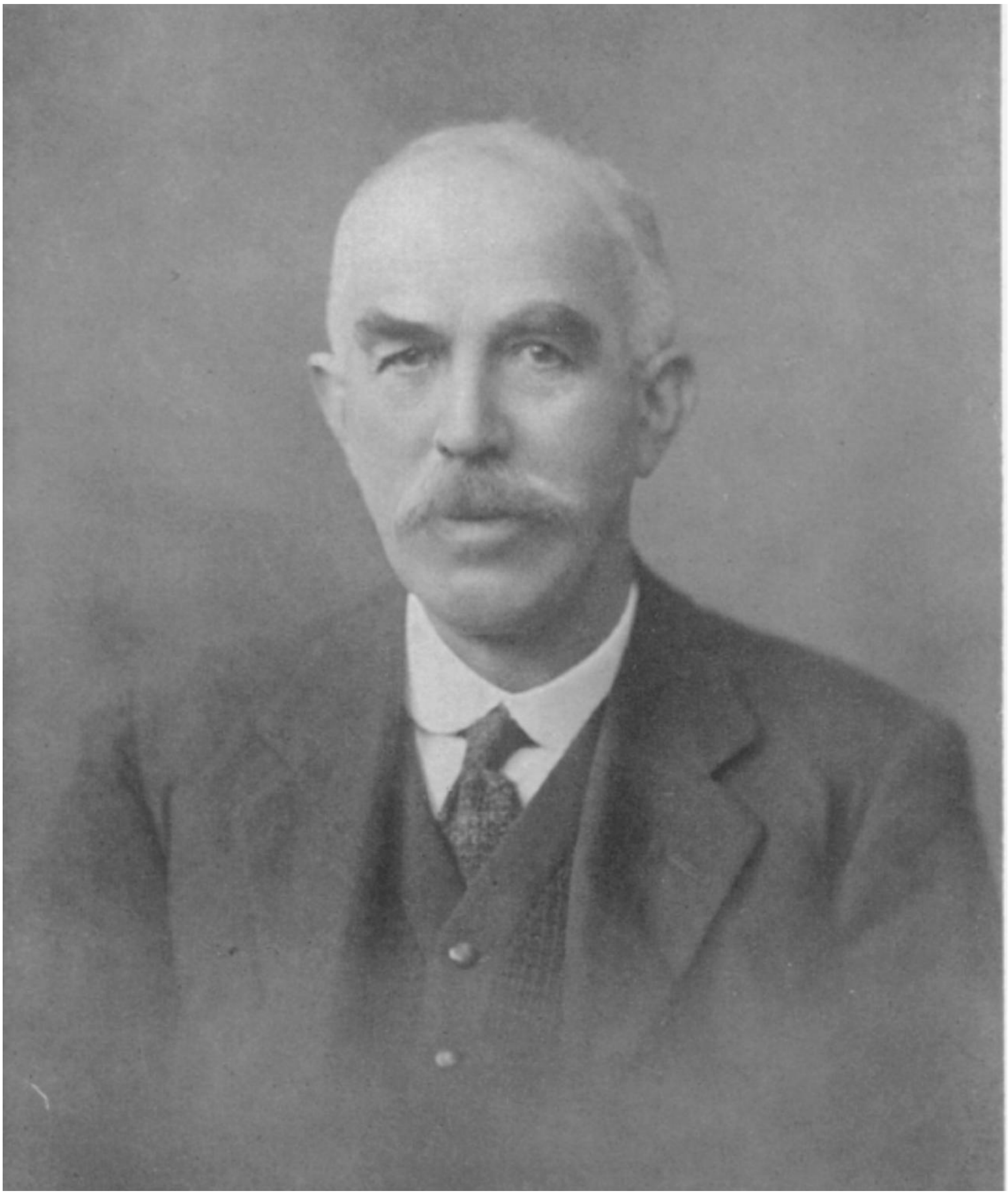

Figure 1.2 Thomas Wood (1869-1929). Reproduced from Wood's obituary in the *Proceedings of the Royal Society*, F. G. H. 1931.

Agriculture, which dealt with the daily running of the department. They also became members of the Board of Forestry (an allied sub-department largely funded by the Tata family in India), various examination committees and the Agricultural Building Syndicate. They were very much part of the department's decision making processes,

Figure 1.3 Rowland Biffen (1874-1949) at matriculation in the late 1890s. Image reproduced from John Innes Archives courtesy of the John Innes Foundation.

right down to the choice of fixtures and fittings for new buildings.[32]

In January of 1905, while still without its own buildings, the department launched an in-house journal, the *Journal of Agricultural Science*. The journal was edited by Biffen, Wood and Thomas Middleton – a Mendelian authority on cotton in his own right – from the department, and A. D. Hall, a Mendelian ally who was then director of the Rothamsted Experimental Station.[33] William Bateson provided further support to the editorial board. The journal was published by the Cambridge University Press and funded by the editors. This was an important forum for transmission of knowledge between workers in the department and those at Rothamsted. The intention was to connect work at the two institutes with that conducted by other workers across the country.[34]

In the period of Wood and Biffen's editorship the journal was a natural home for much Mendelian work originating from the department.[35] While Biffen and the other editors were preparing the journal Biffen wrote to Hurst:

> [T]he study of problems in heredity is going to be of such importance to agriculture that we propose to lay ourselves out to publishing it.[36]

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[32] *Reports of the Board of Agriculture 1909–1922*, University Registry Guard Books, Cambridge University Archives, C. U. R 108.2, section 3a.

[33] See the frontispiece of the first edition for details of the editorial board. Hall and Hurst went on to become friends sharing a long correspondence from 1912 onwards, held in the C. C. Hurst Papers at Cambridge University's University Library, see also the correspondence between Hall and Bateson in the John Innes Archive's Bateson Letters Collection and biographical details on Hall from Russell 1942 and Dale 1956. Middleton brought a collection of cotton varieties and much knowledge back with him from a period working in India at the Baroda College, see Middleton 1896.

[34] Palladino has explicitly linked the editorial style of the journal, which excluded farming articles, to the science prioritising ethos of the department, see Palladino 1993: 309.

[35] Biffen and Wood's work on wheat and sheep took pride of place in the first issue, see Biffen 1905a and Wood 1905.

[36] Biffen to Hurst, 26th October 1904, CC Hurst Papers, Mss add. 7955/5/58.

The journal's tone is perhaps best caught in its glowing reviews of Reginald Punnett's "admirable little primer" *Mendelism*, and Robert Lock's *Recent Progress in the Study of Variation, Heredity and Evolution* both of which ran to several editions and became Mendelian classics.[37] Former and present students at the department, including H. Martin Leake, William Balls and W. O. Backhouse, many of whom were now working in the colonies, contributed to the journal; it seems to have been largely Mendelian work which filled the journal's pages in these early years. The journal was a great success, as Biffen explained to Hurst, his supply of copies of the first January edition "ran short all too soon".[38]

Students who stayed at the department for more than the first year largely went on to have careers around agriculture rather than in it; few of them went on to become farmers. According to Frank Engledow, students fell into two groups; future estate managers, and future agricultural scientists. Engledow optimistically hoped that positions in both roles would be available at home and in the empire, although jobs in Britain remained scarce.[39] Leake, Balls, Backhouse and Robert Lock all left the department to take jobs overseas in administration or research.[40] We can add to this list A. B. Bruce who, after working as a research assistant at the department, and secretary to the Cambridge Board of Agricultural Studies, went on to make a career in the Government Board of Agriculture and later the Indian civil service.[41] As there were not many jobs for agricultural scientists, especially at home, some students, including Engledow, who went on to fill the Drapers' Chair of Agriculture in 1930,

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[37] See "Reviews" 1907: 217-220, Punnett 1905 and Lock 1906. See also Kevles 1980: 450 on Mendelian opportunities to publish.
[38] Biffen to Hurst, 28th May 1905, C. C. Hurst Papers, Mss add. 7955/5/58.
[39] Engledow 1957. For biographical details on Engledow see Bell 1986.
[40] Balls and Backhouse are discussed in more detail in chapters 2 and 5 of this thesis respectively. For more on Leake, see Leake 1911a, 1911b (a paper read on his behalf to the Royal Society by Bateson) and 1921. On Lock, see the biographical note in the fourth edition of *Recent Progress*, Lock 1916.
[41] For Bruce's key Mendelian experiments see Bruce 1910 and 1917.

stayed on to work as departmental demonstrators. W. H. Parker and S. F. Armstrong also took this career path, staying on to work at the department. Finally, a very small fraction of students went on to become agricultural educators for the county councils or the Royal College of Agriculture at Cirencester as Henry Chaplin had hoped.

As the department's students became researchers and administrators they also connected Cambridge Mendelism to the rest of the world. Leake went to India to work with wheat and cotton, Balls went to the Botanical Laboratory of the Khedivial Agricultural Society in Cairo to work with cotton and Robert Lock became Acting Director of the Royal Botanic Gardens at Peredeniya, Ceylon.[42] Many of these men sent back information or varieties to Biffen at Cambridge. When Backhouse went to Argentina to work on wheat production there the department noted his departure as proof of the demand for agricultural scientists and the possibilities of this career path.[43] However the blessing was a mixed one, the students' departure sapped intellectual life from the school, as Biffen told his close friend Lawrence Weaver, many of his best students were "tempted away by offers from the Colonies".[44]

Equally important to the department's expansion as its students, was Biffen's Mendelian work. In 1906 King Edward VII apparently showed a special interest in several of Biffen's exhibits at the Royal Agricultural Society's Derby show.[45] Prompted by this royal interest the then Duke of Devonshire (who the King had been staying with when he visited the show) set up a committee to collect £20,000 to upgrade the department into a new School of Agriculture. The Drapers' society gave another £5,000 to this fund and renewed and increased their endowment of the Chair

[42] Olby 1989: 504.
[43] *Agricultural Education 1909–1922*, section. 53: "Twelfth Annual Report of the Board of Agricultural Studies 12th November 1912".
[44] Biffen to Weaver, 3rd December 1917, NIAB Archives, 1917-1919: p. 9.
[45] Wood 1922: 229.

of Agriculture (which Wood occupied from 1907) for another ten years. When Wood took the Drapers' Chair of Agriculture, Biffen was given a specially created Chair of Agricultural Botany, again with Drapers' Company money.[46]

1.1.3 The Cambridge School of Agriculture

The department became a school, opened officially by the subsequent Duke of Devonshire on 26th April 1910.[47] A formal banquet was held to celebrate the school's opening and the new premises, adjoining the Botany School and the Sedgwick Museum of Geology, were put on display in the day beforehand. Punnett and Wood's work on Mendelian segregation and recombination in rabbits and sheep, along with Biffen's work on "the improvement of wheat", formed the centre piece of the display in the new laboratories.[48] At the opening ceremony, Biffen and Wood sat at the high table and Bateson and his wife Beatrice, Hurst and his wife Rhona, and Punnett, joined what must have been quite a social occasion.[49] The spacious new buildings (see figure 1.4) contained chemical, botanical and physiological laboratories, lecture rooms and private research areas.[50] There was space for around 100 students, however this was soon taken up as student numbers swelled.

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[46] On the Chair of Agricultural Botany see *Professorship of Agricultural Botany 1907-1919*, University Registry Guard Books, Cambridge University Archives, C. U. R. 108.1.
[47] See the public notice in *Nature*, "Opening of the New School of Agriculture, Cambridge" 1910.
[48] "Opening of the New School of Agriculture, Cambridge" 1910: 260.
[49] *Agricultural Education 1909–1922*, section. 13. See Macleod and Tan 2007 and Richmond 2006b on the importance of these sorts of celebrations.
[50] Board of Agriculture, *Annual Report on the Distribution of Grants for Agricultural Education and Research in the years 1910-11 and 1911-12*, Cd. 6025 (1913), 7.

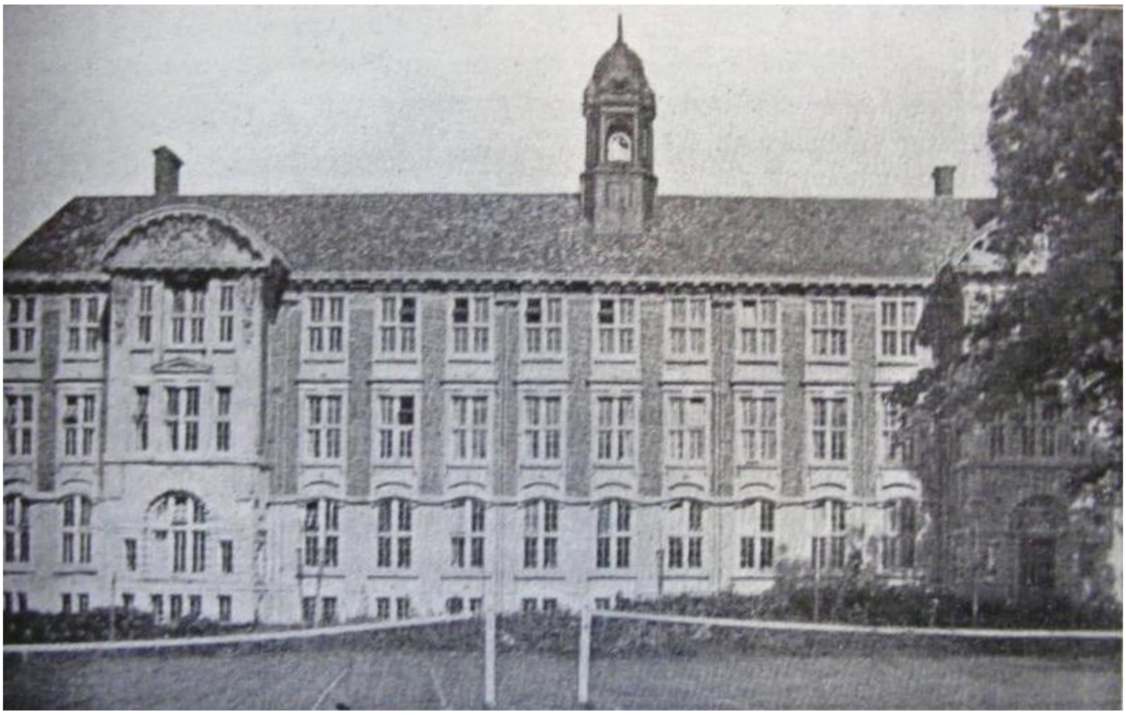

Figure 1.4 The Cambridge School of Agriculture's new buildings, from Wood 1922.

In 1910 the school also began looking for a new farm and the lease was acquired on one closer to the university, and owned by Trinity College, Gravel Hill Farm on Huntingdon Road. At 250 acres, Gravel Hill was considerably larger than Burgoyne's Farm, and the terms of its lease from the college were probably favourable. The land on the farm consisted of well-defined areas of good and poor soils making it ideal for teaching and research purposes, as it offered a range of conditions. The farm also provided space for Reginald Punnett's Mendelian poultry and rabbit breeding experiments. These farms were crucial in another way too; large parties of farmers from local and international clubs and associations regularly came to visit the farm and see Biffen's work.[51]

[51] In 1909 the Cambridge Board of Agriculture reported that there were some 1000 visitors to the farm including a party of 50 from the Royal Agricultural Society of Hungary, *Agricultural Education 1909–1922*, section 5 "Tenth Annual Report of the Board of Agricultural Studies 11th May 1909". See also visits by farmers recorded by the Government Board of Agriculture, *Annual Report on the Distribution of Grants for Agricultural Education and Research for 1907-08*, Cd. 4802, (1909), 29.

The school also provided a focal point for fund raising. Wood and Biffen both received a healthy salary from the department and this increased as they were promoted, in 1909 Wood was on £800 per annum and Biffen £200 which was supplemented by fellowships from Catherine College and Gonville and Caius. From 1909 onwards Biffen also received a £200 yearly research grant from the Board of Agriculture and this was paid through the university.[52] There were undoubtedly many more such spill-over benefits for Mendelians looking for funding at the new school. The scholarships which the department had attracted from county councils, the Board of Agriculture and other bodies increased as school's student numbers grew. This income was supplemented by various small grants and bequests, often hunted out by Biffen and Wood personally. Indeed Biffen became so embedded in this world that in 1922 he complained to Hurst, "I'm thankful to say that the one board I'm not on in this blessed University of Cambridge is the one dealing with applications for admission [to] research students".[53] Yet despite his feigned reticence, he and Wood came to rule over a vast and sprawling Mendelian empire within the Cambridge School of Agriculture, so much so that the *Daily News* could proclaim, "The Agricultural College [*sic*] at Cambridge is doing probably the most extensive work in the department of Mendelism in the whole world".[54]

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[52] For details of the school's finances see the yearly financial reports submitted by the Board of Agriculture to the University, *Agricultural Education 1909–1922*.

[53] Biffen to Hurst, 26th April 1922, C. C. Hurst Papers, Mss add. 7955/14/14. Hurst was himself applying and hoped his friend Biffen might get him in.

[54] "New Wheats: Professor Biffen on his Experiments at Cambridge", 15th July [1912] in *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University*, Rowland Biffen Papers. Biffen's importance in the department was physically represented after his death in the naming of the Biffen Lecture Theatre which is still used to this day.

1.2 Other Mendelian Organisations

In this early expansive period Biffen and Wood fostered links with many external organisations. Although the idea of applying Mendelism to agriculture was new, the idea of aiding agriculture through science was not. The Royal Agricultural Society had been preaching "Practice with Science" since its foundation in 1838. Unsurprisingly, this older institution was generally enthusiastic about its newer peers. Biffen served as Botanist to the Royal Agricultural Society, writing yearly reports from 1910 until 1940. This obviously pleased Biffen's colleagues who collectively noted in the school's annual meeting:

> This appointment is a gratifying recognition by practical men of the value of the school, and affording as it does many opportunities of becoming acquainted with the needs and difficulties of farmers, is of considerable value to the staff.[55]

Along with the Royal Agricultural Society, the Royal Horticultural Society also lent great support to the Mendelian cause, most famously and directly to Bateson, but also to Biffen and his wife Mary who won awards from the society for her Mendelian sweet pea varieties.[56]

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[55] *Agricultural Education 1909–1922*, section 15, "Eleventh Annual Report of the Board of Agricultural Studies 10th May 1910". The first of Biffen's reports as botanist appeared in 1910, followed by yearly reports running up to 1940, Biffen 1910. In the first report Biffen explained that he had received 360 enquiries in the year, "the majority of these were concerned with the purity and germinating capacity of agricultural seeds", Biffen 1910: 311. Biffen's concern with seed purity ran throughout his career, it also featured in his published studies on the quality of agricultural seed in 1914 and 1916, see Biffen 1914 and Biffen 1916b.

[56] On Mary Biffen (nee Hemus) see Judith M. Taylor and Simon Wilkinson [2008], "Miss Evelyn Hemus", *Horthistoria,* <http://horthistoria.com/?p=217> [accessed 6th June 2011] and

1.2.1 The Home Grown Wheat Committee

One organisation in particular formed a close knit relationship to the Cambridge School of Agriculture. The Home Grown Wheat Committee (HGWC) was established in 1901 by the National Association of British and Irish Millers (NABIM).[57] It was run on a grant of £50 per year from the Board of Agriculture and £100 a year from NABIM. Much of the committee's work was undertaken free of charge by its members and supporters.[58] Initially the committee was mainly composed of millers who farmed a little or had personal connections to agriculture.[59] However, the committee was enlarged to include several academics and plant breeders including Biffen, Wood and their co-editor at the *Journal of Agricultural Science*, A. D. Hall. They were joined by John Percival and E. S. Beaven. A. E. Humphries – the committee's longstanding secretary and a former student of the Cambridge Department of Agriculture – formed a particularly close relationship with Biffen which ran from around 1903 through several collaborations and on into the 1930s.[60] The two men published several papers together, working closely on breeding new varieties of wheat and testing their performance.[61]

Collaboration with Humphries paid several dividends for Biffen. Humphries appeared before government on several occasions to argue that Biffen's research and

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Judith M. Taylor, Simon Wilkinson and Keith Hammett [2009], "Miss Hilda Hemus", *Horthistoria*, <http://horthistoria.com/?p=219> [accessed 6th June 2011].

[57] I follow Biffen here in referring to the Home Grown Wheat Committee. Wood used the grammatically correct Home-grown Wheat Committee and Humphries, Home-Grown Wheat Committee. At some point in the 1930s the committee's papers were transferred to NIAB. Unfortunately these papers have now disappeared. For more on the committee's work in the 1930s see Greer 1949.

[58] Board of Agriculture, *Report of the Departmental Committee Appointed to Inquire into the Subject of Agricultural Education in England and Wales*, Cd. 4207 (1908), 494-5.

[59] Biffen and Engledow 1926: 91.

[60] See Hall 1905; "Home-Grown Wheat Committee", *The Times* (28th August 1911), 4a and Biffen and Engledow 1926: 91-93.

[61] See for example Biffen and Humphries 1907 and Humphries 1931.

Mendelian breeding in general should be given more support.[62] Humphries even requested that he might give evidence on the first occasion, before the Board of Agriculture's Departmental Commission on Agricultural Education, especially to plead the case that Biffen might be relieved of his teaching duties in order to devote more time to research.[63] The Home Grown Wheat Committee wholeheartedly endorsed Mendelism, lending their facilities and supporting Biffen in print and at government. Biffen fondly described his relationship with Humphries in a letter to the Australian plant breeder William Farrer:

> With regard to the milling side of the situation I am very fortunately situated. Humphries to whose words you refer has an experimental milling plant, a trained baker and a most extraordinary 'eye' for quality man ever had–all these I draw on fully.[64]

In roughly 1905 the committee began to advocate the All-English Solution to the wheat growing sector's problems. At the heart of this strategy was the belief that raising the quantity of home grown wheat used for bread making would act as a boon to both inland millers and wheat farmers. In order to revive the fortunes of inland millers and wheat farmers the quality of English wheat varieties would have to be raised through breeding.[65] The committee undertook research into this problem in several areas. Between 1901 and 1906 this meant studies of the performance of

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[62] Most notably at the 1923 Linlithgow Commission which is considered in more detail in chapters 3 and 4 of this thesis.
[63] *Report of the Departmental Committee Appointed to Inquire into the Subject of Agricultural Education in England and Wales,* 496-510.
[64] Biffen to Farrer, 9th January 1905. Reprinted in Sutherland 2001: 176.
[65] Ministry of Agriculture and Fisheries. *Departmental Committee on Distribution and Prices of Agricultural Produce. Interim Report on Cereals, Flour and Bread,* Cmd. 1971 (1923), 74-75.

different wheats in different locations around Britain.[66] Perhaps the most famous of this early work was conducted over twenty-one years to test the effects of British climatic conditions on the quality of a Canadian wheat variety called Red Fife.[67] The committee published reports and leaflets on this work for distribution to millers and farmers. Several committee members also published individual accounts of their work in the *Journal of the Royal Agricultural Society* and the *Journal of Agricultural Science*.[68]

Wood and Biffen worked in particularly close synergy on these projects. As Biffen was developing higher quality wheats, Wood was also working with the HGWC, investigating the chemical analysis of quality in wheat.[69] Biffen and the HGWC's fortunes became so entwined that in 1911, when the *Times* reported to its readers on the committee's work the newspaper recorded the committee's view, "That they would have to depend upon the ability of Professor Biffen and other expert plant breeders to evolve new varieties". The article concluded by noting that, "the committee speak in hopeful terms of the final result of [Biffen's] labours".[70]

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[66] Biffen 1908: 86-87.
[67] This report is attached as an appendix to the second issue of the *Journal of the National Institute of Agricultural Botany*. See Humphries and Hutchinson 1924.
[68] See Hall 1905, Biffen 1908b, Biffen and Engledow 1907 and Wood 1907b and 1907c.
[69] See the small controversy that Wood's claims to priority on a chemical test of strength caused in the pages of *Nature*, Wood 1907a, Armstrong 1907 and Banks 1907.
[70] "Home-Grown Wheat Committee", *The Times* (28th Aug. 1911), 4a.

1.2.2 The John Innes Horticultural Research Institution

The John Innes Horticultural Research Institution (the John Innes, or JI, to its staff and friends) opened its doors in Merton, Surrey in 1910. The institute was an important centre for Mendelian research, the first in Britain outside of the university context.[71] However, the John Innes had strong links with Cambridge and in particular with the School of Agriculture. Biffen was a regular presence at Merton along with several other Cambridge Mendelians. Indeed, there was a near continuous trade of students, ideas and seeds between the institutes.

The JI was dependent on private funds for its foundation and maintenance. These came from a bequest by John Innes, a wealthy landowner and developer in the London area. Innes gifted his home at Merton, and the surrounding land, which together comprised £300,000 worth of his estate, for the foundation of a school for horticultural instruction. William Bateson was chosen to be the institute's first director by a committee made up of Biffen, J. B. Farmer (then Professor of Botany at Imperial College and pursuing Mendelian work himself) and Adam Sedgwick, an old Cambridge friend. After receiving some thirty applications the committee chose Bateson for the role without a formal interview. There was some considerable wrangling over how the bequest was to be spent but it was decided by compromise that the institute should pursue mainly research rather than education.[72] Bateson proceeded to negotiate very favourable terms for his directorship which allowed him full control over the institute's newly formulated research programme, considerable

[71] Olby 1989a: 497.

[72] For details of this negotiation see Olby 1989a. Although this was a horticultural institute, as market gardens expanded to serve urban populations the distinction between plants of the field (agri) and of the garden (horti) became blurred and garden vegetables were increasingly grown on a field scale. The scale of market garden operations was extended partly as a result of new techniques in glass manufacturing developed towards the end of the nineteenth century.

space in which to conduct his own work, and a salary of £1000.[73] He remained as director at the JI and "dominated the programme" until his death in 1926.[74]

Bateson, with Biffen's help, skilfully translated his research program from Cambridge to the JI and the two institutes operated an effective open-door policy.[75] Biffen sat on the institute's governing body until the 1920s along with several other friendly Mendelians such as Sir Daniel Morris who was fond of praising Biffen's work at the meetings of the Royal Horticultural Society.[76] Thomas Wood was also frequently at Merton, either on one of the institute's committees or its lunchtime seminars. Furthermore, Bateson staffed the institute with postgraduates – rather than established academics – and these mainly came from Cambridge University. Muriel Wheldale, William Backhouse, M. S. Pease and J. W. Lesley all transferred to the John Innes directly from Cambridge.[77] Punnett, who was given a Chair of Genetics at Cambridge in 1912, after Bateson turned it down and recommended his friend, often visited to conduct research at the JI. In later years visits from eminent plant breeders such as Nikolai Vavilov and Otto Frankel often included a stint at both Cambridge and the JI.[78] Biffen and Bateson also shared varieties and ideas together. In 1914 Bateson sent Biffen samples to grow on at the Cambridge University farm and Biffen in turn told Bateson about interesting cases of wheat inheritance he thought was analogous to Bateson's work on peas.[79] Despite the distance between them the JI and the School of Agriculture liberally shared space, leadership and staff.

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[73] This was very high by the day's standards.
[74] Olby 1989a: 507.
[75] Olby 1989a: 508.
[76] See the minutes of the governing council and Reports of the Director at the John Innes Archive for the day to day attendance of Biffen and other Mendelians. Biffen barely missed a meeting until well into the 1920s. See also Morris 1907: 319.
[77] On Backhouse's time in Argentina see chapter 5 of this thesis. On Backhouse's work on fruit at the JI see Olby 1989a: 509.
[78] See Evans 1999 on Frankel and Harland 1954 on Vavilov's visits at the John Innes.
[79] See Biffen to Bateson, 14th March 1914, Bateson Letters Collection, 3269.

Bateson, through his correspondence, acted as a fulcrum for the Mendelian community. From Merton he kept in contact with several former Cambridge Mendelians in far flung corners of the globe, including Backhouse in Argentina, Balls in Egypt and Lock and Leake in India. This network of letters also included Wilhelm Johannsen and Herman Nilsson-Ehle, perhaps the two most famous European botanists of the period after Hugo de Vries. Even during the First World War this steady stream of correspondence continued from Bateson to other Mendelians in Europe.[80] Much of this information would undoubtedly have found its way back to Cambridge, passed from Bateson to Biffen and Wood. In order to further strengthen communication amongst Mendelians, in 1911 Bateson and Punnett established the *Journal of Genetics* – also published by Cambridge University Press. After the War the same desire to strengthen communication, led Bateson and Punnett to establish the Genetical Society, in which Biffen was an executive committee member.[81] Biffen, Engledow, Hall, Hurst, Punnett and Bruce all played an active role in the society and, of course, Biffen's wheat varieties were once again on display at the fifth of the society's regular meetings.[82]

The display of Biffen's wheat varieties at the John Innes points to one more feature of the institutional environment at the JI; much of the early work conducted there was aimed at creating new varieties. Over the years Bateson passed his friends the Suttons, at the famous nursery of the same name, several new varieties. Furthermore, he was consulted by the Board of Agriculture on plans for seed production before the War and agricultural reconstruction afterwards. Despite Bateson's reputation as an ardent supporter of pure science, the John Innes was

[80] See in particular the series of letters to Ostenfeld and Nilsson-Ehle in the Bateson Letters Collection, the whole catalogue is searchable online <http://www.jic.ac.uk/corporate/search/batesonletters/default.asp> [accessed 1st August 2011].
[81] On the foundation of the Genetical Society in 1919 see Crew 1969.
[82] See *Committee Meetings of the Genetical Society*, Genetics Society Archives, GS/3.

deeply connected to the practical world.[83] This was, at least in part, because the John Innes was also strongly influenced by Biffen. When Bateson died suddenly, Biffen was the first candidate for his directorship. Biffen declined, and the post was taken by A. D. Hall, however something of Biffen lives on at the institute, in the form of a statue now standing in the current day John Innes Centre, relocated to Norwich, guarding the entrance to the Biffen Building (see figure 1.5).[84]

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[83] On the early productive aspects of the work at the John Innes see "The John Innes Horticultural Institution" 1936: 1061 and the JI's own historical timeline, <http://www.jic.ac.uk/centenary/timeline/1910s.html> [accessed 20th Feb. 2011].

[84] On the importance of statues see MacLeod 2007 and MacLeod and Tan 2007.

Figure 1.5 Statue of Biffen by Edith Simon, installed at the Plant Breeding Institute, Cambridge 25th October 1981. Now located at the modern-day John Innes Centre's Biffen Laboratory. Image by author, reproduced courtesy of the John Innes Foundation.

1.2.3 The Development Commission

> In 1909, however, the Development Fund was set up and this gave the opportunity that Middleton, A. D. Hall and T. B. Wood had long wanted of working out a proper system of agricultural education, advisory work and research to apply to the whole of the United Kingdom. ... Between Middleton at the Board of Agriculture and Hall at the Development Commission new schemes, national in scope, were worked out, and for the first time science could be systematically applied to the problems of agriculture in all parts of the United Kingdom.[85]

In 1911 a new organisation began operations. It was neither an educational nor a research institute. The Development Commission, staffed by eight commissioners was set up to advise the treasury on the allocation of the Development Fund. This was a £1million fund (although it grew significantly over the years) established by David Lloyd George in order to stimulate development in rural Britain. Lloyd George hoped this could be achieved through public works projects including harbour restoration, canal dredging and the creation of co-operatives for small farmers. Part of the fund was also earmarked for agricultural research and education. The commissioners met roughly every month to consider applications to the fund which were submitted to them through government departments such as the Board of Agriculture. After the first year of operation this arrangement was altered so that

[85] Russell 1944: 563.

departments made block applications rather than passing on each application to the commission individually.[86]

The eight initial members of the commission were Richard Frederick Cavendish, Sir Francis John Stephens Hopwood, Sir Sainthill Eardley-Wilmot, Henry Jones Davies, Michael Andrew Ennis, William Stowell Haldane, Alfred Daniel Hall (who we have already met) and the well-known Fabian Society member, Sidney Webb. Hall was a member of the commission until 1917 when he was replaced by Wood. Wood held his position for two years and was succeeded by Middleton in 1919. Hall went on to become the permanent secretary for the Board of Agriculture before moving to the John Innes to fill Bateson's job. Connections to Cambridge were further strengthened through Bateson, Biffen and Punnett's on-and-off work between 1910 and 1917 for the Development Commission's Advisory Committee on Agricultural Science.[87] This body was established to provide advice on the allocation of small grants for special investigations in agricultural science. All in all, Cambridge Mendelians certainly had a great deal of influence on the Development Commission. These public connections were probably strengthened behind the closed doors of Hall's London club.[88]

At the Cambridge School of Agriculture, the Development Commission bankrolled a huge expansion of activity in the years after 1910. Although other organisations like the animal nutrition research group at Leeds University benefited, the lion's share of the fund's money for agricultural research and education went to

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[86] Development Commission, *First Report of the Proceedings of the Development Commissioners for the Periods from 12th May, 1910, to 31st March, 1911,* Cd. 199 (1911).

[87] For a feel of the composition of the members of the Commission and the year to year operations see the yearly reports published by the Development Commission from 1911 onwards.

[88] Obviously, there is no evidence of what happened in these private contexts but I would agree with Olby that it is more than likely these relationships often blurred the line between personal and professional responsibilities, see Olby 1990 and 1991.

the Fens.[89] Two of the largest awards made by the commission, before and after the War, were for the foundation of a Plant Breeding Institute and a National Institute of Agricultural Botany at Cambridge.

1.3 New Funding and New Institutes

1.3.1 The Plant Breeding Institute

In 1911 the Development Commissioners, on the advice of the Agricultural Science Advisory Committee made £3,285 available to the School of Agriculture at Cambridge as an interim grant towards the creation of two new institutes.[90] In 1912 the Plant Breeding Institute (PBI) and the Animal Nutrition Institute (ANI) were both opened with a further grant from the development fund of £18,000 towards capital expenses.[91] The PBI shared some of the fields at Gravel Hill Farm and made use of the School of Agriculture's laboratories, figure 1.6 shows the institute's field station on the University's farm. Two hundred and fifty acres at Howe House Farm located next door to Gravel Hill Farm was also leased from the same owners, Trinity College, for the PBI's experiments.[92] Private gifts and Board of Agriculture money paid for some staff salaries and collaboration with the Home Grown Wheat Committee. Biffen became the first director of the institute, a position which he held until his retirement in 1936.

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[89] See Olby 1991 and Raina 1997 on the possible impropriety of this.
[90] *Annual Report on the Distribution of Grants for Agricultural Education and Research in the years 1910-11 and 1911-12*, vii.
[91] The history of the Animal Nutrition Institute remains to be written.
[92] Wood 1922: 299.

Figure 1.6 The Plant Breeding Institute Field Station, Cage Field, University Farm, c. 1952. Trumpington Local History Group, "Notes on the History of the Plant Breeding Institute, Trumpington".[93]

Biffen employed a small staff from within Cambridge University, George Udny-Yule was the statistician, H. V. Sherringham the farm manager, S. F. Armstrong and Wilfred Parker, who both came from the school were assistants.[94] Miss A. M. Taylor was employed as the institute's pathologist and M. Buck as an analyst.[95] They were joined by two research students, also from the school, J. W. Lesley and Frank Engledow.[96] Lesley left for the John Innes in 1914 and staff numbers dwindled during the First World War. In 1917 Biffen went to work for the Food Production Department as part of the war-effort and work on the farm practically stopped.[97] He told the Food Controller there, Lawrence Weaver, that he

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[93] Located online here <http://www.trumpingtonlocalhistorygroup.org/subjects_PBIhistory.html> [accessed 21 August].

[94] See Yule's interesting thoughts on reconciling Biometry and Mendelism, something he held in common with Balls, Yule 1907.

[95] See Board of Agriculture, *Annual Report on the Distribution of Grants for Agricultural Education and Research in the years 1913-14 and 1914-15*, Cd. 7450 (1914), 56 for the first list of staff at the institute.

[96] For details of J. W. Lesley's scholarship at the JI see *Annual Report on the Distribution of Grants for Agricultural Education and Research in the years 1913-14 and 1914-15*, 114.

[97] *Agricultural Education 1909–1922*, section 112: "18th Report of the Board of Agricultural Studies 20th Nov. 1917".

had to ask his wife and her servants and staff to take on what little work was being done.[98] After the War, however, the annual wage bill at the institute soared from around £700 to nearly £3,000. The expansion continued into the early 1920s when two new research stations were attached to the institute, the Horticultural Research Station (1922) and the Potato Virus Station (1926) and two more members of staff hired; A. E. Watkins, who later described the chromosome number in wheat, and Herbert Hunter, Biffen's eventual successor as director.[99]

Of course, there was also plenty of publicly funded agricultural research in America at this time, perhaps even more than in Britain, but there was undoubtedly a different configuration of purpose in the two countries' public research stations. For a measure of this contrast, compare American biometrician Raymond Pearl writing in 1906 to his University College London based mentor, Karl Pearson, on conditions at his new institutional home, the Maine Experimental Station, "I am under no restrictions as to giving the work a practical turn. On the contrary I am expected to work exactly as if I were taking up the study of heredity for my own purely scientific ends", with Biffen's reports to Whitehall on his own publicly funded research.[100] In 1916 he informed the Board of Agriculture in his report for the year, "A great deal of the work is now of a routine nature, and the results, consisting mainly of records of yields of new varieties, which may or may not be put on the market later, are of too little general interest to publish".[101] Research was very much directed towards "economic investigations" at the PBI and in particular the production of new varieties.

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[98] Biffen to Weaver, 3rd December 1917, p. 3, NIAB Archives, Relations with the PBI folder.
[99] For more on the changes that occurred post-Biffen at the PBI see Bell 1976: 13-14.
[100] Raymond Pearl to Karl Pearson, 9th December 1906, Pearson Papers, University College London, Pearl file, quoted in Kevles 1980: 452. On the subtle configurations of research at American institutes see Kimmelman 2006.
[101] Board of Agriculture, *Annual Report on the Distribution of Grants for Agricultural Education and Research in the years 1914-15 and 1915-16,* Cd. 8066 (1916), 59.

The majority of the work conducted by the institute in its first years involved cereals and this was under Biffen's direction. A significant part of the day to day work of the institute was the multiplication of stocks of seeds to distribute to farmers. This activity continued multiplication efforts that had begun on the School's Gravel Hill Farm.[102] Biffen even hired a boy to run a small seed cleaning plant. Even so, Biffen could not grow up enough seed to enter the market himself. However, if he passed seeds to one or other dealer for multiplication and distribution people might ask why any particular seed dealer should benefit from publicly funded research? In response to this problem, two years after the PBI was established, Biffen began exploring new ways of distributing the seeds he was producing at the PBI's Howe House Farm.

1.3.2 The British Seed Corn Association

The British Seed Corn Association (BSCA) was founded in 1914 by Biffen, William Hasler, a seed dealer from Dunmow, Essex, and Biffen's sometime collaborator at the school and the Home Grown Wheat Committee, E. S. Beaven.[103] According to a prospectus produced in 1914, the association was to be composed of shareholders, and several thousand shares were to be made available. In 1915, ten shares each were sent to Hasler, Biffen and Wood to formally establish the association. The association, they hoped, would head a network of licensed seed dealers and growers who became agents upon payment of a fee and possibly also a percentage of their sales. The association and an allied botanist, probably intended to be Beaven, would

[102] This land was also used for multiplication of seeds for sale, see Beaven 1947: 239-41.
[103] A draft prospectus for the Cooperative Seed Corn Association Limited is held at the Museum of Rural Life in Reading along with various correspondences between Hasler, Beaven, Biffen and Wood see the Papers of the Guinness Barely Research Station, Museum of English Rural Life.

certify seed from producers of new varieties, distributing them to dealers and growers (who would grow the stock on).

The BSCA was one facet of an on-going attempt started by Beaven as early as 1909 to provide some kind of voluntary regulation in the seed trade. Indeed, the association was based at the offices of Hasler's jointly owned seed dealership, Hasler and Clapham. However, Biffen also began to distribute quantities of the new varieties he was producing at the PBI through the BSCA. In 1916 he gave Hasler 200 quarters of seed to distribute. With several agents ready everything seemed to be in place for the association to go to work distributing certified stocks of Biffen's varieties. However, as the PBI's activity diminished during 1917, the BSCA disappeared.[104] Its function lived on though and was embodied by a new institution established after the War with the help of Beaven, Hasler, Biffen and Hall.

In 1918, Hall wrote to Bateson, asking him to, "draw up an ideal programme of research and development, representing a maximum of possible attainment in ten years, and, thereafter, to translate it, so far as practicable, into terms of men and money".[105] Bateson's reply was not a simple one. On one hand, he was adamant that the John Innes should remain free from government funding, "purely scientific" work became "trivial and unfruitful" in Bateson's view, when economic concerns were foremost. He believed that no matter how hard one safeguarded against it, public funds would drag research in an economic direction. On the other hand, he saw no reason why a financially free John Innes could not interact with a publicly funded system of institutes. Indeed, it turned out he wanted rather a lot from Hall.

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[104] Hasler to Beaven, 23rd August 1916, Papers of the Guinness Barely Research Station, TR GUI: 132.

[105] Hall to Bateson, 25th January 1918, Bateson Letters Collection, 2147. See Dale 1956: 72 on the close personal relationships between Hall and Biffen and Wood and Hall 1913 for an account of his and Wood's pilgrimage of British farming reprinted from a series they had published in *The Times*. For more on Hall's post-war plans for reconstruction see A. D. Hall 1916 and 1936.

Bateson gave two general recommendations, as well as some thoughts on specific research lines. First of all Bateson felt that Mendelism needed a “liaison officer”, to work with the “practical breeders, seedsmen etc”. He even had a man in mind for the job; W. O. Backhouse. Secondly, Bateson told Hall that the biggest aid government could provide to Mendelian plant breeding efforts would be to provide resources for the commercial production of new seeds. In a much fought over passage of his reply to Hall he claimed, “Hitherto our efforts to get this done have been failures. … In work of this kind we are in the hands of the grower and a bad result can always be ascribed to an inherent peculiarity of the material”.[106] Bateson went on to indicate that, “Perhaps the Cambridge Svarlof [*sic*] may supply this want”.[107]

1.3.3 The National Institute of Agricultural Botany

The National Institute of Agricultural Botany, the Cambridge Svalöf which Bateson had referred to, took over much of the intended role of the BSCA.[108] When it was established in 1919, Hasler, Biffen and Beaven all took roles in NIAB; Hasler and Beaven as members of the council. The driving force behind the creation of the institute was Lawrence Weaver who had worked with Biffen at the Board of Agriculture’s Food Production Department during the First World War, see figure 1.7. This role impressed upon him the need for institutionalised security and regulation in the nation’s food supplies. The head of the Board at the time, Rowland Prothero (Lord Ernle) had also been making suggestions about the need for such an

[106] Hall to Bateson, 25th January 1918, Bateson Letters Collection, 2147. This passage is a re-write of an earlier version which is crossed out.
[107] Hall to Bateson, 25th January 1918, Bateson Letters Collection, 2147.
[108] For an alternative institutional history of NIAB see Wellington and Silvey 1997 or Palladino 1990.

institute.[109] In 1916-7 Weaver helped drawing up the Temporary Testing of Seeds Order of 1917 which was enforced by the Board of Agriculture and the seed testing station the order established in Streatham, London.[110] The order, which aimed to regulate sales of seeds, stated that all seeds should be certified for germination level and purity, both in terms of identity and freedom from weeds or disease.[111]

From 1917 Weaver began to canvass both his friends and members of the agricultural establishment, for ideas on the form the new institute should take and for donations. When Weaver began planning the institute's foundation he turned to Biffen for advice. Biffen was rather excited about the opportunity, he suggested the PBI should be remodelled to incorporate the new institute and wrote to Engledow suggesting he could be the director.[112] Weaver maintained the need for physical separation and a freestanding NIAB but the two men continued collaborating despite their differences on this point. Biffen even helped Weaver with the fundraising and seemed to rather enjoy himself. He wrote to Weaver in 1918 about one possible benefactor, "my Welshman is nibbling well... it's an exciting sport this".[113] Over the next two years Weaver drew up a constitution for the institute to delineate its role and raised around £40,000 pounds in donations. Half of this money came from his personal contacts with Sir Robert McAlpine, the construction magnate fondly known as "Concrete Bob", and Lord Elveden of the Guinness family.[114] The other

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[109] See *Memoranda on the Establishment of a National Institute of Agricultural Botany,* 1917, NIAB Archives, folder marked 1917.
[110] See: Ministry of Food, *Statutory Rules and Orders no. 55. Defence of the Realm*: *The Testing of Seeds Order 1917,* 000 (1918), 1-6.
[111] See the similarities of NIAB's intended role to that performed by experimental stations in America who issued blue ribbons to "pure" tested seed, Kevles and Bugos 1992: 86.
[112] See Biffen to Weaver, 3rd December 1917, NIAB Archives, 1917-1919 and Biffen to Engledow, 8th March 1918, Rowland Biffen Papers.
[113] Biffen to Weaver, 11th October 1918, NIAB Archives, 1917-1919.
[114] For biographical details see Iain F. Russell, "McAlpine family (*per. c.*1870–1967)", *Oxford Dictionary of National Biography,* Oxford University Press, 2004; online edn, Oct 2008 [accessed 3 Aug 2011] and R. G. Wilson, "Guinness, Rupert Edward Cecil Lee, second earl of Iveagh (1874–1967)", *Oxford Dictionary of National Biography,* Oxford

half was raised by donations from various bodies representing the seed and milling trades and other agricultural organisations, including the National Farmers Union, and various regional farmers' associations.[115] This sum was then matched by the Development Commission who provided £20,000 in loans and £20,000 in grants.

The institute's business was executed by a council. There was considerable wrangling over the composition of the council, largely because the seed and milling trades felt underrepresented. In their opinion it was unfair for the two universities (Oxford and Cambridge) who had not contributed any money to have an equal representation. Furthermore, the composition of the executive committee which sat above the council also gave equal representation to the seed and milling trades, the universities and the Government.[116] In response to complaints raised by seed firm owner Martin Sutton, the representation of the seeds trade in the council was increased by one. Counter objections were, however, made by S. W. Farmer, a potential contributor who believed the institute risked becoming dominated by the seed trade's interests.[117] Weaver personally reassured Farmer that this would not be the case. It seems in the end, neither seedsmen nor academics had quite the control of the council they might have hoped for.[118] Humphries and Hasler were the obvious choice for the places that the trades had managed to secure. Hasler was the

University Press, 2004; online edn, May 2006 [accessed 3 Aug 2011]. Curiously neither man's involvement with NIAB is mentioned.

[115] See for example the Board of Agriculture's report of Rowland Prothero, Hall, Biffen, Weaver and Humphries at an evening event with the National Association of British and Irish Millers who subsequently donated £5000. "National Institute of Agricultural Botany" 1918: 383.

[116] Sutton to Weaver, 16th July 1918, NIAB Archives, NIAB Council notes 1918-1927, box No.26.

[117] See correspondence between Weaver, Prothero and Farmer, NIAB Archives, folder marked Mr. Prothero's correspondence with Mr. Farmer. For background on Farmer see Hall 1936: 386.

