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ABSTRACT

From around 1900, advocates of "Mendelism" stressed the usefulness of Mendelian principles for breeders. Ever since, that usefulness – and the favourable view of Mendelism it supposedly brought about among breeders – has featured in explanations of the rise of Mendelian genetics. Alongside that tradition of commentary, however, there has developed a counter-tradition, emphasizing the ways in which early Mendelian theory in fact fell short of breeders' needs. Attention to intellectual property, narrowly and broadly construed, can bring these traditions together in a novel way, by enabling both a more complete description of the theory-reality shortfall and a better understanding of how changing practices functioned to render the shortfall unproblematic. In the case of plant breeding in Britain, a perennial source of lost profits and disputes over ownership was "roguing": the appearance within a variety of individuals departing from the type. Mendelian plant varieties acquired a reputation for being rogue-free less because of the correctness of Mendelian principles than because Mendelians gradually took control of the means for distributing their varieties. In doing so, they protected their products physically from rogue-inducing contamination, and in such a way that when rogues did appear, the default explanation – that contamination had occurred – ensured that there was no threat to the underlying principles.

Keywords: Mendelism, intellectual property, plant breeding, W. F. R. Weldon, Rowland Biffen, rogues.

Intellectual property, plant breeding and the making of Mendelian genetics

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1. Tradition and counter-tradition in connecting Mendelism and breeding

"In these pages, I have only touched the edge of that new country which is stretching before us, whence in ten years' time we shall look back on the present days of our captivity." So wrote the Cambridge biologist William Bateson in 1902, near the end of what became the first textbook in the science that Bateson, a few years later, would enduringly dub "genetics". He went on: "The breeder, whether of plants or of animals, no longer trudging in the old paths of tradition, will be second only to the chemist in resource and in foresight."[2] When, not long after his short volume, entitled *Mendel's Principles of heredity: A defence,* came out, Bateson addressed plant breeders directly about the new science and its practical promise, he was notably less condescending about what breeders knew already about their literal stock in trade. But – as documented in a later paper in this set – he never stopped talking up the transformative potential of Mendelism as an applied science or offering himself as an expert in its applicability. A Cambridge protégé, Rowland Biffen, was no less confident in his predictions of what Mendelism would do for breeders – and no less committed to making those predictions come true. "Of one thing we may be certain", Biffen concluded in 1904, in a paper in the *Journal of the Royal Agricultural Society of England* on his Mendelian experiments with wheat and barley, and "that is that ...

research in the right direction will make clear what now appears as mysterious, as did the results of the breeders who had to work in ignorance of Mendel's discoveries."[3]

The usefulness of Mendelism for breeders, and vice versa, has ever after remained a major theme for commentators on the rise of Mendelian genetics. In 1910, Bateson's American counterpart Charles Davenport, writing in the first issue of a new "journal of genetics and eugenics" sponsored by the American Breeders Association, noted the passing of "the scholastic biologist of our universities" who little appreciated artificial breeding and looked down on the men involved with it. (He recalled one of these colleagues asking him what he did at Association meetings – "inspect 'hawgs', pass around 'pertaters' and show up your biggest ears of corn?")[4] In 1950, Davenport's former student William E. Castle told of the warm welcome that Mendelism received in America from those "interested in the study of evolution from a pure science viewpoint" and those who "saw in Mendelism a new tool for the production of new and improved varieties of plants and animals."[5] Nearer our own time, this perspective continues to be well-represented in the professional historiography on genetics. Diane Paul and Barbara Kimmelman on the American reception of Mendelism, Robert Olby on the British reception, Kyung-Man Kim on the biometrician-Mendelian debate in both countries: all have stressed the importance to the Mendelians' success of their reaching out across the divide to breeders – and with a theory that illuminated breeders' problems as none had before.[6]

This version of the rise of Mendelism is, for certain sensibilities, perfumed with an air of "history from below." Against, say, a complacently elitist view of Mendelism as needing only the backing of the big-question biologists, we learn that it was the breeders ("associated in the mind", wrote Davenport of the scholastic snobs, "with the cowboy, the stable boy, the 'hayseed', the country jay, and the peasant of

Europe"), who made all the difference to its success.[7] Even so, its start in the polemics of partisans means that the notion that breeders were early converts to Mendelism can hardly be treated as unproblematic. Recent historical studies of Mendelism's international life are already well on their way to a more complicated picture of Mendelism's connections with breeding. Christophe Bonneuil's work on the situation in France, Jonathan Harwood's on Germany, and Bert Theunissen's on the Netherlands have gone some way to querying the dependence on Mendelism of the advances in breeding in those countries in the first half of the twentieth century. For the Anglo-American case too, there have been, over the past quarter century, some splendid openings, notably Paolo Palladino on successful breeders who were non-Mendelian or half-heartedly Mendelian, and Richard Lewontin and Jean-Pierre Berlan on whether hybrid corn was the Mendelian wonder-plant of legend.[8]

One aim in what follows is to show how much historians have to gain by trying to reconcile these two traditions, one taking seriously the gradual success of Mendelians in positioning their science as the begetter of successful breeding, the other taking seriously how surprising that success appears once we adopt the points of view of many of the breeders. A related aim is to suggest that, once Mendelism's usefulness to the plant breeder has been turned into a question, historians can learn a lot, both about the obstacles faced initially and how they were eventually overcome, by attending closely to intellectual property arrangements among British plant breeders in the decades around 1900. We shall see how these arrangements fed into developments on either side of Mendelism's rise. At its beginning, they provided the fiercest of Mendelism's early critics, W. F. R. Weldon, with data used in the 1902 article that provoked Bateson to his book-length *Defence* (and so, arguably, to advocacy of Mendelism as a full-time, lifelong job). Drawing especially on the record of nineteenth-century controversies over a pea variety called *Telephone*, Weldon contended that rogue plants, of the sort that regularly led to property disputes among breeders, were a fact of the breeding life which Mendelian principles utterly failed to account for. And, on the other side of the "Mendelian revolution", it was changes in these very arrangements – changes in which the Mendelian breeder Biffen played a key role – that helped secure the reputation of those principles as true and, connectedly, of the varieties credited to those principles, notably Biffen's wheat variety *Yeoman II*, introduced in the early 1920s, as rogue-free and otherwise excellent.[9]

Although the historical literature on the rise of Mendelian genetics is large, there has been little until now on issues to do with rogue plants. Here we will encounter these issues as bearing centrally not just on the correctness of Mendelian principles but on associated claims of wider relevance: for the usefulness that was held to follow from correctness, and for the rights of breeders, Mendelian or otherwise, to profit from the literal fruits of their labours. In the terms of the analytical program outlined in the introduction to this set of papers, these are, respectively, productivity claims, relating to intellectual property or IP in a broad sense, and patent claims, relating to IP in a narrow sense. Before examining these claims as they were contested and consolidated over Mendelism's rise, from uncertain beginning (with Weldon) to confident end (with Biffen), the paper offers a couple of scene-setting background surveys, both to do with assertions of ownership. The first takes in the era's practical biology, in particular the matter of how British breeders down the decades managed new varieties and their commercialization. The second takes in the era's academic biology and the matter of how Mendel's name came to be attached to certain kinds of successes and, relatedly, other names not to be so attached.

Name-attaching priority claims too are "IP-broad" claims; in highlighting their part in the story of Mendelism, and putting them in the company of productivity and patent claims, the paper seeks to relocate the story even more firmly within the wider story of intellectual property and the sciences.

2. Ownership of new plant varieties in Britain in the decades around 1900

We begin, then, with the surveys, starting with intellectual property among the British plant breeders.[10] "Intellectual property" was not a term that anyone in Britain in the decades around 1900, plant breeder or otherwise, would likely ever have come across. But then as now, plant breeders had problems for which the phrase is apt. Budding breeders who read the chapter on plant breeding in John Percival's textbook *Agricultural botany*, first published in 1900, were put on their guard. Do not, Percival warned, assume that just because you have bred into being and named a new variety, or have purchased seeds for a supposedly new variety from somebody else, that all is well or will long remain so. First of all there is the problem of "rogues" – a term Percival put both in quotation marks and italics, and defined as individual plants "departing considerably from the type" and which "appear among the offspring at irregular intervals." In these departures, he went on, rogue plants "most frequently exhibit characters possessed by the ancestors of the variety in which they are found." And these unwelcome representatives of the past were bound to turn up, Percival counselled. Indeed, so familiar was the phenomenon of the "tendency of plants to revert to long-lost characters" that he reported several labels: "*atavism*, '*throwing-back*,' or '*reversion*'." Against reversion, the breeder had but one weapon: the hunting down and destroying of rogues when they appeared. But not everybody could be counted on to be vigilant, and that made for constant trouble:

> Very few if any varieties of plants propagated by seeds remain like the type first sent out by the raiser for more than a limited number of years. In a great many instances where almost everybody raises seed, destruction of "rogues" is not efficiently or thoroughly carried out, and through the consequent mixing with the progeny of the reverted plants, the type rapidly degenerates in purity.

Nor was that all. Even the most zealous breeders, working with utmost skill to preserve a new variety, would often contribute nevertheless to its disintegration. Percival used a (fictional) variety of pea as an example:

> [T]hree different raisers of seed of "Gubbins' 'Incomparable' pea" are almost certain to hold different views from Mr Gubbins and from each other in regard to the relative importance of the various characters of a good pea; selection is therefore carried out from three different standpoints, and in a few generations the "Incomparable" variety no longer exists except in name, unless Mr Gubbins himself also carries on the propagation: three different types bearing the same name would arise. It is therefore very necessary for the farmer and gardener not to be led away by the fascination of an old name, for it does not follow that anything useful is obtained with it; at the same time it must be remarked that a new name does not necessarily represent any new quality or character in the seeds to which it is applied; new names may easily be applied to old articles when the latter cannot be sold by their original names.[11]

The vagueness and passive-voice construction of that last clause enabled Percival to pass in silence over the sometimes nasty commercial realities that made these difficulties so much worse in practice. For, those other raisers of seed were not just Gubbins' customers; they were, potentially if not actually, his rivals. And, given the success of his variety, it could very well be in their interests, and not at all in his, for them to attempt to sell rogue-riddled or otherwise inferior pea seeds under the "Gubbins' Incomparable" name; or conversely, to sell his pea seeds under names of their own.