[118] Palladino claims the institute was established to annul the PBI's position in the agricultural seeds market, something which the seed companies of the day would no doubt have enjoyed. However the institute's archives reveal the seed companies had much less power than they might have wished. See Palladino 1990. See also clashes with the seed trade over the use of the word national in NIAB's trademark, folder marked Trademark Dispute.

representative for the seed trade and Humphries represented the millers. The council was headed by a director, William H. Parker, and honorary vice presidents, initially three although this was expanded to five in subsequent years before shrinking and settling to four in 1924. The rest of the council consisted of four representatives from the trade associations which had contributed donations, three from the Ministry of Agriculture, one from the NFU, one from the Royal Agricultural Society and three from the universities of Cambridge and Oxford; twelve in total including NIAB representatives.[119]

When the institute was established Biffen was elected honorary vice president. Although he relinquished that role after one year, he remained the institute's chief scientific advisor until 1936. He also remained on the institute's working committees until 1929. Wood had a place in the institute too, as the Cambridge representative on the council, a position which he held in conjunction with a position on the executive committee from 1919-1924 and a working committee position. Wood also convinced the Dean of Cambridge University to allow the university's representatives time off to work for the institute, telling him, "its prime object is the distribution of the seed of new varieties of farm crops produced by professor Biffen".[120] Biffen's student Engledow held the same roles until 1942. There were also many other Mendelians in powerful positions in the institute, R. N. Salaman was a Vice President along with Biffen in the institute's first year and he remained in the role until 1944. He was chairman of the potato advisory committee until 1952. F. W. Keeble was the Oxford representative and member of the council until 1940 and worked for much of that time on the crop

[119] The Board of Agriculture became a Ministry in 1920 but it also went through spells as the Board of Agriculture and Fisheries and the Ministry of Agriculture Farming and Fisheries. Today it is the Department for Environment Food and Rural Affairs (DEFRA).
[120] The letter is reproduced here, *Reports of the Board of Agriculture 1909–1922*, section 128. Wood to the Vice Chancellor, 4th January 1919.

Figure 1.7 Sir Lawrence Weaver (1876-1930) by Walter Stoneman. National Portrait Gallery Number x28057.

improvement and potato committees.

The institute's work was divided between several committees each responsible for reporting back to the council on their particular area. The duties and names of the committees changed, but during the first 20 years of the institute's working their membership remained fairly constant with a small group of individuals, many of whom were Mendelians, forming the bulk of the members. Biffen and his student Engledow as well as Wood were amongst them. The most important and long-running of these working committees were the Crop Improvement Committee, the Official Seed Testing Station Committee and the Potato Advisory Committee. The institute's work was embodied in its slogan "Better Seeds : Better Crops" and this was elaborated into three stated aims; crop improvement, seed testing and combating the potato wart epidemic. After the War, the 1917 Testing of Seeds Order became the Seed Adulteration Act of 1920. As part of these changes the Streatham Seed Testing Station was moved to NIAB where it continued testing seeds in accordance with the new act. Along the way the station was renamed the Official Seed Testing Station. The Seed Adulteration Act demanded the use of certificates for all seeds, produced at point of sale. The Ministry of Agriculture provided inspectors to take samples from thousands of businesses who sold seeds, including farms who sold to other farms, and even blacksmiths or grocery stores that sold seed only seasonally and in very small quantities.[121] The OSTS at NIAB undertook the copious amounts of testing this generated. Work on potatoes, which pre-dated NIAB, continued at the institute's newly acquired grounds at Ormskirk in Lancashire, fulfilling the third aim. How the first of these aims – crop improvement – was to be achieved, was left underdetermined by the institute's constitution. In the 1920s this ambiguity led to considerable debate and several shifts in policy. The first decade of the institutes' work on crop improvement saw the relinquishment of initial aspirations to compete in the wholesale seeds market and a restriction of this work towards varietal-trialling

[121] See "The First Year's Working of the Seeds Act" 1922, and subsequent sporadic reports published in the *Journal of the Ministry of Agriculture.*

and the provision of small stocks for the trade to multiply and distribute. Beaven, in particular, argued that growing seed in bulk was not going to be profitable.

The institute was homeless for the first two years of its existence but in 1921 it moved into purpose-built accommodation on the Huntingdon road – directly opposite the Cambridge School of Agriculture and the PBI's farms, see figure 1.8. In the same year the Ministry of Agriculture's seed testing station at Streatham was relocated to the institute where it filled the second of the institute's aims, seed testing. The move was accompanied by a visit from the King. Further support for the institute was created through a fellowship of subscribing members who after 1922 also elected a representative to the council. The honorary president of the fellowship was the then Prime Minister David Lloyd George. Bateson's friend Wilhelm Johannsen was another early honorary member. When it moved into its new buildings the institute was gifted the use of the farm next door by Fred Hiam, a local farmer and benefactor. Although the plan was initially to use the farm for the multiplication of seed stocks to sell on in bulk, when this idea was abandoned as unprofitable the farm was sold in 1929. The institute was gifted or loaned the use of several sites around the country including the Ormskirk site for potato work. These allowed it to perform cross national trials on new varieties. The institute also undertook several other smaller projects in trialling, it kept a living museum of varieties from around the world – including those brought back from the Everest expeditions – and observation plots and pure seed stocks of various varieties were kept under monitoring.[122]

[122] See the "Register Recording the Receipt of Seeds etc. by the Manager of Field Plots Commenced 1922", NIAB Archives.

Figure 1.8 The National Institute of Agricultural Botany. Image reproduced from the NIAB Archives, courtesy of NIAB-TAG. Image published in the *Times* (12th October 1921), 15c, d and e, with the announcement, "On Friday, October 14, the King and Queen, accompanied by Princess Mary, will visit the headquarters of the National Institute of Agricultural Botany".

The institute was not only run by Mendelians, it was run in concert with the School of Agriculture, the PBI, the HGWC and the JI to Mendelism's benefit. The JI even sent over their deeds of trust so that Weaver might use them as a model for NIAB. From the very beginning NIAB was conceived of as an "aid to Professor Biffen's researches".[123] The institute's wider aims of ensuring purity in seed stocks and taxonomical clarity were also conducive to Mendelism, all Mendelian experiments had to start with pure-breds.[124] The observation plots and living museum kept at NIAB provided a valuable resource of raw biological material for Biffen. Much space was freed up on the PBI's farm as NIAB took over the role of distribution. The HGWC committee also conducted a series of trials for the institute – these were undertaken by Humphries as the millers' representative on the

[123] See *Memoranda on the Establishment of a National Institute of Agricultural Botany*.
[124] See Parker and Chambers 1921 on institute's aim to overhaul nomenclature.

institute's council. The first variety that NIAB distributed for Biffen was specifically intended as a new, better-milling wheat.[125] In sum, the institute played a central role in the development of Mendelian plans for agricultural improvement; one that has not been previously recognised.[126]

In the mid-1920s Biffen, Wood and Bateson were at the height of their powers, presiding over a Mendelian system that was firmly established. However, in 1926 Bateson died suddenly from heart failure; three years later Wood died in similar circumstances and in 1930 Lawrence Weaver followed Bateson and Wood. In the intervening years NIAB abandoned its plans for commercial distribution of seeds and while Biffen remained busy until 1936, in 1931 funding for the Mendelian system was reorganised under the newly formed Agricultural Research Council.

1.3.4 Popular Reception of the Mendelian System

The forgoing events hardly went unnoticed in their day, so in closing it is worth spending a moment looking at reactions to these developments. The reactions of the British Association for the Advancement of Science, and the *Times* spoke directly of approval for the institutional expansion from the ranks of their members. Wood and particularly Biffen's work featured heavily at the meetings of the British Association for the Advancement of Science.[127] Biffen's first appearance, as a Mendelian, was at the 1904 meeting at Cambridge, a year in which the association launched an

[125] See Chapter 3 of this thesis for more on Biffen's varieties.
[126] Palladino 1990 presents relations between PBI and NIAB as being rocky, he bases this on the veiled musings of Engledow in an interview conducted in 1975. See Hugh Rogers, *Plant Breeding: the Early Years*, Plant Breeding Institute Collection. However examination of the correspondence between Biffen and Weaver reveals the close and often fond relationship between the two. Furthermore, minutes of the working committees of the institute reveal Biffen's active involvement up until the mid-1930s.
[127] On the BAAS see Morrell and Thackray 1982.

agricultural sub-section.[128] In the following year at the association's meeting in South Africa, A. D. Hall waxed lyrical about the "great practical importance" of Biffen's work. Biffen featured in speeches to the association by Bateson, and, the RHS and Board of Agriculture's Sir Daniel Morris, who in particular supported the institutional expansion orchestrated by his friend Biffen. The new institutes, he told the Bournemouth meeting in 1919, were, "essential to the welfare and safety of the nation", men like Biffen, "workers in pure science", were, he argued, required to solve, "those problems of national importance which confront us".[129] As to the success of the institutes, the Prince of Wales voiced this approval in his 1926 presidential address to the British Association for the Advancement of Science, "At the plant-breeding institute at Cambridge, Sir Rowland Biffen has provided several new wheats, of which two are generally grown throughout the country; the extra yield and value of these wheats must already have more than repaid the whole expenditure on agricultural research since the institute was founded".[130] The Prince obviously felt that the expense of public breeding was more than justified by the benefits brought to the nation. The fact that NIAB was actually struggling to realize self-sufficiency at this point should not detract from the importance of these sorts of perceptions of its success.[131]

In the *Times* the inspiration provided by Bateson to the Cambridge Mendelians was remembered in an article on the inauguration of the School of Agriculture:

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[128] See the inaugural address to the subsection from former Cambridge Chair of Agriculture William Somerville 1905 and Biffen's appearance in this sub-section and the zoology section, Biffen 1905b and 1905c.
[129] Morris 1920: 319.
[130] The contents of this presidential address were (as in most years) republished in *Science*, no doubt increasing Biffen's fame internationally. See, "The Presidential Address" 1926: 146.
[131] Biffen was knighted in the New Year's honours roll in 1925, receiving, as Middleton and Hall had previously, a KBE.

> This work is but one manifestation among many in Cambridge of the inspiration which Professor Bateson communicated to the men around him when he took up the long forgotten principles of Mendel on the inheritance of parental characters and began to apply them to the problems of the breeders of animals and plants.[132]

Twelve years later the Prime Minister, David Lloyd George, the chief architect of the Development Act which had established the Development Commission, used the *Times* to voice his support for the work at NIAB. Lloyd George, now at the end of his second coalition term as Prime Minister, enthusiastically supported the institute and its developing plans for releasing commercial varieties. In 1922 he wrote an open letter to the *Times*, addressed to Lawrence Weaver:

> Dear Sir Lawrence. – I have been following with great interest the rapid progress of the National Institute of Agricultural Botany, and congratulate you and your colleagues on the serious and useful work the Institute is already doing for the farming community… I gladly show my appreciation of what you are doing by asking to be enrolled as one of the first Life Fellows of the Institute.[133]

The announcement precedes a report of the year's Royal Agricultural Show, at which NIAB, the Cambridge School of Agriculture and the Plant Breeding Institute were linked in a joint public display of Biffen's new varieties.

[132] "The Cambridge School of Agriculture", *The Times* (25th April 1910), 7e.
[133] "Letters to the Editor", *The Times* (16th January 1922), 4c.

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Biffen, Wood and Bateson's activities fit a pattern of system building. They were responsible for bringing together the resources for creating and maintaining a system. The School of Agriculture, the JI, HGWC, PBI, BSCA and NIAB interacted towards these men's aims. Furthermore, Biffen Bateson and Wood's preoccupation with production marks Mendelians out against a standard research school.[134] Accordingly, we might view Biffen's efforts as much in line with the system-builders of his time such as Edison as the canon of heredity studies. The executive involvement of Mendelians with the institutes covered in this chapter can be seen in figure 1.9. Shared executive control was not the only form of interaction in this system; students, staff and researchers circulated freely between institutes. Varieties used for breeding experiments often travelled with workers or were passed to friends and colleagues. Several journals, including the *Journal of Genetics*, the *Journal of Agricultural Science* and the *Journal of the National Institute of Agricultural Botany*, were established to give staff a platform from which to publish ad share their work with peers.[135]

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[134] On the things a research school needs to flourish see Jack Morrell's standard setting, "The Chemist Breeders: The research schools of Liebig and Thomas Thomson", Morrell 1972. Morrell revisits his analysis of research schools in Morrell 1993. Although Perkins, the focus of Morrell 1993, developed links with commerce, he never retained control of the means of production as Biffen did at Cambridge. See also Morrell 1996.
[135] On publication rates at a later period see Palladino 1996a: 126.

CSA	HGWC	JI	PBI	BSCA	NIAB
Biffen	Biffen	Biffen	Biffen	Biffen	Biffen
Wood	Wood	Wood		Wood	Wood
		Bateson			
Hall	Hall	Hall			
Humphries	Humphries			Humphries	Humphries
Parker			Parker		Parker
Engledow			Engledow		Engledow
				Hasler	Hasler

Figure 1.9 The Mendelian System. Figure shows the shared executive control at the Cambridge School of Agriculture, Home Grown Wheat Committee, John Innes Research Institute, Plant Breeding Institute, British Seed Corn Association and the National Institute of Agricultural Botany. Note: Parker Humphries and Hall never took executive positions at the CSA, but were all trained at the School or its predecessor the Department of Agriculture.

Scarce resources were shared amongst the system as when Bateson wrote to Biffen in 1914 asking Biffen to grow a sample of seeds for him on spare land at the PBI's farm.[136] There was also an increasing division of labour as new institutional components were added to the system. This division of labour was in part a response to the internal dynamics of the system, as seen in Biffen's work with the BSCA. The PBI did not afford Biffen the means to distribute seed adequately and so he sought to outsource this part of the work.

We have now seen that agricultural science experienced a huge institutional expansion at the same time as the emergence of a Mendelian system. The sceptic's reply at this point might be, "So what?" Well indeed, was there a connection or were these two events entirely unrelated phenomena that happened to occur at the same time? Perhaps the expansion of agricultural science might have occurred without

[136] Biffen to Bateson, 4th March 1914, Bateson Letters Collection, 3269.

Mendelism. Or vice versa, the expansion of Mendelism might have occurred outside of an agricultural context, perhaps in the elite university context and not in a muddy agricultural department? As Biffen and his peers often pointed out, good men were in short supply, especially after the War; perhaps Mendelians were the only academics available to fill the new roles of agricultural scientists? Worse still, perhaps Mendelism had very little to do with plant breeding or any agricultural concerns at all really? In order to answer these questions, as to whether agricultural science and Mendelism actually influenced each other, and if so how, we need to proceed to look at the theoretical development of Mendelism in this agriculturally located Mendelian system.

Chapter 2

A New Theoretical Map: Purity as a Theoretical Problem

MANY AGRICULTURAL SCIENTISTS have claimed that the first decades of the twentieth century were a period of great intellectual freedom, during which workers could focus on theory with little consideration for practical outcomes. As Sir John Russell, then director of Rothamsted, put it in 1924, speaking to the agricultural section of the British Association for the Advancement of Science, "This period ... may be called the period of free exploration, since the workers were not usually tied down to any particular technical problem". Russell's reasons for giving this characterisation became clear as he concluded his speech, "the safest way of making advances, even for purely practical purposes, is to leave the investigator unfettered".[1] Despite such claims, the next two chapters demonstrate exactly how Mendelians and their theories became tied to one particular agricultural technical problem in this early period. The central problem was that of rogues, or out of type plants. These were taller, shorter, wilder plants which did not behave as they should, showing little to no resemblance to their parents. The presence of rogues, often freakishly dwarfing their neighbours in the field or garden, pointed to impurity of the hereditary factors in the gamete. Yet purity of hereditary factors, or gametic purity, was a central plank in early Mendelian thinking.

Gametic purity was a conceptual starting point from which two fundamentally Mendelian dispositions developed.[2] The first was a disposition to reduction, epitomised by the move to treat organisms as reducible to single

[1] Russell 1925: 256. See also Seward 1917, Engledow 1957, Bell 1976 and Hugh Rogers, *Plant Breeding: the Early Years*, Plant Breeding Institute Collection, for nostalgic visions of a free past used as polemical fodder for the need to unfetter scientific research in the future.
[2] Although these dispositions are common knowledge in the historiography, they have been identified most clearly by Müller-Wille 2007b: 799-800.

alternating and pure character pairs, often exemplified by an imaginary cross between a green and a yellow pea, all other differences, beyond colour, being reduced to nothing. The second disposition was one to construction, epitomized in the Mendelian belief that it was possible to use character pairs as though they were building blocks from a Meccano set – the toy construction kits which also appeared in Britain for the first time in 1901.[3] Assuming gametic purity was an essential pre-requisite to both these dispositions. Breaking down organisms or building them up required stable, bounded building blocks. However, in order for consensus to emerge around the purity of hereditary factors, an older Darwinian conception of purity as a rare and unstable phenomena had to be overturned. On a Darwinian view there might always be some impurity in the pedigree waiting to pop up; rogues were to be expected. Danish botanist Wilhelm Johannsen's work on pure lines helped underwrite the genetic consensus on gametic purity in America and on the Continent, leading some to believe that Darwinian selection on small variations was impotent to create new species or even varieties.[4] But how did British Mendelians deal with this problem?

This chapter seeks to analyse several aspects of British theoretical work on rogue plants and the problems they posed for gametic purity, all conducted by Cambridge trained Mendelians between 1901 and 1926. Following theory, and its interactions with context, expands our understanding of the scientific development of Mendelian genetics. In explicitly Hughesian terms, the rogues can be thought of as reverse salient; a part of the system which lagged behind the advance of the rest, a problem which required realignment of other components, including theory. When

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[3] On Meccano sets see <http://en.wikipedia.org/wiki/Meccano> [accessed 6 August 2001]. For more on the constructive element of Mendelism, and the fluidity of these categories see Sapp 1983.
[4] On this shift see Bonneuil 2008.

Biffen and his colleagues promised a theory driven revolution in breeding, the rogues, just off stage, were the most obvious sign that such promises were overstretched. Accordingly Mendelians spent great efforts, firstly in attempting to show why rogues should not exist, and secondly, when the rogues refused to disappear, why they did not undermine Mendelian claims or the theory on which they were based. Some of these arguments were exculpatory; they phrased the rogues as being entirely a problem of distribution, not production (as we will see in the next chapter, focused on Biffen's breeding and distribution activities and the Mendelian tendency to construction). On the other hand, some of these arguments subtly changed the scope and operation of Mendelian theory. In a sense this shift strengthened the disposition to reduction. Anomalies forced Mendelians to concede the complexity that underlay what they had claimed to be simple Mendelian factors but as such problems came to be defined as anomalous they ceased to threaten core Mendelian beliefs.

The chapter is split into six sections. The first part covers the state of the art in the new discipline of agricultural botany in 1900, in the period immediately before the arrival of Mendelism. The essentially Darwinian nature of agricultural botany in 1900 is then contrasted with the new theory of Mendelism. The outline of Mendelism given here is taken from successive editions of Reginald Punnett's key Mendelian text, *Mendelism.* In the third section we focus in on one key theme in *Mendelism*, variation. The issue of variation, and especially its form, was the chief point of contention between the young William Bateson and the Darwinian tradition in which he trained and then reacted against. Unsurprisingly then, in the years after the "rediscovery" of Mendelism, rogues and the type of variation they represented, were at the forefront of Bateson's interpretation of the new theory. This was a project taken on by Bateson's students and exported around the world. When

William Balls, the focus of the chapter's fourth section, went to work on cotton in Egypt, the rogues were waiting for him there, hidden amongst the cotton fields. Balls's correspondence with Bateson and the published accounts of his work reveal Balls' view of rogues as unavoidable, at best reducible to acceptable statistical thresholds. Meanwhile back in Britain, Biffen, whose theoretical powers have often been under-appreciated, began a series of experiments that sought to resolve the problem of rogues in wheat. This research project, along with Balls's, influenced Bateson's later work on rogues in peas, produced in collaboration with Caroline Pellew. Despite Bateson and Pellew, Biffen and Balls's best efforts, and their different attempts at assimilation of Johannsen's work, closure on the issue of rogues was never fully achieved; at best they were reduced to the status of peripheral phenomena which posed no threat to the stabilisation of the Mendelian gene concept.[5]

[5] Eventually rogues became a matter of statistical variance within acceptable limits. Nowadays their presence is measured against a threshold which acknowledges the impossibility of fully removing them. See for example the uniformity criteria in the UPOV system's Distinctiveness, Uniformity and Stability (DUS) standards for the registration of new varieties see Dutfield 2011. A recent study on the same rogues that Bateson dealt with, now known as paramutations, concluded "Whereas Mendelian rules, together with the concept of genetic transmission via the DNA sequence, can account for most inheritance in sexually propagating organisms, paramutation-like phenomena challenge the exclusiveness of Mendelian inheritance". Stam and Scheid 2005: 283. On the stabilisation of the gene concept see Keller 2002, Lewontin 2000a, 2000b and on the recent move away from genes to genomes, Barnes and Dupré 2008.

2.1 The Arrival of Mendelian Theory

2.1.1 Pre-Mendelian Inheritance and the Rogue Problem

In 1900, the Cambridge-trained botanist John Percival published a discipline defining textbook, *Agricultural Botany.* Percival shared a mentor with Biffen at Cambridge in Henry Marshall Ward, but his vision of a new discipline was very different to the Mendelian one offered by Biffen and his colleagues in the following years.[6] Percival published *Agricultural Botany* to answer a perceived need. "Practical men and the agricultural press", he explained, "have from time to time complained of the absence of text-books of botany suited to the wants of the student of agriculture".[7] *Agricultural Botany's* subtitle, *Theoretical and Practical*, reflected the hybrid nature of the content of the book, which contained theory and technical tips interspersed together. Percival believed science had much to offer in the way of aiding agriculture, but for him the correct sciences to apply were physiology, morphology and systematic botany – the study of relations between species.[8]

In three chapters, towards the middle of the book, Percival deals with reproduction and plant breeding. The first two chapters on reproduction, dealing with vegetative and sexual reproduction, are interesting because they say nothing about heredity at all. The chapter on vegetative reproduction focuses on techniques of making cuttings and grafts, the chapter on sexual reproduction continues Percival's previous discussions of flower morphology. The essence of reproduction for Percival was, "the fusion of two special kinds of cells ... which after complete coalescence or co-mingling of parts, give rise to a single cell capable of growing

[6] For biographical details of Percival see Palladino 1993.
[7] Percival 1900: preface.
[8] See Percival 1921 for Percival's fullest systematic account of the wheat plant, and Biffen 1922c for a gently critical review. See Palladino 1993: 319-320, for more on Percival's views about the application of science to breeding and farming.

into a new organism".[9] For much of the sexual reproduction chapter his focus is on the shape and development of pollen and ovule cells and the mechanics of pollination and fertilisation, on the act of coalescence or co-mingling or what might be mingled Percival had very little to say. After a brief detour through flowers' evolutionary adaptations to attract insects, the rest of the chapter lays out a taxonomy of hybrid plants. The chapter closes with a discussion of the best techniques for artificial transfer of pollen to create hybrids.

In the following chapter, "Cultivated Plants and their Origin: Plant Breeding", Percival was slightly more revealing about his theoretical allegiances on the subject of heredity. The chapter begins with a discussion of sports. Percival uses the English definition of the term, referring to new buds which vary from the rest of the plant. These are distinct from seminal sports, which Percival defined as, "a seedling which differs very appreciably from its parent in some of its morphological or physiological characteristics".[10] Seminal sports were, Percival believed, behind most of the varieties then grown for food. Only recently had breeders started to use hybridisation as a means of artificially inducing the type of variation seen in seminal sports.[11] In either case, once in possession of some useful variation a breeder was best off trying to capture this in some way, in case the variation changed again into something less useful. On one hand, Percival advised, they could propagate the variant vegetatively, in effect capturing the variation in freeze frame. All of the cuttings or grafts taken from the plant would be identical but its seeds might produce something completely different. If given the chance to develop seeds, co-mingling might occur and the resulting plant would be altered as a result. On the other hand a

[9] Percival 1900: 268.
[10] Percival 1900: 295.
[11] See H. E. 1886 on some of the first experiments with hybrid wheat, in which hybridisation was used to create variation.

breeder could attempt to "fix" the variant so that it "came true from seed".[12] To do this a breeder would have to conduct continued selection from amongst the progeny of the sport for several generations, choosing only those which showed the desirable new feature and throwing away the rest, until the desirable plants were the only plants in the new population.

However, a breeder's work was not finished here, even after finding some useful variation and successfully fixing it, there could still be problems:

> 'Fixation' is, however, a relative term, for even in cultivated varieties in which the process of destruction has been systematically carried out and which have 'come true' from seed during many generations, *'false plants*' or *'rogues*' departing considerably from type appear among the offspring at regular intervals.[13]

Percival defined rogues in the following (distinctly Darwinian) terms, as part of a family of variations, all of which pointed to reversions back to what Darwin called an aboriginal state:

> 'Rogues' most frequently exhibit characters possessed by the ancestors of the variety in which they are found.
>
> The tendency of plants to revert to long-lost characters is termed, *atavism, 'throwing-back'* or *reversion.*[14]

[12] Percival 1900: 305.
[13] Percival 1900: 306.
[14] Percival 1900: 307.

This was a huge problem as if the breeder did not take care rogues might undo all their hard work. Percival warned would-be breeders, if the “destruction of ‘rogues’ is not efficiently or thoroughly carried out … the type rapidly degenerates in purity”.[15]

The account of plant breeding given by Percival and his identification of the central problems of fixing a variety and guarding against fluctuation, in the form of rogues, were underpinned by a Darwinian conception of the nature of varieties and species.[16] On Percival’s Darwinian view, species and varieties merged into each other insensibly by gradual changes in variation. The idea that species were inherently unstable and that hybridisation could upset them further, and moreover, that selection was required to guard against variation, were essential parts of Percival’s account of a plant’s ability to degenerate, vary and produce rogues. The need for successive generations of selection fits right into his Darwinian model of the plasticity of plants.[17] Selection, for Percival, acted as a guard against the usual and expected variation. Co-mingling of large numbers of hereditary factors which interacted together was the exact opposite of the clean and precise union and separation of unit factors being proposed by the Mendelians.[18]

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[15] Percival 1900: 307.

[16] For Darwin’s account of rogues see Darwin 1859: 32-34 and Darwin 1868: 46. See also Richards 1994: 409-411 on Darwin and the domestication of extreme variations and Bartley 1992: 315-318 and Secord 1981 and 1985.

[17] Darwin 1859: 33-34.

[18] Percival’s response to the Mendelian thinking that arrived in Britain in the following year is very interesting. In 1902 a second edition of *Agricultural Botany* appeared but having been drafted in November of 1901 this contained no mention of Mendel. The third edition of *Agricultural Botany* appeared in 1910 with a new section on “Mendelian Laws of Inheritance”. However, the prefacing text remained identical to the 1902 copy. Percival noted in his discussion of the theory that it “greatly assists the efforts of the plant breeder, inasmuch as it indicates the lines along which crossing must take place”. Percival 1910: 298. However, he took up a position that was familiar at the time. Mendelism, to Percival, while interesting, represented a special case with limited applicability beyond a few characters in a few species. There was one feature of Mendelism that Percival seems to have been much struck by, and this is where he ends his discussion of the theory; Mendelism’s presumptive ability to explain rogues. Percival did not see any contradiction in using Mendelism to explain rogues, while at the same time advocating a Darwinian understanding

2.1.2 The New Theory: Gametic Purity and Segregation

Agricultural Botany was a hugely successful publication; it went through another five editions, the last appearing in 1949. However, this success was outstripped by that of Reginald Punnett's *Mendelism*, a key example of the now long forgotten genre, the Mendelian best-seller. *Mendelism* went through seven British editions, numerous reprints, American editions and translations into several languages. Over the course of its life the initial 63 page volume swelled to 236 pages. This evolution is instructive. From an initial brief statement of just the most important features of the theory *Mendelism* swelled to a lengthy and sometimes rambling account of the many theories that came to be part of Mendelian thinking.

The first edition of *Mendelism* opens with an account of Mendel and his work, introducing the phenomena Mendel observed; dominance, recessiveness, and the 3:1 ratio.[19] After demonstrating the wide applicability of these phenomena by referencing work that confirmed Mendel's results in other organism – including Wood and Biffen's, see figure 2.1 – Punnett moved to the theoretical explanation of the patterns of inheritance Mendel observed. The point of departure for a classic Mendelian cross is two pure breeding populations of organisms, differing from each other by a single discreet character. Punnett used figure 2.2 to represent this type of cross. For our purposes we can imagine two populations of peas, labelled in figure 2.2 as P1. One variety has black seeds and the other white seeds. Upon our first cross between a black seeded and a white seeded pea plant the progeny, the F1 in figure 2.2, are all black. In Mendelian terms the black seed making factor that our

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of the plasticity of plants and the efficacy of successive round of selection. The plant breeding chapter following his explanation of Mendelism was also unchanged from 1902 and still contained his Darwinian explanation of rogues.

[19] On dominance and recessiveness see Falk 2001.

AN ILLUSTRATION OF INHERITED CHARACTERISTICS IN SHEEP.

The black-faced Suffolk ram (at left) when crossed with the white-faced ewe of the horned Dorset breed (in middle) results in animals with speckled faces, of which the males are horned and the ewes hornless. Breeding from the latter leads to a type of ram (at right) combining the white face of the Dorset with the hornlessness of the Suffolk.

At left "Fife" Wheat—strong, but scanty yield. At right, wheat developed by crossing "Fife" Wheat of large cropping capacity.

The two pure strains of White Sweet Peas (at right and left) reverted, on crossing, to the Purple (shown in the middle).

Figure 2.1 Punnett's *Mendelism*, frontispiece from the third edition showing Wood's Mendelian sheep and Biffen's Mendelian wheat as well as a specimen of Punnett's Mendelian sweet peas. Punnett 1909: frontispiece.

parent plants transmit is dominant to the white making factor. In contrast, the white seed making factor is recessive to the black making factor. As a result the Mendelian believed, when both black and white making factors are present in a plant it will only produce black seeds. However, each of the black seeded F1 individuals still contains both black and white making factor. If we then took two of these black-seeded progeny and crossed them together, we would get a mixture of black and white seeded progeny in this second generation from our first cross, the F2. Indeed, the Mendelian claimed the F2 would consist of a definite ratio of 3:1 black to white seeded plants (if the population was big enough). The possible combinations of gametes produced by the F1 mean that in three of every four of their progeny there will be at least one black making factor present, making these black seeded plants. In one in every four plants (on average) only white making factors would be passed on. In these individuals the white seed making factor could be expressed as the black making factor is absent.

Mendelians invoked several further concepts to explain what they took to be the empirical fact of the 3:1 ratio.[20] The key concept, for Punnett, was “gametic segregation” and, as he noted later, “segregation implies gametic purity”.[21] The twin concepts of segregation and gametic purity implied that if a differentiating character, like short or tall, was the result of a pair of factors, then, “a fundamental property of the gamete is that it can bear either one such a pair of characters, though not both”.[22] Mendelians assumed that each plant was only capable of passing on one or other factor in its ovule or pollen (represented as circles in figure 2.2). In other words factors never influenced each other. They came together and disassociated cleanly; black and white never mixing into grey. Working from this assumption, Mendelians believed that not all of the 3 in a 3:1 ratio were alike. As the four central boxes in the bottom half of figure 2.2 illustrate, of the three in every four plants which produced black seeds two would have a mix of factors like their F1 parents but one would have only black making factors. So the 3:1 ratio of characters can be broken down

[20] For an overview of Mendelism see Olby 1966, 1979 or Radick 2005.
[21] Punnett 1905: 23 and 49.
[22] Punnett 1905: 49.

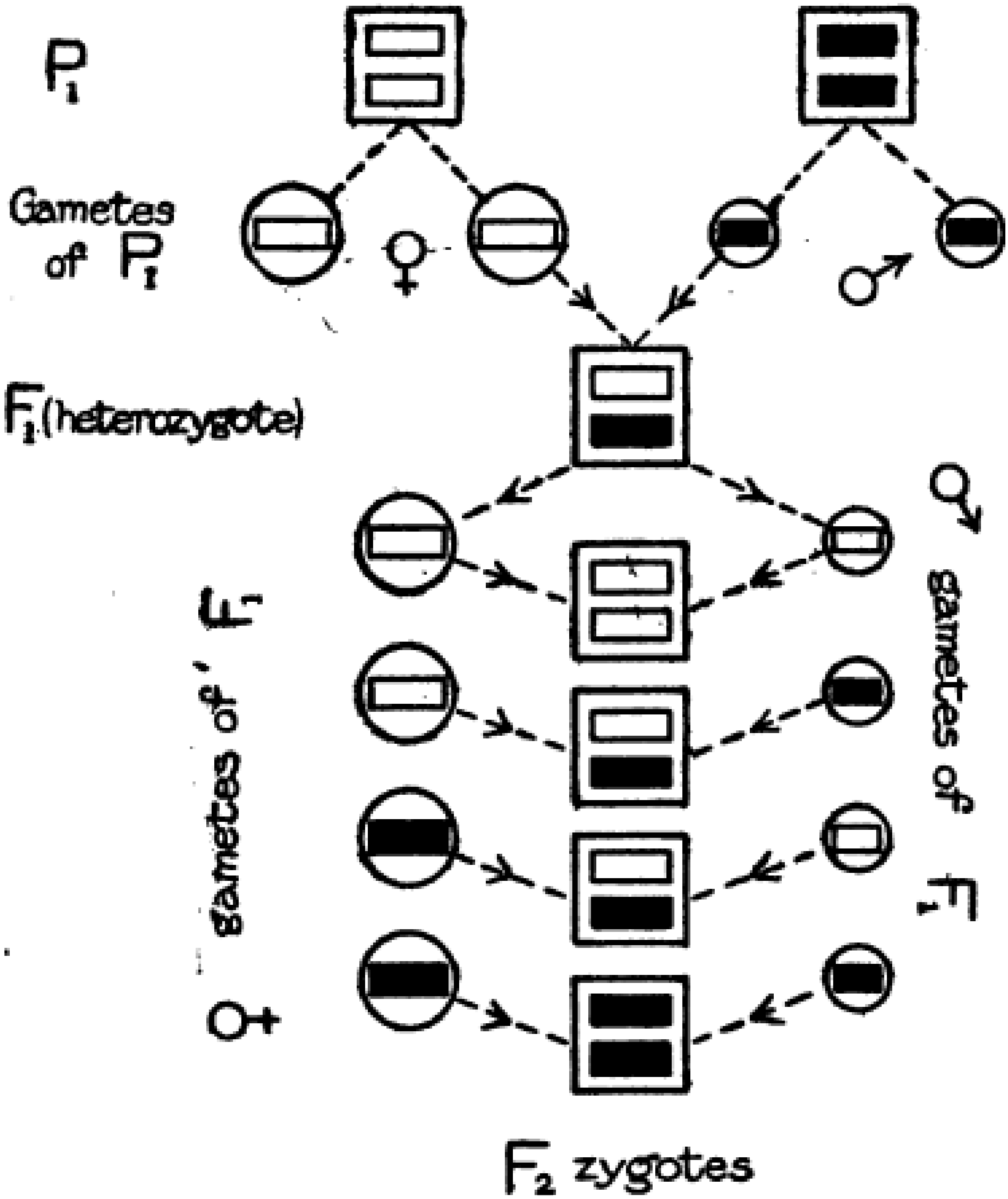

Figure 2.2 Gametic purity and segregation, figure from the second edition of *Mendelism*, Punnett 1907: 25.

into a 1:2:1 ratio of factor composition; one in four plants would only have black making factors, two in four would have both types and the final one in every four F2 individuals would have only white making factors.

The Mendelian account of factors coming together and separating cleanly stands in stark contrast to Percival's Darwinian concept of co-mingling. Moreover, the differences between the two theories impacted on the way each sought to explain and eradicate rogues. On Percival's reckoning, rogues were caused by the presence of ancestral influence, reflecting the plants previous plasticity, and so the only way to guard against them was through selection. On a Mendelian understanding of factors and their interaction, however, the purity of that interaction disbarred the effects of either ancestry or selection. The first Mendelian explanation of rogues came, unsurprisingly, from William Bateson.

2.1.3 William Bateson and the Heterozygote Explanation of Rogues

William Bateson, as is well known, was an arch anti-Darwinian. He believed in evolution, but not Darwin's mechanism of evolutionary change by small continuous variations. This disagreement with Darwinism was the context from which Bateson interpreted Mendel's work. In 1899, before he came into contact with Mendel's work, Bateson presented to the Royal Horticultural Society his differences with Darwinism on "the species question". Bateson's quarrel with Darwinism was twofold and it hinged on the mechanism of speciation:

> 1. By what steps—by integral changes of what size—did the new form come into being?
> 2. How did the new form persist? How was it perpetuated when the varying individual or individuals mated with their fellows? Why did it not regress to the form from which it sprang, or to an intermediate form?[23]

[23] Bateson 1900: 61. This is essentially the same objection as that raised by Fleeming Jenkin shortly after the publication of Darwin's *Origin*, see Jenkin 1867 and Gayon 1998: 94.

The inability of the standard Darwinian school of evolutionary thought to answer these questions, pointed, to Bateson, to a need for a return to the breeders' empirical data. He called to the Royal Horticultural Society to aid his investigation of variability, telling the society's members, "Our business, then, is to test and examine these different kinds of variabilities according to their behaviour when the different varieties are crossed together".[24] Bateson believed the answer to his questions for Darwinism lay in discontinuity. Bateson's major pre-Mendelian work, *Materials for the Study of Variation,* made clear that he believed that big discontinuous variations – those fully formed from the start of the organism's life – were the important ones when it came to speciation. Discontinuous variation, in Bateson's eyes, provided a solution to the problems inherent in his Darwin baiting questions; discontinuous variations were immediately useful and less likely to be swamped by blending. However, in order to fulfill this role they would have to be stable. When Bateson came into contact with Mendelism the central element of theory he pounced upon – especially during the Biometrician-Mendelian debate – was gametic purity.[25] Using this concept Bateson was able to make claims about reduction, as in his famous comparisons of the new Mendelian science of heredity to chemistry.[26] Bateson was also, working from an assumption of gametic purity, able to make constructive claims as to the promise of practical application.

[24] Bateson 1900: 65.
[25] See Charnley and Radick 2010. In this paper we demonstrated that purity was an intellectual issue for Mendelians at the turn of the century. In his critique of Mendelism (one that spurred Bateson to write Mendel's laws a defence) W. F. R. Weldon pointed back to plant breeders' debates over purity in the 1870s, discussed in detail in the prelude to chapter 3. In particular the impurity of one pea variety had caused trouble for a seed firm trying to claim the variety as their intellectual property. Weldon took the firm's troubles to be instructive, the instability of their variety pointed to gametic impurity in his eyes. This was a direct intellectual attack on one of the central tenants of Mendelism – the purity of the gametes. This issue of purity ceased to be a problem for many Mendelians over the next 25 years. For more on Bateson and the importance of gametic purity see Gayon 1998: 276, Radick 2005, Allen 2004: 216 (point 10 in his list) or Olby 1966.
[26] On Bateson's view of Mendelian science as precise and comparable to chemistry see Coleman 1970 and Radick 2011.

Bateson gave his own interpretation of gametic purity to the zoological section of the British Association for the Advancement of Science meeting held in Cambridge in 1904. We can continue using Punnett's diagram in explicating this speech. Although the diagram was published a year later, the thinking displayed is identical to Bateson's. According to Bateson the "qualities or characters" whose transmission he was concerned to study, were distributed among gametes, "according to a definite system".[27] In Punnett's diagram each plant is represented by a square containing two smaller rectangles. These smaller rectangles represent the pairs of gametes and their transmission (from the top of the page to the bottom) through two crosses. In the first cross the gametes are shown leaving the plants (inside circles) and recombining in the F1. As Bateson explained to the BAAS in 1904:

> [E]very zygote – that is, any ordinary animal or plant – is formed by the union of two gametes, it may either be made by the union of two bearing similar members of any pair, say two blacks or two whites, in which case we call it *homozygous* in respect of that pair, or the gametes from which it originates may be bearers of the dissimilar characters, say a black and a white, when we call the resulting zygote *heterozygous* in respect of that pair.[28]

The thing that most excited Bateson was the nature of the cell division which formed the gametes. The nature of this division could be read from its results. On the one hand, "If the zygote is homozygous, no matter what its parents or their

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[27] Bateson 1904a: 581. Like most years the speech was republished in *Nature*, see Bateson 1904b: 407. Bateson uses the word system repeatedly in this passage.
[28] Bateson 1904a: 581.

pedigree may have been, it breeds true indefinitely unless some fresh variation occurs". But on the other hand, "If, however, the zygote be heterozygous, or gametically cross-bred, its gametes in their formation separate … so that each gamete contains only one … character of each pair".[29] In other words the offspring from heterozygotes might always be mixed and could never, in contrast to the homozygotes, be assumed to be pure. Crosses between heterozygous plants could always produce both black and white off spring. If the offspring of heterozygous and homozygous plants behaved in this way, Bateson reasoned, it must be dues to a feature of cell division and the way in which it parcelled out factors between gametes during reproduction. As Bateson put it:

> At least one cell division in the process of gametogenesis is therefore a differentiating or *segregating* division, out of which each gamete comes sensibly pure in respect of the [factor] it carries, exactly as if it had not been formed by a heterozygous body at all. That, translated into modern language, is the essential discovery that Mendel made. ... [T]he discovery of gametic segregation is, and will remain, one of the lasting triumphs of the human mind. [30]

This had been the core message of Mendelian thinking that Bateson had propounded for some time. Two years earlier he made precisely the same point, this time using As and Bs, instead of black and white, as his example characters:

[29] Bateson 1904a: 581.
[30] Bateson 1904a: 581.

> [T]he Mendelian principle of heredity … declares that the cross-breeding of parents *need* not diminish the purity of their germ-cells or consequently the purity of their offspring. When in such cases individuals bearing opposite characters, A and B, are crossed, the germ-cells of the resulting cross-bred, AB, are each to be bearers either of character A or of character B, not both. Consequently when the cross-breds breed either together or with the pure forms, individuals will result of the forms AA, AB, BA, BB. Of these the forms AA and BB, formed by the union of similar germs, are stated to be as pure as if they had had no cross in their pedigree, and henceforth their offspring will be no more likely to depart from the A type or the B type respectively, than those of any other originally pure specimens of these types.[31]

Some sixteen years later, while giving the Croonian Lecture, Bateson was still making just this point:

> Mendel proved the existence of characters determined by integral or unit factors. Their integrity is maintained by segregation, the capacity, namely, to separate unimpaired after separation with their opposites.[32]

To come back to the black and white seeded peas we started with, the all black progeny of the first cross between the P1, are heterozygous, containing both black and white factors, but two in four of the offspring of this F1 will return to a homozygous composition like their all white or black factor containing P1

[31] Bateson 1902a: 114, emphasis in original.
[32] Bateson 1920b: 358.

grandparents. Gametic segregation implies that these two white and black seeded plants, with matching gametes, will always breed true and never produce rogues. Working within this framework, Bateson presented a very different view of rogues to Percival's, while speaking to the New York Horticultural Society in 1902:

> [A] breeder or seedsman introduces some strain of a new variety of his seed – peas or whatever it may be. He finds a number of rogues which are not true to the character which he desires to put on the market – rogues which he is unable to eliminate. Formerly we said it was only a question of time; he must hoe out the rogues and go on, and he will gradually fix his type. But now we begin to see what the facts really mean … We have lost forever, I think, the conception that purity of character is solely or chiefly a function of the number of generations during which that character has been manifested, or of the number of successive selections of that variety which have been made. *Purity of strain or fixity of character is, on the contrary, due primarily to the union of similar gametes in fertilization.*[33]

Bateson did not believe that rogues marked the start of a new species or the return of an old one. Believing gametes were pure, to Bateson, rogues could not be a sign of a hidden aboriginal state; they were far more likely to be the result of unions between dissimilar gametes produced by a heterozygote harbouring a recessive trait. In the years around 1902 Bateson believed most rogues resulted from crosses between heterozygous plants. Pure gametes should mean pure breeds and no rogues,

[33] Bateson 1902: 3-4, emphasis in original. See also Darden 1977 on the promises held out for construction by gametic purity.

so long as dissimilar unions, between heterozygote gametes, were recognised and avoided.

2.2 The New Theory in an Agricultural Context

2.2.1 Pure Line Theory and the Statistical Solution

In 1903 Biffen began working on Mendelism and wheat. One of the few people to see his first crosses was William Balls, see figure 2.3. Balls was a student of Biffen's, and also received much advice and help from Bateson.[34] In 1904, having trained at the Cambridge Department of Agriculture Balls went to work as a botanist for the Khedivial Agricultural Society in Cairo – initially to work on a predicted wilt disease epidemic. The epidemic turned out to be a false scare due to mis-diagnosis, so when he arrived Balls had a free hand to investigate Mendelian inheritance in cotton and establish, "a system of Seed Stations in various parts of Egypt".[35] In 1905 Balls wrote to Bateson to tell him he had begun working on "establishing Mendel in Egypt".[36] Sponsored by Prince Hussein, Balls began collecting all the facts he could as he felt, "they were bound to be useful at some point". As he put it to Bateson:

> I am starting work out here on Cotton, on the lines of Biffen's Wheats, or rather in modest imitation of the same, that is, Mendelian work with a technical bias. I have got very impure stocks to work with as far as I can

[34] For biographical information on Balls see Harland 1961.
[35] Balls 1908: 347. See also the announcement of the new Mendelian station in Cairo in the *Journal of Heredity*, Phillips 1910.
[36] Balls to Bateson, 19th February 1905, Bateson Letters Collection 2554. See also Balls 1907.

judge from seed inspection & fragmentary information in reports of American & other work.[37]

Figure 2.3 William Balls (1882-1960). Reproduced from Balls's obituary in *Biographical Memoires of the Royal Society,* Harland 1961.

[37] Balls to Bateson, 19th February 1905, Bateson Letters Collection 2554.

Everything seems to have gone well in these early years, Bateson was even faintly jealous of the freedom Balls described in his reports back to the John Innes. In 1908 Bateson wrote to Balls, "I am very glad to hear from you again and know you are so prosperous. Everybody, it seems, will have a station soon, except poor old Cambridge!"[38] During these years Balls began working on a manuscript, which would eventually become *The Cotton Plant in Egypt.*[39] Proofs were sent back to Biffen who passed them on to Bateson, who along with Biffen, offered encouragement and support. In his work Balls had a recurring problem; despite being generally self-fertilising he found that 5-10% of his cotton plants were out-breeding. The progeny of these out-bred crosses he called, in keeping with Bateson's interpretation, rogues. The problem, as he saw it, was essentially one of stray pollen creating heterozygotes:

> The amount of natural crossing which takes place in cotton under field conditions was formerly supposed to be negligible; but the author in 1905 showed that about 5 to 10 per cent, of the cotton-seed in an Egyptian field crop was not self-fertilized, and since then it has been elsewhere shown that most other commercial cottons intercross to about the same extent. The effect of this crossing is gently to mix, and to keep mixed, the pedigree of the plants composing the crop, so that even if a variety consisted of only two elementary species when first introduced, it would soon be complicated. [40]

The mixing of pollen, he associated with impurity, "The cause of the impurity – which soon appears even when the original strain was pure – is to be found in the act

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[38] Bateson to Balls, 7th January 1908, Bateson Letters Collection, G2F04.
[39] Balls 1912.
[40] Balls 1915: 15-16.

of natural cross-fertilisation, or vicinism".[41] Notice that this does not suggest an impurity of the gametes themselves, but an impurity of their combination. In 1905 Balls half-jokingly suggested to Bateson that he might start, "growing seed-plants under … mosquito-nets!"[42]

Over the next five years Balls became concerned with, "conservation of purity by the most refined techniques of isolation possible -techniques almost bacteriological in their thoroughness".[43] In practice this actually meant using the mosquito nets he had joked about, along with clean brushes for transferring pollen. He also spent several years trying to produce a cotton plant which physically could not be cross fertilised. At first he thought he might find a variety with a short style, surrounded by anthers, which would reduce the escape of pollen and increase chances of self-fertilization. When such a plant failed to appear in his extensive search of Egyptian and foreign varieties, he began trying to breed one. However, as with his attempts to preserve his plants' purity through netting and handling procedures, he was never entirely successful. By the time Balls left Egypt in 1912 he was convinced that rogue cotton plants were the result of contamination with foreign pollen but he remained unsuccessful in entirely controlling them.

In *The Cotton Plant in Egypt*, Balls suggested a third way to deal with rogue plants; to tolerate them by exclusion. He developed a system to define which deviants were really rogues, believing some plants with unusual features might be the result of rare re-combinations of characters, in his multi character crosses. In order to distinguish the rarities from the rogues he devised a system of voting. Each deviant character earned a plant a vote. When a plant had several votes, representing several deviant characters he classified it as a rogue and excluded it from his results:

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[41] Balls 1912: 110.
[42] Balls to Bateson, 15th March 1905, Bateson Letters Collection, 2555.
[43] Harland 1961: 5.

> [V]ery often, however, such decisions as to vicinistic origin are based on the appearance of abnormal characters which might very well be due in reality to some rare gametic combination following self-fertilisation; we thus argue in a circle; a plant shows an unexpected characteristic, therefore it is a rogue. We have endeavoured to reduce the probability of such unjust decisions by a system of voting, whereby no plant can be condemned unless it shows incredible abnormalities in several characters.[44]

This statistical analysis of rogues allowed Balls to exclude them from his crosses and treat his populations as if they were self-fertilised pure lines.

Balls was very enthusiastic about Johannsen's pure line theory. As his obituarist recalled, "he believed the pure line concept of Johannsen to be as important, possibly even more important, than the principles of Mendel".[45] In his treatment of rogues as caused by contamination with pollen his debt to the concept of a pure line, a self-fertilized colony in which you always know who the father is, is obvious. As Johannsen put it in 1906, "'Pure line' is a mere genealogical term ... It indicates nothing more than the warranted purity of descent".[46] The problems Balls was having he believed to be exactly about warranting descent. Johannsen inspired Balls to pursue the "isolation of pure lines from mixed populations", which characterized much of his work.[47] Despite his protests that his work was mainly economic, Balls's work was actually alive to evolutionary and cytoplasmic themes.[48] He was an early convert to the chromosome theory of heredity and his talk of

[44] Balls 1912: 127.
[45] Harland 1961: 5.
[46] Johannsen 1906: 103, for more on Johannsen see Roll-Hansen 1978 and 2009.
[47] Balls 1960: 435.
[48] See Balls 1910.

elementary species above was not accidental. Balls did not believe there was a sharp division to be drawn between species and varieties, in his interpretation of evolutionary thought, Mendelism hybridisation was a possible source of speciation. In trying to establish Johannsenian pure lines of self-fertilised plants he felt himself to be fighting evolution that was constantly occurring around him and rogues played no part in it. Balls defined them as neither elementary species nor regressions to lost ancestry but merely the result of stray pollen. Evolution, as he saw it, was essentially progressive but the rogues were a statistical anomaly, caused by impure parentage. In fact the problems of the cotton crop were, to Balls, almost entirely due to impure parentage (and by implication, management). In accepting the Batesonian line that anomalies like rogues, were caused by heterozygotes, resulting from cross fertilisation, Balls, who was much more sympathetic to Darwinian selection than Bateson, explicitly rejected the idea that they might be the result of evolutionary degeneration:

> This absence of differentiating characters … has been responsible for a fund of fatalistic ideas about deterioration …. The "running-out" of varieties, miscalled inevitable, need no longer be the bogey of the cultivator. A recognition of the incontrovertible fact that the nominal varieties are more or less heterogeneous complexes of heterozygotes, even when first introduced to commerce, should enable us in the future to dictate the history of Egyptian cotton.[49]

So, compounding the problem of stray pollen was an inability on the part of previous breeders to distinguish between closely allied varieties resulting in the

[49] Balls 1912: 5.

creation of heterozygous populations. Still, Balls was hopeful, once the heterozygous nature of the cotton populations in Egypt was recognised, that rogues could be avoided.

2.2.2 Biffen's Rogue Wheat and Unwieldy Ratios

> Unit-characters have become too familiar to require more than brief illustration. … Their interest lies especially in the fact that they are transmitted independently of one another, as if they were separate and independent things. By appropriate crossing experiments, such as we have just seen, particular groups of such characters may be split up and recombined, over and over again, in constantly new combinations, with no alteration of their individual character.[50]

This was how the famous American cytologist, E. B. Wilson, described the essential nature of the Mendelian insight. Tellingly, the example he used to illustrate the inheritance of unit characters in a Mendelian scheme was Biffen's work on wheat. Biffen was working with wheat simultaneously with Balls, and in communication with him. After the first experiments in 1903, witnessed by Balls, Biffen had continued working, and published his first big paper in 1905 just as Balls was

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[50] Wilson continued, "Let us look at one or two examples of this. Here are the results of crossing two different races of wheat (from experiments by Biffen). One parent is a bearded variety with short, dense heads; the other a beardless form with long, loose heads. The hybrid is intermediate in shape, and is beardless. On pairing the hybrids together all combinations of the four original characters, and of the hybrid character, appear in the grandchildren, namely, (1) short beardless, (2) short bearded, (3) hybrid bearded, (4) hybrid beardless, (5) long bearded and (6) long beardless. These six types appear in definite numerical ratios, and it is evident that the bearded or beardless character has been transmitted quite independently of the shape of the head". Wilson 1913: 814-815. See also the instructive diagrams used by Wilson in this article which gives an American's view of developments in Britain.

arriving in Egypt. This was a flagship moment for Mendelism. Biffen's results were referenced continuously in both the popular and technical textbooks of the day, by scholars as far afield as E. B. Wilson in America. The paper was important to Mendelians for several reasons; it was not coincidentally the first article in the *Journal of Agricultural Science*. It was important, firstly, because of its role in confirming and extending the applicability of Mendelian theory, including gametic purity. Secondly, it was taken to show that Mendelian principles applied to an economically important organism; wheat. In concluding this paper Biffen was sure to point out that no reversion had occurred in his crosses:

> No indisputable case of "reversion" has occurred. Where hybrid varieties of known parentage are crossed with other varieties no indications of the parentage of these hybrid varieties, excepting the characters they themselves show, have been met with.[51]

In other words, Biffen's plots at the Cambridge University's experimental farm (see figure 2.4) were rogue free. In 1907, in an article published in *Science Progress*, Biffen made clear his allegiance to Bateson's interpretation of rogues as heterozygotes, returning once again to the black and white characters used by Punnett and Bateson:

> The heterozygous blacks would throw off the recessive whites, and though these would have been rogued out, a fresh generation of heterozygotes would

[51] Biffen 1905a: 47.

repeat the phenomena season by season. ... [o]n the contrary the recessive white ... would come true from the first.[52]

Figure 2.4 Biffen's Experimental Test Plots. Reproduced from Engledow 1950: plate 1. Notice how the photos, intentionally or not, illustrate the dead level growth of the test plots. Something which might be a rogue, growing above its neighbours, can be spotted on the far left of the bottom image.

[52] Biffen 1907b: 708.

As we have seen, Mendelism offered an explanation for those plants which reverted back to a recessive parental type, to look like the white P1 plants in figure 2.2. If they had been produced without Mendelism it could be presumed that these were the progeny of a cross between heterozygotes, the F1 in Punnett's diagram; plants which could produce both types of gametes. Putative reversions to ancestral type were, however, more troublesome for the Mendelian theory. So long as the P1 parents of the hybrid F1 plants were homozygous, the hereditary composition of the P1's parents and ancestors – according to Mendelism – should be irrelevant. That ancestral influence certainly should not show up in the later generations but this is what seemed to be happening when wild, ancestral looking plants were found amongst their domesticated relatives.

Biffen offered up a practical explanation for rogues of both types, parental and ancestral, he believed they were the result of accidental mixing of several varieties in a single batch of seeds. "The common belief that [varieties] tend to 'hark back' to the parental forms", was to Biffen, "but a relic of pre-Mendelian days".[53] Biffen believed instead that the "accidental admixture" of forms was caused by the use of "travelling threshing machines, and where these are not employed it does not occur".[54] Threshing machines were used to separate the corn from the husk and to separate both from the straw, much as combine-harvesters do now.[55] At the time, threshing machines, being expensive, were usually transported from farm to farm, and were rented, rather than owned by individual farmers. In the process of

[53] Biffen 1922b: 37.
[54] Biffen 1922b: 37.
[55] The threshing machine was developed by Andrew Meikle in the 18th century and replaced the former practice of using flails to separate corn from the rest of the wheat plant. The name combine harvester derives from combined harvester describing its function to combine the processes of cutting and processing crops. Wheat farmers would also use seed cleaning machines to further separate debris and weed seed from their corn if they intended to use it for planting in the following year. Biffen believed these also caused contamination of seed stocks and specially designed his own – which were easy to clean – for use on the experimental farm at Cambridge, Biffen 1925.

threshing, some corn would become lodged in the machine, which would then travel to the next farm. Biffen thought the corn from the first farm would become mixed with corn from the second farm intended for planting in the following season. Biffen even collected samples of rogues from farmers and grew them on to prove they were distinct varieties and not reversions to the parental or ancestral types.[56] By 1926 he was so adamant in his belief of this explanation that the "still common view regarding the fractional representation of the parents in their direct offspring, their grandchildren, great-grandchildren and so on", was dealt with in a section of his fullest report on his breeding activities, titled "Obsolete Theories".[57]

In 1912 Biffen published a follow-up paper on his 1905 work, titled "Studies in the Inheritance of Disease Resistance II". Biffen continued to propagate the offspring from his 1905 crosses, investigating disease-resistance in the intervening period, to show there was no loss in gametic purity; that the characters he had followed since 1905 remained constant. The upshot of these experiments was that Biffen's plants showed no increase in susceptibility to disease, one of the characters he had studied in 1905. This evidence, that resistance had not fallen off, Biffen interpreted as a sign that there had been no reversion:

> The oldest hybrid varieties I have any personal knowledge of are now at the F8 stage. They have been under observation continuously both on the University Farm and in general cultivation, and it is safe to say they are still as resistant to as the original F2 plants.[58]

[56] Biffen 1924a: 7-8.
[57] Biffen and Engledow 1926: 21-22. The view regarding fractional representations of ancestors having an influence on their offspring was chiefly associated with Francis Galton's rather vague "Ancestral Law of Heredity". See Galton 1897 and 1898 and Charnley and Radick 2010.
[58] Biffen 1912c: 429.

But if it was not happening in Biffen's wheat fields on the experimental farm, there was, despite Biffen's claims, plenty of troubling rogueing happening in general cultivation. In order to quell this suggestion that this was a sign of reversion Biffen collected rogues from farmers and tested them by growing them into adult plants. From these results he concluded:

> The commonest cause of "reversion" in ordinary farming practice is failure to take sufficient care to keep stocks true to type. Probably 99 per cent. of the "rogues" found in crops described as "reverting" can be recognised immediately as commonly cultivated wheats.[59]

In one of Bateson's most important Mendelian books, *Mendel's Principles of Heredity,* published in 1909, Bateson indicated that he shared this view with Biffen:

> The rogue-plants may be of various kinds ... the guess may even be hazarded with some confidence that in numerous examples the cause of impurity in seed-crops will often be found to be nothing more recondite than an unsuspected admixture of another variety.[60]

As Biffen told the members of the National Institute of Agricultural Botany some years later, "There is no difficulty in fixing these types; so-called cases of reversion are traceable to mixture of stocks".[61] These beliefs led Biffen to a longstanding concern over seed stocks and their purity. To Biffen's mind, if only pure, certified

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[59] Biffen and Engledow 1926: 41.
[60] Bateson 1909: 292.
[61] Biffen 1922b: 45. Johannsen was made an honorary foreign member of the NIAB fellowship in the same year.

seed stocks could be created, the rogues would disappear. This stance as we will see in much more detail in the next chapter, essentially made rogues a non-theoretical, practical issue.

Biffen's ability as a theoretician has been doubted on several occasions, by peers and later by historians.[62] But Biffen's work was not entirely devoid of theoretical analysis. Some of his most theoretical work was on the problem of rogues. In 1914 he wrote to Bateson to tell him about two new types of rogues:

> Yesterday I heard of a case which seems [?] parallel to your rogue peas one of the spring sown Tares known as "[Gore] Tares" occasionally throws a plant or two with round white seeds. These I understand differ from any other tares grown here. The particular "strain" is grown in the Cotswolds & the natives spot it by looking for the pea-seed.
>
> My Informant is getting samples for me.
>
> Here's another repulsion case for you in wheat.
>
> Smooth Black X Rough White give in the f2 –
>
> Rough Black – Rough White – Smooth Black – Smooth White in proportions fitting very closely to the 1:3:3:1 scheme. Putting the characters in the [other way] round gives a long long coupling. The f2 is practically all Rough Black & Smooth White the other two classes being represented by about 3 plants in 2000. Ugh! [63]

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[62] Palladino cites Bateson to Hardy, 12th July 1922, William Bateson Letters Collection, in which Bateson casts doubts on Biffen's knowledge of genetics and whether he should have been given the Darwin Medal by the Royal Society, Palladino 2002: 46, as well as Edgar Anderson 1952: 77, who in turn relates an anecdote from Vavilov..

[63] Biffen to Bateson 4th March 1914, Bateson Letters Collection, 3269. Coupling was a term used generically for unusual ratios caused when factors seemed to associate. For an early case study of coupling explained by repulsion see Pellew 1913, on repulsion see also Gregory 1911: 124-130.

In the following year these thoughts became the basis for a short paper in Bateson's *Journal of Genetics*, Biffen's only contribution to the journal, "The Suppression of Characters on Crossing". Biffen suggested two further possibilities for bringing rogues into the Mendelian scheme. The example given of repulsion between characters in his letter to Bateson, he now explained on the basis of suppression. The idea was that factors could interfere with each other when present in the same individual. Biffen also invoked the concept of multi-factorial inheritance as a third possible cause for the ratio upsetting rogues:

> [R]ed-grained varieties crossed together frequently produce white-grained forms even though the parents breed perfectly true to their red colour. In the commonest cases the ratio of red to white is as 15 : 1. The ratio has been thoroughly established by Howard. Nilsson-Ehle, who first called attention to the fact, has suggested that it is due to there being various red-producing factors.[64]

Although highly speculative, these brief statements from Biffen on rogues, suggest the range of interpretations available for explaining rogues after the first decades of the century as Mendelism became much more sophisticated in dealing with problematic results.

Developments in Biffen's thinking were linked to his interpretation of pure line theory. If rogues were either a non-theoretical problem, caused by seed stock impurity, or at worst a unique case to be explained by the postulation of ancillary theories such as coupling, repulsion or suppression, then this was good news for pure line theory, and especially the claim many derived from Johannsen's work, that

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[64] Biffen 1916a: 226.

pure lines were immutable. As Biffen put it in 1924 while speaking to the Farmers Club in London:

> The belief that by "adding together successive small variations," as the exponents of selection have it, it is possible to effect improvements in any direction has been shown to be incorrect. It is now admitted by most plant-breeders that all the process of selection can do is to isolate existing forms from a mixture, and that once these have been picked out and obtained in a stable condition further selection is powerless to alter them. In fact, they are, as far as human efforts go, immutable.[65]

This was a point that he emphasised two years later in a monograph on his activities, co-authored with Engledow and produced by the Ministry of Agriculture:

> [T]he view is now general that the plant – as long as it is self-fertilized or pollinated by a plant similar to itself – is, as far as human efforts go, unchangeable. To use a modern term, a stock of any wheat uncontaminated by admixture with other sorts is a "pure line".[66]

For Biffen then, rogues held no significant challenge to Mendelism or pure line theory. Darwinian programs, based on selection, were ineffective after an initial "clearing of house" stage in which mixtures of varieties were disassociated. Biffen's non-theoretical treatment of rogues is explored in more detail in the next chapter,

[65] Biffen 1924a: 7.
[66] Biffen and Engledow 1926: 8.

this chapter's focus turns now William Bateson, who was working on rogues over at the John Innes.

2.2.3 Bateson's Rogue Peas and the Physiological Solution

Sometime after he took up the directorship of the John Innes Horticultural Research Institution, Bateson was approached by his good friends the Suttons of the internationally renowned, eponymously named nursery. The Suttons had found that there was a tendency in their high class garden peas to produce (or throw) rogues, see figure 2.5.[67] Staff at the institute began to work on producing rogue-free strains of peas in three commercial varieties. The first rogue-free strain was grown in bulk in 1915 and bought by Suttons.[68] Alongside these productive efforts, Bateson launched an extended research program into the cause of the rogues. Working with Caroline Pellew (see figure 2.6) he soon found that his initial explanations of heterozygosity or impure seed stocks could not explain rogue peas in varieties which the Sutton's had developed, not least because they were in correspondence with Bateson, and presumably following his instructions for avoiding this problem.[69] These were rogues which did not seem to be due the usual scepticism Bateson felt towards such cases. As he said in his Silliman Memorial Lecture at Yale, "The literature of horticulture for example abounds in cases alleged, but I do not think

[67] The Suttons often supplied the JI with their unusual plants, see also Pellew and Sverdrup 1923: 125, for an account of another type of rogue pea handed to the JI.
[68] See "1911 Research on Rogue Peas Begins", <http://www.jic.ac.uk/centenary/timeline/1910s.html> [accessed 10 August 2011].
[69] Biographical details on Pellew are sketchy but there is a brief outline of her career at the JI's website; < http://www.jic.ac.uk/centenary/timeline/info/pellew.htm> [accessed 10 August 2011].

Figure 2.5 A Rogue Pea. The narrow leaves indicate this cutting is from a rogue. Image reproduced from John Innes Archives courtesy of the John Innes Foundation.

Figure 2.6 Caroline Pellew (1882-[1963]). Cartoon of Caroline Pellew by Dorothy M. Cayley. John Innes Archives courtesy of the John Innes Foundation.

anyone can produce an illustration quite free from doubt".[70] Most of the time rogues, Bateson still believed to be "introduced by accident", or the result of "a cross with a pre-existing dominant", or the "meeting of complementary factors".[71] However, sometime after 1911, when the Suttons had passed him their unusual batch of rogue peas, Bateson came to believe that there might be at least three types of rogues, some caused by dissimilar unions, some by mixing of seed, and some caused by another more mysterious process. Bateson started publishing on the mysterious rogues with Caroline Pellew in 1914. Over the next five years his work with Pellew made several appearances at the Royal Society and in *Nature* and Bateson's own *Journal of Genetics*.[72] In 1915, in perhaps the fullest yet most tentative account Bateson published with Pellew, the pair outlined the problem like this:

> The term "rogue" is applied by English seed growers to any plants in a crop which do not come true to the variety sown. … When peas are grown for seed on a commercial scale it will be readily understood that untrue plants are introduced in various ways, mixture, crossing by insects, and the persistent recurrence of a recessive form being the most obvious sources of such plants … but the facts preclude the supposition that the special rogues with which we are here concerned are introduced either by mixture or crossing, nor can they be regarded as recessives coming from a heterozygote.[73]

[70] Bateson 1913: 89.
[71] Bateson 1913: 89.
[72] See Bateson and Pellew 1915 (which mentions the Telephone and Telegraph varieties, discussed in chapter 3, as being particularly rogue prone), a report of this paper in *Nature*, "Notes" 1915, Bateson and Pellew 1916 (also reprinted in part in *Nature*, "Societies and Academies: Royal Society" 1916) and Bateson and Pellew 1920.
[73] Bateson and Pellew 1915: 14.

The discussion section of the paper was prefaced with the following warning, "The general course of the phenomena is quite unlike anything with which we are familiar in ordinary Mendelian inheritance".[74]

These mysterious rogues had curved pods, narrow leaves and a bitter taste, see figure 2.4, unlike their straight poded, broad leaved, sweet tasting "type" counterparts. Bateson and Pellew initially offered a physiological explanation of rogues that came in two parts. The first was that plants which produced rogues were "mosaics"; that is, their cells were of different types in different areas of the plant.[75] The rogues arose from rogue cells, hidden against a background of normal ones in the type plants. The other concept they invoked was of somatic segregation, "which prevents the type-elements from reaching the germ cells" in crosses between rogues and types which produced more rogues.[76] By moving to the cellular scale Bateson and Pellew avoided the possibility that gametic impurity might be to blame. Plants might be mixtures of cells but the cells themselves were purely rogue or type in terms of the factors they contained.

Observing that some plants were normal at the base but rogue-like at the top, and that these different areas of the plant produced types and rogues respectively, Bateson and Pellew, referencing Biffen's work, believed this was further evidence for a physiological cause of rogues. Normal type gametes, they inferred, were left behind in the lower parts of the plant as it grew upwards, allowing rogue like features that would normally be masked to be expressed. In other words the type-elements were segregated at the base of the plant from the rogue-elements which

[74] Bateson and Pellew 1915: 30.
[75] Mosaicism is responsible for the different patches of colour on a calico cat, the differently coloured patches result from genotypically distinct cells.
[76] On the possibility of different types of segregation see Pellew 1916.

carried on growing upwards. Bateson and Pellew drew a rather startling inference from this physiological explanation:

> The persistent recurrence of rogues among the offspring of types must indicate some liability to an error in cell division. ... It was a common place of practical breeders and of conventional evolutionists that when selection is suspended, a breed "degenerates". This doctrine, promulgated, as it commonly was, without any reservation as to crossing or reference to critical purity of line is fallacious as an expression of physiological truths however much the objective consequences may seem to fulfil the prophecy. In the present example the popular conception of degeneration is precisely realised. So far as we know it [is] unique.[77]

The idea of the need for continual selection to guard against regression was anathema to Bateson's anti-Darwinian thinking, which like Biffen's, tended to go against the efficaciousness of selection, based on a Johannsenian understanding of pure lines. Perhaps this is why the final sentence of the paper tries to limit the damage by claiming this phenomenon is "unique" in rogue peas.

By 1925, just a year before his sudden and unexpected death, Bateson seems to have become remarkably sanguine about the power of Mendelism to deliver on some of his earlier promises to eradicate rogues and produce permanently fixed types. In response to an inquiry from Biffen's star student, Engledow, he gave the following words of reassurance to the younger man, "I see no occasion for disappointment in the fact that unfixable wheat remains a problem". Indeed the rogues might even be a good thing, as Bateson continued, "Probably the elucidation

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77 Bateson and Pellew 1920: 195.

of some of these queer phenomena will give us our next move. I would rather be disappointed if they prove amenable to the common factorial schemes".[78] In other words, in just under a quarter of a century, rogues had ceased to be a critical problem. By 1925 they were, instead, interesting starting points for further investigation. Bateson had accomplished the theoretical equivalent of Balls' statistical exclusion of the rogues.

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This chapter gives only a small sample of the work conducted by early British Mendelians on rogues, and there were of course additional workers in other countries looking at similar subjects. There were also a whole range of (often ambiguously defined) words to describe deviations of varietal type. Rogues were a small part of a large family that included sports, reversions, atavisms and mutations. Hopefully with more study of these subjects, we can extend our picture of the importance of purity in the early history of genetics through analysing the deployment of these terms in situations of presumed impurity.

During the expansion of agricultural science and Mendelism, Mendelism took on the problem of rogues, an essentially agricultural problem. The foregoing discussion sheds light on the relationship between these two areas, undermining the sceptical view that these might have been simultaneous but unrelated developments. The influence seems to have been deep. In the agricultural sector there is an emphasis on purity and uniformity which has lasted almost a century and is still largely on-going. Approaching the development of Mendelian thinking by following one of the key problems it sought to resolve also gives us insights into the wider

[78] Engledow to Bateson, 29th May [1925], Bateson Letters Collection, 2990.

development of British Mendelism. It has been suggested that during the 1920s the centre of genetic inquiry moved from Britain to America following the explication of chromosome theory at Thomas Morgan’s fly laboratory. Several reasons for this shift have been suggested, chief among them Bateson’s refusal to accept chromosome theory.[79] This chapter offers another possible factor to consider, the British Mendelians’ fixation on the problem of rogues was at its height just at the moment when chromosome theory began its ascendancy. The British research effort’s focus on rogues suggests a somewhat tighter linkage to the problems of its agricultural context than in Morgan’s Fly Room. However, while this was possibly a more constrained research program, it was not a less important one.

These debates also illustrate the development of Mendelian thinking from a revolutionary science to a normal science situation. In the years after 1901 Mendelian claims were bold and extensive, based on a belief in the ability to reduce complex phenomena to an understandable scheme. After two and a half decades of research, the will to reduction was an essential feature of Mendelian thinking. Further complications were now an extension of this essential truth, which only became distorted by exception, not the rule. Such exceptions could be dealt with by ancillary theses such as repulsion or suppression. Accounts of the theory given by system builders nearly always started with this essential truth, complications or extensions, were naturally brought up after several pages, once most readers’ attentions might well have wavered. Arguably, the reductionistic “gametically pure unit factors direct organismal characters” description of the theory became a component in the system, interacting with the theory itself and the other components it interacted with.

[79] Olby 1989a: 508-510.

Having seen the Mendelian tendency to reduction form, and its subsequent struggles with empirical data which required further explanation we now turn to that other fundamentally Mendelian disposition; to construction. In the next chapter we will see how a belief in reduction aided Mendelian breeders as they tried to build up new plant varieties. Rogues, which have been discussed here as a theoretical problem, also plagued these practical efforts and induced Mendelian system builders to new practical responses.

Chapter 3

Managing New Mendelian Varieties: From Experimental Plot to Market (and Back Again)

> The actual methods of plant breeding are now fairly well-established. Preliminary difficulties have been overcome, and for the most part the hybridising work is a matter of routine. The farmer appreciates, as a rule, the broad principles involved. He is becoming accustomed to talk of Mendelism and inherited "factors," and understands that by scientific methods of breeding it may be possible to combine in one variety several desirable qualities, and to introduce that variety to agriculture as an improvement.[1]

IF THE BRITISH FARMER in the 1920s was becoming accustomed to talk of Mendelism, unit-factors, and the scientific methods of breeding, it was in some measure because of the work of Biffen. As we saw in chapter 1, Biffen was a key Mendelian, one of the system builders who helped put in place many important institutional components of the architecture that stabilised Mendelian theory in the early twentieth century. His conceptual innovations helped stabilise Mendelism, despite the problems raised by rogues. In understanding the rise of Mendelism, an analysis of the production and marketing of Biffen's new varieties is just as important. Their success, and in particular that of a wheat variety called Yeoman, demonstrated the utility of Mendelism to agriculture.[2] But as we will see, Yeoman was neither easy to produce, nor instantly successful. Its superiority and reliability had to be argued for through skilful promotion and distribution. Yeoman was

[1] Wilkins 1926: 50.
[2] Throughout this thesis varietal names are capitalised.

promoted as Biffen's creation; the product of his application of Mendelism to breeding. In many of the places where this promotional work was done, Biffen, Yeoman, and Mendelism became firmly associated with each other.

Biffen's varieties were actively promoted to encourage farmers to buy them; at the same time they interacted with the forums in which they were marketed. They were distributed by another group of Mendelian organisations, again interacting with them along the way. The style of promotion and distribution itself interacted with Mendelism, and fused together the theory and the products, so their success reflected back on the theory. In the scientific community's forums and the general press read by scientists, the varieties' success was held up as vindicating the theory. While Biffen's varieties did not make money for Biffen directly, their promotion and distribution helped him protect the "credit stream" that he and Mendelism received from the varieties' success. This was an important credit stream indeed – even Bateson came to see the importance of these products. In 1907, Bateson told the audience of the Royal Horticultural Society's Third Conference on Plant Breeding and Hybridisation:

> The science of heredity must be pursued in the same spirit astronomers pursue their science... what economic truth does astronomy teach us? Why, that the sun never sets on the British Empire! Any other? No![3]

Yet after one year at the John Innes, Bateson had changed his mind drastically. He told the agricultural subsection of the British Association for the Advancement of Science in 1911, "If we are to progress fast there must be no separation made

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[3] Bateson 1907a: 77.

between pure and applied sciences".[4] Tracking the nature of these interactions between Mendelism, Biffen, Mendelian varieties and their promotion and distribution reveals the workings of a system for the varieties' production and release.

This chapter is about Mendelian products going out into the world: the Mendelian innovation process. It begins with a brief introduction to the context in which plant breeders of the period protected their new innovations, the moral economy of plant breeding. What exactly did Mendelism bring to this context? To answer this question, the next part of the chapter gives a demonstration of Mendelism applied to breeding as taught at the time. The focus then closes in on Biffen's views about how Mendelism changed plant breeding and provided a means to conceptualise what was happening in breeders' crosses, including Biffen's own plant breeding work and especially the production of Yeoman. The next part of the chapter demonstrates that when Yeoman was ready to leave the experimental plot and be used in the field, Biffen and his supporters marketed the variety to convince farmers to buy it. In this marketing they took on the tropes of promotion already established in the moral economy of plant breeding. When Yeoman was in use the changing nature of market regulation, enforced by the National Institute of Agricultural Botany, helped Biffen attempt to solve Yeoman's own rogue problem that, as we saw in the last chapter, was also a problem for the Mendelian account of heredity.

The next part of this thesis looks much more closely at what was supposed to be better about individual Mendelian varieties, and how they performed on those terms. In this chapter the focus is on the production and distribution of Mendelian

[4] Bateson 1912: 27, for more on Bateson's views about science and practice see Punnett 1952: 343-344 and Radick 2011.

varieties. The aim is to follow the process by which Mendelian varieties were made, advertised and sold in an increasingly integrated system. However, in order to get a sense of the operation of the moral and market economies of plant breeding this chapter starts with a prelude. The following incident, which was well known to Mendelians, clearly illustrates the context into which Biffen's varieties were launched. The incident involves two new varieties of pea, Mendel's own experimental material, one called Telephone and the other Telegraph.

3.0 Prelude: The Moral Economy of Plant Breeding

One important and previously overlooked context in which the relationship between science and plant breeding mattered was a moral economy of plant breeding. The term "moral economy" as used here is drawn from the original use by social historian E. P. Thompson.[5] For Thompson, a moral economy described an alternative to a market economy when it came to setting the price of corn (in the British sense). This distinction was identified by Thompson in order to provide analytic depth to the actions of the mob in eighteenth and early-nineteenth century Britain. Where other historians have described spasmodic riots caused by hunger alone, Thompson traces a much richer lineage to the actions of the mob. When groups of people gathered to demand corn to make bread, at reasonable prices, they were surprisingly organised and disciplined. Their actions often fitted a definite pattern; one which included the moral idea that the basic necessities of life should not be the objects of profiteering. The disappearance of this tradition and the mob's

[5] E. P. Thompson 1971.

ability to demand a set price *on moral grounds*, came, for Thompson, with the ascendency of the market economy, championed by the proponents of Adam Smith.[6]

In the years since its coinage, the term moral economy has been adopted by historians of science to describe the spheres in which the value of certain objects unavailable in the market economy are set. Such goods include intangible ideals such as empiricism, objectivity and accuracy and, furthermore, means of regulating the relationships between scientists.[7] How can this notion help us better understand the role of science in making British plant breeding in the long nineteenth century profitable? In a context without patents or any sort of formal intellectual property regime, the value of breeders' varieties was gauged by an intricate system of publicity, shows and medals, reported on by a specialised press, and hosted by learned societies which for a significant part, operated outside of the market economy.[8] Breeders' commercial strategies were codified by the morals of the plant breeding community in which they operated. This community was very big, and included a mixture of professionals and amateurs with no clean differentiation of power between them. However, the moral economy was also not absolute, as breeders became more commercialised they increasingly operated in the market economy and the concerns of their fellow breeders were less important. Part of the moral economy of plant breeding was the interaction between plant breeders and naturalists such as Charles Darwin and Joseph Hooker in the nineteenth century, and

[6] E. P. Thompson 1971: 89-90.
[7] See Gooday 2004: esp. 23-30 for an overview of the use of this notion in history of science circles. For even more on moral economies in science see Daston 1995 and Kohler 1993, 1994 and 1999. See also Palladino 1996c. The notion of moral economy employed here lies closer to Thompson and in between Kohler and Daston's uses of the concept.
[8] For more on breeders views on intellectual property see "Copyright for the Raisers of Novelties" 1907 and for the historical development of plant breeders rights in Britain see Rangnekar 2000. For an alternative view of the commercial aspects of early genetics see Thurtle 2007 and especially this author's thoughts on "the space of flows".

as we will see, these interactions continued into the 1920s with the rise of the professional plant breeding scientist.

One particular incident towards the end of the nineteenth century illustrates even more clearly the operation of a moral economy that formed around plant breeding.[9] In 1878 James Carter & Co., a large and well established seed firm, introduced a new pea variety; Telephone, that they claimed was a single selection from the older variety Telegraph.[10] On June 27th Telephone was issued a first class certificate by the Royal Horticultural Society's Fruit Committee at Cheswick.[11] In December Carter's took out an advert in the *Gardeners' Chronicle*. Under the title, "Sterling Novelties", Carters' advert proudly announced the variety's first class certificate received at the RHS's, "crucial trial" at Cheswick.[12] A three quarter page illustration of a pod of "Carters' Telephone", as it was ubiquitously known, accompanied the text. In Carters' *Vade Mecum* catalogue, published in the following year, Telephone was advertised with no less than three mentions of its first class status and glowing testimonials from several gardeners, see figure 3.1.[13] Carters' status as suppliers of seed to the Queen was also prominently displayed on the front cover of the catalogue as was their award of five gold medals at the Paris Exhibition. And Carter's also offered their own cash prizes for outstanding samples of their varieties displayed at the RHS's shows. In 1879 Carter's offered a cash prize to growers for

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[9] This case study was also particularly important to Mendelians and their main intellectual rivals in Britain, the Biometricians, who claimed that the dispute over Telephone undermined the theoretical integrity of the Mendelian hybridisation discussed in section four. See Charnley and Radick 2010, forthcoming and on the moral economy of plant breeding Charnley forthcoming a.

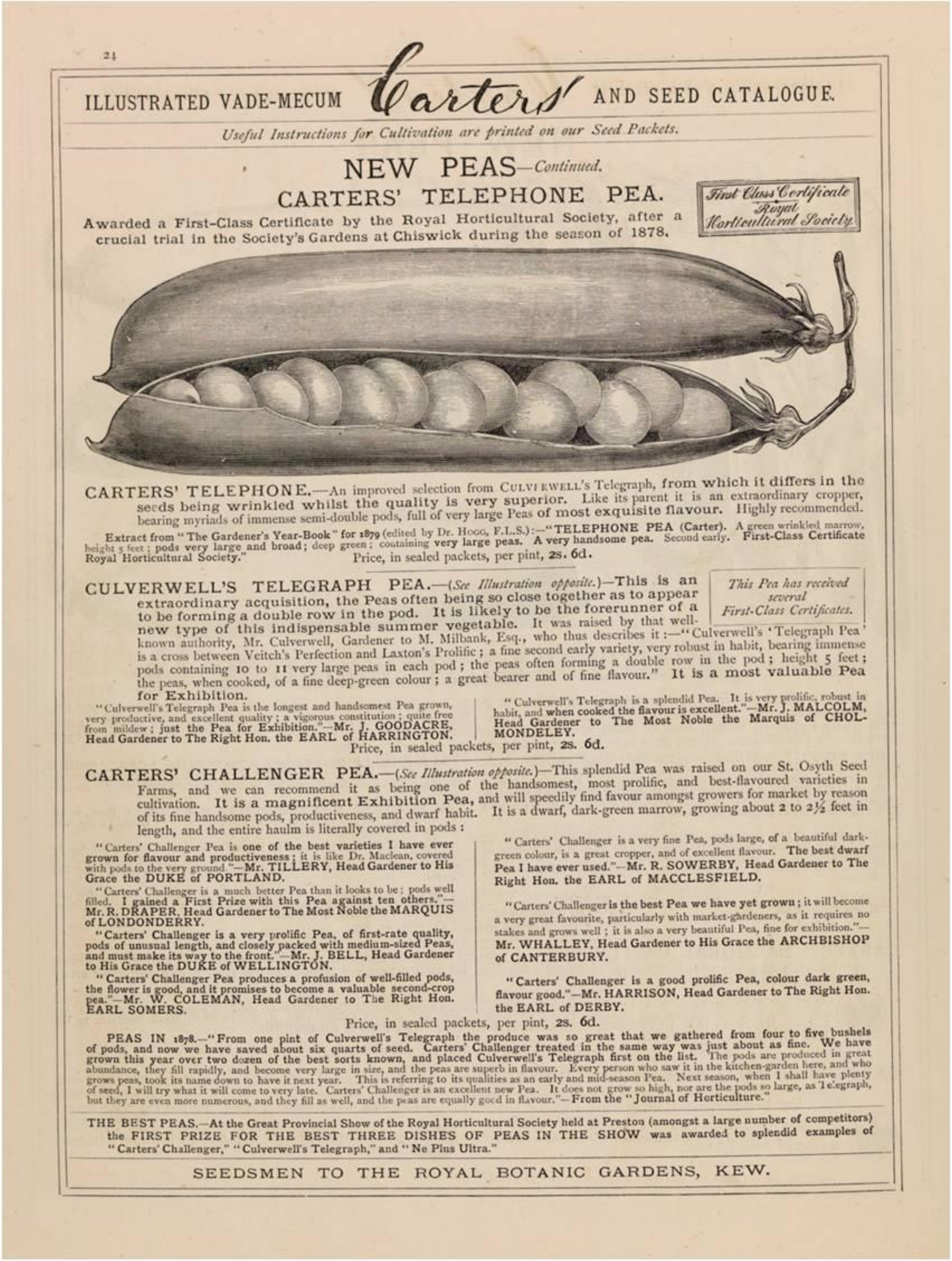
[10] I follow Carters' use of apostrophes in referring to the company's products as "Carters'". Elsewhere I use the standard history of economics notation of Carter and Co. being abbreviated to "Carter's" to refer to the company.

[11] "Awards of the Fruit Committee at the Cheswick Trials, 1878" 1879: xciv.

[12] "Sterling Novelties", *Gardeners' Chronicle* (28th December 1878), 825.

[13] Carter's 1879: 21.

outstanding samples of Telephone. This display of mammoth samples was great advertising.



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ILLUSTRATED VADE-MECUM *Carters'* AND SEED CATALOGUE.

Useful Instructions for Cultivation are printed on our Seed Packets.

NEW PEAS—*Continued.*

CARTERS' TELEPHONE PEA.

First Class Certificate Royal Horticultural Society

Awarded a First-Class Certificate by the Royal Horticultural Society, after a crucial trial in the Society's Gardens at Chiswick during the season of 1878.

CARTERS' TELEPHONE.—An improved selection from CULVERWELL'S Telegraph, from which it differs in the seeds being wrinkled whilst the quality is very superior. Like its parent it is an extraordinary cropper, bearing myriads of immense semi-double pods, full of very large Peas of most exquisite flavour. Highly recommended.

Extract from "The Gardener's Year-Book" for 1879 (edited by Dr. HOGG, F.L.S.):—"TELEPHONE PEA (Carter). A green wrinkled marrow, height 5 feet; pods very large and broad; deep green; containing very large peas. A very handsome pea. Second early. First-Class Certificate Royal Horticultural Society."

Price, in sealed packets, per pint, 2s. 6d.

CULVERWELL'S TELEGRAPH PEA.—(*See Illustration opposite.*)—This is an extraordinary acquisition, the Peas often being so close together as to appear to be forming a double row in the pod. It is likely to be the forerunner of a new type of this indispensable summer vegetable. It was raised by that well-known authority, Mr. Culverwell, Gardener to M. Milbank, Esq., who thus describes it:—"Culverwell's 'Telegraph Pea' is a cross between Veitch's Perfection and Laxton's Prolific; a fine second early variety, very robust in habit, bearing immense pods containing 10 to 11 very large peas in each pod; the peas often forming a double row in the pod; height 5 feet; the peas, when cooked, of a fine deep-green colour; a great bearer and of fine flavour." It is a most valuable Pea for Exhibition.

This Pea has received several First-Class Certificates.

"Culverwell's Telegraph Pea is the longest and handsomest Pea grown, very productive, and excellent quality; a vigorous constitution; quite free from mildew; just the Pea for Exhibition."—Mr. J. GOODACRE, Head Gardener to The Right Hon. the EARL of HARRINGTON.

"Culverwell's Telegraph is a splendid Pea. It is very prolific, robust in habit, and when cooked the flavour is excellent."—Mr. J. MALCOLM, Head Gardener to The Most Noble the Marquis of CHOLMONDELEY.

Price, in sealed packets, per pint, 2s. 6d.

CARTERS' CHALLENGER PEA.—(*See Illustration opposite.*)—This splendid Pea was raised on our St. Osyth Seed Farms, and we can recommend it as being one of the handsomest, most prolific, and best-flavoured varieties in cultivation. It is a magnificent Exhibition Pea, and will speedily find favour amongst growers for market by reason of its fine handsome pods, productiveness, and dwarf habit. It is a dwarf, dark-green marrow, growing about 2 to 2½ feet in length, and the entire haulm is literally covered in pods:

"Carters' Challenger Pea is one of the best varieties I have ever grown for flavour and productiveness; it is like Dr. Maclean, covered with pods to the very ground."—Mr. TILLERY, Head Gardener to His Grace the DUKE of PORTLAND.

"Carters' Challenger is a much better Pea than it looks to be; pods well filled. I gained a First Prize with this Pea against ten others."—Mr. R. DRAPER, Head Gardener to The Most Noble the MARQUIS of LONDONDERRY.

"Carters' Challenger is a very prolific Pea, of first-rate quality, pods of unusual length, and closely packed with medium-sized Peas, and must make its way to the front."—Mr. J. BELL, Head Gardener to His Grace the DUKE of WELLINGTON.

"Carters' Challenger Pea produces a profusion of well-filled pods, the flower is good, and it promises to become a valuable second-crop pea."—Mr. W. COLEMAN, Head Gardener to The Right Hon. EARL SOMERS.

"Carters' Challenger is a very fine Pea, pods large, of a beautiful dark-green colour, is a great cropper, and of excellent flavour. The best dwarf Pea I have ever used."—Mr. R. SOWERBY, Head Gardener to The Right Hon. the EARL of MACCLESFIELD.

"Carters' Challenger is the best Pea we have yet grown; it will become a very great favourite, particularly with market-gardeners, as it requires no stakes and grows well; it is also a very beautiful Pea, fine for exhibition."—Mr. WHALLEY, Head Gardener to His Grace the ARCHBISHOP of CANTERBURY.

"Carters' Challenger is a good prolific Pea, colour dark green, flavour good."—Mr. HARRISON, Head Gardener to The Right Hon. the EARL of DERBY.

Price, in sealed packets, per pint, 2s. 6d.

PEAS IN 1878.—"From one pint of Culverwell's Telegraph the produce was so great that we gathered from four to five bushels of pods, and now we have saved about six quarts of seed. Carters' Challenger treated in the same way was just about as fine. We have grown this year over two dozen of the best sorts known, and placed Culverwell's Telegraph first on the list. The pods are produced in great abundance, they fill rapidly, and become very large in size, and the peas are superb in flavour. Every person who saw it in the kitchen-garden here, and who grows peas, took its name down to have it next year. This is referring to its qualities as an early and mid-season Pea. Next season, when I shall have plenty of seed, I will try what it will come to very late. Carters' Challenger is an excellent new Pea. It does not grow so high, nor are the pods so large, as Telegraph, but they are even more numerous, and they fill as well, and the peas are equally good in flavour."—From the "Journal of Horticulture."

THE BEST PEAS.—At the Great Provincial Show of the Royal Horticultural Society held at Preston (amongst a large number of competitors) the FIRST PRIZE FOR THE BEST THREE DISHES OF PEAS IN THE SHOW was awarded to splendid examples of "Carters' Challenger," "Culverwell's Telegraph," and "Ne Plus Ultra."

SEEDSMEN TO THE ROYAL BOTANIC GARDENS, KEW.

Figure 3.1 Advert for Telephone and Telegraph peas taken from Carters' *Illustrated Vade Mecum and Seed Catalogue,* 1879. The catalogue was also intended to function as a handbook, a *vade mecum,* which translates literally as 'go with me'. Image supplied by the RHS, Lindley Library. © RHS.

Not everyone was happy with the arrival of Carters' Telephone. Sometime in the last week of January 1879, a Yorkshire breeder William Culverwell (private gardener to one M. Milbank Esq.), wrote a letter to the *Gardeners' Chronicle* launching an attack on Carter's and their new pea. Culverwell was the originator of another variety, Telegraph, which he had produced by hybridisation between two other varieties; Daisy and Early Morn. In 1876 Carter's had purchased the stock of Telegraph from Culverwell for a high one off price and it was from this stock that they claimed one of their breeders had selected the new variety, Telephone, by a single selection. Culverwell claimed that Telephone was not a new variety, but merely the wrinkled peas selected from Telegraph, which gave both round and wrinkled peas. Culverwell felt that isolating the wrinkled peas from Telegraph would ultimately detract from the stock since the wrinkled peas were reckoned to be more desirable than the round ones. In this way Telegraph would eventually become an inferior sample of the same variety. Culverwell felt that if this were to happen, his reputation, as the originator of Telegraph, which was largely known as Culverwell's Telegraph, would diminish as the quality of Telegraph diminished. Meanwhile the quality of Carters' Telephone would increase. For Culverwell, then, it was above all his reputation as a breeder that was at stake.[14]

Conversely Carter's felt that the attack from Culverwell undermined their reputation. Their reply to Culverwell stated:

> We have never sought to disparage either Mr. Culverwell or his Telegraph Pea; they are we believe both good of their kind – both the man and the Pea,

[14] This letter is not present in RHS's copies of the *Gardeners' Chronicle* held in their Lindley Library, however its content can be inferred from later letters and Weldon: 1902.