Within the history of biology, 1900 is of course associated not with Gubbins' peas but with Mendel's. That was the year when Mendel's studies in experimental hybridization from decades before suddenly – and for reasons we shall examine below – became a talking point among experimentally inclined botanists interested in heredity. Percival's textbook, which went off to the publisher in March 1900, made no mention of Mendel. The Brünn monk's experiments and explanations, clearly and respectfully summarized over ten pages (out of eight hundred plus), show up only in the fourth edition of 1910. Not, it should be noted, in the plant-breeding chapter – where coverage of the role of hybridization in the breeder's armoury remained unchanged from the 1900 edition – but in the preceding chapter, on reproduction. That placing notwithstanding, Percival saw Mendelism as of more than strictly theoretical interest. After introducing the Mendelian basics, he drew attention to a couple of ways in which, in his words, the "Mendelian conception of distinct unit characters" – the notion that, for example, the colour of a pea seed may be all-yellow or all-green, but not something in between, and that seed colour gets inherited independently from other inheritable characters – "… greatly assists the efforts of the plant breeder." First of all, the breeder seeking to combine characters from different plants might draw on Mendelism to plan out a simpler and more direct series of crossings than would otherwise be possible. Furthermore, the new science brought some order to the chaos of rogue plants. “Mendelism”, wrote Percival, “… throws considerable light on various forms of reversion” – notably, those cases where “‘reverted’ individuals … are merely recessives which have never had the chance of showing themselves.” So, for example, the breeder perpetually disappointed in his attempts to breed uniformly yellow-seeded peas but forever finding green-seeded ones among them would, with Mendelism’s help, come to appreciate that the starting, yellow-seeded stock must have been hybrid. In the Mendelian vocabulary, “dominant” characters such as yellow are visible whether the plant contains only yellow-making factors or whether it contains both yellow-making and green-making factors. Given a hybrid ancestor, offspring generations will eventually – and with a regularity which Mendel quantified and explained – produce plants containing only the green-making factors and so displaying greenness (the “recessive” character). Of the return of characters from much further back in a plant’s lineage, however, Mendelism was silent.[12]

In Percival’s perspective, then, the Mendelian conception was helpful to the breeder, but it was hardly revolutionary. The main how-to messages on plant breeding in his textbook – standard reading in applied biology throughout the first half of the twentieth century – remained the same in 1910, and indeed in 1950, as they had been in 1900.[13]

What of the breeders’ world more widely? “Synonyms” was the term used in connection with the IP problems gestured toward by Percival, referring both to the situation of different plant varieties bearing the same name and, confusingly enough, to the opposite situation of one variety bearing several names. Synonymy was the plant world's version of piracy. On the whole the market was unregulated, so the sale of synonyms could be quite widespread. Furthermore, the hierarchical structuring of the market meant that, although less established breeders could be "discouraged" more or less effectively from the practice, better established breeders sold synonyms with relative impunity.[14] But even in these cases, there were various informal means of protection, and there gradually emerged an increasingly organized set of instruments and even legislation to deal with synonyms and other problems such as deliberate mislabelling and the sale of inferior seed. We shall first describe some informal practices which persisted throughout the period, then look at some changes in formal structures.

In Britain at the time there were several big seed firms, including Garton's, James Carter & Co., and Sutton's. The literature produced by these companies, including seed catalogues and farmers' guides, played an important role in protecting new varieties from piracy. Carter's, in particular, produced lavish yearly catalogues and farmers' guides, often with photographs that sometimes even showed how different varieties compared with each other in order to aid identification. In general, the reputations of these companies served as a guarantee and a means to keep buyers coming back to the same source of seed; this promotional literature was one way of maintaining these hard-won reputations. Carter's were particularly keen to trade on their name and position as suppliers of seed to the royal household – a distinction they proclaimed in much of their advertising literature. Another set of means for securing identity, and so protection, was the use of sacks and seals. Seed was often sold in sacks with the name of the seed variety written on them, and sometimes these sacks were also sealed (see figure 1 for an example of a seal used by Carter's). The sealed sacks would then be supplied directly to the purchaser by post, further reducing opportunities for tampering.

Even in the nineteenth century there was an important institutional dimension. Informally, the Royal Horticultural Society (RHS) provided some protection against synonyms. The Society ran several committees which rated new varieties, awarding certificates based on quality: first class, second class, botanical commendation and commendation. Awards of these certificates were reported in the Society's journal. Notice of awards would often be made in firms' advertising material, where they functioned as a warrant to the originality and quality of a new variety (see figure 2 for an example of Carter's advertising, illustrating the company's use of prizes). These certificates were augmented by the awarding of prizes at flower shows (the most famous of which, the Chelsea flower show, is still running). Another institution, the Royal Agricultural Society, also ran some trials of new varieties, publishing the results in its journal. Readers would often write in with the results of their own trials. The Royal Agricultural Society's shows also awarded prizes and so credit in a similar way to the RHS. Independent journals such as the *Gardeners' Chronicle* – which enjoyed a similar circulation to the *Guardian* and the *Economist* in the period, and is well known to historians of biology for contributions from the likes of Darwin and Hooker – provided forums for the promotion of new varieties and, through their correspondence pages, the airing of disputes as to originality.

Thanks to these and related institutions and publications, seed dealers accrued reputations around particular varieties. To a large extent it was this informally established and maintained reputation coupled with direct sales which protected the largest, most established dealers, at least, against the sale of their varieties as synonyms. Nevertheless, there were also, as noted, some developments over the period of more formal means for protecting the identity of varieties. The oldest piece of legislation was the Adulteration of Seeds Act of 1869. This Act arose from concerns about adulteration and dealt mainly with outlawing various practices used to make old or bad seed saleable. But the Act was largely seen as toothless, since there was no official body to enforce it. Not a great deal more happened until the twentieth century, in particular the period after the Great War. The disruption to the agricultural status quo during the war concentrated minds on the question of how government could best help agriculture, especially as the threat of isolation from trading lines had now become a very real prospect. One of the results of this shift in official attitude was the temporary Testing of Seeds Order of 1917, instigated as a special measure during a part of the war when all agriculture was under government control. The Order stated that all seeds should be certified for identity, germination level and purity in terms of freedom from weeds or disease.