> therefore we cannot understand why he should wish to disparage either us or our Telephone Pea.[15]

Several other gardeners weighed into the debate with letters, one, Mr. W. Iggulden, questioned Culverwell's claim on the basis of Telephone's RHS certificate, asking, "if they are synonymous how came the certificate of the Royal Horticultural Society to be awarded to Telephone?"[16]

The debate then shifted to another obvious locus of a breeder's reputation – his skill and knowledge of breeding. Carter's accused Culverwell of having a poor knowledge of hybridisation. Culverwell retaliated that the three years in which Carter's had owned Telegraph was never enough time to develop and bring a new variety to market. Culverwell even conducted his own "experiment", in which he repeated the process of selection he believed Carter's had used to create Telephone, or as he believed it to be, merely a stock of the best seeds selected from Telegraph.

Iggulden, Carter's and Culverwell each sent the editors of the *Chronicle* samples of seeds to prove their point. But as yet another gardener, Thomas Keetley observed, "Separate the wrinkled seed from the non wrinkled [*sic*] and in appearance you have two distinct Peas; sow them side by side and they will prove undoubtedly one and the same". The problem was that "comparison in ripe seed is no real test" because seed becomes more wrinkled with age.[17] Accordingly the *Chronicle* called a halt to the furore and refused to publish any more correspondence until the seed samples could be grown on. Fittingly the RHS's gardens at Chiswick

[15] Carter's, "Culverwell's Telegraph and Carter's Telephone Peas", *Gardeners' Chronicle* (1st February 1879), 148.
[16] William Iggulden, "Culverwell's Telegraph and Carter's Telephone Peas", *Gardeners' Chronicle* (1st February 1879), 148.
[17] Thomas Keetley, "Culverwell's Telegraph and Carter's Telephone Peas", *Gardeners' Chronicle* (8th Feb. 1879), 180.

(the site at which Telephone had first been trialled) was chosen as the site to perform comparative trials.

Finally, in August, the *Chronicle* published its verdict on the case: Culverwell was in the right; Telephone was not distinctively different from the stock of Telegraph, but was merely an isolated sample of its wrinkled peas. The *Chronicle's* verdict was this, "To Mr. Culverwell belongs the credit of raising and sending out Telegraph – an undoubtedly fine Pea, and it is to be hoped we shall hear no more of the name Telephone".[18] Credit was indeed the thing at the heart of this dispute. Reputations and credit seem to have been especially important as it was impossible to tell how good a seed was until after it was grown – seeds were essentially bought and sold on trust. Breeders' names, prizes and certificates invited gardeners to place their trust in new varieties.[19]

There was another device used by breeders like Carter's to protect their new innovations, the sacks, often times sealed, in which they supplied their seed direct to postal buyers (see figure 3.2 for Carters' own seal). These arrangements considerably reduced opportunities for tampering or relabeling of seed. Sacks, seals and direct postage were all part of a system designed to control the supply of seeds to defend against the type of accusation Culverwell was making to Carter's. At the time there was little effective legislation, so these types of strategies, coupled with promotion in the appropriate forums, and defence of criticisms was the best way to build and protect a variety's reputation. One surprising feature in the analysis that follows is just how widespread the appropriate forums for promoting new Mendelian varieties became. Mendelian wheat varieties were possibly the first ever to be released through the pages of the *Times.*

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[18] "Telegraph and Telephone Peas", *Gardeners' Chronicle* (2nd Aug. 1879), 146.
[19] For more on prizes and innovation at world fairs see Moser 2004.

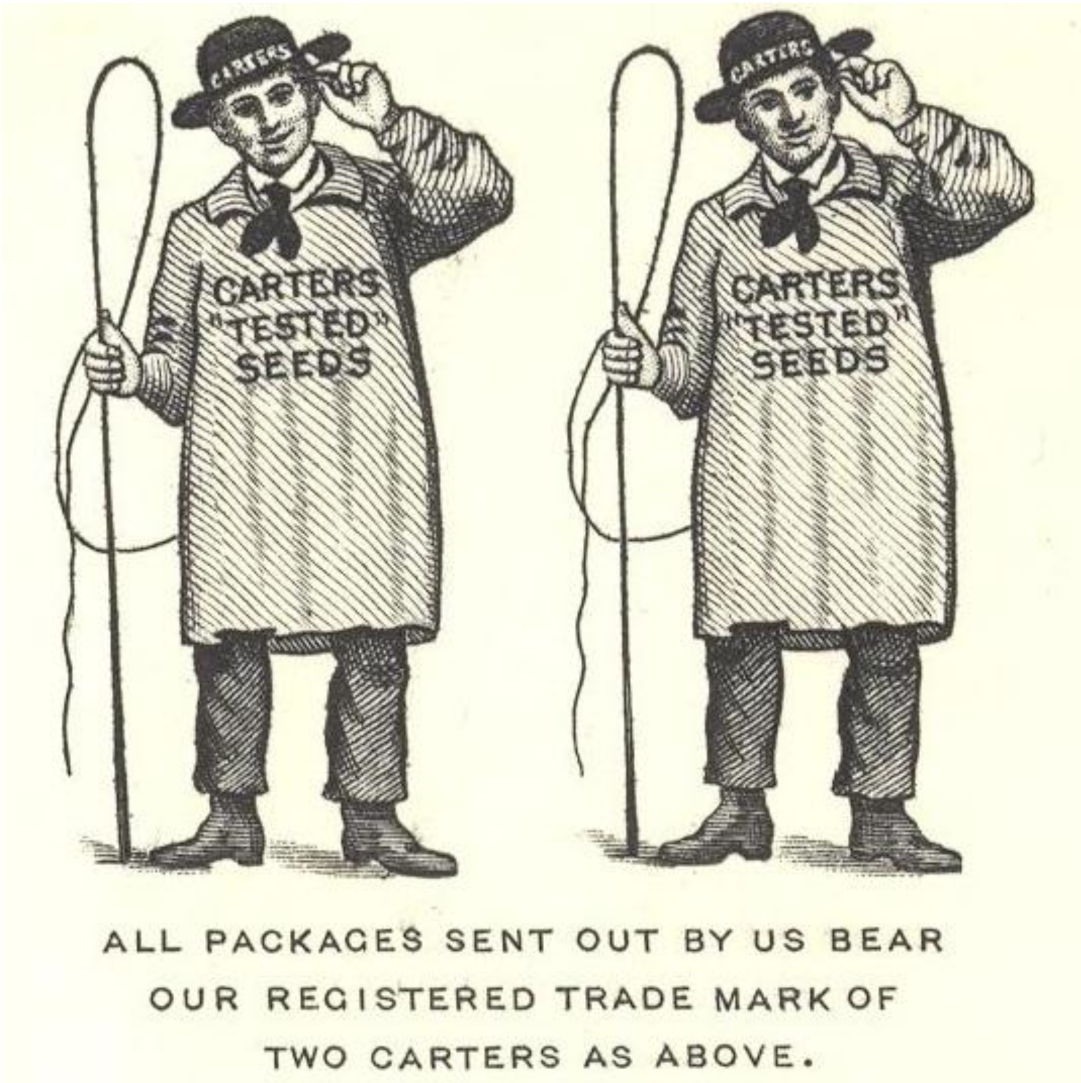

Figure 3.2 Seal for seed sacks used by Carter's at around the turn of the century. Weldon Papers, UCL. Image reproduced courtesy of UCL. Many thanks to Gregory Radick for finding this image and sharing it with me.

3.1 Mendelism and Wheat Breeding

3.1.1 Breeding and the Mendelian Discovery

Contemporary historians of science still find the question, "what exactly changed for breeders after Mendel's rediscovery?" a fruitful one. Despite Wilkins's claims, with which this chapter began, the relationship between Mendelism and breeding was a very complex one. Indeed, breeding and Mendelism had a very complicated relationship from the outset.[20] Breeding was an analytic tool for Gregor Mendel. For him hybridising varieties was a means to elucidating the underlying mode of inheritance. [21] The characters he observed from one generation to the next demonstrated an underlying pattern. By contrast, for later Mendelians – and especially Biffen – the mode of inheritance that Mendel's work described became a means of explaining breeding. Knowing how characters were inherited gave Mendelian breeders a way to transfer characters between varieties. Furthermore it gave them knowledge of how those characters were constituted once they had been transferred. Knowledge of what constituted those characters was essential because this information made it possible to tell if a new combination was stable and would breed true.

To demonstrate the power of Mendelism for breeding, consider the thought experiment produced in a 1911 textbook written by a contemporary of Biffen's – A. D. Darbishire. The cross between two pea varieties recounted by Darbishire, is intended to illustrate how characters can be passed between varieties.[22] The pea varieties that are crossed differ from each other in two characteristics and breed true for each. One has yellow round seeds  and the other wrinkled and green seeds . We can represent these types as RRYY (round and yellow) and rryy (wrinkled and

[20] For more on this relationship in the context of Cambridge University see Palladino 1993 and 1994.
[21] See Müller-Wille 2007b: 800.
[22] Darbishire derived this description from Mendel's work, I have changed the nomenclature and introduced a new illustration to make the cross easier to follow, but the characters described, and the use of a Punnett square are the same as those in Darbishire 1911: Ch. 13, "Theory to Account for the Results which Follow a Cross Involving Two Pairs of Characters". Biffen never actually expressed his crosses in print in the form of a Punnett square as shown in figure 1 – although he was acquainted with Reginald Punnett who was also at Cambridge University when he developed this type of annotation – Biffen would instead use long strings of letters to represent crosses, this was complicated further by his use of a single letter (A or B) to denote homozygous characters in the same style as Mendel even though the dual nature of what Biffen called "unit characters" (and Johannsen called genes in 1909) was recognised by Bateson who started to use the terms alleomorphs, heterozygote and homozygote in 1902. Although the treatment of the cross in figure 1 and the discussion is slightly anachronistic I hope it gives the reader a much clearer example to keep in mind during the discussion of Biffen's own work that follows.

green). R represents a round making factor which is dominant to wrinkled, r. Y represents a yellow making factor which is dominant to green, y. What happens if we cross these two peas together hoping to transfer one of their characters? How can we breed for a variety with wrinkled yellow seeds or round green ones? When the two parental strains are crossed together the progeny of the cross (the F1) will all look the same; round and yellow as these are the two dominant characters. However, instead of having the same RRYY combination of factors as their round yellow parent, the F1 have an RrYy combination of factors. When the F1 are crossed with each other (or self-fertilised) there are four possible gamete types which go into the cross (RY, Ry, ry, and rY) and sixteen possible offspring (the F2) shown in figure 3.2.

Amongst the F2 two new types of pea arise, a wrinkled yellow and a round green. But not all of the peas of the new types are the same. According to Darbishire, we can see that the rrYY and RRyy will respectively only ever produce rY and Ry gametes and only ever produce the new wrinkled yellow and round green types of offspring. However, to take the case of the new wrinkled yellow type, while rrYY will remain stable, rrYy – which looks wrinkled and yellow – might still produce ry gametes and so might produce wrinkled green offspring. The point of applying Mendelism to breeding for Darbishire was that if a wrinkled yellow pea produced only wrinkled yellow offspring over three generations of self-fertilization then the odds were that it was rrYY and would remain in the new wrinkled and yellow form forever. In breeders' terms the new combination of characters would be "fixed" from the F2 generation onwards, further rounds of breeding were merely used to confirm this fact. Without Mendelism a breeder who wanted a new wrinkled yellow variety might spend literally years working with a heterozygote rrYy wrinkled yellow pea in order to try to fix the new combination. At least occasionally, however, an rryy wrinkled green offspring would appear and the new type would seem to have "reverted" back to the wrinkled green appearance of the parental variety the breeder had started out with.[23]

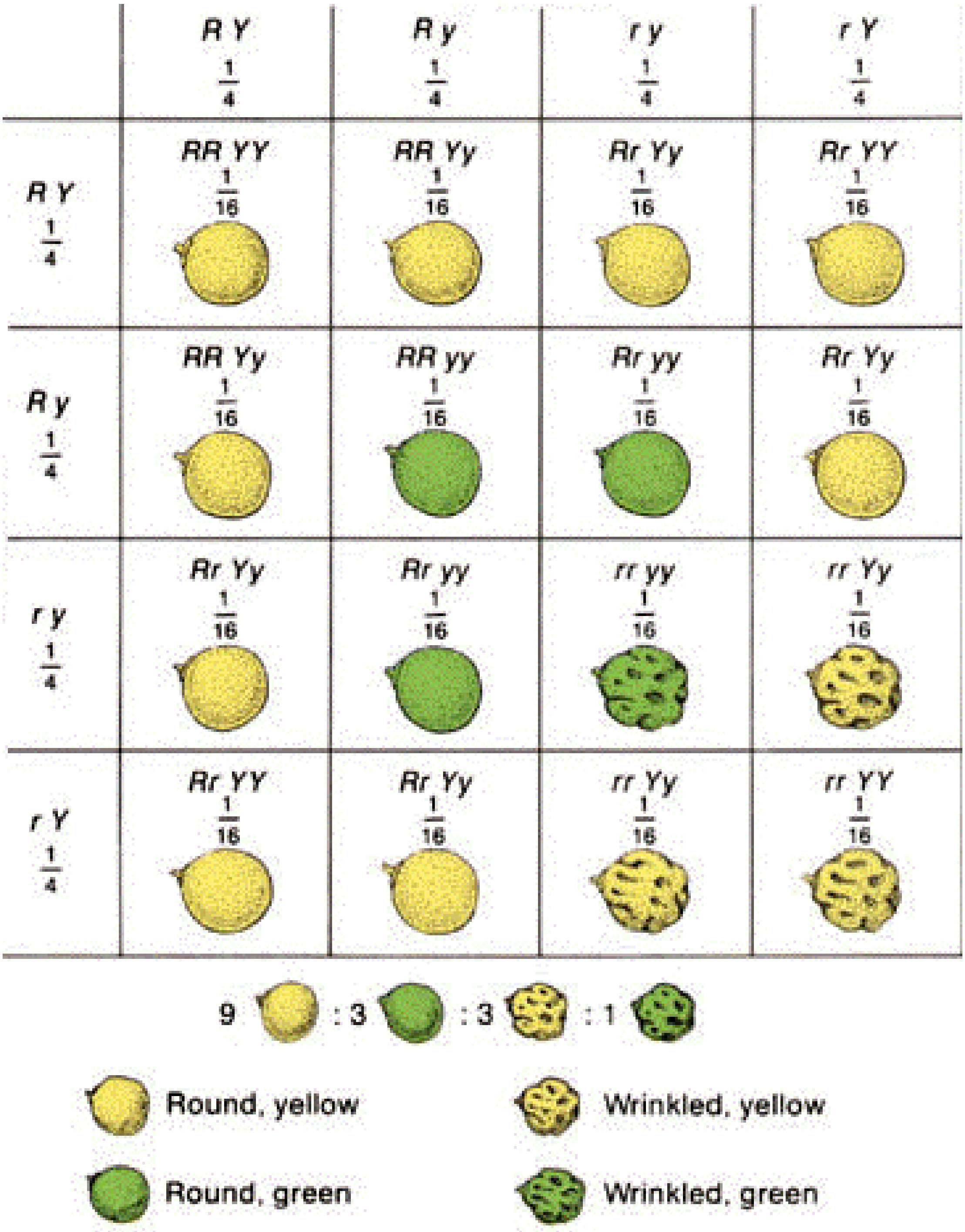

Figure 3.3 Two factor cross. Ratio of pea types in the F2 progeny of a hybrid (F1) resulting from a cross between a round and yellow seed producing variety of pea and a wrinkled and green seed producing variety of pea.

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[23] This is essentially the same heterozygote interpretation of rogues that we saw Bateson, Biffen and Balls offer of rogues in the previous chapter.

Viewed like this, working without Mendelism was like playing a lottery, in which the breeder only had a one in three chance (in this simple illustration) of picking the stable new rrYY type from amongst the other seemingly identical wrinkled yellow peas. Mendelians such as Darbishire and Biffen believed Mendelism revealed which of the new forms amongst the progeny of their hybrids were inherently fixed. They suggested that without the theory a breeder could do next-to-nothing to fix a variety apart from hoping that at some point in his successive rounds of breeding he would by chance produce a homozygote. Darbishire argued that if the breeder used ancestry – the pea's pedigree – to inform his choice as to which type to try to fix he was even more likely to pick the wrong one. But he continued:

> The breeder, according to the new principles, only requires a knowledge of the offspring, and he only needs this as an indication of the germinal contents of their parents; he *does* go beyond the characters of the offspring to the germinal contents of their parents, and bases his prediction on this, and not on the characters themselves.[24]

In other words by self-fertilising the progeny of the hybrid cross and looking forwards one could tell whether the plant they came from contained a heterozygous rrYy or a homozygous rrYY combination of factors. From this information the breeder would know whether or not he was working with a stable recombination of characters. According to Bateson the transformation this new way of explaining breeding would bring about was going to be radical one:

[24] Darbishire 1911: 243 (emphasis in original).

> The grower devotes much time and expense in keeping the rogues down, but the idea that they can be got rid of altogether does not generally occur to his mind. Nevertheless in many such cases Mendelian observation at once provides the means of carrying out this radical treatment with success.[25]

3.1.2 Biffen on Mendelian Plant Breeding

Biffen began exploring the possibility of applying Mendelism to the practice of plant breeding very swiftly after the "rediscovery" of Mendel's work in 1900. According to Engledow, he found "practical breeding irresistible", and "his ruling purpose" was "improving agricultural plants".[26] Viewing Biffen as the man who proved that Mendel's laws applied to wheat as well as to peas, and to physiological as well as morphological characters, misrepresents the vast body of his work after about 1905 which was actually more concerned with changing the wheat-growing industry. Producing new varieties for use by farmers was the central problem Biffen addressed for much of his career. His early conceptual work showed a wider applicability of Mendelian principles but it was no coincidence that he chose to extend the generalisation to a commercially valuable plant such as wheat, and its commercially important physiological characters such as disease resistance. In thirty-five years of research beginning in 1901, Biffen published a total of thirty-five papers on the application of Mendelism to plant breeding. His first and last were titled, "Wheat Breeding" and "The Thing We Call Wheat: Wheat Breeding", respectively.[27]

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[25] Bateson 1909: 292.
[26] Engledow 1950: 14, 19.
[27] Biffen 1904a and 1935.

In 1903 while speaking to the Cambridge Philosophical Society, Biffen recalled having begun crossing wheat in the summer of 1901, not just to test the wider applicability of Mendel's laws, but also "with the object of raising improved varieties from the point of view of the farmer".[28] Even in Biffen's most theoretically ambitious investigations, discussed in detail in chapter 2, his simultaneous concern with the economic application of Mendelism was obvious.[29] The 1903 talk at the Philosophical Society was published in its *Proceedings* in 1904, and Biffen published two more articles on his early experiments, one in *Nature* (1903), the other in the *Journal of the Royal Agricultural Society of England* (1904).[30] In 1905 the culmination of these experiments was published in Biffen's own *Journal of Agricultural Science*. This paper, as we have seen, detailed the mode of inheritance in wheat; Biffen used the paper to demonstrate that it was possible to breed disease resistance into wheat by Mendelian hybridisation. He started, however, by arguing that disease-resistant varieties would be economically beneficial to farmers.[31] After this report Biffen published several more along similar lines, and gave papers on his initial experiments in forums as diverse as the Royal Horticultural Society's meetings, the journal *Science Progress*, the supplements of the *Journal of the Board of Agriculture*, and the 1912 Conference of Agricultural Teachers at Cambridge.[32] Biffen energetically promoted the utility of Mendelism to breeding in general, and to plant breeding in particular in a surprising range of forums, over an extended period of time.[33]

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[28] Biffen 1904a: 279.
[29] William Bateson often publicly cited the importance of Biffen's theoretical work see for example Bateson 1912: 596. However Biffen's fullest analysis of inheritance in wheat, "Mendel's Laws of Inheritance and Wheat Breeding" begins with a lengthy discussion of the economics of wheat growing and milling, Biffen 1905: 4-6.
[30] Biffen 1903 and 1904b.
[31] Biffen 1905: 4-6.
[32] Biffen 1907a, 1907b, 1910a, 1912a and 1912b.
[33] For a discussion of the application of Mendelism to livestock breeding see Biffen 1906.

For Biffen, the essence of Mendelian breeding was the use of hybridisation to cross two varieties, in a predetermined manner, towards a definite goal. He phrased his method of hybridisation in relation to the work of a previous generation of hybridists, which he characterised in the 1905 *Journal of Agricultural Science* paper as un-predetermined and "haphazard".[34] For Biffen, Mendelism added the ability to transfer characters at will. If prior to the rediscovery of Mendelism, hybridising was – in words he borrowed from John Lindley – "a game of chance played between man and plants", Mendelism now held out "prospects of, so to speak, picking out the valuable characters from different varieties and building up an ideal type".[35] Biffen's relationship to older breeders is evocative of what social historian Samuel Hynes described as the "Edwardian turn of mind", caught simultaneously looking backwards with admiration and contempt to the Victorians and forwards, to the unknown, with optimism and trepidation.[36] While drawing upon the knowledge previous hybridists provided, Biffen also sought to distance his work from theirs. The pre-Mendelian hybridisation work of William Farrer in Australia, for instance, analysed in detail in chapter 5, received much attention from Biffen. He obviously admired Farrer's skill as a plant breeder but criticised his work because it lacked the Mendelian conceptual underpinning which informed Biffen's own crosses. As Biffen said in 1917 in a piece entitled "Systematised Plant Breeding":

> Farrer's success, however, and this is written in no spirit of depreciation, was due to his endless patience and that happy gift some breeders have acquired of recognising the merits of one individual plant amongst thousands and

[34] Biffen 1905: 8, 41.
[35] Biffen 1905: 6-8, the phrase, "A game of chance…" is quoted from Lindley, *Gardeners' Chronicle* (6th July 1844), 443.
[36] See Hynes 1968.

> selecting it as the basis of a new variety. In such work chance plays an important part, whilst the aim of the breeder today is to eliminate chance and work definitely for the end in view.[37]

Here Biffen only put at greater length a view announced in his Royal Horticultural Society's Masters Memorial Lecture in 1913: with William Bateson in the audience, "The days of chance results in plant-breeding are over".[38]

Biffen's work showed a wider applicability of Mendelism and did much to show how Mendelism might inform plant breeding but he added little to our understanding of heredity. In most of his breeding, as we shall see in the next section he simply did the crosses and picked out the most promising recombinant hybrids and self-fertilised them. But this simplicity was part of what Biffen thought Mendelism could do for breeding. It allowed him to identify the hybrid progeny of a cross that were a stable recombination of characters. In contrast to the continuous selection which Percival warned would be necessary, under a Mendelian scheme stable new combinations could be expected from the first cross. Moreover, Biffen (and Darbishire) believed that by self-fertilising the recombinant progeny of a hybrid he could check whether he had picked the individuals showing a stable new combination from amongst the offspring of his cross. He reported this view to the *Journal of the Royal Agricultural Society* in 1904, "the breeder must note, then, that external appearances are no guide to the purity of any individual that depends solely on its gametic condition; the one test is to breed from it".[39] Biffen was so certain of the power of this assay that he did not bother keeping records after the third generation believing "no further numbers are as a rule necessary, as the following

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[37] Biffen 1917: 147-8. For more on Biffen's treatment of Farrer's work see Chapter 5.
[38] Biffen 1913: 319.
[39] Biffen 1904b: 341.

generation [the F3] shows whether the individuals … will breed true or not".[40] If a variety was stable for a few generations then he assumed he had the homozygous form of the combination and that it would always be stable. After this Biffen might also perform selection to get the best of his F2 progeny but he saw this as a means of purifying, rather than improving, his varieties.

Biffen consistently asserted the futility of Darwinian selection to create new varieties. For Biffen, working under the Johannsenian belief that selection was ineffective in changing a variety, the method amounted to little more than selecting in the more mundane sense of choosing one parental variety from a mixture. Citing Wilhelm Johannsen and Hugo De Vries, Biffen in 1907 maintained that the most selection could do was isolate "the best types" from the heterogeneous group of types which were often lumped together under the same variety name.[41] Selection was, to Biffen at various points in his career, of limited use, "powerless", and a cause of, "discredit", to the plant breeding industry.[42] In 1926 Biffen even chastised Carters & Co., the seed firm with which this chapter began, in a thinly veiled attack on their use of "selection by our usual methods" to improve Yeoman, and produce, "Yeoman King", or "A's 'XYZ'", as he put it.[43] Selection, Biffen argued, might be of use to a limited extent, but it could not create a new combination of characters, or even change the existing characters of a stable variety. For Biffen, only hybridisation was capable of this, and it was only by applying Mendelism to

[40] Biffen 1906: 13.
[41] Biffen 1907b: 703. Biffen does not cite any specific work by De Vries or Johannsen in this piece and apart from some inconclusive small scale tests of the affectivity of selection in producing new wheat varieties (Biffen and Humphries 1907: 15) he did not make any effort to replicate the findings of their most significant works, De Vries 1901 or Johannsen 1903. For more detail on Johannsen and the futility of selection see Roll-Hansen 1978, for De Vries and mutation theory see Gould 2002.
[42] Biffen 1924a: 7.
[43] Biffen and Engledow 1926: 9. The catalogue containing the advert Biffen quotes, Carter's Tested Seeds 1923, includes Carters' claim that they had improved Yeoman by "re-selection". The catalogue is part of an extensive collection held at the Archives of the National Institute of Agricultural Botany.

hybridisation that one would be able to do this in a pre-planned manner and create fixed new varieties with certainty.

3.2 The Mendelian Innovation Process

3.2.1 Biffen the Mendelian Wheat Breeder

The remainder of this chapter will concentrate on Biffen's Yeoman and Yeoman II varieties. They are the most important to the argument here for three reasons. Firstly, they both made the news – or to be more accurate news was made about them – so there is far more evidence of their marketing, on which I base the arguments in the next section. Secondly, they were released before and after the establishment of the National Institute for Agricultural Botany which managed the marketing of Yeoman II with important consequences. Finally, they were respectively the most, and one of the least, commercially successful of Biffen's varieties so the numerous reports of Yeoman's success form the basis of my analysis of the Mendelian harvesting of success.

How did Biffen go about his breeding? To produce Yeoman, Biffen needed to find a parental variety which retained its quality when grown in England. Up until this point the common belief was that British weather conditions caused high quality foreign varieties to deteriorate when transplanted in to British soil.[44] Biffen and those he enjoined to help him grew quality wheats from Hungary, Russia, the USA, and Canada at locations across Britain in a series of variety trials.[45] A Canadian

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[44] There were various theories on this subject but the majority view was that it was the damp which caused this deterioration.

[45] These trials were conducted with the help of the British Seed Corn Association, the Home Grown Wheat Committee and the National Association of British and Irish Millers. For more on the history of these organisations and Biffen's involvement with them see chapter 1,

wheat variety called Red Fife was identified which was low yielding but retained its quality when grown in Britain over successive years. Biffen then set about crossing Red Fife with a high yielding English wheat variety called Browick. To prepare for the cross Biffen grew pure cultures of Browick and Red Fife by selecting and self-fertilising typical plants. He then crossed the varieties assuming the characters of quality and yield would behave in exactly the same way as pea shape and colour. In 1907 Biffen described how amongst the progeny of his Red Fife and Browick cross, "Ruthless selection is practiced, and unless wheats prove satisfactory in all features they are destroyed at once … so far about 40 types … have survived the ordeal".[46]

Despite Biffen's views on Mendelian breeding discussed in the previous section, his methods actually resembled those of pre-Mendelian breeders very closely. Biffen performed trials on varieties from around the world and picked the best of these to perform crosses. After crossing he picked the best of the hybrids to self-fertilise and from the progeny of these he once again selected the best to propagate. Biffen then kept records of his crosses by assigning each cross a number which was then attached to the hybrid progeny. Each individual plant was marked with a further number so 1-8 would denote the eight individuals resulting from the first cross. The characteristics of the individual plant would also be recorded alongside their identity.[47] In all of this work Biffen's skills in recognising promising individuals were crucial. Mendelism really only played a part in the last round of hybridisation, amongst the progeny of the F1 hybrids, when Biffen was checking the stability of their new combination of characters. In short Mendelism did not change Biffen's practices or diminish the level of skill required to breed wheat – it still took

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see also Punnett 1909: 78-84 and a later account from Biffen's student A. B. Bruce, Bruce and Hunter 1926: 41-42.

[46] Biffen 1907: 16.

[47] For more on the way these sorts of organisational and note taking practices can become epistemological constraints see Müller-Wille 2005.

at least nine years to get from the first crosses in 1907 to the release of Yeoman in 1916. Instead Mendelism informed Biffen's choices about what crosses to attempt and which individual plants to use for propagation. From the beginning of his work on Yeoman, Biffen, in public, believed the manner in which Mendelism informed his choices was absolute:

> Breeding has entered upon a definite stage, … order can be traced in a subject which hitherto has appeared chaotic. The breeder has now to recognise that new breeds can be built up with certainty by recombining characters.[48]

3.2.1 Marketing the New Varieties: The Case of Yeoman

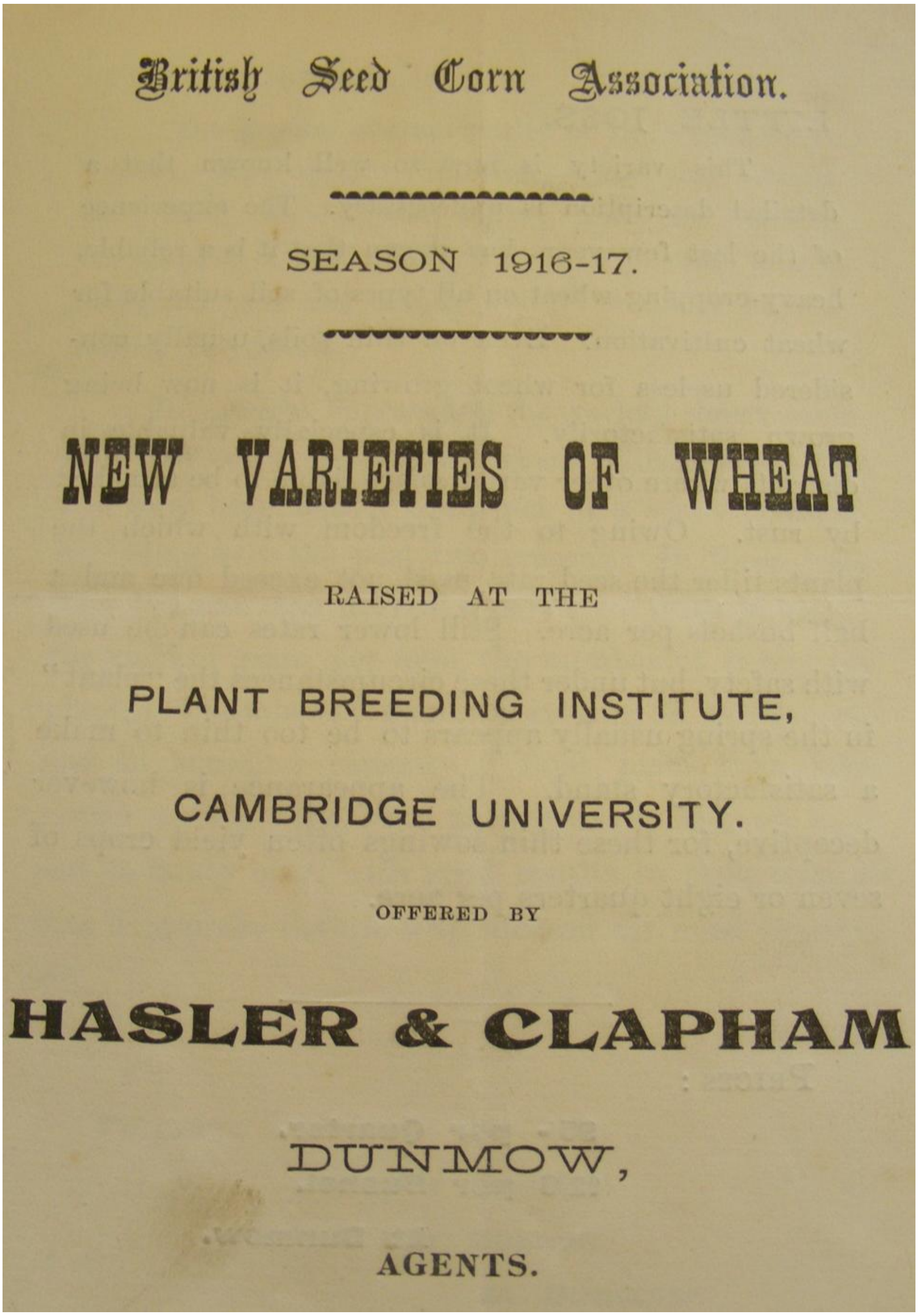
Yeoman was actively marketed to farmers and the agricultural community at large by Biffen and his supporters. Where Carter's for instance, sold the selected "Yeoman King, Carters' New Ennobled-Strain Wheat", Biffen's varieties were often sold under a variation of, "Professor Biffen's Yeoman, a product of Mendelism".[49] This use of eponymy draws from established traditions in the plant breeding world, the moral economy of plant breeding, in which a variety's good standing, its moral calibre even, was established through a system of public display, prize giving, and testimonials. As we saw in the prelude to this chapter, the identification of originator, commercial name and method of production, with prizes and other indicators of merit, was a marketing trope first established by breeders such as Carter's to protect

[48] Biffen 1906: 63.
[49] For more on advertising in the period see Church 2000, for more on trademarks, which many seed firms and even NIAB established, separately to these variety names, see Bentley 2008.

revenue streams derived from their new varieties. Yeoman was marketed in the same way and in the same forums as other breeders' new varieties. In what follows we will track the promotion of Yeoman, paying particular attention to how Biffen's deterministic reading of Mendelism supported promotional claims about Yeoman's superiority and stability.

Before promotion could begin however, the first problem Biffen identified with marketing new Mendelian varieties was one of distribution. How could he transfer stocks of seed from the Plant Breeding Institute at Cambridge to innumerable farms across the country without favouring any particular farmer or member of the seed trade? Biffen initially turned to the British Seed Corn Association which took on this distributive role. With a means of distribution in place, Biffen and his supporters now had to convince farmers that Yeoman was worth the cost of bought seed. Indeed, even after it was successful, efforts continued to promote the variety's wider use.[50] The British Seed Corn Association printed up a little brochure in 1916 to circulate amongst farmers and promote the superiority of Yeoman, see figure 3.4.[51] The brochure announced to farmers the arrival of a wheat variety whose grain was, "a distinct advance on that of any other English wheat".[52] To mark the release of this special variety the *Times* also printed a short article announcing that the distribution of Professor Biffen's Yeoman was to be placed in

[50] Most farmers either saved back wheat seed from the previous year's crop or bought it from each other. Any breeder had to convince farmers to buy expensive seed in what was a period of perceived decline. If they wanted to continue making money they also had to convince farmers to continue buying that seed from year to year, Brassley 2000: 523.
[51] The British Seed Corn Association, *New Varieties of Wheat Raised at the Plant Breeding Institute*, Dunmow Agents (1916), Guinness Barley Research Station Papers, TR GUI AD, 2/6.
[52] *New Varieties of Wheat Raised at the Plant Breeding Institute*, 2.



British Seed Corn Association.

SEASON 1916-17.

NEW VARIETIES OF WHEAT

RAISED AT THE

PLANT BREEDING INSTITUTE,

CAMBRIDGE UNIVERSITY.

OFFERED BY

HASLER & CLAPHAM

DUNMOW,

AGENTS.

Figure 3.4 *New Varieties of Wheat Raised at the Plant Breeding Institute*. Catalogue held at the Museum of English Rural Life, Reading, Guinness Barley Research Station Papers TR GUI AD 2/6.

the hands of the British Seed Corn Association.[53] Biffen's varieties were probably the only wheats to ever have their release announced in the *Times*. A few growing seasons later news of the successful first season of Yeoman's use was the focus of an article in the *Journal of the Board of Agriculture* in 1919.[54] The success of Yeoman was brought forwards by the Board as part of a recommendation to use the new variety.

The promotion of Yeoman continued at the Royal Agricultural Society of England's Show in 1922 where Biffen set up an exhibit of his plant breeding activities in the show's Education section. Yeoman was central to this exhibit. Biffen described his own wheat, in a report of the exhibit published in the Society's journal, as "markedly superior in quality to other English wheats". Biffen then gave a lengthy explication of the "simple" procedure of plant breeding, which he described as aimed at the production of an "ideal type".[55] In the *Yearbook and Annual Report of the Essex County Farmers Union* for 1923, Biffen described Yeoman, in almost exactly the same word, as, "markedly superior in … quality to any other English wheat" and he also gave (as he had before) a description of Mendelism applied to breeding, could be used to produce "fixed" and "ideal type" varieties.[56]

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[53]"New 'Yeoman' Wheat", *The Times* (14th September 1916), 3b.
[54] "Official Notices and Circulars: New Wheats" 1919: 457-8. Biffen recalls in Biffen and Engledow 1926 that during the initial release only a "small stock of seed could be distributed" in 1916 due to the lack of staff on the experimental farm. It took a year to grow enough seed stock for the next year's distribution and Biffen was always short of space and seed on the experimental farm. As a result the initial release of Yeoman was a long winded affair hence the gap between 1916 and the varieties next appearance in the Board's journal in 1919. After the collapse of the BSCA, the first large scale distribution of Yeoman was probably conducted in 1918 with the co-operation of the Board of Agriculture through the Food Production Department. Interestingly, this is one of the few reports of Yeoman's success in which Biffen is not named, a rare exception that proves it was possible to talk about them individually.
[55] Biffen 1922b: 35, 39.
[56] Biffen 1923: 226, 231.

At the Royal Agricultural Society's show and in the articles which followed, Biffen gave a very optimistic portrayal of Mendelian breeding's power to produce almost any desirable new combination of characters at the breeders' whim. At the same time, Biffen, Yeoman and this reading of Mendelism were united in display and publication. Biffen's name lent credence to his claims to Yeoman's superiority, the Mendelian nature of Yeoman's production assured the stability of Yeoman as a variety. Finally the variety's name, Yeoman, invoked images of the trustworthy, sturdy and reliable Yeoman Farmer.

When the Ministry of Agriculture and Fisheries' Departmental Committee on Distribution and Prices of Agricultural Produce published its *Interim Report on Cereal, Flour and Bread* on 9th October 1923, it was full of praise for Biffen and Yeoman. One of Biffen's former students at the Cambridge University School of Agriculture, and long-time ally at the Home Grown Wheat Committee, A. E. Humphries, gave evidence to the committee and on his advice they heartedly endorsed both Yeoman and the Buckinghamshire National Farmers' Union's advice to its members to grow the variety. One of the recommendations of the report was that this advice should be extended to farmers nationally by the National Farmers' Union. Biffen was named as the creator of Yeoman, and much praise lavished upon him. His methods were described as "scientific" by the Committee and Mendelism was absent from the discussion only in name.[57] Growing Professor Biffen's "scientifically produced Yeoman" was now officially advised by the Ministry of Agriculture and Fisheries:

[57] Ministry of Agriculture and Fisheries. *Departmental Committee on Distribution and Prices of Agricultural Produce. Interim Report on cereals, Flour and Bread*, Cmd. 1971 (1923), 70.

> [T]he National Farmers' Union should recommend its members to produce "Yeoman" wheat … Millers, for their part, should concentrate on the production, advertisement and sale of all Yeoman flour, while bakers should make enlightened use of its proved and recognised qualities.[58]

Biffen's wheats were part of orchestrated effort to improve agriculture, one that the committee felt "can only be accelerated by the continued improvement of the varieties of wheat grown and the full, ready and enlightened use by farmers of the improved and authenticated varieties placed at their disposal".[59]

The promotion, which had started with a small brochure printed up by the British Seed Corn Association, culminated with the Government's recommendation to farmers to buy and grow Yeoman.[60] In the time between the release of Yeoman in 1916, and the Government's endorsement in 1923, Biffen, Yeoman and Mendelism became so fused, that in 1924 A. B. Bruce, writing in the *Farmer and Stock Breeder*, pronounced, "His triumphs are known to all. Everyone has heard of Professor Biffen's ... wheats which were deliberately 'made' according to plan for certain purposes … these results owe their origin to the application of certain laws of which the master principle is associated with the name Mendel".[61]

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[58] *Interim Report on cereals, Flour and Bread*, 86.
[59] *Interim Report on cereals, Flour and Bread*, 87.
[60] *Interim Report on cereals, Flour and Bread*, 86-87.
[61] A. B. Bruce, "Aspects of Agriculture: Mendelism Applied to Business", *Farmer and Stockbreeder* (13th Oct. 1924), 2396.

3.2.2 Harvesting Success

Biffen was keenly aware of the importance of public display. At the start of his career he had devised a new system for producing rubber by centrifuging raw latex. On a trip to the West Indies he had left a copy of the prototype of his specially constructed centrifuge in Trinidad. When the "uncivilised peoples" of Trinidad began using the device after Biffen's departure he was outraged and insisted, "The merits or demerits of this mode of preparation must rest entirely with me, but I cannot be responsible for any statements made in Trinidad, where a copy of my experimental machine was recently exhibited without my knowledge or consent".[62] Given this concern for his reputation and that of his innovations Biffen must have been pleased in 1919 when the Board of Agriculture pronounced that Yeoman's success bore "striking testimony to the value of scientific research in agriculture … carried on by arrangement with the Board".[63] At the British Association for the Advancement meeting in Bournemouth in 1919 Sir Daniel Morris, talking to Section K (Botany), presented Yeoman's success as a justification of the Mendelian research that produced it, and "the prominent position now occupied by plant breeding on Mendelian lines".[64] Indeed, Morris believed, "the further development of plant-breeding and the distribution of pure seed may be regarded as essential to the welfare and safety of the nation".[65] Reports of Yeoman's success used as a vindication for Mendelian breeding were not unanimous. However, my argument

[62] Biffen 1898b: 114-115.
[63] "Official Notices and Circulars: New Wheats" 1919: 457.
[64] See Morris 1920: 319, Morris was at this time the president of the Royal Horticultural Society and working at Kew gardens, although his long career also included several stints working for the government, see Olby 2000b: 1045-6.
[65] Morris 1920: 319/

rests not on their ubiquity, but their prominence in the key forums of the scientific community; *Nature*, the meetings of BAAS and to some extent the *Times*.[66]

Biffen and his work gained another prestigious mark of success on the 30th November 1920, at the anniversary meeting of the Royal Society, when Biffen was awarded the Darwin Medal. Previous holders included Alfred Russell Wallace, Thomas Huxley, Hugo de Vries, August Weismann, and William Bateson. Along with the £1000 awarded with the medal, Biffen gained considerable credit and an exoneration of his methods. The award placed Biffen in a scientifically respectable lineage and invoked the success of his wheat varieties to do this. As a *Nature* report of the evening put it, Biffen deserved the medal for having "worked out the inheritance of practically all the obvious characters of wheat". The report then explained that "Biffen's activity is not by any means to be measured by his published work. Two of his new wheats … are among the most popular in the country, and together account for something like a third, or even a half of the wheat crop of England".[67] *Nature's* coverage – including the report of the Darwin award – frequently noted Yeoman's success. Once again, many of these reports record Biffen's name, and the "scientific", if not Mendelian nature of Yeoman's production.[68] *Nature* was obviously committed to thoroughgoing coverage of Biffen's activities but these reports were not intended to be read by farmers, they were intended to demonstrate to scientists the remunerative benefits of applying

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[66] In a later appearance at the 1924 BAAS meeting held at Toronto, Biffen's work was not given nearly the same level of significance. Sir E. John Russell, described the importance of Mendelism to plant breeding and the importance of Biffen's work to that application, but in the next sentence he highlighted the importance of selection as a method and Yeoman was not mentioned at all. Russell 1925.For more on the way in which credit accrues around new technologies see Biagioli, 2006c and on the importance of reputation attached to scientist's names see Biagioli 2006b.

[67] "Anniversary Meeting of the Royal Society" 1920: 453, see also the same report with the same claims in *Science*, "Medals of the Royal Society" 1920.

[68] See also, Morris 1920, "Wheat from Seed Bed to Breakfast Table" 1921: 614-5, "Current Topics and Events" 1923.

science to agricultural practice. The *Times*, which was not specifically a scientific publication, but which was still important to scientists, also carried news of Yeoman's success.[69] In 1924, while praising Biffen's research, the newspaper reported the success of Yeoman, which was, "being grown to the satisfaction of the farmer".[70]

Yeoman's success was good news to many people. It won Biffen the complete backing of the Board of Agriculture. The head of the Board, Rowland Prothero (Lord Ernle), proclaimed to the house of Lords in 1918, "We are using our best efforts to encourage the work of Professor Biffen and his colleagues at the Cambridge Institute, and we have also set up a commercial side to it on the lines of the Svaloff Institute in Sweden, which brings the seed rapidly into the market on a commercial scale".[71] The extent to which Biffen's reputation gained from his varieties' success can be seen in the Knighthood he received in 1925. Seven years later he was invited to edit *the* agricultural textbook of the period – *Fream's Elements of Agriculture.* By 1932 Mendelism was part of textbook breeding strategy and Mendelians were writing the textbooks.[72] The Board of Agriculture, Sir Daniel Morris, the Royal Society and *Nature's* correspondents all felt that Yeoman's

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[69] The importance which Biffen attached to the reports of *The Times* can be gauged from a letter he sent, in conjunction with several prominent Botanists, Zoologists and Agricultural Scientists, to the newspaper in 1911. *The Times of Ceylon* had published an article suggesting that botanical research at the Royal Botanic Gardens at Peradeniya, Ceylon was no longer being conducted and accordingly the scientific reputation of the Gardens was languishing. Biffen's letter (his was the first signature) argued that this did a great injustice to the scientific research now being conducted there. The letter shows he was concerned with the paper's report and alive to its ability through such reports to bestow credit upon scientific research. R. H. Biffen, "Botany in Ceylon: to the Editor of the Times", *The Times* (27th May 1911), 6b.

[70] "The Quality of British Wheat: Professor Biffen's Researches", *The Times* (3rd November 1923), 7f.

[71] Rowland Prothero, "The Government and Agriculture", *Hansard Parliamentary Debates*, 5th ser., vol 34 (1919), cols. 526-527. For more on Prothero see G. E. Mingay, "Prothero, Rowland Edmund, first Baron Ernle (1851–1937)", *Oxford Dictionary of National Biography*, Oxford University Press, 2004; online edn, May 2009 [accessed 13 Aug 2011].

[72] Biffen 1932.

success not only justified Biffen's research but also underwrote the necessity to continue producing new Mendelian varieties.

3.2.3 Protecting the New Varieties: From Yeoman to Yeoman II

There was, however, one problem with Yeoman. Fields of the variety contained a good many rogues, of just the type described in chapter 2. The problem was not particularly important to farmers, who took little notice of "talls" except to note that this would not be seed for sowing next year, and if there were a huge number, perhaps not a seed dealer for future use. However, for Biffen and his claims about the way in which gametic purity guaranteed the fixing of his varieties this was disastrous. Biffen proclaimed in *Nature*, "the sooner Yeoman is off the market the better".[73] The problem was partly to do with threshing machines mixing seeds around, as we saw in chapter 2, but there was another aspect to this problem; the hopeless mess of seed resellers. Open the pages of the *Mark Lane Express* or the *Farmer and Stockbreeder* and one could find countless adverts – large and small – for Yeoman seeds and new derivative varieties like Yeoman King.[74] Wilfred Parker, the National Institute of Agricultural Botany's first president put the point like this:

> Such chaos [of varietal names] as has been shown is obviously bad. It prevents the raiser of a good new variety from reaping the full value of his

[73] "Current Topics and Events" 1923: 734.
[74] See the significant collections of agricultural newspapers and journals and seed catalogues at the Museum of English Rural Life and the NIAB Archives.

> discovery, as the name he selects will be lost amongst the many new names that are continually appearing.[75]

As one of the Board of Agriculture's correspondents had noted in 1917, "it is necessary to draw a marked distinction between the new varieties put on the market by the usual trade agencies and those produced by approved scientific methods of hybridisation and selection".[76] The problem was that the varieties put on the market by the usual trade agencies, which oftentimes had been developed from Mendelian varieties, tended to degenerate. While there was, "a popular belief that the degeneration [was of] the variety itself", the Board's correspondent, and Biffen, were adamant that, "This is not so... there can be no doubt ... that degeneration if it appears is the result of admixture of foreign seeds of which the commercial migratory threshing machine is the principal cause".[77]

To solve this problem, Biffen, along with his friends at NIAB, set about releasing a new, improved and, crucially, pure stock of a new variety; Yeoman II. The new variety was released in 1924, despite much wrangling on NIAB's Crop Improvement Committee.[78] There were several problems with the release, not least the weather, which reduced the seed crop grown by Fred Hiam on the farm he had gifted to the institute. William Hasler organised the cleaning and preparation of the seed for sale. As with the first Yeoman, a small catalogue was printed up, this time focused exclusively on the new Yeoman II variety, see figure 3.5.

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[75] Parker and Chambers 1921: 171, the authors were sure to avoid accusing any one dealer of causing these problems, "It is not implied that wilful misrepresentation is habitual, or even common, among British Seed Producers or Dealers". For more on the confusion of names in English wheat varieties see F. L. Engledow, "English Wheat"[Pamphlet reprinted from the *Essex Farmers' Union Year Book* 1927 by the Cambridge University School of Agriculture and *Essex Weekly News*,] A. D. Hall Papers, 188-8-61: pp. 11-14.
[76] "Plant Breeding and Controlled Seed Farms" 1917: 1080-1081.
[77] "Plant Breeding and Controlled Seed Farms" 1917: 1082.
[78] "Minutes of the Crop Improvement Committee 24th May 1923", pp. 49-50, *Crop Improvement Committee Minutes*, NIAB Archives.

YEOMAN II

National Institute of Agricultural Botany,

Huntingdon Road, Cambridge.

Telegrams : Niab, Cambridge. Telephone : Cambridge 1001.

Figure 3.5 Yeoman II. Front page of a catalogue released by NIAB in 1925. A. D. Hall Papers, 188-8-66.

Once again, with a means of distribution in place, the problem became one of convincing farmers to buy Yeoman II. Biffen gave papers to the Farmers' Club of London and the Cambridge University Agricultural Society in 1924 and samples of Yeoman II were displayed at the Royal Agricultural Society's Royal Show.[79] In both he recommended a change of seed as beneficial to farmers in order to maintain the purity of the wheat they grew. At Cambridge he outlined the cause of impurity in stocks of seeds:

> Keeping stocks true to type, though not entirely a modern problem, is of far greater importance than it was in the past …. The coming of the threshing machine has made the problem a more pressing one. The type of machine which travels from farm to farm seems almost to have been designed especially to mix the various sorts together.[80]

Later in the year, Biffen recommended Yeoman II in an article written for the *Journal of the Ministry of Agriculture*. In this article and further announcements of the new wheat Yeoman II, published in NIAB's journal, *Nature* and the *Times*, Yeoman II's release was explained as a measure to solve problems of purity in Yeoman stocks.[81] Biffen's article for the *Journal of the Ministry of Agriculture* is typical of reports of Yeoman II's release. In it Biffen explained the need to release Yeoman II because "pure stocks of the older Yeoman, owing to admixture with other wheats, are getting difficult to obtain". Accordingly he felt that, "It is very desirable that farmers should know how they are to identify genuine Yeoman II",

[79] "The Royal Show: Roots and Seeds" 1924: 419.
[80] Biffen 1924a, 1924b.
[81] Biffen 1924b, "Meetings of the Fellows of the Institute: First Annual General Meeting 10th Nov. 1922" 1922, Parker 1923, "Current Topics and Events: The Council of the National Institute of Agricultural Botany Announces…" 1924, "New English Wheat, Yeoman II Available for Sowing", *The Times* (28th July 1924), 18c.

which could "only be obtained in sacks closed with the seal of the National Institute of Agricultural Botany" (see figure 3.6).[82] As NIAB's catalogue warned, "none but this will be genuine".[83] The very means of distribution of Yeoman II presumed rogues were a problem of admixture and not reversion.

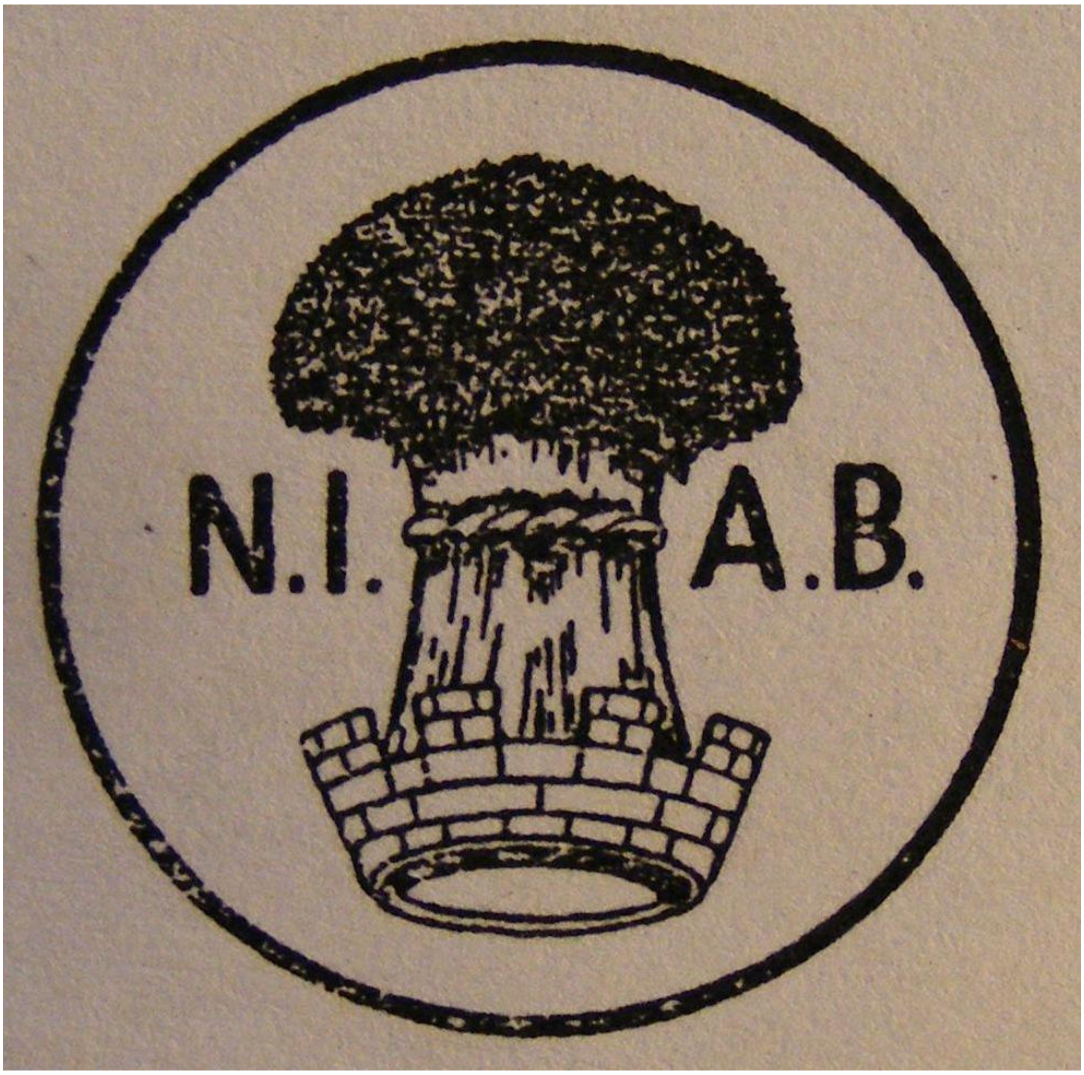

Figure 3.6 The seal used by NIAB to close bags of the new Yeoman II wheat. Image reproduced from Biffen 1924b.

In 1924 the first chairman of NIAB's Council, Sir Lawrence Weaver, proclaimed in his last speech as chairman, "we have brought to the point of

[82] Biffen 1924b: 510, 512. NIAB's reports are an exception to the above in stating that Yeoman II was recommended, as Biffen's Mendelian production but solely because it was of a superior quality to Yeoman.
[83] *Yeoman II*, p. 3, A. D. Hall Papers, 188-8-66.

distribution but one new variety, Professor Biffen's Yeoman II, but that variety happily of an importance that justifies us abundantly".[84] That justification meant more money, as he continued:

> The Ministry [of Agriculture and Fisheries] have expressed without qualification the view that the existence of the Institute is fully justified by its work in testing new and established varieties, altogether apart from its service in distributing such new varieties as Yeoman II, and the Ministry proposes to use part of the new funds available for agricultural research to aid us in this work.[85]

So, even before they were successful, Biffen's new varieties provided justification for NIAB and were instrumental in garnering further funding for the institute.

In the following year, several reports of the success of the distribution of Yeoman II appeared in the *Times*, and the *Journal of the Ministry of Agriculture*.[86] Each of these reports stressed that, despite the bad weather, a considerable amount of seed had been made available through the fixed-price tendering system. Worryingly, there was even still some seed left to distribute. The minutes of the Crop Improvement Committee later in the year reveal that the variety was in fact a failure.[87] Furthermore, it was a failure that put paid to Mendelian plans for the

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[84] Weaver 1925: 54.
[85] Weaver 1925: 54.
[86] "Yeoman II Seed Wheat: 2480 quarters Marketed", *The Times* (6th January 1925), 8e, "Results of the Marketing of Yeoman II Seed Wheat" 1925, See also "A Great Home Grown Wheat", *Yorkshire Herald* (8th January 1925), reproduced in *Newspaper Cuttings,* NIAB Archives, main library section and the clippings from the *Peterborough Advertiser* and the *Farmers' Express* on the marketing of Yeoman II.
[87] "Minutes of the Crop Improvement Committee 24th May 1923", p. 108, *Crop Improvement Committee Minutes,* NIAB Archives. Sales of the variety only just made £2500 which after the cost of producing the seed left no profit. The last 90 qtrs. of seed was sold for milling.

institute to operate as a financially self-sufficient distribution centre. The failure of Yeoman II to pay for itself made it look increasingly unlikely that the institute would be financially self-sufficient. Beaven led the way in re-orienting the institute to distribute, instead, small batches of seeds that commercial seed dealers would multiply and resell.[88] At the same time the Development Commission converted the loans it had initially made to the institute into grants, recognising that the commercial failure of Yeoman II meant the institute's dreams of financial self-sufficiency would not be realised. Furthermore, rogues and purity of seed stocks were still on the agenda at the 1949 second Conference of Plant Breeders. Nearly 40 years after the release of Yeoman II, academic plant breeders, including G. D .H. Bell, were still looking askance to NIAB, for a "greater control by plant-breeding stations over the maintenance and multiplication of their products".[89]

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Applying Mendelism to plant breeding to produce new varieties was a key element in Biffen's career. He believed that Mendelism allowed the breeder to make pre-determined crosses towards definite goals, and quickly recover fixed hybrid progeny from those crosses as stable and commercially useful new varieties. The benefits of these varieties were not self-evident, especially to farmers who were keen to reduce the costs of wheat production. Accordingly, when Biffen and his supporters marketed Yeoman they lent on the variety's Mendelian origins and Biffen's reputation to assert the variety's superiority and stability. When Yeoman was

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[88] See NIAB archives and the announcement in the *Times*, drawn from NIAB's reports and accounts that institute's financing and work would be changing, "Agricultural Botany: Work of the National Institute", *The Times* (21st November 1927), 21b.
[89] "Problems and Policy in Plant Breeding" 1949: 53.

popular, the close associations drawn between Biffen, Mendelism, and Yeoman in its marketing meant that success reflected back on Biffen and his use of Mendelism. When Yeoman began to misbehave in the field, in a way which undermined Biffen's claims about the power of Mendelism applied to breeding, he and his supporters sought to use direct sales and sealed sacks to help them recast that troublesome behaviour as the result of an aspect of Yeoman's distribution or use rather than its production.

Throughout the marketing of Yeoman and Yeoman II, the varieties interacted with numerous organisations that leant space to their promotion and enabled their distribution. The Yeoman varieties brought capital and support into a Mendelian system. The accolades of the Essex Farmers' Club, and the Royal Society, or the space provided to run an exhibit at the Royal Agricultural Society's Show meant all of these organisations were complicit with Mendelism, and so became instrumental in the Mendelian system. If the Mendelian gene was a "territorializing technology', then the Mendelian gene incarnate, realised in Biffen's Mendelian varieties was of equal importance in the emergence of a Mendelian system which had by the 1920s grown so widespread that British farmers had become accustomed to talk of Mendelian factors, inheritance, and scientific breeding.

Once Yeoman II was released in 1924 it totally disappeared from the news. The *Journal of the National Institute of Agricultural Botany* which had practically been the *Journal of Yeoman II* for much of 1923-1924, was devoid of news of the variety in 1925. The *Journal of the Ministry of Agriculture* was similarly quiet. Even *Nature* and the *Times* had very little to say. The only reports in the year after Yeoman II's release, were reports of the success of its distribution; not the variety itself. Biffen mentions Yeoman II in his 1926 monograph but was agnostic about the variety's success. If the variety was a failure, and most farmers, as Engledow suggests, went back to using Yeoman why were they not troubled by the variety's rogues? In the next chapter the focus turns to the question of how Biffen's varieties were or were not integrated into farming practice. Each of his varieties was linked to a particular agricultural strategy; increasing yield through avoiding losses to disease, or aiming to grow wheat of a higher quality for the bread market. Sometimes farmers did not use Biffen's varieties in line with his recommendations for changes in practice, but sometimes they did, and when they did, they helped Biffen and his varieties change the world.

Chapter 4

Remaking the Field: How the New Varieties Meshed with the New Farming

> Mr. Patterson made the point that it pays one to grow what I may call indifferent quality wheats merely for the purpose of chicken food. Do not for a moment go away with the impression that I want to teach you how to farm. I know nothing about farming, but I am going to make the suggestion that Yeoman wheat may be as good for chickens, and therefore it might be worth trying the double event. If the chicken food trade does drop, then the human subject might be worth feeding.[1]

ROWLAND BIFFEN WANTED to improve wheat in specific directions. The new wheats would, he hoped, in turn change farming. Despite his feigned reticence about telling farmers how to farm, in 1924 at the Farmers' Club in London, he went on to do just that. Indeed, much of the evening's paper revolved around tips on how to grow Yeoman correctly. Each of Biffen's varieties was meant to fulfil certain functions on the farm and in the rural economy. Biffen's first variety, Little Joss, for example, was meant to help farmers increase yields by reducing losses to disease. However, the disease to which Little Joss was resistant, yellow rust (*Puccinia glumarum*), was only a small threat to farmers and went unrecognised by most. Even by Biffen's reckoning losses of five per cent in yield might be the worst they would usually have to worry about.[2] It turned out that Little Joss had other useful qualities

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[1] Biffen 1924a: 18.

[2] Biffen 1905a: 4. Despite Biffen's initial estimates, by the time he gave evidence to the Selborne Committee in 1918, he believed rust resistant varieties could increase yield by 10-

which were more attractive to farmers than disease resistance. Following up on this discordance between Biffen's intentions for, and farmers' use of, the new Mendelian varieties, this chapter focuses on agricultural successes and failures of the Mendelian system in the field, relating them to contemporary agricultural economic patterns.

The agricultural importance of Mendelian varieties has still never been seriously assessed.[3] This is surprising given the claims made on their behalf. Fortunately, reports of the varieties being used are available and by working from these records it is possible to start making inroads on this question. The evidence presented here is far from exhaustive, but it points towards a consistent pattern. Biffen's varieties were oftentimes popular with farmers and important to the agricultural economy for reasons Biffen did not principally intend. Historians of technology refer to this process as "re-scripting".[4] The notion (but not the word) is invoked here to conceptualise how farmers reacted to the Mendelian system and its products. On one interpretation of events in this period, Biffen and his colleagues held the farmers in chauvinistic contempt, believing they should be passive recipients of the benefits of Mendelism. Mendelians, on this view, became frustrated with farmers when they clung on to backwards-looking traditions.[5] However, Biffen, time and time again, sympathised with famers' concerns and explained to his peers why their reticence about new varieties was to be expected.[6] Neither does this view give a fair representation of farmers. From their perspective, Mendelian varieties

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12 per cent practically putting an extra £1 on price of an acre of wheat. Reconstruction Committee, *Report of the Agricultural Policy Sub-Committee of the Committee on Reconstruction, Part 1*, Cd. 8506 (1917-1918), 27.

[3] See Fitzgerald 1995 for an early call to look at varietal improvements more seriously. Studies of the British situation have focused on the post 1930s period. In the main they suggest that publicly funded plant varieties only began to have a significant share of the agricultural seeds market from the 1950s onwards, see Thirtle et al. 1997 and 1998.

[4] On users and use, and especially, "re-scripting" of innovations by users see Oudshoorn and Pinch 2003.

[5] See Palladino 1993: 318-319 and Vernon 1997.

[6] See Biffen and Engledow 1926: 8.

fitted two clear patterns of agricultural development. Before the First World War, Little Joss fitted into a pattern of reduced expenditure on inputs. After the War, Yeoman was part of a pattern of increased expenditure on the intensification of agricultural production.

This chapter begins with an overview of the British agricultural economy between 1880 and 1930. Agricultural depressions, rural depopulation, bad weather, the First World War and a massive increase in competition from abroad all had an impact on British agriculture. As we will see in the second section, Biffen believed he was tailoring varieties to solve these problems. By his own description, much of his work was of “economic” significance. The popularity of Biffen’s varieties, with farmers, is then explored in comparison with reports of the varieties’ use in a pair of sections aimed at getting closer to the question of Mendelian wheat’s success in the field. In closing, the chapter reflects on the significance of Biffen’s varieties for the history of biological innovation in agriculture. The case of Biffen’s wheat illustrates an interesting lesson for anyone attempting to assess the weight of impact of biological innovations; these varieties remained malleable and, indeed, alive to the contexts in which they were deployed.

Before moving to the business at hand in this chapter, a word or two about farmers. Farming is a catchall term for a very heterogeneous group of activities. Matters are not helped by the prevalence of a strong stereotype of farmers in common currency today. To make full disclosure, I grew up farming in a small valley in Devon, in the south west of Britain. My parents farmed a small holding of 25 acres; they kept cows and goats and made cheese from their milk. We also kept chickens, ducks and pigs for eggs, meat and fun. My parents were middle class idealists, who ran the farm with a minimum of chemical intervention, and a romantic view of farming; we never stopped milking the 30-odd goats by hand. In contrast, at the top of the valley, there were the Beers, four siblings who had grown up together on a sheep holding. They farmed roughly 2000 sheep, over several hundred acres. They had the newest tool for every job and made use of chemicals whenever they thought it appropriate. Compared to the sleepy idyll on our own farm theirs roared and rumbled with the noise of industry. The final farmer in the valley was John Thorne. Although he owned more land, his farm was left to woodland and scrub in which he hunted, shot and fished for pleasure; what farmland he did have was rented to the Beers.

These three approaches to farming – we might call them idealist, commercial and pleasure farming – have long histories.[7] The ideals they embody can be seen in action throughout the nineteenth and twentieth century. In the period, and more so now, these ideals were rarely, if ever, espoused by the stereotypical country bumpkin. In what follows the focus is on commercial wheat farmers in the south east, farmers like McFarlane Grieve or Fred Hiam who had gifted whole farms to the Department of Agriculture at Cambridge and the National Institute of Agricultural Botany. If this restriction of view seems unduly narrow, it is worth remembering that wheat was the principle crop of the period, and the south east of England was considered to be the centre of wheat production. In many eyes the area was the faltering heart of Britain's failing agricultural industry.[8]

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[7] The contemporary terms, tenant, landowner, dilettante do not quite capture this broad range of differences in approach to farming as they derive from class concerns and land ownership. For an example of pleasure or dilettante farming see Le Couteur's career, discussed in "Seed Wheats" 1835 and Berlan 2001.

[8] For more on farming in the south east of Britain see the archived reports of the Morley Agricultural Foundation running from 1908 and detailing the activities of the Norfolk Agricultural Station of which Wood was an executive committee member, <http://www.tmaf.co.uk/archived-reports> [accessed 11 August 2011].

4.1 The Importance of Agricultural History

An appreciation of agricultural history and the history of agricultural economics is essential to understanding Biffen's varieties and their impact on agriculture.[9] The historical backdrop for the events described in this thesis was a perceived crisis in British agriculture. Just one expression of this view, drawn from many, can be seen in this extract from Thomas Middleton's obituary:

> There were indeed plenty of problems, for British agriculture was then at a desperately low ebb. Prices were low and uncertain, farmers were losing heart, arable land was going out of cultivation and simply allowed to seed itself with whatever wild vegetation would come: 'tumbling down to grass' was the phrase used.[10]

The following overview of that crisis and the periods before and after, is not intended to be exhaustive. It should, however, serve its purpose by illustrating the types of problems Biffen was responding to in his work.

4.1.1 British Agriculture in the Doldrums, 1880-1930

The history of British agriculture in the decades before and after the First World War can be very roughly divided into four periods. This periodization can be read from figure 4.1. Roughly put, 1879-1895 was a period of depression, 1895-1915 a

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[9] See the introductions to Harwood 2005a and Collins and Thirsk 2000 for general concerns about the decline of interest in agricultural history.

[10] Russell 1944: 560. For a similar contemporary tale of depression, followed by recovery, see Orr 1922. Orr was a keen sponsor, along with Biffen, of Otto Frankel's work which features in the next chapter. See also Muir 1895: 181.

slow recovery and stabilisation, 1915-1930 a boom during the War followed by two bad growing seasons and generally thwarted expectations in the 1920s before the onset of the Great Depression after 1930. Prior to 1879, and the period covered in this thesis, the national area of wheat production stood well above four million acres. British farming was in a boom period, following the first agricultural revolution, and caused in part by the widespread adoption of threshing machines.[11] The period is characterised by the arrival of the Royal Agricultural Society in 1838. The society's motto "Practice with Science" reflected the progressive ethos under which it was established. Other developments in this period, which reflected the growth of growing, included the foundation of the Royal Agricultural College at Cirencester (circa. 1842), the granting of a Royal Charter to the Horticultural Society of London to form the Royal Horticultural Society (1861), the foundation of the Farmers' Club of London (1842) and the foundation of an agricultural press, including the *Gardeners' Chronicle* (1841), the *Agricultural Gazette* (1844) and the *Mark Lane Express* (1832) among others.[12]

This supposedly golden age of high farming lasted until the end of the 1870s. During the 1880s the country's agriculture entered a period of intense depression.[13] The steep decline in acreage and production can be clearly seen on the left hand side of figure 4.1. Two developments are often thought to have conspired to produce this depression. One, evocatively named since, "the grain invasion", was the arrival on British markets of huge quantities of foreign imports of grain.[14] Cheap shipping and

[11] Up until the mid-century wheat had been threshed largely by hand using flails. The pre-1879 boom seemed to occur in spite of the repeal of the Corn Laws in 1849 and the effective removal of any sort of trade tariffs. See Levy 1911: 59-60.
[12] For more on the Victorian agricultural press see Goddard 1996 and for more on the Farmers' Club of London see <http://www.thefarmersclub.com/club_history.php> [accessed 11 August 2011].
[13] For a lengthy account of this period see Hall 1936.
[14] On the underlying patterns of development in global trade, from an American perspective, see Williamson 1974. On the European grain invasion see O'Rourke 1997.

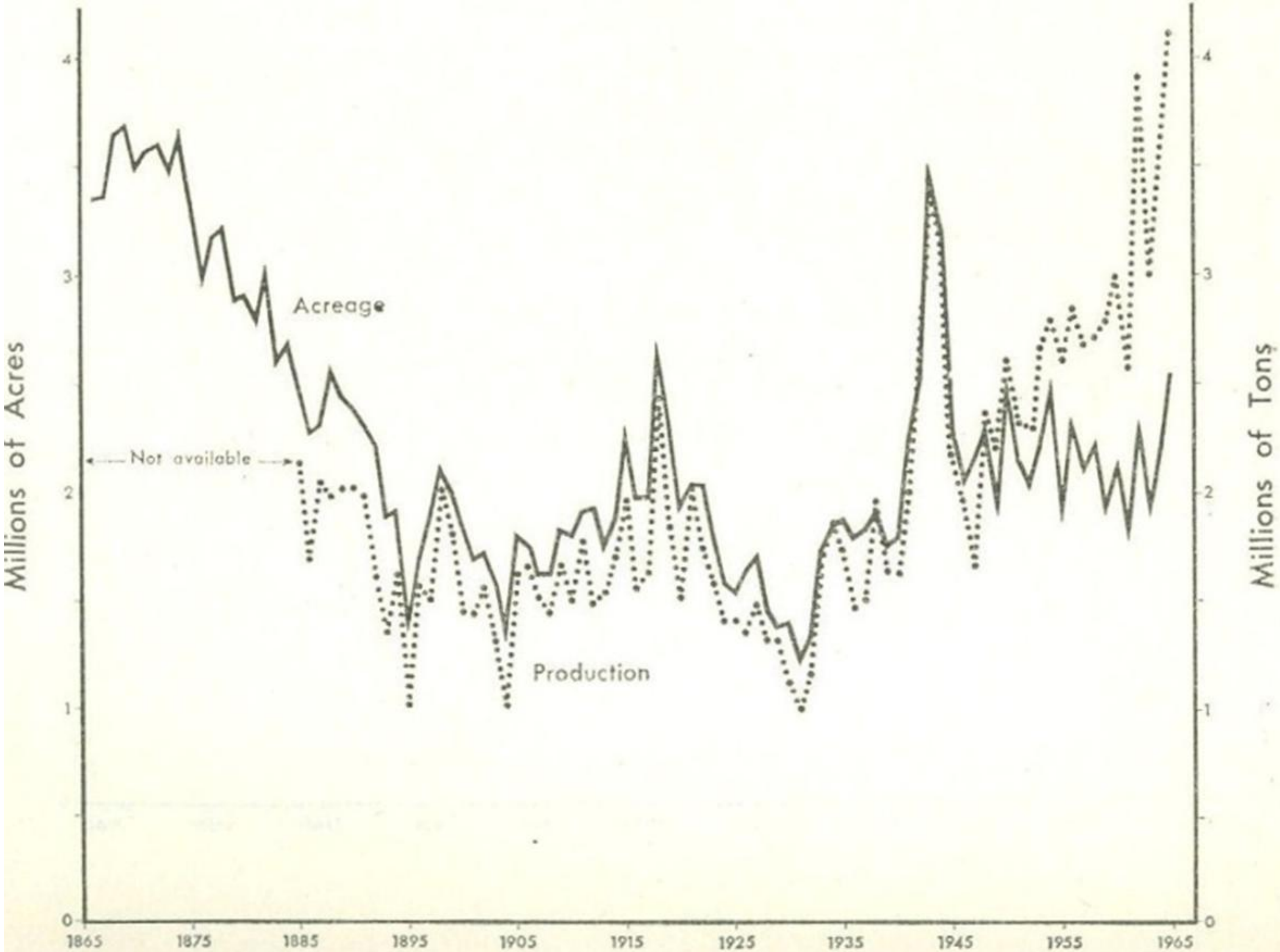

Figure 4.1 Graph of wheat production and the area under wheat cultivation, 1865-1965. Ministry of Farming and Fisheries 1968.

the breaking of virgin soil meant that the New World – both in the Americas and Australasia – was now becoming a net exporter of wheat.[15] The other development on which the recession was blamed, in part because it facilitated the grain invasion, was the ascendency of free market views as expressed by Joe Chamberlain and the diverse Free Trade movement.[16] The policy upshot of the second free trade movement was, for farmers, the same as the first, a refusal to place tariffs on agricultural imports, and so protect farmers from the grain invasion.[17] Two Royal Commissions into the state of agriculture were convened during this period and reported in bleak terms; numerous popular texts also decried the ruin of agriculture.[18]

A second picture of this period of depression has emerged in the history of agriculture.[19] Contemporary concern with agriculture was, apparently, bound up with certain beliefs about what agriculture should be. The reports of the Royal Commissions, both of them, and more popular concern with the subject, belied a prejudice about the relative importance of wheat farming – considered the highest of high farming – over dairy or mixed farming.[20] For farmers other than those in the south eastern wheat growing areas the recession was much less felt. Some farmers

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[15] Of particular emblematic importance was the length of the grain races from Australasia to European markets. These annual events, which were the source of much national pride took just 82 days for the fastest ships to complete in the 1930s. See Collins 2000: 39 or for a popular account see Newby 1956.
[16] See Palladino 1990 on the political economy of agricultural free trade, see also Bernstein 1986.
[17] See Collins 2000a: 45 on the 20th century Free Trade movement and Johns 2009: 51-52 and 275, on the first free trade movement. See also Cox 1903 for a contemporary account.
[18] The two commissions (which produced three reports) were Agricultural Commission, *Report from Her Majesty's Commissioners on Agriculture*, C.3309, C.3309-I, C.3309-II (1882), Royal Commission on Agriculture, *First report of Her Majesty's Commissioners Appointed to Inquire into the Subject of Agricultural Depression*, C.7400, C.7400-I, C.7400-II, C.7400-III (1894), Royal Commission on Agriculture, *Second report of Her Majesty's Commissioners Appointed to Inquire into the Subject of Agricultural Depression*, C.7981 (1896). For an example of a popular account of agricultural rack and ruin see G. Anderson [1899].
[19] See Flethcher 1961 and Collins 2000b: 140-146.
[20] See Hall 1936: Chapter 17 and p. 363 on high farming. See also Collins 2000b: 71.

even benefited as arable land became available for milk production.[21] Even considering the south-eastern wheat growing areas, the bastion of the English landed gentry, the extent of losses in this period, especially to landowners, has been questioned.[22] On this view, the crisis in British farming in the years running up to the turn of the century might more accurately be described as a crisis in wheat farming.

After this period of perceived depression many sources describe a slow recovery hampered by government indifference, beginning just before the turn of the century and ending in 1910.[23] Indifference, according to the orthodoxy, was followed by intervention from David Lloyd George, which began, as we have seen, with the creation of the Development Commission and took off in the years around 1917, as agriculture was placed under government control in the interests of national security. Acreage and production spiked in the years 1915-18. Following the work of agricultural reformer Thomas Middleton, many authors have stressed the importance of direct government intervention as a stimulus to agriculture.[24] Middleton and his supporters focus on the policies during 1917-18 of forced ploughing up of pasture land to be converted to cereal production and government backed fixed prices for wheat. In recent years the analysis given by Middleton has been questioned by historian of technology P. E. Dewey who argues convincingly that a much broader range of factors influenced agriculture before and during the War, including mechanisation, increased use of fertilisers and greater employment opportunities for women. However, even in this enlarged analysis new varieties play only a minor part.[25]

[21] See Hall 1936: 384 on Scottish dairy farming around London.
[22] On landowners and the landed gentry see F. M. L. Thompson 1963 and Beard 1989.
[23] On problems in the 1920s see for example Crowther 1924.
[24] See Middleton 1923 and Dewey 1980:71-72 on the hegemony of this view.
[25] See Dewey 1980, 1989 and Pollock 1991.

At the close of the War, concerns with agriculture remained. Successive government commissions produced much hand wringing about new directions. However, in the early 1920s horrendous weather marred harvest after harvest. To make matters worse, government pricing regulation introduced during the War was repealed in 1921.[26] Despite a bad start many, including Biffen, saw hope in the mid-1920s. Indeed, as the previous chapter argued, Biffen was in large part responsible for inspiring that hope through the release of Yeoman II. The end of the period covered by this thesis coincides with the close of the fourth period of agricultural development described in this thumbnail sketch. In the 1930s, agricultural research and many of the institutes run by Mendelians were reorganised under the Agricultural Research Council.[27] At the same time, British agricultural prices and production slumped, in line with the global economic downturn of this period.

In general outline, as figure 4.1 demonstrates, with its jagged fall, rise and fall pattern, the developments covered by this thesis fell between two periods of agricultural depression, the first from the 1870s-1890s and the second from the 1930s. Despite questions about the depth or generality of the first years of depression, the period was certainly perceived to be one of decline, associated with the grain invasion and a cross party refusal to apply trade tariffs.[28] Whatever recovery there was during the time between the turn of the century and the 1930s is generally attributed to government intervention, either directly during the War, as in the plough schemes and fixed prices implemented by Middleton, or indirectly in the programme of government funding established by the Development Commission from 1910. Although some historians have pointed to a wider range of factors and

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[26] On the repeal of fixed prices in 1921 see Penning-Rowsell 1997.
[27] On the ARC see Dejager 1993.
[28] On declinism see Gooday 2000.

various modifications to this narrative, the role of new varieties, if they played one, has been somewhat under-analysed.

This under-emphasis on the importance of plant breeding is surprising given the long-term government support for this area. Moreover, Wood, Biffen and Humphries were frequently invited to give evidence, and submit reports to government commissions on agriculture, as was Bateson. For example, Humphries was invited to the 1908 commission on agricultural education and later to the Linlithgow Committee's deliberations on post-war agricultural reconstruction. Hall, as head of the Development Commission authored its reports, as did Wood when he took the position after Hall. Mendelians were used to making expert testimony on the wheat economy in government proceedings.

One moment of Mendelian representation to government is particularly instructive. It occurred at the Selborne Committee's deliberations in 1918, one of the two big post-war commissions on agricultural reconstruction. On this occasion Biffen laid out the history of British agriculture as seen through his eyes. The Selborne committee was a self-consciously historical group. A long précis to the committee's report, authored by Alexander Goddard, secretary of the Surveyors' Institution, outlined British agricultural development from the early nineteenth century. Goddard picked out the same periods discussed above. The committee, which included Hall, was appointed by Asquith in 1916 with the following terms of reference, "Having regard to the need of increased home-grown food supplies in the interest of national security, to consider and report upon the methods of effecting such increase".[29] For many of the witnesses and the committee, the answer to this question was, at least in part, an extension of plant breeding work. Middleton gave

[29] Ministry of Reconstruction. *Report of the Agricultural Policy Sub-Committee of the Reconstruction Committee*, Cd. 9079, 9080 (1918), i.

glowing evidence of the importance of Biffen's wheats, claiming that, "[U]ntil Professor Biffen's wheats began to appear there was no substantial improvement in the wheat plant".[30] Biffen himself argued for the need for a better system of distribution for his new varieties, one under the Plant Breeding Institute's control. To a large extent the committee agreed with him and from his evidence in particular it drew the following view:

> The evidence that has been laid before us has amply shown the ultimate value of pure scientific research and the dependence of the industry upon investigation that is independent of any apparently immediate practical end.[31]

This was not quite the line taken by Biffen himself, who had complained of a lack of workers, "who were really interested in their crops from the point of view of agriculture".[32] The view of pure science recommended by the committee was also not shared by another witness, Professor John Wrightson, late president of College of Agriculture at Downton, who, in his own evidence claimed:

> An immense amount has been spent on agricultural education and agricultural science, but the average production has not varied to any great extent yet. ... it would be a most extra ordinary thing if by the application of science the yield of wheat was increased by one bushel an acre over the United Kingdom during the next ten years. Agriculture [is] a very old

[30] *Reconstruction Committee*, 161.
[31] *Reconstruction Committee*, 48.
[32] *Reconstruction Committee*, 215

> occupation not developed through invention or discoveries so much as by a process of evolution and cumulative experience.[33]

Tellingly, he also believed, "The crux of the whole matter [is] the margin of profit between arable and grass farming".[34] Despite Wrightson's evidence the general tone of the committee was in favour of increasing support for plant breeding (amongst other measures including: increasing security of tenant farmers, conversion of pasture to arable land and the continuation of fixed prices for wheat). Unfortunately, Biffen did not have any new varieties ready to oblige.

Biffen did, however, author a memorandum on wheat production that was appended to the committee's interim report. In this memorandum Biffen, affirming Wrightson's view, claimed that price was the determining factor of the area under wheat production in Britain. When prices went down, he reasoned, farmers would turn to other crops. In describing the long run of wheat production he gave this account:

> From 1870-1894, prices and acreage fell rapidly, but since 1897 the fairly steady rise in price has been accompanied by a gradual rise in the acreage. ... The fall between 1870 and 1894 was due mainly to the enormously rapid increase in the area under wheat in the United States.[35]

The problem, Biffen explained was the US and Canadian imports that had flooded the market. He then went on to give a summary of wheat growing in Argentina,

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[33] *Reconstruction Committee*, 149.
[34] *Reconstruction Committee*, 149.
[35] Ministry of Reconstruction *Report of the Agricultural Policy Sub-Committee of the Committee on Reconstruction, Part I,* Cd. 8506 (1917-18), 22.

Russia, India and Australia, all countries which provided sources of cheap wheat. Cheap wheat had driven down prices and with it the acreage farmers were willing to use for wheat growing. However there was, Biffen reckoned, some hope that the situation would soon change:

> It may be assumed that the course of events in America will prove typical of that of other countries where there has been a rapid rise of production under prairie conditions. The soils gradually become exhausted and in place of continuous wheat cultivation a system of rotation has to be adopted.[36]

In other words, the grain invasion would soon dry up and consumption would once again outstrip global production. Britain's best hopes for rejuvenating its wheat growing industry lay in several directions including, improved varieties, more intensive production and an increase in the wheat growing area. On the first of these options, improved wheat varieties, Biffen was most hopeful:

> Increased production may be expected from any improvement in the kinds of wheat grown. … of late the systematic study of cross-breeding has opened up great possibilities of improvement.[37]

Biffen's engagement with agricultural economics and its history is striking. The picture he painted, illustrated with figure 4.2, encompassed decline and slow improvement. Moreover, Biffen went further than describing agricultural economics

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[36] *Committee on Reconstruction, Part I*, 26.
[37] *Committee on Reconstruction, Part I*, 27.

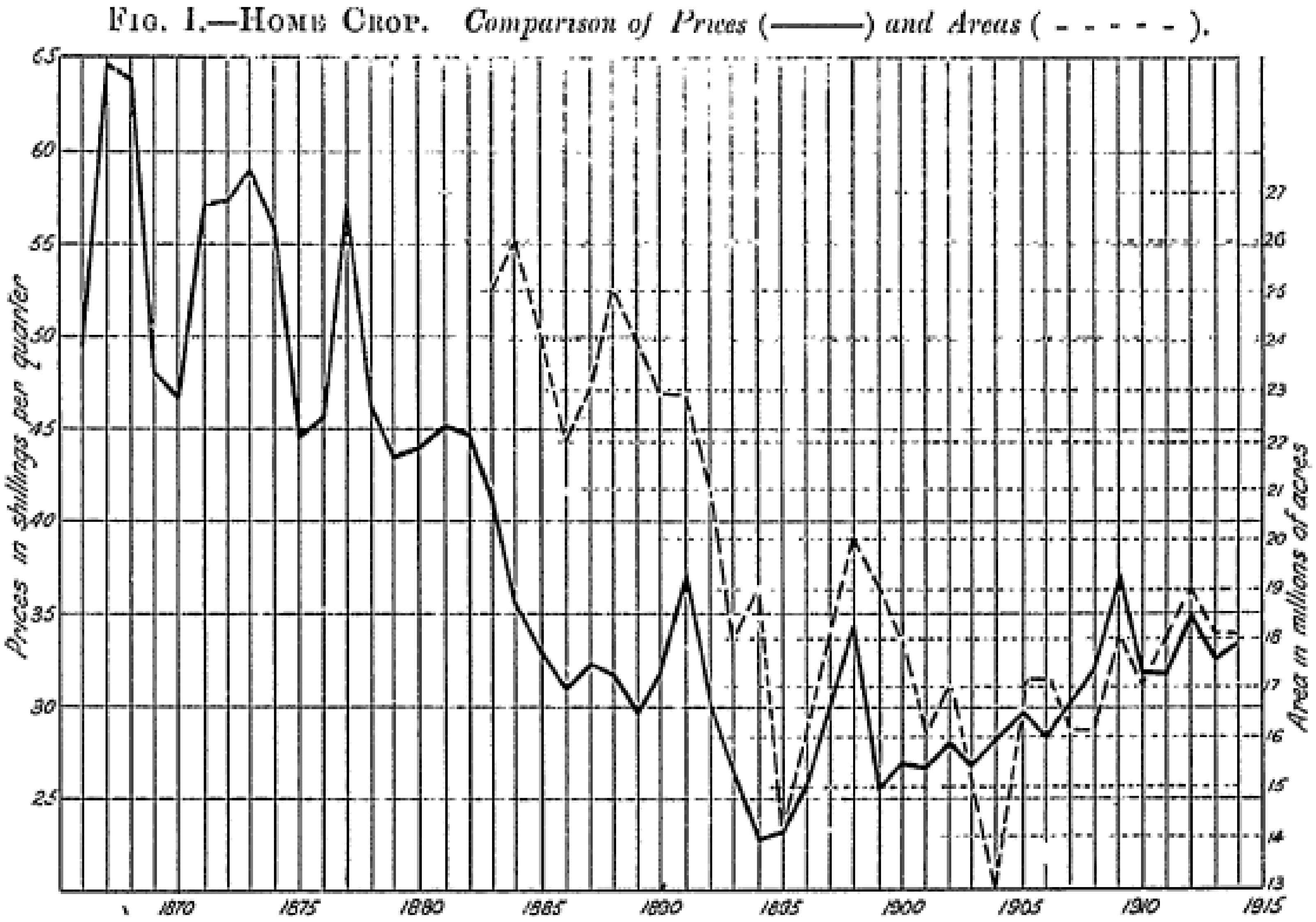

Figure 4.2 Home Crop. Diagram produced by Biffen for the Selborne committee in a report on the wheat industry. Reconstruction Committee, *Report of the Agricultural Policy Sub-Committee of the Committee on Reconstruction, Part 1*, Cd. 8506 (1917-1918).

and made several suggestions about the causes of these developments.[38]

4.2 Mendelian Strategies for Agricultural Development

Biffen's plans for British agriculture were rooted in a belief that new plant varieties were the best means to combat the problems faced by farmers. However, this belief, for Biffen, did not stem from a simplistic view of agriculture, but rather from a pragmatic one of what he felt could be achieved. As he put it in a significant article written for the *Times* on the eve of the First World War, "uncertain factors such as improvement in yield through better cultivation, the possibility of extending the known wheat area, the diminishing fertility of the prairie lands, the increased use of wheat by nations who do not at present use it as a foodstuff, all have to be considered". Given that data on these factors was, however, "utterly inadequate" Biffen felt the best way forward was to "leave it to the plant-breeders to add to the value of the crop per acre by improving the varieties now grown".[39] Biffen worked on several methods of adding value to crops. The following discussion concentrates on the three most significant, in that they informed the production of his two most popular varieties, Little Joss and Yeoman.[40]

Biffen released six new varieties of wheat for commercial use during his career. Little Joss, released in 1910 was named after an off-the-cuff reference to,

[38] For more on Biffen's involvement in economics see his attendance at the 1907 fourth annual meeting of the Association of Economic Biologists, hosted that year at Cambridge, and reported on by *Nature*, "Notes and News" 1907: 238.
[39] Rowland Biffen, "British Wheat: Improved Methods of Cultivation", *The Times* (8th June 1914), 16b.
[40] Besides the strategies presented here Biffen also experimented with wheat varieties that could be sown in spring and harvested at the same time as varieties traditionally planted in the previous autumn; thus reducing the time to harvest, see Biffen 1915a and 1915b.

"that little joss over there".[41] In 1912 Biffen released Burgoyne's Fife – named for the first experimental farm associated with the Cambridge University Department of Agriculture.[42] In 1916 Biffen released two new strains, Fennman and Yeoman, named no doubt to invoke images of the Cambridge landscape and the stout English farmer.[43] Finally, Cambridge Browick and Yeoman II were released in 1924. Each of these wheat varieties was supposed to have a specific function.

4.2.1 Disease Resistance

When Biffen began his breeding program in 1903 he was convinced the wheat plant had reached its yielding limit. Yields in Britain were already three times higher on average than those in New World wheat growing countries. Most British farmers produced 30 bushels per acre but yields of 60-80 bushels per acre, although rare, were not unheard of. Working under this conception Biffen's first variety, Little Joss, was designed to increase yields indirectly. Little Joss was released and recommended to farmers in the years around 1910 in order to combat rust losses. The release of Little Joss was more of an ad-hoc affair than the releases of Yeoman, and certainly, Yeoman II, detailed in the previous chapter. However, the variety still made the news; its release was widely noted in the popular press and as Biffen distributed parcels of seed to local farmers the variety's fame spread.[44]

By Biffen's reckoning Little Joss fell out of his work on modes of inheritance quite fortuitously. Even his initial interest in disease resistance was

[41] "How it was Originated", in *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University*, Rowland Biffen Papers.
[42] Biffen 1912b: 362-3.
[43] The Seed Corn Association, *New Varieties of Wheat Raised at the Plant Breeding Institute, Cambridge University, Offered by Hasler and Clapham, Dunmow Agents* (1916). Pamphlet held at the Museum of Rural Life, University of Reading, TR GUI MERL.
[44] See *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University*, Rowland Biffen Papers.

academic; he had wanted to compare the modes of inheritance of physiological with morphological characters; and disease resistance was a physiological character that had come to mind. Yellow rust resistance just happened to be the ideal character for him to study, quite beyond its putative economic importance. As Biffen said, "[I]mmunity and susceptibility to the attacks of yellow rust form as sharply a differentiated pair of characters as Mendel himself would have wished for".[45] Biffen's interest in yellow rust also followed on from his early career interest in mycology. When he had studied botany under Henry Marshall Ward, mycology formed the basis of the first papers he published.[46] Yellow rust was only one of the many pathogens that could attack wheat. Farmers also had to contend with bunt (or smut), stem rust and mildew. In other countries, as we'll see in the final chapter, there were even more pathogens and pests to worry about.[47] Despite farmers' indifference, Biffen continued believing that disease resistance was an important way of improving wheat.[48] Although many people seem to have agreed this was a good idea, and it is now remembered as such, Biffen failed to create any major support at the time. The problem was simple, if farmers were already producing more wheat in Britain per acre than anywhere else in the world, what was the point in adding an extra five or ten per cent to yields? We now turn to Biffen's key strategy for raising improved wheat varieties; the search for quality.