After the war this legislation in turn became the basis for the Seed Adulteration Act of 1920, which demanded the use of certificates for all seeds, produced at point of sale. The government Ministry for Agriculture and Fisheries – a forerunner of the current Department of Environment, Food and Rural Affairs (DEFRA) – oversaw enforcement and provided inspectors to take samples from thousands of businesses that sold seeds, including farms that sold to other farms, and even blacksmiths or grocery stores that sold seed only seasonally and in very small quantities. Enforcing the Seed Adulteration Act in turn generated a demand for seed testing services. A newly established charity, the National Institute of Agricultural Botany (NIAB), based in Cambridge, became the new home of the chief English seed testing station, the Official Seed Testing Station (OSTS). The OSTS at NIAB was responsible for checking the particulars given in the seed certificates and providing those particulars to vendors. NIAB also published yearly reports on the OSTS's testing activities in its own journal. NIAB also had another regulatory role: the testing for quality and distribution of new varieties raised by publicly funded research. The seeds for these varieties would be sold by NIAB, which acted as intermediary between research centres and the established seed corn dealers. In this way NIAB exerted some control over the sale of seeds while at the same time utilizing established supply channels and advertising of seed companies such as Carter's, Sutton's and Garton's. The Institute was also, significantly, responsible for bestowing credit on the new varieties it tested and then reciprocally harvesting that reputation to bolster its own.[15]

A crucial but easily overlooked, additional innovation of relevance here was the growth, alongside publicly funded agricultural research and seed testing, of an ideal of selfless public service amongst the researchers who depended on that funding. For a glimpse of that ideal in action, consider the following extract from a speech made in 1924, introducing one of the most famous of the publicly funded breeders, Rowland Biffen (about whom more below). The speechmaker was Sir H. Trustram Eve KBE, introducing Biffen at the London Farmers' Club:

> We practical business men, if we have an idea, try to make money out of it; it is human nature, but the scientific man is always working for others without advantage to himself […] There is no patent, there is no copyright in seeds, and yet our scientific friends are spending the whole of their lives in seeing how they can help the farmers of this country.[16]

3. Ownership of the science of heredity in Britain (and elsewhere) in the decades around 1900

Twenty years earlier, Biffen had written of Mendel's 1866 paper that, "judging from the almost absolute lack of reference to it by later writers, it was completely lost sight of."[17] That was an exaggeration, though a persistent one. In the later nineteenth century, Mendel was not quite the forgotten figure of legend. Botanists had regularly and respectfully cited his 1866 paper. But Mendel had taken his place among several investigators who were considered to be working along similar lines. Where he had identified and, in a provisional way, explained patterns of inheritance for particular traits in peas, others had done roughly the same for other plants. No one attributed much significance to the "law valid for *Pisum*", as Mendel had called it.[18] And then, in the spring of 1900, the German botanist Carl Correns published a paper enshrining "Mendel's law" in its title. As Correns explained, his own researches with maize as well as peas had led him to rediscover what, he had belatedly realized – and, he noted, another rediscoverer, the Dutch botanist Hugo De Vries, had apparently yet to realize – Mendel had discovered so long before.[19] Within a few years, Mendel's law or, more commonly, laws came to be hailed, in influential quarters, as the basis for a new and general science of heredity. Soon rebranded "genetics" or "Mendelian genetics", it was initially called simply "Mendelism." Over a century later, in our own genomics age, "Mendelian inheritance" is still a meaningful phrase, and Mendel remains the celebrated founder. In the words of a popular undergraduate biology textbook, his "theory of inheritance, first discovered in garden peas, is equally valid for figs, flies, fish, birds and human beings."[20]

And yet, in the 1900 paper which singled out Mendel's law so consequentially, Correns made rather modest claims on its behalf. He guessed that it would probably turn out to be valid only for "varietal hybrids" – that is, hybrids formed from varieties within a species, as distinct from hybrids formed from different species – and then only for those varietal hybrids where one character in a pair dominated the other. Even some hybrid peas, he continued, were a poor fit.[21] Why, then, make such a fuss about Mendel's having priority as discoverer? As the sociologist Augustine Brannigan observed in a classic discussion of the Mendelian "rediscovery," Correns' generous gesture toward Mendel was at the same time a bid to undermine De Vries. Correns might have lost out to De Vries in the publication race; but now, in stressing how much both men's work shared with Mendel's, down to the "strange coincidence", in Correns' phrase, of De Vries' replicating the abbot's vocabulary of "dominating" and "recessive", Correns got his revenge. If he would get no credit for the discovery, neither would De Vries. (Intriguingly, both the word and the quotation marks around "rediscovery" are Correns'. Even so, he, like De Vries, owed a larger and earlier intellectual debt to the 1866 paper than he would ever admit.) So Mendel entered the wider biological consciousness, at the time that he did, as a means to the end of resolving a priority dispute.[22]