[45] See Biffen 1907b.
[46] Spells as president of the British mycological society at the beginning and end of Biffen's career illustrate his longstanding interest in mycology.
[47] See for example Pauly 2007 on the Hessian fly in America or Olmstead and Rhode 2008 on other US pathogens and pests.
[48] See Biffen 1924d and 1929.

4.2.2 Strength and the All-English Campaign

In 1903, speaking to the Cambridge Philosophical Society, Biffen recalled having begun crossing wheat in the summer of 1901; not just to test the wider applicability of, "Mendel's laws" but also, "with the object of raising improved varieties from the point of view of the farmer and the miller".[49] In his efforts to increase the bread-making qualities of British wheat, Biffen made his most-direct attempts to apply Mendelism to the wheat industry's problems. In 1913 his supporters were already beginning to claim some success for this strategy. Mr. Walter Runciman, president of the Board of Agriculture at the time, laid out the promise of strong wheats while arguing for increased funding for plant breeding, "At Cambridge, already Professor Biffen has proved he can grow wheat in large quantities of the hardness of Canadian wheat and the fecundity of British wheat, and that cannot but be of great monetary advantage to the farmers of this country".[50]

At the turn of the century Britain imported nearly 80% of the wheat used by the country.[51] As we saw in a previous section, many believed this was because an avalanche of wheat from the New World had swamped the British market. But, not only was New World wheat cheap, it was also superior in quality. As a result, port-based millers were dominating the market because of their access to imports; some even made bread purely from the higher quality imports on their doorstep. Inland millers were losing out because they had to pay to transport imports inland. If British wheat was used at all for bread making it was often only to dilute imported wheat. The quality which British wheat lacked was called "strength" and measured

[49] Biffen 1904a.
[50] Walter Runciman, "Board of Agriculture and Fisheries (Class II.—VOTE 11.)", *Hansard Parliamentary Debates*, 5th ser., vol 55 (1913), col. 2320.
[51] Ministry of Agriculture Farming and Fisheries 1968: 48.

by the ability of flour produced from the wheat to make a fluffy and voluminous "well piled loaf".[52] Because it was stronger, imported wheat commanded higher prices in a market which had generally fallen. Accordingly, Biffen argued that farmers should look to improve the quality of their crop. The hope was to revive the industry by producing an "All-English" loaf, made entirely from English flour. The way to do this was to breed wheats which would command a better price because they were of the same quality as imported wheat. Biffen believed if he could improve the bread-making quality of British wheat, inland millers could be induced to pay more for home-grown as they would save on transport costs. All of Biffen's varieties after Little Joss were aimed towards this end: the hope that if the quality of British wheat could be improved its price would rise and millers and farmers would profit.

Biffen worked with A. D. Hall and the Home Grown Wheat Committee on this problem. In 1905, Hall underlined the importance of quality in a piece for the *Journal of the Royal Agricultural Society*, "In all agricultural produce the question of "quality" is of the greatest importance, determining as it often does whether a given crop can be grown at a profit or not".[53] Exactly what constituted quality in a crop was, however, Hall continued, "A very subtle question".[54] Here, however, Biffen's long-time partner in his institutional plans, Thomas Wood, provided valuable assistance. Strength was thought to be dependant in some way on nitrogen content which affected gluten levels. Wood set about using his knowledge of agricultural chemistry to create a test of nitrogen levels and so quality in wheat. Wood was ultimately unsuccessful in this work, he devised a test but it remained less effective in Biffen's eyes than the time honoured method of chewing up seeds to

[52] Biffen and Humphries 1907: 2.
[53] Hall 1905: 321.
[54] Hall 1905: 321.

see how sticky they became.[55] However, Hall and Wood's involvement in Biffen's work on quality shows the widespread support that this strategy for agricultural improvement gained among the leading Mendelian-minded agricultural reformers of the day.

The promotion of Yeoman was widespread, culminating as we saw in the previous chapter, in the endorsement of the Linlithgow Committee's report and the release of Yeoman II. The promotion of Yeoman and Yeoman II ran simultaneously with the promotion of the All-English or Home Grown Wheat campaign. Sometimes the two promotional campaigns were indistinguishable, as reports and recommendations of the varieties were accompanied by recommendations to grow wheat for bread production. The Board of Agriculture's first announcement of Yeoman, for example, was sure to point out that Yeoman, "is sufficiently strong to produce a good quality loaf without the addition of imported flour".[56] However, it is useful to distinguish the two campaigns, as farmers themselves obviously did. While in the previous chapter the focus was on the superior stability and purity of Yeoman and Yeoman II, in this chapter the focus turns to these varieties' places in the All-English campaign. The appearance of many images of and much writing about bread clearly illustrate Biffen's commitment to the All-English Wheat campaign; the results of one of Biffen's many baking tests for strength can be seen in figure 4.3.

[55] Wood 1907c and 1907b.

[56] "Official Notices and Circulars: New Wheats" 1919: 458.

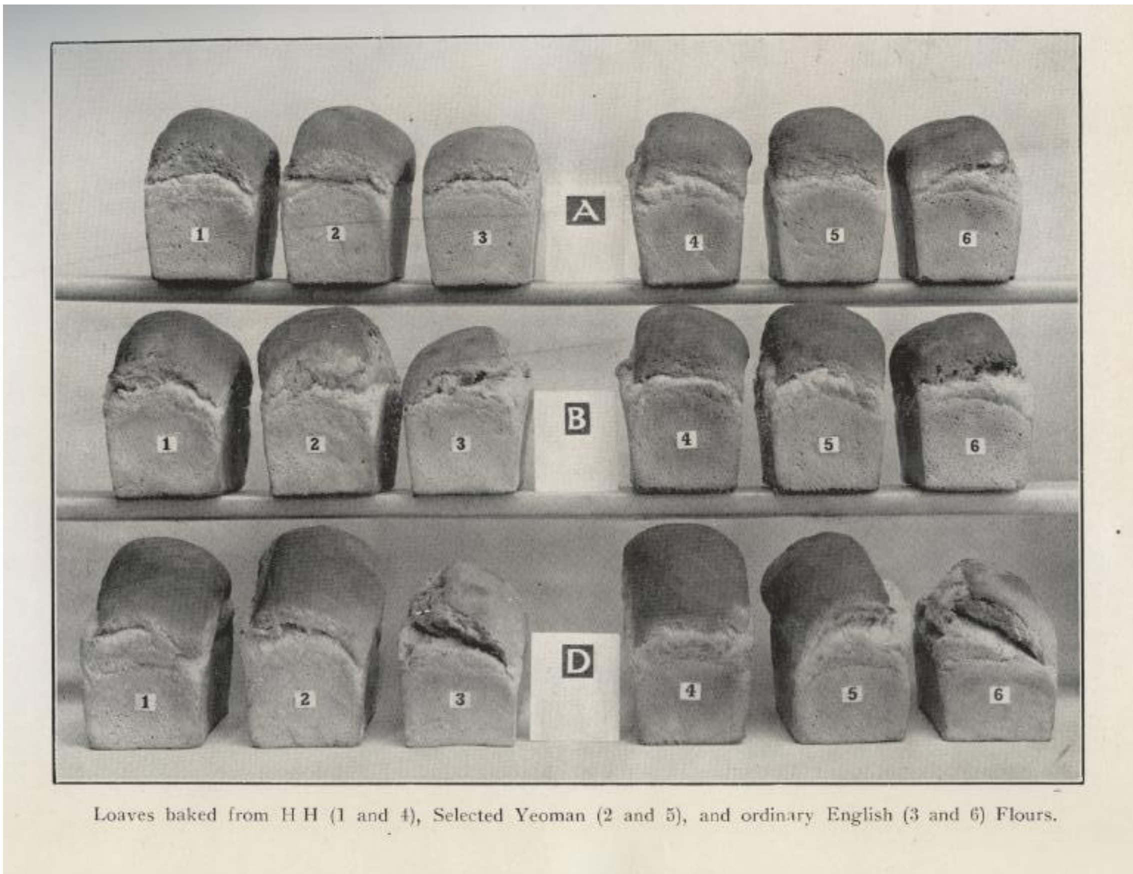

Figure 4.3 Loaves of Yeoman Bread. Reproduced from NIAB's report on the quality of Biffen's varieties, Parker 1923. In each group of three Yeoman is in the centre and ordinary English flour on the right. The loaf on the left is made from HH, a variety which was developed into Yeoman II.

Such images, and the sample loaves they captured, travelled around with Biffen. They were as much a part of his public demonstrations as the new wheat varieties themselves. Once again the popular press played an important part in this campaign. Notices appeared far and wide, especially in the months after the Linlithgow Committee's report.[57] The *Daily Mail* took on the campaign too, proclaiming:

> When the home-grown loaf is made again in this country on a large and useful scale (as hopeful agriculturalists believe it will be made) it will be due

[57] See the many articles on the All-English campaign collected at the National Institute of Agricultural Botany from newspapers as diverse as the *Daily Mail*, *Times*, *Macclesfield Courier*, *East Kent Gazette*, *Banbury Advertiser*, *Farmer and Stockbreeder* and the *Essex Weekly News*, reproduced in, *Newspaper Cuttings*, NIAB Archives, main library section.

> in a great measure to the work of Professor R. H. Biffen head of the School of Agricultural Botany at Cambridge [*sic*].[58]

The two campaigns, for Mendelian varieties and the way they should be used, shared a high water mark with the release of the Linlithgow Committee's interim report in 1923. The committee whole-heartedly endorsed Yeoman as a part of the All-English solution. The report also, however, contained a hint of the problems associated with this course of action. The idea was predicated on the public's preference for a certain type of bread, but some farmers were unsatisfied with this situation and they, "inclined to blame the trade for much of the prejudice that exists on the part of the consuming public in favour of white highly aerated bread".[59] Furthermore, these farmers, "assert[ed] that bakers are interested in encouraging the public taste in this direction".[60] The farmers' suspicion was that this sort of flour suited milling processes better but did little for them.[61] Having identified a shift in public taste the report's conclusions left that shift unexplained, while admitting it was one that was "led". Ominously, for concerned farmers, the report simultaneously recommended that, "the creation of an articulate demand is essential".[62] In fact using wheat to produce flour for making bread was only one option of many open to farmers. So while Biffen, Wood and Humphries had thrown their lot in with the All-English loaf, a strategy which essentially encouraged

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[58] "Wheat Wizard: Sir R. H. Biffen's New Grain", *Daily Mail* (2nd January 1925), reproduced in *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University*, Rowland Biffen Papers.
[59] Ministry of Agriculture and Fisheries. *Departmental Committee on Distribution and Prices of Agricultural Produce. Interim Report on Cereals, Flour and Bread*, Cmd. 1971 (1923), 73.
[60] *Interim Report on Cereals, Flour and Bread*, 73.
[61] See Bensusan 1931 for more on home-grown wheat and conflicts between millers, bakers and farmers, and Tann and Jones 1996 on the technological development of the mill roller. See also Perren 1990 on structural changes in the milling trade due to globalizing trade patterns.
[62] *Interim Report on Cereals, Flour and Bread*, 73, 75.

farmers to tailor their produce to millers' needs, it was far from obvious that this was the only, or even most desirable, option open to farmers.

4.2.3 Intensification

Despite Biffen's beliefs that the wheat plant had reached its yielding limit, there was one strategy which he investigated, and eventually recommended, which did increase yields. This was the strategy of intensification. As artificial fertiliser became commercially available in the years after the commercialisation of the Haber-Bosh process of ammonia production, farmers increasingly used these new fertilisers in the hopes of improving yields.[63] The farmers who could afford this new source of nutrients ran into a significant problem. As the heads of their wheat plants grew bigger they were also more likely to fall over dragging the plant with them and becoming "laid".[64] A laid crop could be ruinous to a farmer; it made harvesting much more difficult especially as it meant mechanical harvesters could not be used; these would simply grind the crop further into the field. Biffen's solution to this problem was to reduce the length of the wheat plant's stem while at the same time making it thicker. As he put the issue to the Selborne Committee, "More intensive cultivation will have to go hand-in-hand with the improvement of the varieties grown. Stiffer straws capable of carrying heavier crops ... will have to be provided before the most can be made of intensive cultivation".[65] Biffen had already made some moves in this direction in 1910 with Burgoyne's Fife. By Biffen's own reckoning this was a gentleman's wheat; it needed care, attention and often intense

[63] On the Haber-Bosch process see Hughes 1969 and Edgerton 2006: 64, 67.
[64] See Biffen 1917: 151 and Amos 1919 on the problem of heavy crops being laid.
[65] *Reconstruction Committee,* 28.

fertilising to reach its full capability.[66] Burgoyne's Fife swiftly disappeared, Biffen believed, because farmers in 1910 were unwilling to spend money on fertilisers and feared the losses that might ensue if a crop became laid. Although Biffen remained sceptical as to the point of growing more cheap wheat, intensification was a secondary aim of both the Yeoman and Yeoman II varieties.[67]

To some extent Biffen's strategies were run together, the aim being to build up an ideal wheat which possessed all of these features. While Biffen himself never claimed to have achieved this, and talked about his varieties as having different qualities, his allies in government pounced on this bold vision. Rowland Prothero, while speaking to Parliament on behalf of the Ministry of Agriculture and arguing for the need for more funding for Biffen's work, explicitly linked the strategies together. He began by introducing Biffen's work:

> I desire to illustrate the possibilities of scientific research in agriculture from the plant-breeding work of Professor Biffen at Cambridge. It is almost one of those romances in which science abounds. … It has been discovered that you can create a new variety of a plant by characteristics of other varieties of the plant. The result of this is most remarkable. Instead of having to wait for the chance discoveries of nature we can deliberately sit down and manufacture the kind of plant that we want.[68]

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[66] Biffen 1917: 173-175.

[67] Standing capacity featured heavily in the 10th report of the Development Commissioners, *Tenth Report of the Commissioners, for 1919-1920*, 230 (1920), 35.

[68] Rowland Prothero, "Mr. Prothero's Statement", *Hansard Parliamentary Debates*, 5th ser., vol 108 (1918), cols. 1265.

In his description of a scientific revolution in plant breeding Prothero was repeating a well-worn line that we have seen several times already from Biffen and Bateson. Prothero then went on to describe the details of Biffen's work.

> The experiments of Professor Biffen with rust-resisting varieties of wheat are typical of the process. After examining a number of varieties of foreign wheat he discovered a Russian wheat called ghirka, which resists rust. Now rust destroys annually thousands of quarters of wheat, but this ghirka wheat was of no use to the British farmer because its yield was miserably low. But Professor Biffen, by using the Mendel system, was able to transfer the rust-resisting quality of ghirka to a high yielding English wheat, and though that wheat has now been in use for several years it has shown no tendency whatever to revert either to the rust tendency of one parent or the low-yielding tendency of the other. He has now produced a wheat which produces a high quality of straw—a fine, stiff, upstanding straw—and a high quality of yield of grain, so much so that without pushing it will produce forty-two bushels to the acre, and by pushing up to seventy-two bushels to the acre. It also possesses a very high quality of disease resistance, and it combines with these qualities the quality of strength, which is so highly valued by both millers and bakers, and which is recognised in increased prices.[69]

The new wheat which Prothero promised to parliament, which would combine all of these characters, was the ill-fated Yeoman II. For Prothero, however, when trying to

[69] Rowland Prothero, "Mr. Prothero's Statement", *Hansard Parliamentary Debates*, 5th ser., vol 108 (1918), cols. 1265.

secure more funding, the promise of success was enough. These sorts of grand projections of the possibilities and successes of Mendelism prompt the question, how popular were Biffen's varieties with farmers?

4.3 A Reassessment of the Success of Biffen's Varieties

4.3.1 The Popularity of Mendelian Varieties

The reassessment offered here seeks to answer two interrelated questions. How popular were Biffen's varieties? And how were they used? In the previous chapter we saw how popular Biffen's varieties were at the Royal Horticultural and Agricultural Societies, the British Association for the Advancement of Science and in *Nature*, the *Times* and indeed, with other breeders. Now the focus turns to how popular Biffen's varieties were with farmers. There is very little data available with which to answer this question but figures from seed testing and crop data and the awards some farmers gave to Biffen point to a broad base of popularity which really did live up to the claims made in more rarefied forums.

At the turn of the century there were hundreds of varieties of wheat in existence – in 1921 NIAB held at least 125 in its observation plots.[70] Figures released in 1923 by the Official Seed Testing Station, housed by the National Institute for Agricultural Botany, indicate that out of these many varieties only eight were grown extensively. As each batch of seed sent to the station was tested for identity, it was a simple task to tabulate the amount of each variety tested. Although these figures only capture the relative use of Biffen's varieties by the types of farmers or dealers who had their seeds tested they are instructive nonetheless. It was

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[70] "Register Recording the Receipt of Seeds etc. by the Manager of Field Plots Commenced 1922", NIAB Archives. For a list of some of the important 19th century wheat varieties see Muir 1895: 182-183.

just these testing-minded farmers that the Mendelian system was trying to reach. Of the varieties tested in 1923, Yeoman made up 20% and Little Joss 9%. Their main competitor, Carters' Red Standard, made up 24% of tested samples and Squarehead's Master 11.5%. However, according to Biffen these were one and the same variety, Carters' had simply renamed Squarehead's Master.[71] The other major varieties in the period were Victor and Marshal Foch, Mendelian wheat varieties produced by Garton's, and the original Squarehead and Rivet, two wheats from the middle of the previous century.[72] Yeoman II barely registered in these results and disappeared after 1929. Of the eight varieties being used widely, four, including Little Joss and Yeoman, had been introduced after 1900, indicating a high rate of turnover of new varieties. Despite this swiftly changing market, Biffen varieties still accounted for 30% of the winter wheat seed that NIAB was testing in 1923.[73] In 1929, the first year in which NIAB produced a recommended varieties list for farmers, this figure had fallen to roughly 22%. At the end of the Second World War, thirty years after they had been released, Biffen's Yeoman and Little Joss still made up an impressive 11% of all seeds tested by NIAB.

Statistics produced in 1933 by Britain's leading agricultural economist of the day, J. A. Venn, suggest that by 1925 Little Joss and Yeoman occupied a quarter of the country's wheat acreage between them.[74] Given this figure is taken ten years after Yeoman's release and fifteen after Little Joss's and that there had been a high turnover in varieties since the turn of the century, this seems like a remarkably swift uptake and long lived success. Venn's data was drawn from agricultural returns

[71] Wellington and Silvey 1997: 29. This analysis based on yearly reports published in the *Journal of the National Institute of Agricultural Botany*, from 1922 onwards. Biffen's suspicions were somewhat reflected in the year of introduction NIAB ascribed to Red Standard; "1850-1900".
[72] On Garton's varieties see the seed catalogues from this period held in the NIAB archive.
[73] The same figure is also given in Thirtle et al. 1998: 131.
[74] Venn 1933: 564.

completed by farmers in the south eastern counties. On several occasions Engledow and Beaven asked farmers to complete questionnaires on the types of varieties they were growing.[75] Farmers resisted these requests (probably feeling the time would be better spent farming) so these sorts of farm returns are patchy at best. However, one further survey instigated by the Ministry of Agriculture in the early 1920s supports the data produced by Venn if only roughly.[76] In both data sets it is clear that Yeoman was principally grown in the rich loam soils of Essex and eastern Hertfordshire. Little Joss was more popular in the Fens around Cambridge were soil was more mixed and could be quite poor. While Little Joss had the longer lived popularity of the two varieties, making up 5% of the 1945 total of seeds tested, Yeoman was long lived in another sense. As figure 4.4 shows, it provided the starting point for a whole genealogy of new varieties in publicly-funded plant breeding. Some five varietal generations on there were still Yeoman genes in the Plant Breeding Institute's new varieties. Figure 4.4 is a map to a series of demonstration plots, displayed at a Plant Breeding Institute open day. Each of the varieties was grown in a little patch connected to its relatives in the other patches using ribbons, represented as green lines in the diagram. At the National Institute of Agricultural Botany both Yeoman and Little Joss are still grown today in observation plots, in much the same place that they were first planted there in the 1920s.

[75] See for example F. L. Engledow, "English Wheat" [Pamphlet reprinted from the *Essex Farmers' Union Year Book* 1927 by the Cambridge University School of Agriculture and *Essex Weekly News*], A. D. Hall Papers, 188-8-61: pp. 10.
[76] The Ministry of Agriculture collected 107 samples from the Eastern Counties in 1926. Of these samples, twelve were Yeoman, thirteen were Little Joss and seven were Yeoman II. See "Seed Wheat in the Eastern Counties" 1926: 15-16.

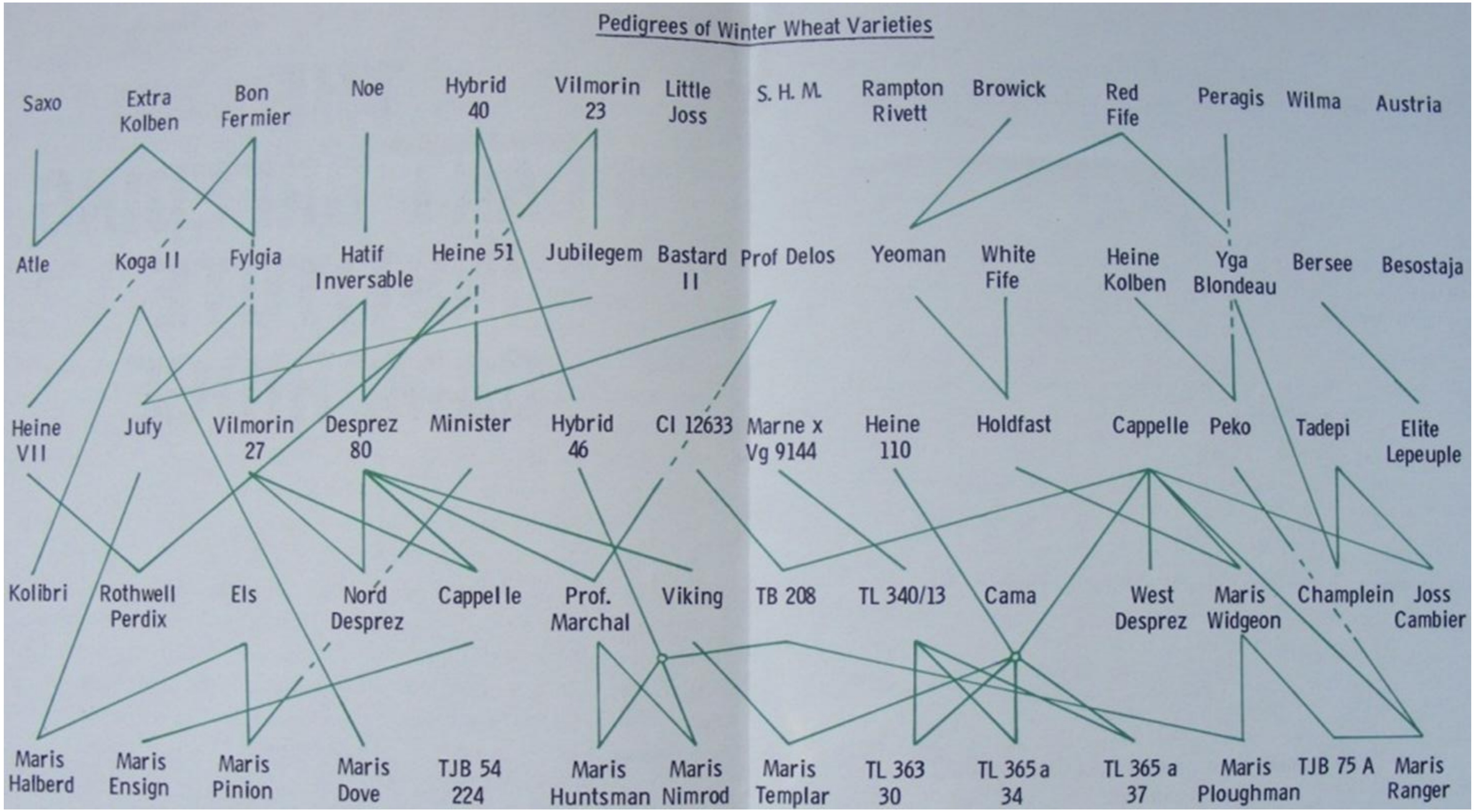

Figure 4.4 Varieties derived from Little Joss and Yeoman. Produced by the Plant breeding Institute circa 1975. Little Joss is just left of centre in the top row, Yeoman just right of centre in the second row from top. The legend reads, “The purpose of the exhibit is to demonstrate improvements in straw height, straw strength and disease resistance which have been achieved during the last fifty years. The most recent varieties are grown in the front row and successively older varieties in the rows behind. The relationship between varieties is indicated, sometimes in simplified form, by the coloured tapes”. Plant Breeding Institute Collections. Image reproduced from John Innes Archives courtesy of the John Innes Foundation.

On 16th December 1921 Biffen and Yeoman were awarded a silver bowl by the Essex Farmers' Club, see figure 4.5. The bowl, weighing 82 ozs., was offered up and explained as recompense, as "this great research has been without any financial gain".[77] The nature of the occasion, held at The Shire Hall in Chelmsford on Christmas market day and pontificated over by the local Dignitary Hon. E. G. Strutt, was a provincial affair, to recognise the success of Yeoman on a local level, in Essex. Despite its local character the event was reported on nationally in *Nature* and the *Daily Mail*.[78] The award signalled a genuine appreciation by Essex farmers for Biffen's varieties. However, an identical bowl was given at the same time to Edwin Sloper Beaven, Biffen's colleague at NIAB who had been responsible for the realignment of the institute away from its early plans for large scale distribution. Beaven was sceptical of the power of Mendelism to transform breeding and felt that the theory had not played any role in the production of his own varieties.[79] Beaven's presence indicates that the Essex event was not really a celebration of Mendelism, it was rather a celebration of varieties.

[77] More detail on this ethic of public service to Nation and Empire is given in chapter 6.
[78] "Notes" 1921: 543 and "Wheat Wizard Honoured by Farmers Grateful for Fine Harvests", *Daily Mail* [1921], reproduced in *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University*, Rowland Biffen Papers.
[79] For more on Beaven see Palladino 1994.

Figure 4.5 Award from the Essex Farmers' Club. Biffen, on the right, is posed receiving his bowl from E. G. Strutt. The man smoking on the far left at the back is Lawrence Weaver.

4.3.2 The Use of Mendelian Varieties

Considering how Biffen's varieties were intended to be used, and their popularity with farmers, we might expect to see an increase in yields following the widespread adoption of Little Joss and an increase in price following the adoption of Yeoman as its stronger flour commanded higher prices. Figures 4.1 and 4.2 (above) show that no such increases in yield per acre occurred; acreage and production growth and decline tracked each other pretty consistently throughout the period. Increases in price during the war (shown in figure 4.6 below), can be attributed to the Government offering guaranteed prices for wheat from 1917. The decline in prices in 1922 indicates the effect of the removal of these measures with the repeal of the Corn Production Act in 1921.[80] Slight gains in price in the mid-1920s are attributed to bad weather by the Linlithgow Committee; despite their best hopes these

[80] The repeal was mooted in 1919 but after several amendments enacted in 1921, see *Corn Production Acts (Repeal)*, 1921.

increases were not the result of millers offering more for Yeoman wheat. The few co-operatives which were established to grow and mill all-Yeoman flour had been unsuccessful.[81]

Biffen's solutions were not the only possible response to the problems of the wheat growing industry. Farmers could, instead, sell their wheat for biscuit-making or animal feed, accepting a reduced price but countering it by increasing production with old varieties and avoiding the cost of new ones. They might try to increase production through better husbandry, or they could use the wheat to feed their own animals, any of these options would make them less money but would also cost them less. There was also the possibility of selling straw for thatching, which required a particularly high grade of straw.[82] Once government-backed fixed prices were introduced this created yet another dynamic in the wheat market. With fixed prices, despite what Biffen said, it paid to grow more wheat of whatever quality.

At the Farmers' Club meeting which began this chapter, Biffen did not have the evening entirely his own way. William Hasler, Biffen's long-time ally in plans for seed distribution, refused to believe there could be a limit to improvements in yield in cereals and pointed to gains made via hybridity in livestock breeding. Mr. Patterson and Mr. Sherwood pointed to the success they were having growing wheat for chicken feed and straw respectively, Patterson growing Rivett and Sherwood Little Joss. A glimpse of the tensions which underwrote this encounter between Biffen and the farmers can be seen in Biffen's arch reply to Mr. Patterson's belief he was making more money from growing wheat for chicken feed. In response Mr. Patterson snapped, "[Yeoman] will not do on my light land".[83] Finally Mr. Alfred

[81] Ministry of Agriculture and Fisheries. *Departmental Committee on Distribution and Prices of Agricultural Produce. Interim Report on cereals, Flour and Bread*, Cmd. 1971 (1923), 71-72.
[82] See Engledow 1950: 17.
[83] Biffen 1924a: 17.

Amos, whose report for the *Journal of the Board of Agriculture* in 1919 had been used by Morris to underwrite the success of Yeoman at the British Association for the Advancement of Science, came to Biffen's aid and expanded on the problem of rogues, pointing out that the admixture of other wheats was actually the principal cause of millers reticence to pay a premium for Yeoman.

As we saw in the first section of this chapter, the total acreage of wheat in Britain was in decline; from 1885 to the start of the War half a million acres were lost, see figures 4.1 and 4.2. Faced with competition from abroad and a lack of import tariffs, farmers moved to other crops. Those who did stay with wheat often moved their crops onto poorer land freeing up their prime land for other more remunerative, or less input-intensive purposes, such as rearing cattle. This move was encapsulated in the slogan, "Down Corn, Up Horn".[84] This is where Little Joss came into its own, not because of its disease resistance, but because the variety flourished on poor soils without much fertiliser. To be sure its flour was only really good for biscuit making or chicken feed, neither of which paid as highly as bread making, but if it required fewer inputs to grow then this loss of price was often acceptable to farmers. By 1919 the Ministry of Agriculture was recommending Little Joss on the basis that it was a "hardy variety" that did well on poor soils while, "it is too weak in the straw for rich land".[85] The War also increased the demand for biscuit flour and Little Joss was recognised as a variety which produced flour well suited to biscuit production.[86] Furthermore, when compulsory ploughing-up of land for conversion to cereal crops was introduced by the Government in 1917, large amounts of less fertile land needed to be planted and Little Joss was often the variety of choice.[87]

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[84] See Hall 1936: Chapter 17 and p.363 and Orr 1922: 86-88.
[85] "October on the Farm: Wheat Varieties" 1924: 670.
[86] Engledow 1950: 17.
[87] See also the plans, never realised, of Mr. Wright, Member of Parliament for Rutherglen. "For the people of this nation, we can drain those extensive areas of land which are

Year	Wheat		Barley		Oats		Year	Wheat	
	s.	d.	s.	d.	s.	d.		s.	d.
1870	10	11	9	8	8	2	1920	18	10
1871	13	3	10	2	9	0	1921	16	8
1872	13	4	10	5	8	4	1922	11	2
1873	13	8	11	4	9	1	1923	9	10
1874	13	0	12	7	10	4	1924	11	6
1875	10	6	10	9	10	3	1925	12	2
1876	10	9	9	10	9	5	1926	12	5
1877	13	3	11	1	9	4	1927	11	6
1878	10	10	11	3	8	9	1928	10	0
1879	10	3	9	6	7	10	1929	9	10
1880	10	4	9	3	8	3	1930	8	0
1881	10	7	8	11	7	10	1931	5	9
1882	10	6	8	9	7	10	1932	5	11
1883	9	8	8	11	7	8	1933	5	4
1884	8	4	8	7	7	3	1934	4	10
1885	7	8	8	5	7	5	1935	5	2
1886	7	3	7	5	6	10	1936	7	2
1887	7	7	7	1	5	10	1937	9	4
1888	7	5	7	10	6	0	1938	6	9
1889	6	11	7	3	6	4	1939	5	0
1890	7	5	8	0	6	8	1940	10	0
1891	8	8	7	11	7	2	1941	14	8
1892	7	1	7	4	7	1	1942	15	11
1893	6	2	7	2	6	9	1943	16	3
1894	5	4	6	10	6	2	1944	14	11
1895	5	5	6	2	5	2	1945	14	5
1896	6	1	6	5	5	4	1946	14	10
1897	7	0	6	7	6	1	1947	16	9
1898	7	11	7	7	6	7	1948	21	0
1899	6	0	7	2	6	1	1949	23	3
1900	6	3	7	0	6	4	1950	25	10
1901	6	3	7	1	6	7	1951	28	8
1902	6	7	7	2	7	3	1952	29	7
1903	6	3	6	4	6	2	1953	31	2
1904	6	7	6	3	5	10	1954	28	3
1905	6	11	6	10	6	3	1955	22	11
1906	6	7	6	9	6	7	1956	25	6
1907	7	2	7	0	6	9	1957	21	7
1908	7	6	7	3	6	0	1958	21	9
1909	8	7	7	6	6	9	1959	21	0
1910	7	5	6	6	6	3	1960	21	4
1911	7	5	7	8	6	9	1961	20	7
1912	8	1	8	7	7	9	1962	21	10
1913	7	5	7	8	7	9	1963	20	11
1914	8	2	7	7	7	6	1964	21	11
1915	12	4	10	5	10	10	1965	22	6
1916	13	8	15	0	12	0	1966	22	4
1917	17	8	18	1	17	11			
1918	17	0	16	6	17	9			
1919	17	0	21	2	18	9			

Figure 4.6 Corn Returns Prices 1870-1966. Ministry of Farming and Fisheries 1968.

frequently under water for several months in the year, deteriorating the value of the land, and we can use the scientific knowledge of men like Sir Rowland Biffen, who are giving us new types of wheat year by year in this and other countries". William Wright, "Ministry of Agriculture and Fisheries", *Hansard Parliamentary Debates*, 5th ser., vol 187 (1925), col. 1363.

The story of Yeoman also shows that farmers used the variety in ways which were not necessarily intended. Figure 4.6 shows that prices for wheat did not rise appreciably after the release of Yeoman or Yeoman II (except as a result of government price fixing).[88] On the other hand, two of the most striking patterns of agricultural development in the first part of the twentieth century were the increasing size of farms and, after the War, the increasing ownership of farms by farmers who had previously been tenants.[89] Financially the War was in fact a good thing for many tenant farmers as production and prices spiked. With profits they made during the period of government control of wheat prices many farmers looked to own their own farms, increase their size and farm more intensively.[90] Yeoman fitted in with this trend perfectly. One of its most obvious botanical features was a far shorter length of straw than other varieties. This meant that farmers could use far larger amounts of artificial fertiliser without fear that the increased weight of the ear would cause the plant to fall over. As the Ministry of Agriculture put it in 1924, "Yeoman excels in milling quality, and also in strength of straw–an important consideration for the farmer who desires to make the best use of artificial fertiliser".[91] Despite the millers' unwillingness to pay a premium for Yeoman, larger crops still meant better profits even if a farmer could only sell his crop for chicken feed. In 1930 when NIAB released a list of recommended varieties both Little Joss and Yeoman featured, "Little Joss should be chosen for the lighter wheat soils … or where fertility is low", Yeoman was one of the, "varieties to grow on the richest soils or under intensive manuring".[92]

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[88] On wheat prices see Clark 2004.
[89] See Dewey 1989.
[90] Increasing farm size was a trend which had started before the war and which some blamed for rural depopulation, see Levy 1911: 61-70.
[91] "October on the Farm: Wheat Varieties" 1924: 670.
[92] First Recommended Variety List 1930, NIAB Archive.

From the 1930s tractors increasingly became a key feature of this kind of intensive production. Tractors supplemented the labour needed to fertilise and harvest large crops after the War, when manpower and horses were in short supply.[93] Although many farmers returned to draught power as horses became available again, the largest continued to use tractors. For those farmers that used tractors and combine harvesters Yeoman was *the* wheat, and the start of a whole string of descendants. It was less likely to be laid and furthermore, its short straw reduced the amount of by-product material that needed to be processed helping the thresher or combine to run more smoothly.[94] Yeoman and mechanisation were part of the same system of intensive farming which was increasingly employed after the War. This, rather than the All-English solution, was the pattern of farming into which farmers, whether they were following the Ministry of Agriculture's advice or not, most often integrated Yeoman.[95] This pattern of increased corn production, "Up Corn, Down Horn", was one identified in 1934 in an editorial from *Nature*:

> The corn dominant view is that England grows some of the best wheat in the world and with yields that are higher than those of most other countries. Thanks to mechanisation production costs bid fair to fall to a level comparable with those in the great wheat growing lands.[96]

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[93] See Edgerton 2006 on the continued use of old horse technology.

[94] On government support for increased use of tractors see the findings of the commission into agricultural machinery prices in 1920, Board of Trade, *Reports, by the Standing Committee (and Sub-Committees) on the Investigation of Prices, Relating to: Agricultural Implements and Machinery,* Cmd. 1315 (1921), 7.

[95] In the 1970s the semi dwarf Norin-10 genes used in green revolution varieties were inserted into winter wheat by F. G. H. Lupton and John Bingham at the Plant Breeding Institute. The new shorter wheat varieties were hugely more productive and some see these as the triumph of the move towards shorter stiffer strawed wheat, see Lupton 1987.

[96] "Economic planning and agricultural management" 1934.

Biffen seems to have become sanguine about the failure of the All-English solution at the end of the 1920s. Writing for the *Yearbook and Annual Report of the Essex County Farmers Union* in 1930 he looked approvingly to a competition that had been held in Italy in the previous year.[97] The idea behind the competition was to test the use of heroic quantities of fertiliser on wheat crops. The farmers involved, including Professor Gibertini, a "scientific propagandist", had produced yields of up to 80 bushels per acre. This result inspired Biffen to revise his previous view that the wheat plant had reached its yielding capacity. Biffen's simultaneous allusion to the plant as machine is a fascinating foreshadow of a view that has been rejuvenated at the start of the twenty-first century in the form of bio-pharming, "The fact that it is possible to produce such crops", as indeed it was for some of his friends in Essex, was for Biffen a "clear indication that the wheat plant – using the word for once in a double capacity – is not often forced to run at its full capacity, and that more might be made of the uncanny machinery which produces grain from raw materials present in the air and soil".[98] Instead of advising farmers to grow stronger wheats, a strategy which disappeared from his public appearances after 1926, he was now, from slightly behind the curve, advising them to grow more intensively.

Little Joss was a wheat used by farmers aiming to get more from less, or even less from less. Yeoman was a wheat used by the sorts of farmers who clubbed together to buy a silver bowl for Biffen as a grand gesture of thanks; farmers who before the arrival of cheap fertiliser had not had to pay much to get more from Yeoman grown in the rich soil of Essex. These were, furthermore, exactly the sorts of farmers who would use tractors after the War to manage their huge farms extending over hundreds of acres. Disease resistance and strength remained

[97] Biffen 1930: 225-226.
[98] Biffen 1930: 225.

secondary considerations for most farmers until the rust epidemics of the 1950s and the introduction of the Chorley Wood milling process. This new way of making flour meant that Biffen and Humphries' dream of a commercially viable All-English loaf was finally realised, even if neither man lived to see the event.

4.3.3 Biological Innovation Prior to 1950

Biological innovations in plant breeding have, until quite recently, existed almost entirely outside legal frameworks of intellectual property.[99] As a result, gauging the frequency and importance of plant breeding innovation in economic history is more challenging; there is no patent record to analyse. Furthermore in the British case (and the American one), the significance of varietal innovation is often overlooked because, as figure 4.7 shows, yields remained fairly constant from 1885-1945. Coupled with a lack of patents, static yield data has led to the period before the 1950s often being represented as being barren of significant biological innovation.[100]

The history of agricultural innovation has for many years been in the sway of the idea of induced innovation encapsulated in the Hayami and Ruttan model.[101] The lack of patents or yield increases is part, in this model, of a larger picture. The classic case studies of agricultural development in which this model has been explored are Japanese and American agriculture. In each country the specifics of land and labour prices, so the thinking runs, induced innovations to occur in those areas. So in America where land was plentiful but labour was expensive, the

[99] On the first Plant Patent Act in 1930 in the US see Cook 1931 in the *Journal of Heredity*, and Kevles 2007, 2008b and Kevles and Bugos 1992. On the development of plant breeders rights in the UPOV framework see Rangnekar 2000 and Dutfield 2003 and 2011.
[100] See Palladino 1997: 210-211 and 1996b on 1950s perceptions of Mendelism no longer being useful to agricultural geneticists.
[101] See Collins 2000b: 86 on stagnant crop yields up until the Second World War.

incentive was to deploy labour saving innovations such as the mechanical reaper. In Japan the opposite occurred. In a country with abundant cheap labour but much less spare land, the innovations that came to be adopted were land saving biological ones, new varieties of rice, or improved methods of cultivation. Europe, has often been thought to lie somewhere in between these two extremes.[102]

New directions for the assessment of biological innovation's impact on agriculture have recently been suggested in the American historiography. Most orthodox analyses of American agricultural innovation, following the induced innovation model, highlight the importance of tractors and combine-harvesters and mechanical innovation in general. In the American case this perception is bedrock, encapsulated in the slogan that Cyrus McCormick (of tractor fame) was the "man who made bread cheap".[103] Such thinking has recently been challenged by Olmstead and Rhode in a reappraisal of American agricultural development.[104] Drawing on their extensive knowledge of agricultural history and economics and United States Department of Agriculture crop returns data, the authors demonstrate that biological innovation played an important and thus far unrecognised role in pre-1950s US agriculture. On one hand, they argue, new varieties maintained productivity that would have otherwise diminished, as a result of expansion into areas less favourable for cultivation or the ravages of pests. On the other hand, the authors claim, accounts that prioritise mechanical developments neglect the interplay between biological and mechanical innovations.

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[102] On Hayami and Ruttan and the induced innovation model see Palladino 1987 and Hayami and Ruttan 1985. For an extension of this model to the European case, see Thirtle and Ruttan 1987 see also Ruttan 1996.
[103] Olmstead and Rhode 2008: 4-7.
[104] See Olmstead and Rhode 2008 for a broad statement of their thesis and Olmstead and Rhode 2011 on varietal innovation as a means to cultivate new areas.

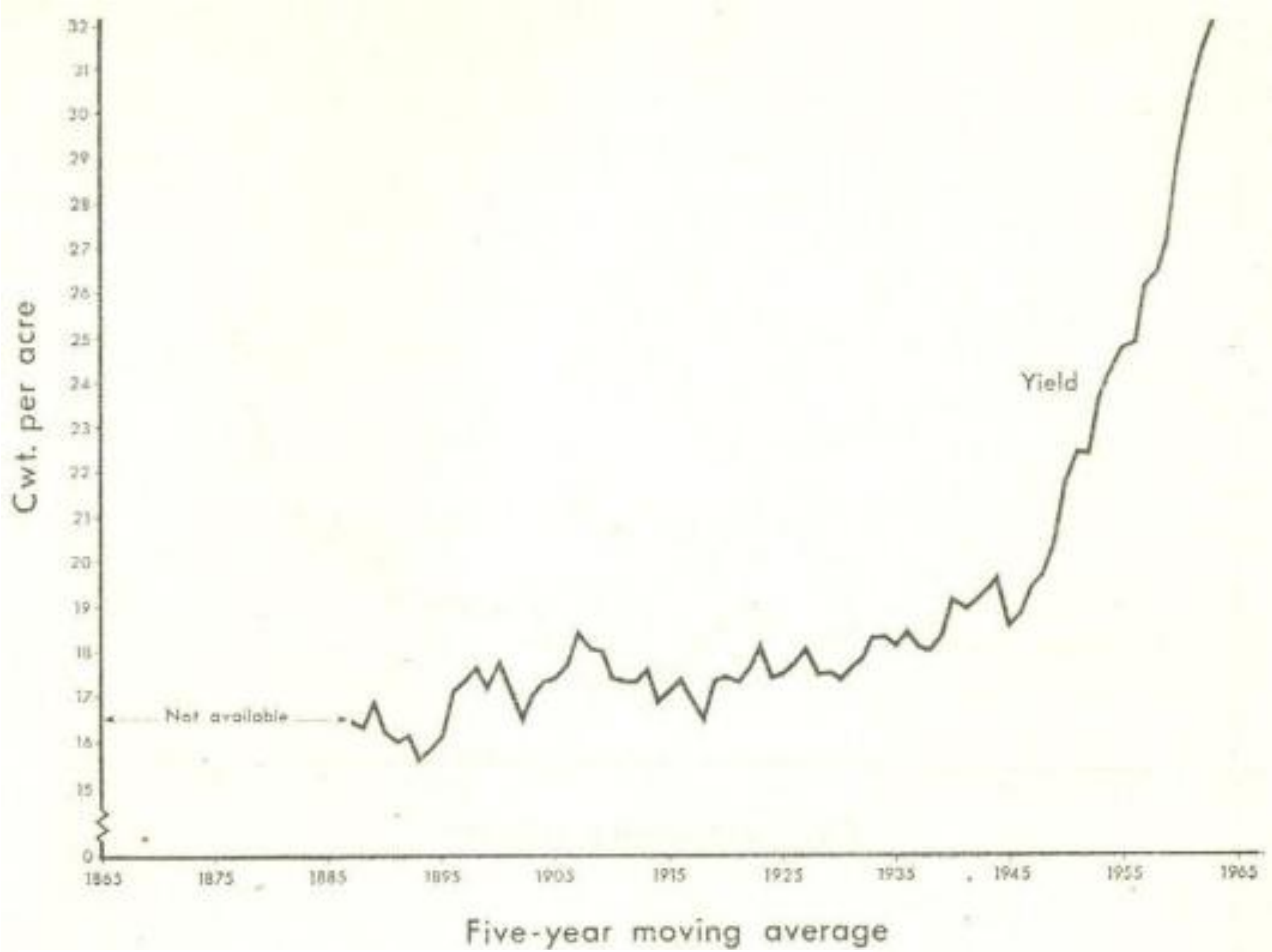

Figure 4.7 Wheat in Great Britain. Ministry of Farming and Fisheries 1968.

Gains in production around 1918, shown in figure 4.1, have been, as we have seen, most often attributed to the Government's war policy from 1917-18 of forced ploughing up of pasture land to be converted to cereal production. However, if we take on board the message from Olmstead and Rhode, Biffen's varieties might have played two important roles; firstly in maintaining yields and secondly in integrating with a generally more intensive, and to some extent more mechanised, system of agriculture. Little Joss allowed new areas of land to be developed and may well have countered some of the worst effects of yellow rust in areas, often by the coast, where yellow rust was prevalent. Yeoman, for the most part, was a variety which allowed farmers to make the most of other developments in fertilisers and mechanisation. The comparison with American agriculture should not be stretched too far. Labour structures and land availability, for instance, were very different issues in each country. However, a little of Olmstead and Rhode's thinking goes a long way to explicating the multiplicity of ways in which varieties could be successful.