In its very name, then, Mendelian genetics is emblematic of the significance of intellectual property (in our broad sense) in the sciences and the controversies surrounding it. And the name rapidly came to stand for far more than merely that science of heredity which took as its starting point the patterns and explanations found in Mendel's paper. Thanks above all to William Bateson, who from 1900 made Mendelism's advocacy his mission and who, as part of that advocacy, invented and popularized the term "genetics," "Mendelian genetics" became the name of the science of heredity *tout court*, with Mendel represented as the trailblazing bringer of scientific method to the problems of inheritance.[23] With Mendel's promotion in this guise came, needless to say, the demotion of other figures whose work could be seen as foundational, above all the English mathematical polymath Francis Galton. An exact contemporary of Mendel's (both were born in 1822), Galton made his debut as a scientific student of heredity with a pair of articles on "Hereditary talent and character" published in 1865, the same year that Mendel presented his pea experiments before the underwhelmed Brünn Natural History Society. From then until his death in 1911, Galton did more than anyone, anywhere, to attract attention and ambition to the problems of heredity as full of intellectual fascination and social utility. He introduced new and enduring methods of attack, including the analysis of pedigrees and studies of twins. He popularized the phrase "nature and nurture" (in the title of an 1874 book on English scientific men) and coined the term "eugenics" (1883). He developed a physiological theory of inheritance which pictured individuals as harbouring hereditary elements some of which had visible effects ("patent" elements) and some of which did not ("latent" ones). His 1889 book *Natural inheritance* was a showcase for his quantitative approach and the statistical concepts he had invented in developing it, notably correlation and regression. His readers included Bateson and Weldon, young men who, over the next decade, gradually abandoned the evolutionary morphology in which they had been trained for Galtonian kinds of research and mentoring alliances with the man himself. Both were enormously impressed with Galton's proposal, in the late 1890s, of a new scientific law governing inheritance, as disclosed in an analysis of data from the breeding of basset hounds. Bateson eventually came to think that the law represented a special case of Mendel's laws, with Weldon coming to very much the opposite conclusion.[24]

We shall say more about Galton's law, which came to be known as the "Law of ancestral heredity", below. For now, the important point to note a propos of ownership claims, in the IP-broad sense, on Mendel's behalf – claims, that is, for Mendel as first (priority) and for his discoveries as useful because true (productivity) – is the way that the early Mendelians made them in the course of writing Galton out of the history of their science. In the 1909 edition of *Mendelism*, a popular textbook written by Bateson's Cambridge colleague Reginald Punnett, a chapter entitled "Applied heredity" (published separately in *Harper's Monthly Magazine* in December 1908), proclaimed that "the great and baffling problem of heredity has suddenly passed from the speculative to the experimental stage. The credit of it belongs to one man."[25] There followed a capsule biography of Mendel and summaries of his principles and their recent applications, in plant and animal breeding – Biffen's achievements as a Mendelian breeder of superior wheat varieties received extensive coverage – and in human heredity, above all as it touched on health and disease. "Professor Biffen's classic experiments with wheat rust have opened up a fascinating field of research in connection with problems of immunity," wrote Punnett in conclusion:

> … We must have full and accurate pedigrees, and for their interpretation we require carefully devised experiments in the breeding of plants and animals. With increase in knowledge will come powers of prevention far greater than those we have to-day. How far we may use these powers must rest with the future to decide.[26]

There is no mention of Galton. How striking to find Galton himself, in the conclusion of an address published that same year, sounding much the same note in relation to recent, but rather different, discoveries about heredity:

> All I dare hope to effect by this lecture is to prove that in seeking for the improvement of the race we aim at what is apparently possible to accomplish, and that we are justified in following every path in a resolute and hopeful spirit that seems to lead towards that end. The magnitude of the inquiry is enormous, but its object is one of the highest man can accomplish. The faculties of future generations will necessarily be distributed according to laws of heredity, whose statistical effects are no longer vague, for they are measured and expressed in formulae. We cannot doubt the existence of a great power ready to hand and capable of being directed with vast benefit as soon as we shall have learnt to understand and to apply it.[27]

Our biology textbooks, so solicitous of Mendel's achievement, tend, like Punnett, to silence over Galton's achievement. Might the situation have turned out otherwise if the Mendelians had lost their debate? We turn next to why Weldon thought they deserved to lose and the Galtonians to win – and to why, as Weldon saw it, Galton's perspective made so much more sense of the plant breeders' own troubles with ownership.[28]

4. Mendelism under attack: W. F. R. Weldon, *Telephone*, and rogue plants as a problem for breeding practice and Mendelian theory

What is known among historians of biology as the “biometrician-Mendelian controversy” ran roughly from the beginning of 1902 until Weldon’s death in 1906, though its roots extend back into the later nineteenth century. In the opening anti-Mendelian salvo, in the journal *Biometrika* in February 1902, Weldon accused the Mendelians of, amongst other things, exaggerating grossly the ease with which cleanly differentiated varieties could be bred from old ones. To illustrate the point, he referred back to a series of letters published in the *Gardeners’ Chronicle* at the end of the 1870s.[29]

At the heart of the 70s furore was a quarrel about the identity of *Telephone,* a putatively new, putatively wrinkled pea variety. *Telephone* was released by James Carter & Co., who claimed to have produced it by selection from an older variety, *Telegraph*, bred by a Yorkshire breeder named William Culverwell. The trouble arose when Culverwell claimed that *Telephone* was not a new variety at all, but merely the wrinkled peas isolated from *Telegraph*, which gave both round and wrinkled peas. Culverwell had sold to Carter’s the whole of the *Telegraph* stock – and so the rights over it, in a situation reminiscent of what was happening in America – but he felt that their isolating the wrinkled peas from *Telegraph* would ultimately detract from the stock, since the wrinkled peas, which tended to be sweeter, were reckoned to be more desirable than the round ones.[30] 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*, would diminish as the quality of *Telegraph* diminished.

For Culverwell, then, it was above all his reputation as a breeder that was at stake. At Culverwell’s and the editors’ behest, various contributors to the *Chronicle* grew the peas together. Finally 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.[31] What mattered to Weldon was less this conclusion, however, than the fact that controversies like this one occurred at all. Protracted disagreement over the novelty of putatively new breeds was, Weldon argued, predictable and explicable on the Galtonian understanding of heredity, but a surprising mystery on the Mendelian understanding.

As Weldon made plain, the biological issue at stake here was the question of ancestral influence and how quickly or easily it could be stamped out. According to Weldon and others on the biometrical side, ancestral influence was extinguished only very slowly and was extremely hard to get rid of completely. The principle was summed up in what, as noted above, was known as Galton's law of ancestral heredity.[32] There was never any consensus about exactly what was governed by the law or how far it applied strictly, but the essential idea was that hereditary influence could be thought of as dropping away regularly as if in a mathematical series, with parents accounting for one half of the offsprings' character, grandparents for one quarter, great-grandparents for one eighth, and so on. Applied to breeding, the point was that, in fixing a new variety, breeders should expect an uphill battle in keeping out unwanted characters – or in other, nineteenth-century words, they should expect rogue reversions to ancestral characters. Even when he wrote, Weldon pointed out, twenty-five generations since the supposedly originating cross, *Telephone* seeds remained stubbornly variable, in colour but also in shape, occasionally exhibiting both of the characters in the Mendelian contrast pairs and even intermediate characters.[33]

As interpreted by Weldon, Mendelism seemed utterly at odds with empirical reality, even on its own home patch of pea varieties. The problem, on his diagnosis, was the Mendelian contention that ancestral influence can go to zero in a single generation: a massive violation of Galton's law. Although this contention is not familiar as a key Mendelian principle nowadays, it is easy enough to discern its importance if we briefly consider a characteristic Mendelian cross. On textbook Mendelism, when "true-breeding" yellow-seeded pea plants are crossed with "true-breeding" green-seeded pea plants, the hybrid plants in the next or "F_1" generation are all yellow-seeded. When these yellow-seeded F_1 plants are in turn hybridized together, the next, "F_2" generation of pea plants are – again on textbook Mendelism – a mixture of yellow-seeded and green-seeded plants, in the ratio of three to one. The question for Weldon was: are the F_2 green peas (known as the "extracted recessives") identical in hereditary constitution to their green grandparents, despite having had yellow parents? Putting the same question another way, should we expect the F_2 greens to harbour no yellow-making factors whatsoever, and thus to show no hereditary influence at all from their yellow parents? The Mendelians answered "yes", in defiance both of Galton's law and, as Weldon's marshalled evidence was meant to show, of the facts familiar to plant breeders, who knew how hard it was to purify away even quite distant ancestral influence. (When Percival, some years later, covered Mendelism in his textbook, he wrote, of extracted recessives: "Such 'reverted' individuals ought to breed true when crossed among themselves or self-fertilised, and *this is sometimes the case*.")[34]

In returning to old numbers of the *Gardeners' Chronicle* and a dispute over breeders' ownership of pea varieties – a dispute that left traces for Weldon to find, thanks to processes institutionalized by breeders in order to manage disputes of this kind – Weldon saw himself as documenting the reality of the long reach of ancestral influence, as against Mendelian teachings. The unit characters described as distinct and segregating by Mendel seemed to be anything but in *Telegraph/Telephone*.

Furthermore, Weldon argued, disputes of this kind, over the genuineness of putatively new breeds, should be expected, because the fixing of new varieties was far more difficult than Mendelian theory implied.

From the foregoing it may sound as though Weldon offered breeders a counsel of despair. Far from it. In an audacious closing section to the *Biometrika* paper, after summarizing his results and the impossibility of squaring them with Mendel's laws of dominance and segregation, Weldon credited a plant-breeder, indeed a highly successful breeder of peas, with having understood all that Weldon was saying a generation before. As Weldon explained, in the very year, 1866, that Mendel published his deeply misleading paper on pea hybridization, a much better paper, far more in accord with the experience of Weldon and pretty much every other observer, was published in English, under the title, "Observations on the varieties effected by crossing in the colour and character of the seed of peas," in the *Report* of the International Horticultural Exhibition and Botanical Congress, by one Thomas Laxton.[35] Weldon gave a preview to his biometrical ally Karl Pearson in a letter of 21 November 1901.

> While Mendel was making his "laws", Laxton, of whom Darwin speaks so often! was crossing peas and making all the main races we now eat. In 1866 he published his impressions of the result of crossing… He says that the peas directly resulting from hybridization "*are sometimes all intermediate, sometimes represent either or both parents in shape or colour, and sometimes both colours or characters, with their intermediates, appear*."[36]

Who was Thomas Laxton? Born in 1830, dead in 1893, he was one of the most successful nurserymen of the era, with a number of varieties of peas, beans and strawberries to his credit, as well as a collaboration-by-correspondence with Charles

Darwin on pea hybridization, memorialized in the pages of *Variation of animals and plants under domestication*. Laxton has rather disappeared from the recent historiography, although H. F. Roberts, in his estimable *Plant hybridization before Mendel* (1929), devoted seven pages to Laxton's hybridization experiments.[37] In the brief paper cited by Weldon (who quoted from it at length), Laxton wrote about the third and fourth hybrid generations as well; by the time of the latter, he wrote, we find "the seed often reverting partly to the colour and character of its ancestors of the first generation, partly partaking of the various intermediate colours and characters, and partly sporting quite away from any of its ancestry."[38]

The upshot was that, on Weldon's reading, Laxtonian breeders did not expect new varieties, especially those formed through hybridization, to settle down into a uniform character at all quickly. Rather they relied, in Laxton's phrase, on "careful and continuous selection" to transform the novelties turned up via hybridization into new and stable breeds.[39] And indeed, such was in summary the counsel on plant breeding offered in John Percival's textbook, in 1902 and beyond.[40]