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In response to the question of whether Mendelian varieties were successful, this chapter has argued for a complicated yes-and-no answer. Mendelian varieties were popular, as the pundits claimed, and they were agriculturally important, but not quite for the reasons Biffen principally intended. This chapter has shown that in the period 1900-1925 Biffen's wheat breeding program produced a string of new varieties. These biological innovations had an impact which has thus far been unrecognised. It seems that despite relatively constant yields, production and prices Biffen's varieties might well have had an impact on agricultural production. On one hand they played an important maintenance role helping to keep yields constant where otherwise they might have fallen as acreage decreased in quality, on the other hand they integrated with, and facilitated the adoption of, new farming patterns. Before the First World War, Little Joss allowed for more extensive production in response to competition from the New World. After the War, Yeoman allowed farmers to move to more intensive patterns of production as farming became increasingly commercialised. Recovering the fortunes of Little Joss and Yeoman helps us to reassess the importance of biological innovation in this early period.

In terms of the system theme advanced so far in the thesis there are two lessons to be taken from this analysis. The first is that an extension to Hughes's notion of systems is required, and the second that a view from outside the system, of its economic success, gives us a better platform for judging early Mendelian impact on agriculture. The extension required to Hughes's thinking is an incorporation of the consumer. The role of farmers here is interestingly paralleled in other hold out, or subverted technologies. Capturing these different ways of using Mendelian innovations brings a richer detail to the operations of the Mendelian system. This detail illustrates well that the Mendelians did not have things all their own way. Farmers' practices could not be bent entirely to the ends that Mendelians wished to pursue. Stepping outside of a focus on systemic parts and relations as this chapter has, also allows for a further contextualisation of the Mendelian system. In the previous chapter we saw Mendelians responding to and adopting the tropes of the pre-existing moral economy of plant breeding. In this chapter we have seen them responding to the exigencies of the national wheat industry and the international wheat markets. These activities put paid to the idea, advanced by Palladino and others, that agricultural Mendelians were operating in isolation from the world of farming.

Chapter 5

Global Connections: Resources, Threats and Beneficiaries

THE STORY THIS THESIS HAS TOLD so far, of disciplinary entwinement between agricultural science and Mendelism, institutional expansion, theoretical entanglement, production and distribution of varieties and their integration into the agricultural economy, has largely focused on what was happening around Cambridge. There was, however, a significant international dimension to these developments. This dimension played out in several ways. As we saw in the last chapter, many believed foreign competition was making British wheat-growing unprofitable and driving farmers to arable and dairy farming. However, seeds and varieties, ones that formed the basis of Biffen's wheat breeding programme, were also sent to Cambridge from the New World. Often these materials came from former pupils, like Balls, who had gone to take positions in the Colonies, Dominions and countries that formed the economic empire. Biffen was deeply concerned for colonial agriculture, and made two significant trips to the colonies, the second in hope of expanding what he called the "agricultural empire". The focus of this chapter is, accordingly, on the nature of the contrasts and connections between systematic developments around Cambridge, and those in the agricultural empire.

Global history has recently become both fashionable (unlike agricultural history) and, as a result, more clearly defined. One significant interpretation of the new global history suggests that moving beyond cultural, racial or national units of analysis to a global scale, revealing connections and contrasts, previously unrecognised, between geographically disparate locations, historians can usefully paint on a larger canvas. The contrastive elements of such analyses are essentially based on comparative history, albeit on a larger scale.[1] The analysis of connections on a global scale is a natural extension of the systems theme of this thesis. Although largely absent from Hughes's work itself – Hughes tended to view the electrical systems that he described as nation specific – this was an obvious part of Biffen and other Mendelians' aspirations. This chapter describes the emergence of a global system that connected Cambridge, Australia, Argentina, Kenya and New Zealand.[2]

The narrative in this chapter begins with the work of William Farrer, a Cambridge man who left the university in the 1860s and emigrated from Britain to Australia where he became a wheat breeder. Farrer's story provides a fascinating contrastive twist to the tale of Mendelian theoretical entanglement with plant breeding. Towards the end of his life Farrer was well known to Biffen, who struggled to incorporate Farrer's accomplishments into the tale of Mendelian triumphalism. Not only had Farrer pre-empted many of Biffen's results, breeding his own rust resistant varieties without the aid of Mendel, but he had done this working from a Darwinian-based scheme of selection. In the second section of the chapter we turn to one of the Cambridge School of Agriculture's most beloved emigrants, William Backhouse, who was working in Argentina from 1911. Backhouse's story highlights the importance of such workers, who took agricultural research jobs

[1] See O'Brien 2006 on the new global history. For national comparative histories of genetics see Kevles 1980 on the US and Britain, Roll-Hansen 2000 on Britain and Sweden and Maat 2001 on agricultural development in the Netherlands and its colonies. This chapter uses O'Brien's analysis rather than taking on the whole of the colonial and post-colonial studies literature, which is vast and for the purposes at hand, distracting. The history of science and colonial studies have less frequently been brought together, Palladino and Woboy's classic paper "Science and Imperialism" informs much of what follows, warning as it does against a repetition of imperialist patterns by historians of science which privilege the centre and ignore the particulars of local colonial contexts, Palladino and Worboys 1993.

[2] Olmstead and Rhode have already drawn together much of the available secondary literature on Backhouse and Farrer, see Olmstead and Rhode 2007. The analysis they draw from this material is concerned with agricultural economics, and particularly framing a supplement to the grain invasion historiography, what they call "the other grain invasion". This was the circulation of germ-plasm in the nineteenth and early twentieth century in just the sort of networks described in this chapter.

around the globe, for the standing of the institutions which had trained them in Britain. These disparately located workers formed a network which Biffen actively cultivated. A decade later, with Mendelism firmly in the ascendency, Biffen was himself doing the travelling, having been invited on a research trip to Kenya by the colonial government. Biffen’s trip to Kenya in 1926 came just on the cusp of a watershed in the situation back in Britain as the National Institute of Agricultural Botany’s scheme to distribute seeds failed. In Kenya, however, when invited to lay out his plans, Biffen revealed most clearly his aspirations for how a Mendelian system should be organised. Finally the chapter turns to the work of Otto Frankel, in New Zealand, where he arrived in 1928, just as NIAB was undergoing a change of mission back in Britain. With Biffen’s backing, Frankel attempted to implement a similar regime of wheat breeding and stock maintenance in Christchurch on New Zealand’s South Island. The contrasts and connections between Backhouse, Frankel and Biffen’s work and their respective contexts throws into even sharper relief the central theme of this thesis; Mendelian concerns with the systematisation of plant breeding to improve agriculture. The chapter begins, however, with the work of an Australian Mendelian-sceptic, one who built up his own systematised plant breeding based on a Darwinian view of selection.

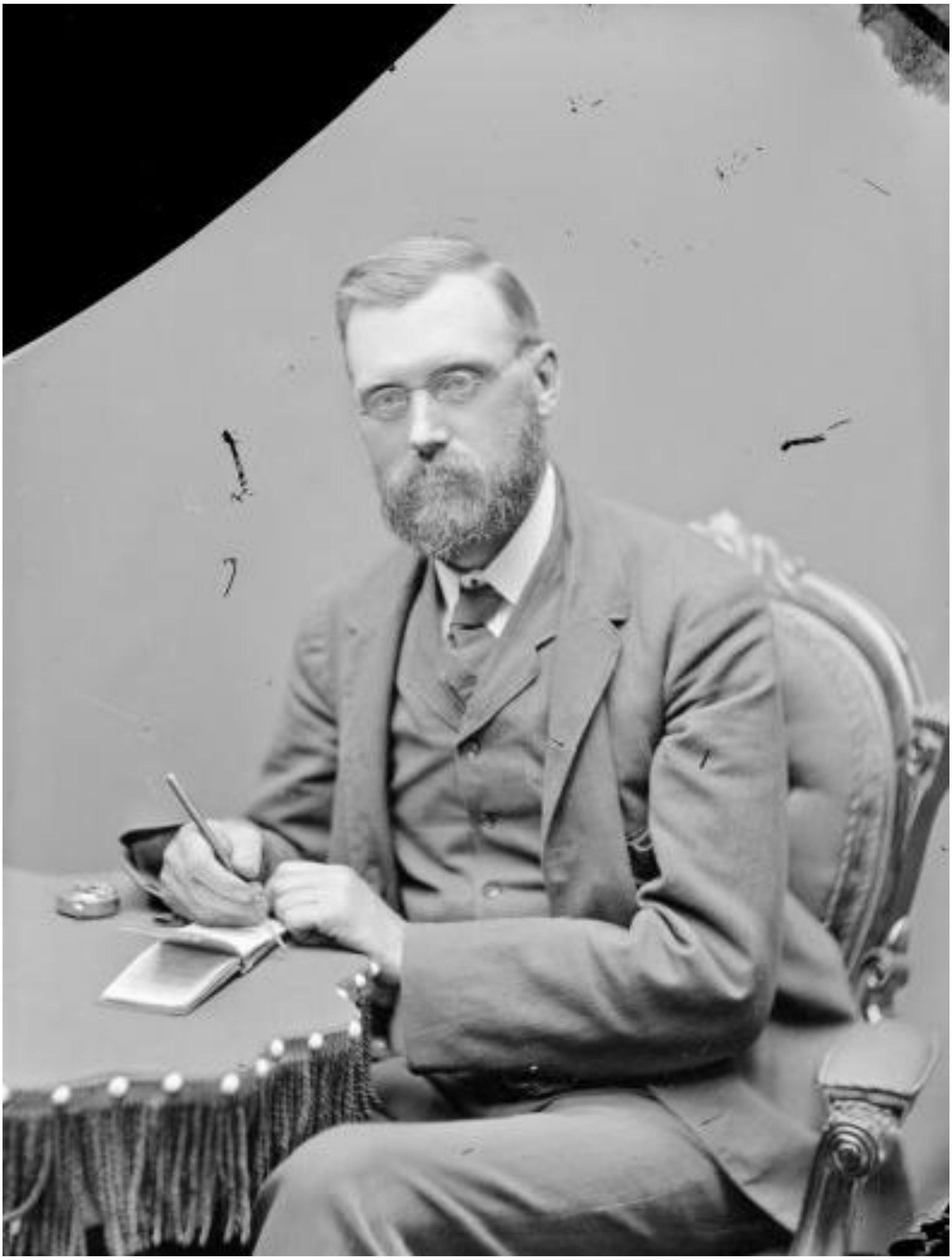

Figure 5.1 William Farrer (1845-1906). Portrait of William Farrer, Lambrigg [?], Australian Capital Territory, ca. 1900, De Salis, Farrer and Champion families photograph collection, National Library of Australia, pic-vn4656093.

5.1 International Networks of Exchange

5.1.1 William Farrer and Darwinian Plant Breeding in Australia

William Farrer (see figure 5.1), so the legend runs, revolutionised wheat growing in Australia. Celebrated as a father of the nation, Farrer has appeared on the country's banknotes, as numerous statues and in the names of equally many schools and colleges. He has been celebrated as one of the *Six Great Australians* and centenaries and annual lectures, organised by a Farrer Memorial Trust, honour his achievements.[3] Farrer and particularly his aptly named variety, Federation, were celebrated in much the same way Biffen's effort were in Britain, and for considerably longer. The success and visibility of Farrer's Darwinian-based breeding program posed serious problems for the claims Biffen was making about the need for a plant breeding programme based on Mendelism. In what follows we will see the outline of Farrer's Darwinian breeding program. An important feature of this program was the network of correspondence Farrer maintained with other plant breeders around the globe. Finally we will see how Biffen came into this ring of correspondence in the years immediately prior to Farrer's death. Despite his best efforts to convince Farrer, the older man remained indifferent to Mendelism. In closing Farrer's story we will see how after his death, Biffen, despite his promises to honour Farrer's work, effectively airbrushed Farrer's Darwinian commitments from the later accounts he gave of the history of Farrer's plant breeding.

William James Farrer was born to poor parents in Cumbria in 1845. He attended Christ's Hospital School on a scholarship and then went on to Pembroke College, Cambridge where he earned a B.A. Shortly afterwards Farrer emigrated from Britain to Australia. During the 1870s Farrer published a speculative paper on

[3] See Cowen 1964.

sheep and grass farming, taught at the Duntroon sheep station, qualified as a surveyor and worked for the Department of Lands.[4] In 1882 he married Nina De Salis, the daughter of the local New South Wales Member of Parliament. Four years after the wedding Leopold De Salis gifted his daughter a large area of land, close to the present-day location of Canberra, which the couple named Lambrigg.[5] Eventually, after trying viticulture, Farrer turned his hand to wheat farming and breeding. Initially his efforts focused more on bringing public attention to the possibilities of wheat improvement by a planned program of breeding than actual field work. Farrer began sending letters about his plans to the *Queenslander* and *Brisbane Courier* newspapers.[6] Farrer advised farmers that they should select the seeds from the rust free plants in their fields. These should then be collected centrally, by a suitable authority, mixed together, and sown under conditions in which rust was likely to occur. If this process of selection and mixing was continued, season upon season, Farrer felt that it was, "more than likely that a number of rust-proof varieties will be secured".[7] At this point, with rust proof varieties in hand, he envisaged a programme of hybridisation would take over in which the milling properties of these rust resistant varieties would be improved. This combination of Darwinian selection and hybridisation became the leitmotif of Farrer's own wheat breeding work which he began a few years later.[8]

[4] Unfortunately the pamphlet has long since disappeared however it seems to have been received well, see "Grass and Sheep Farming", *The Queenslander* (7th June 1873), 5.
[5] For biographical see Guthrie 1922, Campbell 1912 and Sutherland 2001. See also Yong Kang Hou, "Farrer, William James (1845–1906)", *Oxford Dictionary of National Biography*, Oxford University Press, 2004 [accessed 12 Aug 2011].
[6] The Queenslander was a weekly digest of the Brisbane Courier so many of his letters appeared in both titles.
[7] "Letters to the Editor", *The Queenslander* (18th November 1882), 716.
[8] These plans met with sharp derision, especially from a series of correspondents to the *Australasian*. However, by 1894, Farrer was hailed as an, "indefatigable amateur agriculturalist", "Letters to the Editor", *The Queenslander* (18th October 1884), 648.

When Farrer had initially become interested in disease resistance it was through following developments that had been occurring in the United States. Farrer did not just follow these developments, he also became involved in a lengthy series of correspondence and swaps of seeds. Over the years he corresponded with a surprising number of breeders from around the world, in a manner that strongly undermines the common contemporary perception of Australia's relative isolation.[9] At first Farrer began writing to Professor Blount, then at the Colorado State Agricultural Station, breeding under the maxim, "Select the *best* to *cross on the best* to make a *better offspring"*.[10] Blount not only inspired Farrer's methodology, he also sent plant material over to Australia for Farrer to use in his breeding. By the end of the nineteenth century, Farrer was keeping up with correspondents from three continents. He wrote to Henri Vilmorin in France asking for advice on drought resistant varieties.[11] Farrer also wrote to India with samples and analyses, addressing his letters to the General Secretary to the Indian Government. The general secretary replied to Farrer with much thanks and praise informing him his letters and seeds were being forwarded to the directors of individual regional research stations.[12]

Farrer's circle of correspondents in the United States widened as he began writing to, and swapping seeds with, plant breeders working for the United States Department of Agriculture. In 1894 Farrer wrote to Beverly T. Galloway from Lambrigg to say he had heard Galloway, a plant pathologist, was intending to study rust resistance. Farrer was in the end unsuccessful in enticing Galloway into a correspondence but he certainly tried, bombarding the American with letter after

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[9] Indeed Bateson was president of the British Association in 1914 when the meeting was held in Australia, see the BAAS report for that year and Boney 1998.

[10] On Blount's unique life and plant breeding career see Olin 1912: 81. Emphasis in original.

[11] Vilmorin to Farrer, 18th August 1893, *Correspondence, 1891-1905 [microform]*, Farrer Papers, Mfm G 27415.

[12] Director of Land Records and Agriculture to Farrer, 15th June 1899, *Correspondence, 1891-1905 [microform]*, Farrer Papers, Mfm G 27415.

letter. He informed Galloway that he had already packed up “a large number” of samples including some, “‘cross breds’ which have been made by me”, and a collection of varieties of wheat, and that the parcel would be sent over to Washington shortly.[13] He advised, however, that Galloway should not hold out much hope for the rust resistance of Farrer’s samples in the new climate, but that a round of selection and propagation might improve them. Furthermore, he told Galloway, “As the securing of rust resisting varieties of wheat … is essentially a matter of selection, the seeds contained in each packet have, in general, been produced by single plants”.[14] A week later Farrer fired off another lengthy letter full of advice for his new American friend.[15] Galloway, it seems, from Farrer’s third letter to him, was a little underwhelmed with Farrer’s samples and thoughts on plant breeding. Unperturbed, Farrer sent off an eleven page letter to Galloway before signing off by saying that he would instead correspond with Mark A. Carleton (also at the USDA) who was more directly involved with wheat breeding.[16]

Before he signed off his correspondence with Galloway, Farrer boldly proclaimed his belief that his new Darwinian breeding had removed the haphazard nature of previous programmes, “My own work has now given me encouragement enough in the shape of results to make me feel sure that success is certain to follow our efforts”.[17] This was not the only thing Farrer shared with Biffen, who as we have seen was prone, on the right occasion, to talking up the imminent revolution in plant

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[13] Farrer to Galloway, 6th July1894, reprinted in *Letters of William Farrer,* Farrer Papers, NLA ms. 33.
[14] Farrer to Galloway, 6th July 1894.
[15] Farrer to Galloway, 13th July 1894, reprinted in *Letters of William Farrer,* Farrer Papers, NLA ms. 33
[16] Farrer to Galloway, 6th November 1894, reprinted in *Letters of William Farrer,* Farrer Papers, NLA ms. 33
[17] Farrer to Galloway, 6th November 1894, reprinted in *Letters of William Farrer,* Farrer Papers, NLA ms. 33. Farrer also pointed out in a rather snotty way that he had offered to lend Carleton his own hybridising equipment and looked forward to corresponding with someone who would appreciate his wisdom.

breeding inspired by the new Mendelism. Farrer, as well as working on rust resistance, believed that the improvement of wheat rested on simultaneously increasing gluten content, the factor Farrer believed to lie behind strength.[18] Farrer set up his own milling facilities and conducted much the same milling experiments as Biffen. However, instead of endlessly baking bread, as Biffen did, Farrer assumed that varieties which milled better would also make a more voluminous fluffy loaf.

Farrer's view of the wheat plant was of an elastic organism in which changes could be heaped up one on top of the other:

> In order to be able to improve a plant in any given direction, it is only necessary that it should possess a tendency to vary in that direction. Variability being given, by means of selection and by expedients in breeding, man can work wonders (these are almost Darwin's own words). ...We have also on our side to help us the general principle that a quality which is being cultivated or secured through its variability tends to go on varying in the direction in which it has already varied.[19]

The now much faded page from Farrer's field book for 1901-2, reproduced in figure 5.2, shows an example of Farrer's breeding in the years around the release of Federation.[20] One can follow the lines of selection which are numbered on the far left hand side by generation, and the hybridisations denoted by an x and brackets, although little else is legible. Page after page of Farrer's field notes, spanning a four

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[18] Farrer displayed the company he kept by describing the distinction between weak and strong wheats as "flinty and soft" rather than strong or weak. Presumably he picked up this American nomenclature from his correspondents in America.
[19] From the *Sydney Mail* (14th June 1890), reproduced in Farrer to Galloway, 13th July 1894, reprinted in *Letters of William Farrer,* Farrer Papers, NLA ms. 33.
[20] Federation was fixed in roughly 1900 and released in 1902.

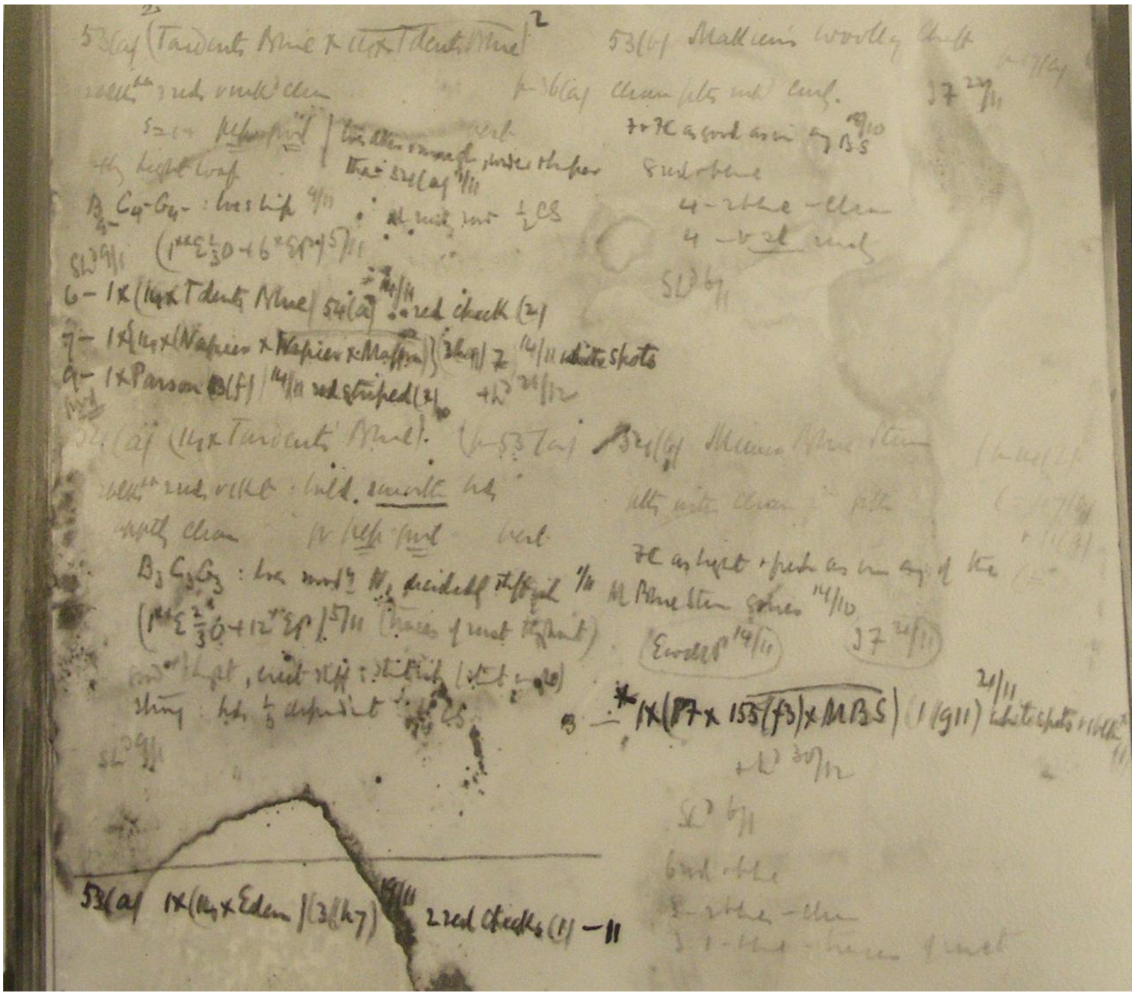

Figure 5.2 Farrer’s Field Notes. Farrer Papers, mss 5856, vol 1.

year period are held at the Australian National University in Canberra. There were obviously many more such notebooks which have since been lost. The intricate record keeping over many generations in Farrer’s work stands in stark contrast to Biffen’s Mendelian programme, where crosses were only followed for a few generations before it was presumed that a variety was fixed.

At some point in 1905 shortly before Farrer and Biffen began corresponding with each other, Farrer heard about Humphries’s work on strength. Farrer thought that strength and milling qualities were related to each other, so he wrote to Biffen to ask about this aspect of the Home Grown Wheat Committee’s work. Biffen insisted that strength and milling qualities were not equivalent, however some of the farmers of Essex might not have agreed with him. As Humphries had pointed out at the

Linlithgow Committee's deliberations (discussed in detail in chapter 4), some farmers thought that the drive for stronger wheats was actually a way to encourage them to adapt their wheat to new milling processes.

In his next letter to Farrer, Biffen poured on the compliments, "I knew of your work with wheats but I have never had the opportunity of realising how comprehensive it was and I find that I have done you a considerable injustice in a paper now in the Press by not referring to it in great detail. I will make up for that as soon as I have the opportunity though, and I will forward the offending paper to you in the course of a week or so".[21] Biffen was palpably excited about the "offending paper", as he continued, "You will see from it that we have for some time fairly definite data to work with and our old bugbear of the 'fixing' of varieties originating by cross-breeding has disappeared, thanks to Mendel's work on heredity". Farrer was not much impressed. In his reply to Biffen he indicated that he thought this was an old problem which had already been overcome:

> In your letter you speak of "the old bugbear of fixing varieties." This work for the last twelve or fourteen years has given me no trouble whatever. It seems to me from what I can see of Mendel's theory of heredity, that the consideration I gave then to the matter of fixing varieties led me to adopt the system, which for all practical purposes, Mendel's theory indicates as being the best.[22]

One thing, however, that did interest Farrer was Biffen's thoughts on spelt and whether they were a reversionary form of wheat. The domestication of wheat

[21] Biffen to Farrer, 9th January 1905, reprinted in Sutherland 2001: 176.
[22] Farrer to Biffen, 8th March 1905. Reprinted in Guthrie 1922: 18.

was, in this period, something of a conundrum. Many suspected that spelt or some other type of grass was the ancestor to modern agricultural wheat in the same way that wild carrots and cabbage were the forbears of their domesticated relatives. However, the point at which wheat was domesticated was so far back in the historical record that no one had been able to provide satisfactory evidence as to whether spelt, or indeed some other grass, was the ancestor of modern day wheat. Farrer's interest in this point was distinctly Darwinian and concerned with reversions. Biffen wrote to Farrer in 1906 recommending Hugo de Vries on sports and mutations; however, without responding Farrer died, unconvinced of Mendelism on the 16th of April.[23]

On several occasions during the course of his career Biffen made reference to Farrer's work. However, Biffen's admiration was always tempered with doubt about whether Farrer's programme was a sustainable one, and he rarely mentioned the Darwinian nature of Farrer's breeding. Sometimes Biffen even doubted if Farrer's work could even be thought of as programmatic, "Farrer's experiments were not planned with any object of testing the possibility of definitely breeding for rust resistance".[24] This view of Farrer found its way back to Australia through students who spent time training in Britain, often at the Plant Breeding Institute or the John Innes. In 1922, one of Farrer's Australian contemporaries, F. B. Guthrie, decided, with support from the Agricultural Department of New South Wales, to set this record straight. He began by outlining Farrer's career and the breeding scheme he was working under. He then moved on to the British portrayal of Farrer, illustrated through the views of a returning student:

[23] Biffen to Farrer, 11th January 1906, *Correspondence, 1891-1905 [microform]*, Farrer Papers, Mfm G 27415.
[24] Biffen 1917: 153.

> [T]he following remarks by Mr J. P. Shelton, holder of the Farrer Scholarship, who has recently returned from England and America, and has had the opportunity of studying, especially at Cambridge, with Professor Biffen, the present developments of wheat-breeding, are of special interest: - The practice adopted so largely by Mr Farrer, of mating unfixed cross-breds … was based upon the old nineteenth century conception that crossing was of value because it produced variations. It is diametrically opposed to the modern methods based on the knowledge of Mendelism.[25]

However, on Guthrie's analysis, there was only a seeming antagonism between Farrer's breeding methods and the new Mendelism and the two methods shared a great deal in common. On Farrer's successes, Guthrie was triumphal in his collected and edited reports from other members of the Agricultural Department of New South Wales:

> Farrer, however, succeeded and his success may be measured from the fact that to-day out of the twenty varieties recommended by the Department of Agriculture as the best, twelve are the result of his labours.
>
> Last year a number of agricultural associations conducted crop-growing competitions in the western districts, and in every case except one the winning crop was a Farrer variety.[26]

The similarity to the prizes and government recommendations that accrued around Biffen's varieties is quite striking, even though in Biffen's eyes the two programmes

[25] Guthrie 1922: 18.
[26] Guthrie 1922: 19.

were diametrically opposed. Beyond receiving the same sorts of accolades, Biffen and Farrer's work also revolved around similar systems of correspondence and exchange with disparately located workers. One of the first of Biffen's students to fulfil this function for the Mendelian system was William Backhouse.

5.1.2 Exporting the Cambridge School: William Backhouse in Argentina

William Backhouse was the first star pupil of the Cambridge School of Agriculture. The School's ruling committee doted over his departure, in 1910, on a scholarship, to William Bateson's John Innes Horticultural Research Institution. The historian of science Robert Olby has identified his work there as some of the most important undertaken by the institute while under Bateson's control.[27] However, within a year, Backhouse left and took a position in the Argentinean Government's Department of Agriculture. His career path reflects that of many other students who passed through the School of Agriculture at Cambridge and on to a foreign career in agricultural research. Moving to a new context presented new problems for each of these students, as we saw in chapter 3, which recounted Balls's struggle with rogues in Egypt. In Argentina, Backhouse set about the process of securing public funding and replicating Biffen's experiments. The contrasts and connections between the political and literal landscapes of Cambridge and Argentina, recounted through a series of letters exchanged between Backhouse and Bateson, form the focus of the first half of this section. In the second half of the section we turn to the international networks of exchange that Biffen created with former students, such Backhouse, in a replication of the network we saw Farrer spend much time and effort establishing and maintaining.

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[27] Olby 1989a.

William Backhouse was born into a family of horticulturalists and plant enthusiasts. His father was a significant breeder of daffodils and his mother the winner of the Royal Horticultural Society's Barr Cup in 1916, figure 5.3 shows a martagon lily which along with a well-known variety of daffodil still goes by the trade name of Mrs R. O. Backhouse. In 1945, when he returned to England aged 60, Backhouse spent his time at the family home breeding red-trumpeted varieties of daffodil.[28] His career was wide ranging including work on stocks at the John Innes, wheat in Argentina, and pigs, apples and honey in Patagonia. However, it is Backhouse's work on establishing wheat breeding for the government of Argentina which we can most usefully connect to the system changes inspired by Biffen in Britain.[29]

Figure 5.3 Mrs R.O. Backhouse. Hybrid martagon lily bred by Backhouse's mother Mrs R. O. Backhouse.[30]

[28] See the well referenced local history websites, <http://www.durhamweb.org.uk/dclhs/Backhouse-family.html> and <http://www.rofsie-estate.com/backhouse-heritage-daffodil-collection-p31.html> [accessed 11th August 2011] for family history on the Backhouses and William Ormston Backhouse (1885 – 1962).
[29] For more on Argentinean wheat growing see Janvry 1973 and Olmstead and Rhode 2007.
[30] Image from Picasaweb, <https://picasaweb.google.com/lh/photo/fJ8a9bgFZo_owACCBUqMLQ?full-exif=true> [accessed 25 August 2011]

When Backhouse arrived in Argentina he wrote back to Bateson on Christmas Eve of 1913. The prospects seemed promising in the new country and Backhouse reported excitedly on a cross he had been conducting to further investigate the patterns of inheritance discovered by Biffen. Not only had Backhouse been replicating Biffen's experiments – with mixed success – he had also been growing maize. In particular, he reported to Bateson, "do you remember a Maize that Blaringham was interested in, ... a certain rich golden yellow maize, [that] would not breed true, and ... always threw another sort, canary yellow". The canary yellow coloured Maize, Backhouse believed, might be a new type of rogue. Upon raising the two sorts another explanation of the differences presented itself, one that chimed with Bateson's own, "they are two totally different maizes, with about a month's difference of season in them". The admixture, Backhouse blamed on his former colleague, telling Bateson, "I fear that Blaringham is not very exact". [31] In this case Backhouse's problem stemmed from the poor material he had brought with him from the John Innes. For Mendelians working in strange places it was not necessarily their new context which caused them problems.

Another feature of Backhouse's first letter, which recurs in later correspondence, was a lack of institutional security for his Mendelian work in a country which was suffering from political instability. In the year he arrived there were rumours of an imminent *coup d'etat* and the country's government was severely weakened. President Saenz Pena had become unpopular in his attempts at "reorganising the electorate", Backhouse believed that, "the old regime is passing away".[32] This uncertainty meant threats to Backhouse's Mendelian breeding program also appeared in the more mundane form of scientific rivalry. Willie Hays –

[31] Backhouse to Bateson, 24th December 1913, Bateson Letters Collection, 2315.
[32] Backhouse to Bateson, 24th December 1913, Bateson Letters Collection, 2315: p.1.

one of America's most famous wheat breeders – was, in 1913, also working in Argentina, on a mission to reorganise state funded wheat research.[33] Backhouse was unimpressed, "Hays is a little behind the times, he talks about all sorts of selection, and one of them is 'Mendelian Selection'".[34] Having "settled three out of the four" proposed research stations for his own work, Backhouse was little concerned by the emphasis Hays was placing on selection, as he continued, "However it doesnt matter twopence as there is nobody here that can say if I am following one both or all the methods set out".[35]

In the following year when he wrote to Bateson with Mendelian gossip, Backhouse's entire department had been sacked around him. However this was no bad thing. Backhouse's new boss was independently wealthy, with a fortune, "sufficiently big to keep him honest".[36] The Mendelian gossip that Backhouse had for Bateson was on an intriguing series of experiments Backhouse had conducted on crossing wheat with rye. This work went on to form the basis of one of Backhouse's few contributions to Bateson's *Journal of Genetics.* In this paper, Backhouse explained that he had discovered a pattern of inheritance for a factor he called crossibility; a factor which determined whether an inter-species cross (in this case between wheat and rye) would be successful or not.[37] If he were right this would be a huge boon to plant breeders who had traditionally viewed the success of inter-species crossing to be the outcome of skill and more than a little luck.

Over the next two decades Backhouse continued maintaining a string of four research institutes across Argentina. His biggest varietal success was released in 1925. Chino-Barletta apparently accounted for up to 20% of the acreage grown in

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[33] On Willie Hays see Kimmelman 1983.
[34] Backhouse to Bateson, 24th December 1913, Bateson Letters Collection, 2315: p.2.
[35] Backhouse to Bateson, 24th December 1913, Bateson Letters Collection, 2315.
[36] Backhouse to Bateson, 30th November 1914, Bateson Letters Collection, 2320.
[37] See Backhouse 1916.

Argentina up until the 1940s. The variety was produced after an extensive search for leaf rust resisting varieties. Leaf rust was a different rust disease to the yellow rust Biffen had been dealing with but Backhouse's method in dealing with the problem was roughly the same. Drawing on a wide-ranging network of correspondents, Backhouse began collecting rust resistant varieties from India, China, America and Britain. Eventually he found a Chinese variety Chino, which he crossed with the Italian variety Barletta, which was the predominant, though rust prone, variety grown in Argentina before Backhouse's arrival. Backhouse's success in Argentina was based firstly on his ability to negotiate the political changes happening around him and secondly on his ability to communicate and share varieties with breeders around the world.

Turning now from Backhouse in Argentina, the chapter moves to a more general look at the importance of such workers for Biffen's plans back in Britain. As we have seen, Biffen's strains were constructed using materials from around the world, Little Joss was derived from an American variety and much of the ground work for Yeoman had involved the testing of American, Hungarian and Russian varieties.[38] As the Mendelian empire in British agriculture expanded, thanks to Biffen's popular support, Biffen increasingly gained access to foreign plant materials. His resources were never enough, especially in comparison to those at the disposal of his American counterparts, but with the help of several former students, now working in research stations in foreign countries, Biffen was able to harvest materials from a wide range of sources. He joked of one particularly complex cross,

[38] Biffen and Humphries 1907.

"By the time this has been accomplished perhaps the League of Nations will be able to turn its attention to deciding what nationality the new wheat is".[39]

Over the years this network included, William Farrer in Australia, William Balls in Egypt and William Backhouse in Argentina (as we have already seen), Albert Howard, H. M. Leake and R. H. Lock in India and W. J. Spillman in America.[40] Biffen also kept in touch with Charles Hurst and William Bateson in Britain and each of these Mendelians kept up their own networks of correspondence and material exchange which they shared with Biffen.[41] Biffen described this network to the Cambridge Agricultural Society in 1925:

> Our organisation lacks ... simplicity. But, even if it is so scrappy as to justify the question whether it exists at all, it works fairly satisfactorily. Indeed, it is so good that I was once privileged to present an American collector, calling at Cambridge on his way home, with cereals from the country he had been vainly sent to explore.[42]

With the foundation of the National Institute of Agricultural Botany these informal networks were consolidated and extended. Biffen's fame was now widespread, and

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[39] Biffen 1925: 23. On the importance of communication across networks see Kaas et al. 2005 on the Maize Genetics Cooperation News Letter or Leonelli 2007 on communication in model organism communities.

[40] Unfortunately Biffen threw away all of his correspondence, as noted by Engledow in his interview with Hugh Rogers, *Plant Breeding: the Early Years*, Plant Breeding Institute Collection. However, there are enough letters, saved by his correspondents, and cross references in the correspondence of other breeders to record the existence of the network. Louise Howard's biography of her husband mentions his correspondence with Biffen and Wood, See Howard 1953: 173. Howard is considered by many to be the father of the modern organic farming movement. See also Howard's description of early work in India inspired by Biffen, using his varieties sent from Britain and some supplied by the French breeder, Henri Vilmorin, Howard 1907. On Leake's collaboration with Russell on a fact finding mission to Sudan see "British Agricultural Research" 1923: 436.

[41] See Olmstead and Rhode 2007 on these types of networks.

[42] Biffen 1925: 20.

his name, along with the new institute's, further encouraged correspondents to send in samples.[43] The plot manager at NIAB kept records of the institute's acquisitions and his notes recorded varieties coming from as far afield as Poland, Nigeria and even the Everest expeditions of the 1920s. The *Daily Mail* described this network quite explicitly in the 1930s in a piece celebrating some of Biffen's achievements:

> From the Cambridge station wide-spread research is directed. One research worker is testing rust-proof varieties from Canada in order to see whether they will be susceptible to the yellow form. In the Argentine, in Australia, in Kenya Colony, and in Germany Cambridge students are conducting experiments for the man who guided their studies in the earlier days; indeed there is no part of the world in which people anxious to carry out research cannot receive direction and inquiry from the experimental station.[44]

In terms of Biffen's plans for British agriculture, then, the rest of the world was a mixed blessing. The grain invasion represented a threat to British wheat farmers, unable to compete on costs or quality with foreign imports, yet Biffen's research depended on his access to varieties of wheat which possessed the characters he wanted to transfer. As he put it in 1914, "The world has been ransacked to find such varieties".[45] The position of Britain's colonies adds an interesting layer of complexity to this picture. To a limited extent they were also competitors on the

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[43] For examples of the busy trade in varieties circulating around the world between research stations see, "Economic Botany, Plant Introduction and Seed Exchange, 1929-1939", DSIR, Lincoln Research Centre Papers, CALM, CH215, Box 19/27/1, 1.

[44] This description of the international scope of Biffen's work is given in the *Daily Mail* in late 1930s, *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University*, Rowland Biffen Papers, the article has had the title removed but appears on page 27 of the scrapbook.

[45] Rowland Biffen, "British Wheat: Improved Methods of Cultivation", *The Times* (8th June 1914), 16b and c.

international wheat market, although British control over exports of colonial wheat meant this threat could be annulled. At the same time the resources available in the colonies suggested a way forward for rescuing British agriculture and placing the rural civilisation of the nation at the heart of a new golden age of agricultural prosperity.

5.2 The Advance of the Agricultural Empire

In the 1890s a young Rowland Biffen went on a research trip to the West Indies and Central and South America. Apparently this trip had a profound effect on Biffen's beliefs and particularly those about how science should aid the empire.[46] He felt that colonial agriculture was incredibly inefficient and could be made much more productive by rationalisation and systematising.[47] The trip resulted in a string of papers on latex and even a patent for a latex processing method. This section follows this previously unstudied strand running throughout Biffen's work. The analysis begins with an interview conducted with Biffen in around 1917, in which he most clearly set out his ambitions for the agricultural empire and Britain's place at the centre of that empire. The section culminates with an analysis of Biffen's 1926 trip to Kenya and the recommendations for Kenyan agriculture he produced in the following year.

In relaxed mode talking to journalist Harold Begbie sometime around 1917 Biffen linked up his beliefs about agriculture, nation and empire. Here we find a clear picture, evoked by Biffen, of a rural civilisation supported by the colonies. Considering the emphasis Biffen placed on the importance of science and progress

[46] Unfortunately Biffen's notebook from this trip has been lost, although Engledow hints it might still be at Cambridge. Engledow 1950: 21.
[47] See Biffen 1897, 1898a, 1898b and British patent no. 3909/1898.

the image he presented is a curiously backward looking one, seeking, as it does, to capture and restore something of a supposed lost greatness. The article begins with a call to revolution:

> "We have got to tune up farming", [Biffen] says. "The farmer is now alert and receptive. The Board of Agriculture is alive to the possibilities of the future. If only the national spirit gets aroused we may accomplish great things. It is not at all impossible that we may create in England a great rural civilisation. That would be a most beneficent revolution".

Finally Biffen's words ended the article with an appeal to the lost greatness of Britain, a greatness that could be recaptured with the sort of revolution in plant breeding Biffen advocated:

> "London" says our professor, "is still the world's chief emporium of the seed trade; very few people know that fact, an important fact; and yet nothing is done on a scale commensurate with this position to improve the quality of our seed. Sweden is far more go-ahead than we are. We ought to wake up to the duty laid upon us by our position as the centre of the greatest agricultural empire in the world. We can give a new impetus to the national life, establish a new stability, if only we give our minds to the business".[48]

[48] "H.B.: Professor Biffen: The Idea of a Rural Civilisation", in *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University*, Rowland Biffen Papers, p. 1. See also the letter from Lawrence Weaver to Harold Begbie thanking him for his high praise for Biffen and his plans, "Lawrence Weaver to Harold Begbie, 28th June 1918", Ministry of Agriculture Papers, MAF 33/22.

In other words, what the country needed to create new stability was a revolution in agriculture, one that would change agriculture in Britain and also the colonies.

One of Biffen's many supporters in these plans was Sir Daniel Morris. Morris began his career working with sugar cane cultivation in the West Indies for the British Government.[49] In 1907, at the Royal Horticultural Society's Third International Conference on Hybridisation and Plant Breeding, Morris explicitly described Biffen's work as a model to be emulated, "Further, the work of Biffen with wheat-breeding should serve as a model on which breeding of sugar cane should be carried on".[50] One of Biffen's many allies in government, was agricultural reformer, Captain Charles Bathurst.[51] Bathurst held Biffen's work in the highest regard and pointed specifically to Biffen's patriotism and the service he was dong for the nation. In a statement made to Parliament on behalf of the Board of Agriculture in 1918, while trying to secure funds for PBI salaries Bathurst reminded his peers just why they should be grateful for Biffen:

> The House may not be aware that Professor Biffen some ten years ago declined to accept a very large salary from the United States of America to go there and develop his Abbot Mendel experiments, which have provided this country with the finest, wheats this country has ever seen. Out of sheer patriotism he preferred to remain in this country to develop this country's wheat. I doubt whether there is any other man in the agricultural world who would have taken the patriotic line Professor Biffen has taken, with the result

[49] See Olby 2000b on the RHS and Morris.
[50] See Morris 1907: 319. Morris was at the time also helping to complete a text-book on tropical agriculture, see Nicholls 1906: frontmatter.
[51] On Bathurst see, F. M. L. Thompson, "Bathurst, Charles, first Viscount Bledisloe (1867–1958)", Oxford Dictionary of National Biography, Oxford University Press, 2004; online edn, Jan 2008 [accessed 13 Aug 2011].

> that to-day we have not only the finest wheats in the world, but consistent types which can always be depended upon to hold an even quality.[52]

Biffen also left a legacy of concern for these issues in his home institutions, the Cambridge School of Agriculture, NIAB and the Plant Breeding Institute. Frank Engledow, who went on to fill the Chair of Agriculture at the Cambridge School of Agriculture, became particularly involved in agriculture in the Empire.[53] In his *Nature* obituary Engledow's outstanding contribution was considered to have been to the Empire, in particular his work, "to perfect a course of postgraduate training for those destined for the agricultural services in the Colonial Territories, and so provide the personnel for the implementation of policies recommended by the Commissions on which he served".[54]

Unsurprisingly, Biffen was asked to turn his attention more directly to colonial agriculture, which posed rather different problems to those in Britain.[55] Establishing agriculture (and favourable trade links) in the Colonies would, Biffen hoped, increase Britain's food security. Cotton from Egypt, wheat from India and rubber and sugar from the West Indies were already established crops which it was hoped could be tuned up.[56] For new colonies like Kenya the plans of British agriculturalists were somewhat more ambitious.

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[52] Captain Charles Bathurst, "Mr. Prothero's Statement", *Hansard Parliamentary Debates*, 5th ser., vol 108 (1918), cols. 336.

[53] See also the Imperial Conferences hosted at Cambridge starting in 1924, Brooks 1924.

[54] "News and Views: Agriculture in the University of Cambridge, Sir Frank Engledow, C.M.G, F.R.S", 1957: 894. See also Engledow's obituary in the proceedings of the Royal Society, Bell 1986.

[55] See other similar advisory trips such as H. M. Leake and E. J. Russell's advisory trip to the Sudan, reported on in *Science*, "British Agricultural Research" 1923: 436, or Hall's 1929 trip to Kenya to investigate soil conditions see, "Kenya's Pastoral Needs" 1939.

[56] For an early example of this thinking about tropical agriculture in the colonies see Willis 1909.

5.2.1 Sir Rowland Biffen in Kenya

In 1926 Biffen was made a KBE. In the same year he was asked by the Kenyan government to spend a year researching the colony's prospects of establishing wheat farming; something the colony's European settlers had, allegedly, been calling for, for some time. In 1927, two seasons after the release of Yeoman II, Biffen departed to Kenya. Upon arriving, he set about surveying the extent of current wheat growing, the types of varieties already in use and the problems anyone wanting to establish wheat growing faced. The results of this survey were recorded in a notebook.[57] The problems in Kenya were quite different to those in Britain. Kenya's wheat growing industry was not in decline as it did not yet exist. [58] It seems that some farmers did very well with wheat crops but there were two big problems, one geographical and one pathological. [59] The topographical map of Kenya reproduced in figure 5.4 shows the geographical situation would-be wheat farmers had to deal with. Kenya can loosely be divided into two areas, a small fertile mountainous region, and a large area of low lying and drought prone land that was less suitable for wheat growing. Much of the interest in growing wheat in the small mountainous area suitable for the plant had arisen since 1910 as new ground was broken when a railway line brought access to the region.

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[57] Held at the John Innes Archive in its collection of Rowland Biffen Papers, see "Notes on Wheat in Kenya".
[58] It is also worth bearing in mind that Kenya only became a crown colony in 1920, it remained so until the 12th December 1963.
[59] Wheat production was first introduced to Kenya with white settlement in the 1890s, for a brief history see Makanda and Oehmke 1993.