5. Mendelism defended: Rowland Biffen, *Yeoman II*, and the protection of Mendelian products and principles against rogues

As noted above, Bateson in 1902 wrote a furious book-length reply to Weldon, including a vigorously contrary reading of Laxton's lessons.[41] Elsewhere in this set of papers, Bateson's efforts on behalf of Mendelism as an applied science, useful not least to rogue-plagued breeders of peas and other crops, are examined at length.[42] For present purposes a more instructive counterpart to Weldon is Rowland Biffen. An early star of Bateson's Cambridge school, Biffen is still remembered among geneticists as the man who first successfully applied Mendelian principles to agriculture, producing new and productive wheat varieties including *Little Joss* (1910) and *Yeoman* (1916) as well as the variety that will be our main concern here, *Yeoman II*.[43] Beyond his Mendelian allegiances, however, Biffen is useful to think with, in that he represented a very different type of breeder from either Culverwell or James Carter and colleagues. For, Biffen, who held a chair in Agricultural Botany at Cambridge from 1908 and the directorship of the Plant Breeding Institute (PBI) in Cambridge from 1912, was one of the new academic scientists who used public monies to advance agricultural research while at the same time using agricultural research to pull in public monies.

In the 1904 paper quoted from earlier, entitled "Experiments with wheat and barley hybrids illustrating Mendel's laws of heredity", Biffen reported the results of trials at Cambridge University's Experimental Farm using these two commercially important cereals and aiming to "test the possibilities of the application of Mendel's discoveries."[44] In prospect, Biffen predicted, was the fixation of new varieties far more rapidly than in the bad old days of futile selection:

> All the evidence which has accumulated … goes to show that the characters of the plant or animal are distributed among the sex-cells according to a definite system, and the possible combinations can be foretold with considerable accuracy. To the breeder the value of this knowledge can hardly be estimated. Once he knows the behaviour of particular characters of the varieties he is working with, he can definitely choose the parents which will give him the combination he desires, and obtain it, fixed, in the first or at the latest in the second generation from the cross-bred. This is worth comparing with one's expectations in the dark pre-Mendelian days. Then one might by chance find

> the required type among the mixture resulting from the cross-breds; more often it was a case of the selection we hear so much of – the picking out of such a form as the rough-chaffed red wheat which in the following generation might breed true, or with far greater probability (the chances can be easily calculated) would break up into a number of forms similar to those from which it was originally chosen. A further selection from the mass would in all probability give the same result. Small wonder is it that competent breeders have given up as hopeless problems the solution of which we now know to be simple.[45]

Biffen, by then, had ample reason to feel confident. His work on the Experimental Farm had already disclosed, to his satisfaction at least, that a range of characters in wheat came in Mendelian dominance-recessive pairs. When, drawing on that work, Bateson had reported to American breeders in New York, in the autumn of 1902, that beardlessness in wheat was a dominant character and, therefore, that beardless wheat plants might contain profit-wrecking factors for the recessive character of beardedness, his audience treated the news as wondrous (see chapter 8). The signs from the Experimental Farm remained encouraging throughout Mendelism's first decade. In the discussion of the applied-heredity horizon in Punnett's 1909 edition of *Mendelism*, readers learned that the most exciting of Biffen's recent additions to the list of Mendelian character-pairs in wheat was susceptibility and resistance to rust, a commercially troublesome disease brought on by a fungal parasite. With rust immunity thus revealed to be, in Punnett's words, "within the control of the breeder to combine with other characters according as he pleased," Biffen was in the midst of doing just that:

> From the knowledge gained through his experiments Professor Biffen has been able to build up wheats combining the large yield and excellent straw of the best English varieties with the strength of the foreign grain, and at the same time quite immune to yellow rust. During the present year several acres of such wheat coming true to type were grown on the Cambridge University Experimental Farm, and when the quantity is sufficient to be put upon the market there is no reason to doubt its exerting a considerable influence on the agricultural outlook.[46]

Yet the old difficulties of fixing new varieties, so that they would reliably "come true to type," remained an ever-present problem for Biffen. *Yeoman,* the most famous product of his Mendelian labours after the rust-resistant *Little Joss* (on which see too the final paper in this set), turned out to suffer disastrously from a rogue problem. When grown by the thousands in a field, a number of out-of-type individuals regularly became obvious, often because they grew taller than the rest. Although his critics suggested that the presence of rogues in fields of *Yeoman* pointed towards a reversion of the strain to ancestral type, Biffen viewed that possibility as a part of hereditary folklore – dismissed a generation before, with Galton's law and the biometrical opposition to Mendelism generally. For Biffen, the stable character of his new strains was guaranteed by the Mendelian principles by which they had been generated. In defending this view and simultaneously defending against the possibility that *Yeoman*'s problem with rogues was a sign that there was a problem with Mendelism, Biffen offered an ingenious explanation. The culprit was the threshing machine, used to separate the corn from the ear, and to separate both from the straw. At the time, threshing machines were often transported from farm to farm, as any individual farmer was unlikely to be able to afford one. In the process of threshing, some corn would become lodged in the machine, which would then travel to the next farm, where, Biffen alleged, the contaminant corn would become mixed with corn intended for planting the following season.[47]