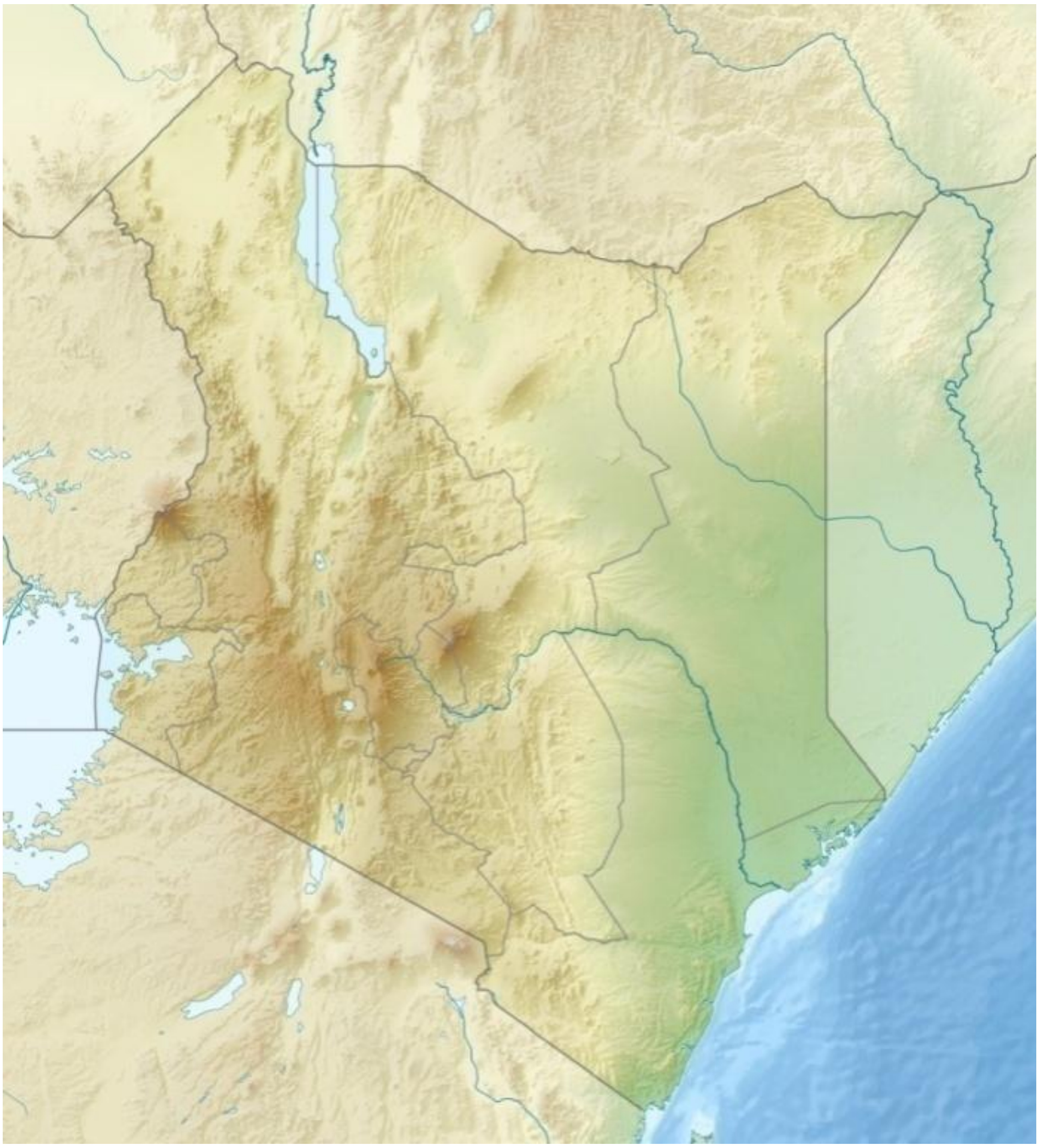

Fig 5.4 Relief Map of Kenya. Source: Uwe Dedering, Green areas indicate lowland and brown highland, <http://en.wikipedia.org/wiki/File:Kenya_relief_location_map.jpg> [accessed 11 August 2011].

Beyond these geographical constrictions; there were also pathological hurdles facing Kenyan wheat production. Kenya was home to three types of rust disease. Losses from these rusts could be catastrophic, ruining a whole crop if they took hold. The three varieties, black rust, *Puccinia grammis*, yellow rust, *Puccinia glumarum*, and brown rust, *Puccinia triticina*, were active at different altitudes. Black rust, the most destructive, was prevalent at 4,500 – 6,500 feet, yellow rust was restricted above 6,500 feet and brown rust grew at any altitude.

Despite the wide range of problems facing Kenyan wheat farmers Biffen was hopeful. In a speech to the white Kenyan farmers he claimed the only way to progress was by increasing support for Kenyan research stations, similar to his own in Britain. Granted such support the prospects were hopeful:

> As a matter of fact you are producing more than in the Argentine ... Given proper cultivation ... I can see no great difficulty in Kenya producing not only enough to feed itself, but to feed its native population as well.[60]

One can imagine the farmers' surprise at being told their country produced wheat on a level with Argentina – one of the great wheat producing countries of the period. Our own surprise today may relate more to the attitude expressed by Biffen towards the "native population". However, the partially redeeming specifier that is missing from this quote is wheat. At the time of Biffen's speech the staple crop of most Kenyans was maize. Part of Biffen's plan for the country's agriculture was the hope that an internal market for wheat consumption – one which included the "native population" – would increase and stimulate the development of a wheat industry. The relationship between wheat and colonial development would, he hoped, be a symbiotic one. As settlers spread European culture, including novel concepts of land ownership and agricultural production, the increasingly civilised native population, so the thinking ran, would turn to eating wheat, thereby supporting the settlers' efforts at introducing the crop to the country. This pattern of development, now long

[60] "Wheat Growing in Kenya", reprinted from the East African Farm and Home Journal, December 1926, p. 3, in *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University,* Rowland Biffen Papers.

forgotten, would have been familiar to Biffen's audience.[61] It turned on the belief that civilisation was deeply associated with a preference for wheaten bread, and ideally, white bread.[62] Developing Kenya's agricultural land also meant civilising the population. Wheat was the key to both ambitions.

At the end of his trip Biffen published the results of his observations and his recommendations in a report produced by the Kenyan Government. This forty page booklet begins with a general survey of wheat growing already being attempted in Kenya. The area on which wheat was cultivated had been steadily expanding. Biffen attributed this partly to access provided by new railways, but mainly to the breaking of new land. Demand for wheat was, Biffen supposed, increasing as the, "native population is beginning to make use of wheaten food-stuffs".[63] Increases in local demand could only benefit those white settlers that tried wheat farming.

Biffen was invited to Kenya, to report on the wheat industry, "with particular reference to the methods of plant breeding now in progress and the organisation of an extended service in the future".[64] Biffen's recommendations were heavily in favour of extending the institutional basis of plant breeding. His first recommendation was the creation of a permanent post of Government Plant Breeder. This position had only been occupied on a temporary basis in previous years, but in order to expand this work, Biffen believed there should be someone working in the post continuously and preferably aided by an assistant. Biffen, believed, furthermore, that key to answering Kenya's pathogenic problems, laid the situations of the research stations at which plant breeding was conducted. At the time Kenya

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[61] See for example William Crookes's presidential address to the Bristol meeting of the British Association for the Advancement of Science, "The Wheat Problem", Crookes 1899. The speech was reprinted with various addenda and went through three separate editions the last of which appeared in 1917, Crookes 1917. See also Brock 2008, esp. chapter 20.
[62] See E. P. Thompson 1971: 80-82, on the switch to white bread that was already underway in Britain at the end of the eighteenth century.
[63] Biffen 1927: 7.
[64] Biffen 1927: 1.

had three plant breeding research stations, Scott Agricultural Laboratories just outside Nairobi and two others one at Njoro and one at Gilgil. Biffen recommended the centre of plant breeding activity in Kenya should be moved to the Njoro station which was situated around 7000 feet, offering a chance to breed resistance to all three varieties simultaneously. This institutional reshuffle would be expensive but it would, Biffen hoped, eventually pay for itself.[65]

Once again Biffen's work appeared in the *Times,* which published a short article on Biffen's report and its main recommendations, the claim that, "the hope that Kenya can supply itself and its neighbours is realizable", was repeated once again.[66] The *Times* article also set Biffen's report in the context of a new census of Kenyan agriculture. The figures show the scale of wheat production as 9% of the area under white cultivation, coffee and sisal 14% and maize 41%. In the years after Biffen's visit, however, settlers on the whole did not follow Biffen's invitation to expand wheat cultivation as they found they could make far more money from growing coffee and barring the natives from doing the same, thereby eliminating their competition.[67] However, Biffen's plans are instructive; despite the failure of NIAB to achieve financial self-sufficiency, Biffen still believed this was a viable, if not the best, option for the research stations in Kenya.

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[65] Biffen 1927: 33-5.

[66] "Future of Wheat Growing in India", *The Times* (22nd January 1927), 11g.

[67] For further agricultural developments in Kenya see "Kenya's Pastoral Needs" 1939, which discusses A. D. Hall's involvement in the Kenya Agricultural Commission of 1929. See also Maxon 2003 for analysis of the divergence in the development of two Kenyan communities, located in similar geographical areas, but which endured two different agricultural development plans.

5.2.2 Otto Frankel and Mendelian Breeding in New Zealand

In 1928, towards the very end of the period this thesis covers, an Austrian geneticist, Otto Frankel (see figure 5.5) spent the year training in Cambridge with Biffen and Bateson. At the end of the year Biffen found Frankel a position with the Wheat Research Institute (WRI) in New Zealand. Following Biffen, Frankel began breeding on Mendelian lines. Frankel, however, believed that unguided research was the best way to proceed in applying science to breeding. In this he was often at odds with his employers at the Wheat Research Institute, who believed research should be directed towards defined purposes. The idea that scientists should be free from administrative control was enshrined in British bureaucracy by the 1930s as the Haldane principle. In New Zealand, under the Department of Scientific and Industrial Research a different conception of the relationship between science and its putative beneficiaries was in sway, despite the department's self-conscious adoption of its British equivalents' departmental title.[68] Frankel's success, in producing important varieties, and becoming director of his own research institute, was tempered by his inability to control the institute's direction and the way it sought to use science. In the end, Frankel became disillusioned with his position in New Zealand and moved to work in Australia. However, Frankel's attempts at establishing a wheat breeding programme, which were to some extent also Biffen's, give us an instructive insight into how such plans were realised in a context in which public benefit was much more directly demanded.

[68] See Clarke 2010 on the British DSIR and Galbreath 1998 on the New Zealand DSIR.

Figure 5.5 Otto Herzberg Frankel (1900-1998), ca. Nov 1954 Collections of the Alexander Turnball Library, Reference Number: PAColl-6388-70.

The history of the wheat industry in New Zealand shares many broad similarities to the British case. Towards the end of the nineteenth century New

Zealand wheat growing was booming as virgin soils were broken and new mechanised farm machinery was introduced. At this point New Zealand was a net exporter of wheat. In the next century a number of factors conspired to reverse this situation.[69] Wheat breeding declined and New Zealand became a new importer. “During the five years before the War”, Frankel’s mentor and boss at the WRI, Professor Frederick Hilgendorf, lamented in 1917, “we were on the verge of not growing enough to feed our own population”.[70] Part of this move to importing was due, as it was in Britain, to the perceived inferior quality of wheat strains then in use in New Zealand. During the First World War this situation was seen as a threat to New Zealand’s security.[71] This broad pattern of boom, bust and decline, followed by government intervention and concern over quality and food security mirrors the British situation, although the causes of the changes were different.[72] New Zealand’s boom was based not just on the arrival of new machines, many of which had been long used in Britain, but also on the breaking of virgin soils. The New Zealand wheat growing bust was, at least in part, caused by the relative increase in profitability in New Zealand’s other famous agricultural exports, butter and lamb. Refrigerated shipping made these two industries more attractive to farmers. One causal factor that played a similar role in the development of the two nations’ wheat industries was the Free Trade movement. In 1930 the New Zealand wheat growing industry’s leading newsletter, the *Wheatgrower* republished, with its own

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[69] See Hilgendorf writing in the popular press on the shrinking wheat acreage in 1917, “A Loaf of Bread”, [New Zealand] *Evening Post* (16th November 1917), 7.

[70] “A Loaf of Bread”, [New Zealand] *Evening Post* (16th November 1917), 7.

[71] See “War Time Wheat: What Britain Did”, *Wheatgrower* (1st December 1930), 9. The *Wheatgrower* was the sporadic newsletter of the New Zealand Wheatgrowers' Co-operative Association Limited. Apparently one of the few situations in which the *Wheatgrower’s* editors did recommend government intervention was in ensuring New Zealand did not pay too much for Australian wheat. On this issue national rivalries were more important than economic ideology. Back issues of the *Wheatgrower* can be found in the New Zealand Wheat Board’s papers.

[72] See “Wheat Growing Industry in New Zealand”, *Report of the Wheat Industry Committee 1929*, pp. 249-252, New Zealand Wheat Board Papers, CAIU, CH84, Box 83.

commentary, a discussion on free trade in the British magazine *Milling.* From the New Zealand perspective it looked like the British Government's "remedy for agricultural depression is the nationalisation of the grain trade".[73] The *Miller* and the *Wheatgrower* as trade journals, naturally, warned that this was, "a Socialist experiment in the nationalisation of trades" which was bound to end in Government bungling.[74]

Responses to the reduction of the wheat industry in New Zealand also shared similarities to those in Britain. Indeed, the New Zealand Government's response in several instances outstripped the British Government's in terms of the directness of its intervention, especially in terms of price control. However, as with the British response to these perceived problems, the New Zealand response started with individual action which later developed into a more systematic and government sponsored intervention. Dr Frederick Hilgendorf began making methodical selections in 1910 at the Canterbury University.[75] In 1922, Hilgendorf visited Biffen to learn about his work on strength. When he returned he began encouraging the New Zealand government to allocate more resources to wheat breeding research. As a piece written about Hilgendorf for the *Ashburton Guardian* put it, "this is a matter for scientific experiment extending over long series of years before results can be verified and a strain definitely established. It is properly a matter for Government enterprise".[76] As a result of Hilgendorf's agitating, in 1927 the Government agreed to part fund the Wheat Research Institute.

[73] "British Wheat History", *Wheatgrower* (1st November 1930), 18-19.
[74] "British Wheat History", *Wheatgrower* (1st November 1930), 19.
[75] For biographical details on Hilgendorf see Pat Palmer, "Hilgendorf, Frederick William – Biography", from the Dictionary of New Zealand Biography. Te Ara - the Encyclopaedia of New Zealand, updated 1-Sep-10 <http://www.TeAra.govt.nz/en/biographies/3h25/1> [accessed 8 August 2011] and his Royal Society of New Zealand obituary, R. C. C. 1943. For more on Hilgendorf's method of selection see Hilgendorf 1939 and Copland and Hilgendorf 1918.
[76] "Wheat Selection", *Ashburton Guardian* (1st November 1920), 4.

The institutional changes in agricultural research that brought Frankel to New Zealand were in many ways a conscious mimicry of the institutions and systems in place in Britain. The Wheat Research Institute Frankel eventually became director of was (like the Plant Breeding Institute and National Institute of Agricultural Botany in Britain) aligned with the nearby agricultural school, Lincoln College, which was in turn aligned to the University of Canterbury.[77] The WRI sought to improve the baking quality of wheat, very much following Biffen's lead. But the institute and its work also varied significantly from the British model. Only part of the funding was from the DSIR, the other part came from direct industry levies. As a result the WRI was focused on practical outcomes even more directly than Biffen's Plant Breeding Institute. The Institute's constitution stated, "The Institute was founded to improve the yield and quality of wheat grown in New Zealand, to improve the flour and bread made from this wheat. It belongs to the farmers, millers and bakers of New Zealand and is administered by the Department of Scientific and Industrial Research".[78]

Sir Otto Frankel was born in Vienna in 1900. His long career saw many changes of direction, he was, "a geneticist by training, a plant breeder by occupation, a cytologist by inclination and a genetic conservationist by acclaim".[79] His early education included both informal stints of farm work for his family and eight years at the Piaristen Staatsgynasiums Wien VIII, where he was Karl Popper's junior by two years.[80] Subsequently he took various courses at the Universities of

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[77] The University was itself previously known as the Canterbury Agricultural College.
[78] "Minutes of the Wheat Research Committee 19th December 1929", *Minutes of the Wheat Research Committee 1927 – 1946*, p 5, Wheat Research Institute Papers, CAXI, 20505, CH999, Box 9/k.
[79] Evans 1999: 167. Evans also published a less extensive obituary in *Economic Botany*, see Evans 2000.
[80] In 1937 Frankel collaborated with philosophy of science Karl Popper, see Frankel's obituary in the [British] *Independent*, <http://www.independent.co.uk/arts-entertainment/obituary-sir-otto-frankel-1076689.html> [accessed 11 August 2011].

Vienna, Munich and Giessen. Finally he moved to the Agricultural University of Berlin where, working with plant geneticist Erwin Baur, he completed a doctorate in agriculture. After graduating, Frankel worked for two years as a plant breeder on a large private estate. In 1927 family connections secured him a job in Palestine working on establishing a plant and animal research programme under the joint auspices of the Zionist Organisation and the Empire Marketing Board. Frankel was apparently unhappy with this position but thanks to the connections he made with the Empire Marketing Board, and in particular with the project's director John (later Lord) Boyd Orr, he returned to London rather than Germany.[81] In Britain, Biffen arranged a research programme for Frankel at the Plant Breeding Institute. In this context Frankel met A. E. Watkins, the cytologist responsible for first counting the number of chromosomes in wheat. He also came into contact with Frank Engledow. The research of these two men, on wheat cytology and yield respectively was to have a lasting influence on Frankel's own research programme.[82]

In 1928 Hilgendorf, through the Wheat Research Institute, contacted Humphries and Engledow in Britain to ask if they could recommend a plant breeder for the WRI.[83] Humphries and Engledow and then Biffen each recommended Otto Frankel. The members of the WRI's executive committee and their DSIR contacts were initially worried that Frankel was both Austrian and Jewish, but after several

[81] On John Boyd Orr see K. L. Blaxter, "Orr, John Boyd, Baron Boyd Orr (1880–1971)", rev. *Oxford Dictionary of National Biography*, Oxford University Press, 2004; online edn, Jan 2008 [accessed 12 Aug 2011]

[82] See for example the series of articles on analysis of yield from Frankel which consciously mimicked Engledow's classic papers on yield in the *Journal of Agricultural Science*, "Investigations on Yield in the Cereals" I-VIII. For instalment I see Engledow and Wadham 1923. Frankel 1935 was the first of a series of papers on this subject, published in British journals, while Frankel was at the same time publishing in New Zealand journals; see Frankel 1939, 1940 and 1947.

[83] "Minutes of the Wheat Research Committee 22nd February 1928", *Minutes of the Wheat Research Committee 1927 – 1946*, p 2, Wheat Research Institute Papers, CAXI, 20505, CH999, Box 9/k.

glowing recommendations from Biffen and Orr he was offered the post.[84] When he arrived in New Zealand, Frankel brought with him a handful of A. E. Watkins and Biffen's strains which he set about hybridising with local varieties.[85] He aimed to improve New Zealand wheat in three directions, like Biffen, but with a different order of priorities. Frankel wanted to improve straw strength, baking and milling quality and disease resistance in this order. Improving straw strength was explicitly a strategy aimed towards improving mechanical harvesting.[86] Frankel amassed eleven hundred varieties and grew some 500 test plots in his first year at the WRI and began a programme of hybridisation. He also conducted tests on different systems of experimental analysis comparing the English chess board system, developed by Beaven as a way of annulling the effects of different soil conditions across a field, with the American rod-row system.[87] The Wheat Research Institute's executive committee was greatly impressed by the expansive sweep of Frankel's initial plans, hoping as much as claiming that, "the result of this investigation will be of importance to the whole Empire".[88]

Over the course of the first five years in his role as plant breeder, Frankel submitted quarterly reports to the Wheat Research Institute. In at least one aspect Frankel's work went beyond Biffen's work in developing Mendelian plant breeding techniques. In 1932 Frankel began using backcrossing as a means of purifying his

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[84] See the correspondence in Frankel's DSIR personnel folder "Frankel, O. H. Part 1 Nov. 1928-Dec. 1938", Frankel Papers, SIR 2, Box 1/41.
[85] Evans 1999: 170.
[86] Evans 1999: 170.
[87] "Minutes of the Wheat Research Committee 19th December 1929", *Minutes of the Wheat Research Committee 1927 – 1946*, p 5. Wheat Research Institute Papers, CAXI, 20505, CH999, Box 9/k.
[88] "Minutes of the Wheat Research Committee 19th December 1929", *Minutes of the Wheat Research Committee 1927 – 1946*, p 5. Wheat Research Institute Papers, CAXI, 20505, CH999, Box 9/k. See also Hilgendorf's report on Frankel's arrival and his "capacity for sustained work", "Director's Report for the 3 Months Ended 31st May 1929", *Wheat Research Institute - Quarterly Reports - Volume I 1929 – 1930*, Wheat Research Institute Papers, CAXI, 20502, CH999, Box 4/a.

crosses. Backcrossing is an essential part of Mendelism as taught now, yet this feature was absent from Mendelian plant breeding until the late-1920s.[89] In 1931 when Frankel began using the method he noted in his quarterly report, "As a new feature of the breeding work, a large number of back-crosses was carried out... between the F1s and the respective parents".[90] The aim was to transfer just a single character from one variety to another. In a cross between two varieties, any number of factors might be passed onto the F1 along with the one apparently responsible for the character a breeder was interested in. To remove unwanted characters the F1 were crossed with the parent that did not exhibit the unwanted characters. The hope was that some of the progeny of this cross would lose the unwanted character but retain the desirable one.

Rogues and the purity of seed stocks were also a problem in New Zealand. Frankel's response to these problems was similar to Biffen's; he became interested in the control of seed stocks. Accordingly a seed station was established at Lincoln College. Instead of centralising this process as the WRI had initially intended, however, Frankel drew up lists of procedures for a seed certification scheme and then commissioned local trusted farmers with the multiplication of pure stocks.[91] Furthermore, Frankel came to be an arbitrator between seed companies and farmers. In 1931 a local farmer, C. F. Rickit, wrote to Frankel, sending him a sample of turnip seeds he had brought from the New Zealand branch of Sutton's. To Rickit's mind, "it looks to me to be as if good was mixed with inferior seed". Although

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[89] For more on the history of back crossing Briggs 1959: 8-9.

[90] "Plant Geneticist's Report: 29th February 1932", *Wheat Research Institute Quarterly Reports: Volume II 1931-32*, Wheat Research Institute Papers, CAXI, 20502, CH999, Box 4/a.

[91] See "Crop Research Division: Wheat – Pure Seed Production", DSIR Lincoln Research Centre Papers, CALM, CH215, Box 68/13/5, 1, for Frankel's procedure's and various correspondence between farmers and the seed station about which varieties would be grown where. For an outline of the initial operation of this scheme see "Report on Seed Wheat Certification 1930", Wheat Research Institute Papers, CAXI, 20505, CH999, Box 9/k.

Sutton's were sending a man to look at the offending plants, Rickit wanted Frankel's advice on whether he should sue, "Can I claim damages in your opinion?"[92]

In 1932 Frankel took a tour of European plant breeding institutes. This was the first of several trips that Frankel made to Britain and Europe.[93] In the introduction to his report of the tour Frankel outlined his main achievements; he had learnt about vernalisation, a new technique being developed in Europe, and about new organisational systems which would allow him to get more work done over the course of the wheat plant's growing cycle. He also learnt something more generally about research ethos, as he noted and underscored "The co-ordination of "fundamental" and "applied" work is the most essential condition for the success of the latter: not only by reason of the applicability of theoretical results to economic work, but even more for the sake of the stimulus which fundamental research exerts on the worker himself".[94] Frankel drew evidence of this from the trend he had observed that, "All institutions with important economic results to their credit are also leading in terms of fundamental research".[95] There was another point which had struck Frankel on his tour, "The isolation of research workers in New Zealand is a very serious handicap to their work".[96] On the one hand, Frankel really did seem to feel lonely, on the other, he was serious about the missed opportunities for the

[92] Rickit to Frankel, 29th April 1931, DSIR Lincoln Research Centre Papers, S.I.R 4/1, "Identification, Genetics, Seeds Samples, Trials".
[93] In 1949 Frankel attended the second annual Conference of Breeders at the John Innes Institute. Frankel seconded a vote for the conference to officially record that it "deplored the dismissal of the leading Soviet geneticists in the last ten years which has culminated in the official repudiation of the established principles and practices of genetics by the Soviet government". The motion was carried with only one vote against. See "Policy and Problems in Plant Breeding" 1949: 51. At the conference Frankel was also adamant that better relationships should be created between academic breeders and commercial seed dealers.
[94] Otto Frankel, "Report on a Tour of Research Institutions for Plant Breeding and Genetics in Great Britain, The Continent and Australia", p. 1, *Wheat Research Institute Quarterly Reports - Volume IV 1934 – 1936*, Wheat Research Institute Papers, CAXI, 20502, CH999, Box 4/d, emphasis in original.
[95] "Report on a Tour of Research Institutions", p. 1
[96] "Report on a Tour of Research Institutions", p. 1-2 emphasis in original.

"exchange of publications and of plant material".[97] Much to Frankel's frustration he was unsuccessful in convincing the DSIR that he should either be allowed to do more fundamental research or have a bigger travel budget.

Frankel became the Chief Executive Officer of the Wheat Research Institute, taking over Hilgendorf's job in 1942. In 1949 the WRI was merged with the DSIR's division of Agronomy and Frankel was made director of the new institutional entity. Frankel wanted to hire more scientific staff and conduct more cytological investigations.[98] However, when he was blocked in these hopes by the DSIR and told to concentrate on plant breeding. Frankel left the institute in 1951, just a year after having been made director, to go over to the Australian Commonwealth Scientific and Industrial Research Organisation. His work there on establishing a phytotron; an artificial biotic environment, hermetically sealed in order to allow more precise botanical investigations, finally realised his dreams for a "fundamental" research program. It seems he also came much closer to the sort of scientific community he had missed in New Zealand. Two years after he arrived in Australia he was elected as a Fellow of the Royal Society of London.

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British Mendelians interacted with plant breeders from around the world. Furthermore, the British Mendelian system was not limited to Britain. Following the correspondence between Biffen and Farrer or the several attempts to establish Mendelian systems outside Britain substantiates these points. Oftentimes the rest of the world was a resource for Mendelians as when Backhouse racked his

[97] "Report on a Tour of Research Institutions", p. 2.
[98] See Evans 1999: 172-173.

international correspondents for a viable rust free strain to use in his breeding program in Argentina. At other times it was the colonies which were intended to benefit from Mendelian breeding, as on Biffen's mission to Kenya. These contexts were, however, not always conducive to Mendelian aims. Considering Farrer's troublesome success or the restriction of Frankel's work to practical projects, the rest of the world was not only either a resource or a beneficiary, it could also be a problem. These were more subtle, less obvious problems than the grain invasion from which Biffen thought many of Britain's agricultural woes stemmed, but they were problems for Mendelian ideas none the less.

The contrasts between these different attempts at creating a system point to the importance of a political context for Mendelian activities. Where in chapter 3 the moral economy of plant breeding was identified, and in chapter 4 an agricultural economic context, in this chapter the political economy of plant breeding abroad is contrasted to that seen in Britain in chapter 1. Often the British Government was so sympathetic to Mendelian aims that it became invisible in facilitating them. In contrast, Backhouse was in constant danger of being on the wrong side of a coup and Frankel was never the master of the New Zealand system to the same extent as his system building contemporaries in Britain. Finally in his plans for Kenya, Biffen, with the Kenyan Government's support, could be expansive to an even greater extent than he could at home. The Kenyan wheat industry represented a blank slate, or at least a slate which could be represented as blank, onto which Biffen could sketch his plans and recommendations.

Conclusion

THE STORY OF Biffen, Bateson and Wood and their Mendelian system is a fascinating one. These were important events in the history of genetics and the history of agriculture. What can be taken from the account given here though? Some might see further evidence of a failure by genetics to impact on agriculture, others might take the opposite reading. After all, Mendelian wheat was both successful and unsuccessful. It grew in many fields and its popularity backed a considerable institutional expansion. However, these varieties never quite fulfilled the ends for which they were intended by Mendelians. By way of conclusion, the analysis will now pause to reflect on how a systems account of Mendelian agricultural involvement might best be interpreted and how far its scope extends.

The first part of this conclusion ventriloquizes the responses of a fictional Mendelian supporter and sceptic. A strong and a weak interpretation of the thesis, as it were, which together highlight the key claims advanced somewhere in the middle. Taking these supporter and sceptic positions seriously, we can also begin to see why they ended up holding so much currency. Considering these alternative interpretations makes clear the need to face down two further issues in order to be serious about the position advocated here. Firstly, how far does the scope of the analysis offered here extend? Could a systems approach help explain events in other countries, or were these developments peculiar to Britain and the colonies? Secondly, how do chromosomes, the next major historiographical mile stone in the history of genetics, after the Biometrician-Mendelian debates, fit into the picture of a Mendelian system? Once these issues have been addressed, the analysis turns to the other type of systems that have been associated with the rise of Mendelism; epistemic systems. On this view the gene was an epistemic thing embedded in an experimental culture. The final section of this conclusion examines how these two visions of the gene might be brought together.

Mendelian Supporter and Sceptic Interpretations of the Thesis

Mendelian supporters, to make a crude generalisation, believe that the rediscovery of Mendel's work at the turn of the century marked a radical point of departure in the progressive improvement of plant breeding and agriculture. Mendelian sceptics on the other hand, believe otherwise. On a sceptic's view Mendelism had little to do with plant breeding. The theory failed to make any real impact on agriculture for quite some time after the rediscovery of Mendel and its early effects were, if anything, sociological; they aided academic professionalisation. This thesis has sought to establish a third view; that the relationship between Mendelians and agriculture was a complex and varied one that arose as a result of a concerted effort to induce change.

The Mendelian supporter's view has been by far the dominant one over the last century, held by many Mendelians and historians of science. From this perspective the operation of a Mendelian system might be further proof of Mendelian intentions to change and improve agriculture. The technocratic elite that developed around the Development Commission, or the many references in Mendelian obituaries to a tight knit collaboration by a small group, point to the existence of a concerted effort to change agriculture. Indeed, the fruits of these collaborations were several new popular varieties. Furthermore, many of the institutes that appeared in this thesis continue to interact with agriculture; the John Innes Centre and National Institute of Agricultural Botany are both still going strongly. These institutes have become part of the grain of agriculture; every nursery man knows John Innes if only as a brand of growing medium and any arable farmer will have heard of NIAB and its recommended variety lists. Many of the links that thesis describes have also continued, the Royal Agricultural Society continued supporting the Plant Breeding Institute, awarding Biffen's successor G. D. H. Bell a society medal in 1956, for, "research work of outstanding merit ... which has proved, or is likely to prove, of benefit to agriculture".[1] Mendelian ideas may have been challenged by this context, but gametic purity was robust enough as a concept, and the ideas around it flexible enough to deal with anomalies like rogues. The supporter could also point out that the Mendelian way of doing things travelled around the world. The governments of Egypt, Argentina, Kenya and New Zealand (amongst others) actively recruited Mendelians such as Balls, Backhouse, Biffen and Frankel to come and work on improving agriculture in these countries. The Mendelian supporter's view was exemplified by E. J. Russell, speaking of his friends' achievements to the *Scientific Monthly* in 1948:

> An important group of agricultural research institutes has developed at the Cambridge University School of Agriculture started by T. B. Wood, one of the greatest of the pioneers. With him was associated Sir Rowland Biffen, whose inspiration came from two great figures in English biological science, Marshall Ward and William Bateson. Marshall Ward was the founder of modern plant pathology in Britain, and Bateson of modern genetics. Biffen, a student of both, combined their subjects and bred wheats to resist rust, giving them also other desirable qualities. Thus, Cambridge became a great center

[1] "Royal Agricultural Society of England Medal" 1956: 456.

for the breeding of wheats for English conditions, and the Cambridge wheats are widely and successfully used.[2]

On the other side of things, a Mendelian sceptic might point to the importance of agricultural trends in determining what farmers got up to on the farm. Mendelian wheat varieties were successfully used but not entirely as Biffen intended. The failure of the All-English solution suggests Biffen's strategies for agricultural improvement were out of step with the economic realities faced by farmers. Furthermore, from this perspective, Mendelism failed as a universal theory; the problem of rogues was one which was avoided, or to use a Mendelian term, segregated, away from centre-stage. The continual expansion of the theory to cover these anomalous phenomena meant that while many talked of Mendelian laws, there was, very soon after the "rediscovery", no single body of Mendelian theory. As the pages of Punnett's *Mendelism* swelled over the first decades of the century it came to document an ensemble of theories rather than the neat and clear laws that had appeared in the first 1905 edition.

Turning to Mendelian fortunes abroad, in New Zealand Mendelism seemed to matter less than institutional expansion. The Wheat Research Institute was important enough in its own right that Frankel's aspirations to conduct theoretical Mendelian cytological work were restricted. On this view it might seem as though the system was much more important than Mendelian thinking. The suspicion behind this view is that in a world which was already in the process of systematising, structuring and formalising, Mendelism was merely a rhetorical fig leaf for the aspirations of professional academics, early capitalists and government

[2] Russell 1948: 131-132.

bureaucrats.[3] In other words, the type of science Mendelians were doing or promoting did not matter as much as the fact that it was scientific.

The weight of evidence presented in this thesis has demonstrated that there is some truth in both of these positions, supporter and sceptic. There were some hits and some misses, some successes and some failures, and by the 1930s there were several new items in the world which had not existed in the 1880s, as well as several surprising survivals. The Mendelian plant breeder was a new feature of the 1930s but some of their practices would still have been familiar to the breeders of the previous century. This might be especially true if they insisted on chewing their corn to find out how strong it was. The more interesting questions are why and how did the relationships between Mendelism and agriculture which did emerge, come to do so in these particular ways? Prior to the 1930s the actions of a small group at and around Cambridge responding to the moral, political and agricultural economic context of their period were in the vanguard of creating new relationships between science and agriculture, the university and the farm, theory and practice. Recognising the existence of this group and their genuine intentions to change agriculture moderates the excesses of the sceptic's position. However, we do not have to acquiesce in Russell's vision of Mendelian success. Reconsidering Mendelian success from an agricultural perspective likewise helps us to avoid the excesses of the supporter's position. Rather than making the case that the success of Mendelism was an all or nothing result, this thesis has shown that benefits were unevenly distributed; they aided certain types of millers and farmers. However, how that unevenness worked out, was not entirely a matter under the Mendelian's sway.

[3] The classic Marxist view is that this is the period in which modern industrial capitalism reached the ascendency. Millers, bakers and farmers were increasingly demanding standardised and larger batches of inputs. On these changes see Palladino 1994: 434 and Amidon 2008.

Little Joss was beneficial to farmers scratching away on poor soil, these farmers were not the corn baron insiders to the Mendelian circle who benefited from using Yeoman.

Claims that Mendelism was either successful in changing, or disconnected from, agriculture need to be viewed as functional in their respective historical contexts. These claims were important to supporters, in the period, and later, for several reasons. The antidote to these claims was also necessary. In one sense, Mendelians, their wives and their students were so good at writing loving praise for their friends, husbands and teachers that a pricking of this illusion was at some point inevitable.[4] On a less naïve view supporter claims about success, translated, as they often were, into institutional reports and parliamentary speeches, were about securing further funding. This was not, however, necessarily about the kinds of appropriation associated with today's agricultural subsidies and pork barrelling. Mendelians and their supporters were at lengths to point out, as we have seen in several instances throughout the thesis, the public benefit that would accrue from what they were trying to achieve and the great amount that Mendelians were sacrificing personally to the cause. Being a Mendelian plant breeder meant working in an area with no patents, or any sort of personal recompense (beyond wages) for innovative work.

Mendelian sceptic claims in the period were equally based on the public good. The idea, as Prof Wrightson noted at the Selborne Committee, was that there

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[4] Mendelians were extensive obituary writers, see for example Bateson 1918 and 1920, Engledow 1949 and 1950, F. G. H 1931, Russell 1930, 1942 and 1944, Biffen 1943 and Bell 1986. Mendelians also effectively maintained each other's legacies, see for example Punnett's little known two volume collection of Bateson's work, Punnett 1928. See also Punnett 1952. Mendelian wives, in addition to collaborating with their husbands were also at pains to collect together their spouses' works. The most famous of these biographies is Beatrice Bateson 1928, see also Rhona Hurst 1949 and the scrap book created by Biffen's wife, *Extracts from Newspapers on Wheat Research of Professor Sir Rowland Biffen MA FRS, Cambridge University*, Rowland Biffen Papers.

might be other – better – ways of aiding agriculture. The reawakening of these claims in the recent historiography reflects the historically situated contemporary view that there might be better alternatives to Mendelism and the specific brand of reductionist science it represents. Writers at the end of the twentieth century were by this time ready to reappraise the promises of science. Writing after the de-romaticisation of scientistic visions, most emblematically by Rachel Carson's *Silent Spring*, latter day Mendelian sceptics have suggested that something was lost when science came to the ascendency; in this case a series of practice-based skills which formed part of the traditional body of knowledge of plant breeding.[5] This thesis has shown that shoe-horning the rise of Mendelism into these broadly pro- and anti-scientific theses risks losing much of the rich detail of these developments.

How Far Did the Mendelian System Extend?

The first issue that needs to be addressed in order to be serious about the position advanced here is, how far does a systems analysis explain developments in other countries? America, France, Germany and Sweden had very different balances of public and private enterprise when it came to plant breeding. In some ways the picture given here of Britain is an inverted image of developments in America. In America public funding was a major part of the nineteenth century development of agriculture. The United States Patent Office distributed seeds in the first part of the century, although these efforts were less aimed at farmers and more at varietal distribution.[6] From the 1860s the United States Department for Agriculture

[5] Carson 1962.
[6] See Kevles and Bugos 1992 on the USPTO's seed distribution programs.

continued this scheme and extended it to support farmers more directly.[7] Towards the end of the nineteenth century the foundation of the land grant universities provided the context which Kimmelman and Paul have described as having being particularly fertile for the development of Mendelism.[8] During the twentieth century the business of breeding, in America, came increasingly to be prosecuted by private companies. In comparison, in Britain, nineteenth century developments were characterised by rather disorganised personal enterprise and benefactions. The Board of Agriculture was initially established by Humphrey Davy in the 1830s but it ceased operations and had to be re-established in the 1880s.[9] Meanwhile, Rothamsted Experimental Station was funded by the Gilbert and Lawes trust, the Royal Agricultural College by private subscriptions and the Cambridge Department of Agriculture, in its early days, by small grants and gifts. In the twentieth century government intervention became increasingly important just as it was dropping out of the picture in America. These differences are partly reflected in the very much earlier adoption of formal intellectual property rights for plant breeders in America. The 1930 Plant Patent Act was explicitly designed to encourage private companies, it was another 24 years before such measures were considered in Britain.[10]

Similar forces were at play but with different outcomes, across mainland Europe. Once again the relative levels of public and private enterprise were (and remain) different. For example the Svalöf Station in Sweden, on which the Plant

[7] See Kloppenburg 1988 on the USDA. See also Powell 1927.
[8] See Kimmelman and Paul 1988.
[9] The literature on Humphry Davy is extensive but there is less on his involvement with founding the Board of Agriculture. For a way in see Forgan 1980 and David Knight's ODNB entry, "Davy, Sir Humphry, baronet (1778–1829)", *Oxford Dictionary of National Biography*, Oxford University Press, 2004; online edn, Jan 2011 [accessed 29 Aug 2011].
[10] Ironically, when intellectual property rights for plant breeders were discussed in Britain the intention was to encourage private firms like Carter's and Sutton's but the outcome of the introduction of UPOV rights in the 1960s actually served to improve the Plant Breeding Institute's position. Under the new scheme the PBI was allowed to claim breeders' rights and these became very lucrative to the publicly funded institute, see Bell 1976.

Breeding Institute and National Institute of Agricultural Botany were initially modelled, remained a joint stock holding company with its allied seed dealing company.[11] The efforts of Svalöf's first two directors, Nils Hjalmar Nilsson and Herman Nilsson-Ehle might usefully be explored as system building activity which cut across the private-public divide, unlike the publicly focused system building undertaken by Biffen, Bateson and Wood. Nilsson-Ehle was on good terms with Bateson. When J. W. Lesley was taken prisoner of war in Sweden, Bateson wrote to Nilsson-Ehle for help. Nilsson-Ehle was also an important Mendelian in his own right, working on multi-character crosses he showed that some colours in wheat grain required several factors to work together in order for them to appear. In France, Henri Vilmorin, of the eponymous nursery, continued his father's efforts at systematisation, although these were, like William Farrer's work in Australia, not particularly influenced by Mendelism.[12] Each of these hybrid and alternative arrangements of Mendelian and non-Mendelian, private and publicly funded activity might fruitfully be explored as system building activity of a particular national flavour, oftentimes connected to the international context as the British system was.

Chromosome Theory and the History of Genetics

On one view of the history of genetics, the next important event after the rediscovery of Mendel was the integration of Mendelian unit-characters with and onto chromosomes. This view was and still is widely expressed as a standard

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[11] For more on Svalöf see Staffan Müller-Wille, 2008. "Plantbreeding at Svalöf: Instruments, Registers, Fieldwork", The Virtual Laboratory (ISSN 1866-4784), <http://vlp.mpiwg-berlin.mpg.de/references?id=art69> [accessed 19 August 2011] and Müller-Wille 2005. See Åkerberg 1986 on Nilsson-Ehle's tour of other European plant breeding institutes in the second decade of the nineteenth century.

[12] On Vilmorin's systematic activity see Bonneuil 2008.

periodisation. From this perspective, “What ultimately served to establish Mendelism on more firm ground was its unification with the cytological work on chromosome structure and behaviour, carried out on a number of fronts, but most well-known through the work of Thomas Hunt Morgan (1866-1945) and his young, enthusiastic team of investigators at Columbia University between 1911 and 1925”.[13] This thesis has shown that there is still much to be learnt about Mendelism before this period and furthermore during this period, which a view focused on chromosomes might miss. That is to say, events which occurred in Britain were influenced by the British agricultural context as much as by Bateson’s refusal to accept chromosomal theory.[14]

The question of Bateson’s attitude towards chromosomes is an attractive historical one. It opens up possibilities for making larger claims about the man and his place in the history of genetics. However, behind this question is the presumption that chromosomes were the next big thing. This is a well-trodden path, from Mendel to Bateson to Morgan to Watson and Crick. However another path through the history of genetics is revealed here. A broader view of the history of genetics, one which encompasses relations with agriculture before and after the rise of chromosomes as a focus of genetic enquiry, draws together several threads of the history. On this view the work of R. A. Fisher at the Rothamsted Experimental Station, though not considered here, seems less misplaced in the wheat fields of Herefordshire, as does Cyril Darlington’s work on cytology at the John Innes. These were not cases of isolated work in unusual places for geneticists. Thanks to the

[13] Allen 2004: 215. This quote is ideal for illustrative purposes but a little unfair on Allen who has also pointed to the importance of the agricultural context, see Allen 1991. For more on chromosomes see Allen 1978, Harwood 1984 and most recently Brush 2002.
[14] On Bateson’s refusal to accept chromosome theory see Cock 1983 and Olby 1989.

developments covered in this thesis these were exactly the sorts of places that important developments in the history of genetics occurred.

Systems thinking could also bring something to the current history of chromosome theory and drosophila genetics. Robert Kohler's view of the production line nature of Morgan's fly room is, in part, so successful because it exposes the enormous task of organisation that lay behind Morgan's work.[15] The creation of new institutes and the production of new fly strains were important features of Morgan's work, as was the management of funding and relations with workers in other laboratories. The opposite is also true, thinking about chromosomes could also add much to our picture of systematic developments for agricultural Mendelians in the years after those covered here. William Balls, Caroline Pellew, Otto Frankel and of course Cyril Darlington all undertook cytological work focused on chromosomes in agricultural institutional contexts, using agricultural model organisms.[16] The picture offered here is not a counterbalance to some egregious error in the view that chromosomes were important. It is instead a supplementary offering which adds another path through the history of genetics.

Hughesian Systems and Epistemological Objects

The historiographical debates this thesis responds to represent one vision of the history of genetics. In the spirit of pluralism it is worth, in closing, considering how a systems view relates to a more philosophically informed view of the history of genetics that has emerged in the last two decades. On this view the major shifts and

[15] It is unsurprising that Kohler also discerns the operation of a moral economy among Kohler's students and colleagues although in this case the morals were largely those of Morgan, forced upon the group. See Kohler 1993, 1994 and 1999.
[16] See also the series of papers from A. E. Watkins started at the Plant Breeding Institute and continued at the John Innes, starting with Watkins 1924.

trends in early, and later, genetics were epistemological, they related to the experimental systems used by Mendelians and the epistemological objects in those systems.[17] The Mendelian gene, viewed as an epistemological object, was successful or not when it allowed stable systems of experimentation to emerge. The gene, along with several other epistemological objects, pure lines, model organisms or heredity itself, for example, were also fertile heuristic tools precisely because these objects resisted easy definition. In other words the ambiguities around these objects provided their heuristic value; if they were totally known there would be little to learn from them. On this view, the rise of Mendelism was part of a kind of constriction of methodologies, terms and objects of study. The crucial shift at the turn of the twentieth century was from the extensive inquiries into heredity of the nineteenth century, to the focussed inquires, upon appropriate objects, of twentieth century genetics.

While experimental systems have taken a back seat in this history of genetics, with the partial exception of chapter 2, there is no reason why these two views should be in tension. The type of Hughesian systems analysis offered in this thesis shows very clearly how the conditions in which Mendelian experimental systems were created, operated and sometimes constrained, were constructed. It seems highly likely that experimental systems interacted with the larger systems in which they were developed. An epistemological view could valuably be folded into the systems analysis given here. The relationship could also be inverted. One of the explicit aims of epistemological thinking is to move beyond the purview of discipline specific history. The gene as epistemological object was important in

[17] On epistemological objects see Rheinberger and Müller-Wille forthcoming, Rheinberger and Gaudillière 2004 and Rheinberger 2000. On experimental systems see also Hans-Jörg Rheinberger, 2004. “Experimental Systems”. The Virtual Laboratory (ISSN 1866-4784), <http://vlp.mpiwg-berlin.mpg.de/references?id=enc19> [accessed 19 August 2011]. See also Hacking 1992.

many disciplines other than genetics. However the history of genetics given here can offer much support to this wider notion of the history of heredity. This thesis could be folded into an epistemological view as a sub-module in that wider history. On this view, rogues were another important epistemological object. Rogues were precisely the type of object which resisted definition and so pushed Mendelian thinking into new directions.

When it comes to history of genetics more really is more, Mendelism was many things to many people.[18] Recovering the system building activity of early British geneticists such as Bateson, Biffen and Wood provides a new path through this multiplicity of meanings, one that tracks closer to, and so illuminates, Mendelian aspirations and the extent of their success.

[18] I would like to thank Jonathan Hodge, who has been an inspiring presence throughout the writing of this thesis, for this brilliant one liner, "Mendelism was many things to many people".

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