While the *Yeoman* rogues were not particularly troubling to farmers – financially, the problem was insignificant – to Biffen, the plant's identity, as a plant of a certain stable character, was crucial, not least because it was so intimately bound up with his own reputation as the great pioneer of Mendelian breeding. Biffen's solution to *Yeoman*'s rogue problem was basically to start again. In November 1922, at the first AGM of NIAB, while giving a lecture in his role as Chief Scientific Advisor to the Institute, Biffen made his first public mention of the new form of wheat that he had passed to NIAB for testing and, if deemed successful, distribution. Especially striking is Biffen's dismissal of reversions as a serious problem for the breeder. The report of the lecture virtually opens with the claim that, "There is no difficulty in fixing these types; so-called cases of reversion are traceable to mixture of stocks in travelling threshing machines."[48] The main reason he gave for the release of *Yeoman II* was, accordingly, to purify contaminated seed stocks. A second reason given was that *Yeoman II* was supposedly a superior variety of wheat. Biffen was quoted in *Nature*, in 1923, as saying, "the sooner Yeoman is off the market the better."[49]

In the *Journal of the Ministry of Agriculture*, in September 1924, Biffen again stated that the new strain was a remedy for the impurities of old stocks. At the end of the article, which announced the release of the new variety, Biffen laid claim to the most obvious form of protection placed upon the release of *Yeoman II*, the seal to be placed on the sacks in which it would be sold: "The attention of farmers is particularly drawn to the fact that genuine seed of Yeoman II can only be obtained in sacks closed with the seal of the National Institute of Agricultural Botany."[50] These seals were the means of protecting the release of *Yeoman II*. Tenders were only to be made to NIAB, the price was fixed, and the seed certified as genuine and superior by the NIAB seal on the sacks it was sold in (see figure 3 for an illustration of this seal).

6. How intellectual property matters for the historiography of genetics: Revisions in prospect

Here we have sketched the outline of an IP-inflected history of the making of Mendelian genetics. To see just how far that history departs from more traditional ways of telling the story, it will help in closing to recall some standard features of traditional tellings. Three in particular will make for instructive contrasts. First, there is the notion that the eventual "synthesis" of Mendelism and biometry shows that there was no empirical or conceptual substance to that debate, just divergent methodologies and ideologies: for and against the postulation of unobserved causes in science; for and against Darwinism; for and against eugenics; and so on. Second, there is the emphasis on supplementary scientific developments which at once favoured and strengthened Mendelism, notably, in its first decade, the "pure line" theory of the Danish botanist Wilhelm Johannsen (who coined the word "gene") and, in 1910-20, Mendelism's fit with the chromosome theory of heredity as developed by the group surrounding T. H. Morgan in the famous "fly room" at Columbia University in New York. Finally, there is what is presented as Mendelism's trump card: the growing appeal of Mendel's work to practical breeders, who literally could not afford to invest in a theory which, however attractive on other grounds, was false and therefore useless.

How does IP-mindedness help us rethink these tropes? To start with the biometrician-Mendelian debate, we have seen that at the centre of Weldon's critique was a phenomenon whose existence the breeders of his day found utterly uncontroversial (but whose consequences led them into ceaseless controversy with each other): the persistence of unwanted variability in the shape of ancestral, supposedly selected-against characters. Yes, Weldon's stress on the need to take rogue plants seriously was of a piece with his allegiances to, among other things, Darwin's theory of natural selection (which, to be an important evolutionary force, requires the existence of copious, small variations), Galton's law of ancestral heredity, and Karl Pearson's positivist concern that scientists had to deal with all of the observations, not just those which fitted overly tidy idealizations. But rogue plants were nevertheless a real problem, and one which, for Weldon, highlighted limitations in the Mendelian scheme which are otherwise easily missed. By Weldon's lights, indeed, it would not be surprising if, for most of the twentieth century, with those limitations well out of view, hereditary returns to deeply ancestral characters went largely unreported, and indeed unnoticed. By 1924, variability in British wheat breeds no longer – as it would have for the likes of Weldon – pointed up an anti-Mendelian conceptual lesson. It merely illustrated a not very interesting practical problem, to be addressed by releasing a pure stock of seed through well-managed means of distribution. The sacks and seals used to protect the plant varieties bred into existence by Mendelian principles thus ended up protecting those principles as well; all apparent reversion to beyond-the-parents plants or character instability could henceforth be presumptively blamed on "external" contamination.

Weldon's and Pearson's biometrical approach is rightly associated with quantitative characters such as height, whose distribution in populations takes the graphical shape of bell curves. From early days it was understood that Mendelian explanations could be stretched to account for the inheritance of such characters, though Mendel had developed his concepts in order to explain patterns of inheritance in qualitative, either/or characters. This was a synthesis; but it was on Mendelian terms, with Mendelian concepts constituting the general principles and biometrical topics relegated to the status of the special case. Weldon wanted something different, and his rogue-based critique is a marvelous vantage point from which to look afresh now at Mendelism's achievements. From a Weldonian perspective, for example, the regular ratios from which all else about inheritance followed for Mendel were not, as Mendel seemed to think, and as Mendelians thought ever after, patterns which were somehow specially revealing about the ways of inheritance as they truly are. They were simply patterns, identified in advance as desirable and then realized thanks to methods which, by ruthlessly excluding any hereditary factors that would mess things up, ensured those patterns would appear. If one designed one's experiments correctly, one could, Weldon suggested, end up showing that a given character is dominant, or recessive, or neither. Here is a point of view never encountered in the standard historiography. Its recovery can help us ask, with respect to Johannsen, about how his pure-line work contributed to a consensus about purification not just as an exciting and achievable goal for practical breeding but as an exciting and achievable goal for scientific students of heredity, who thereby stood to learn their most profound lessons. (Mendelian genetics was, in this sense, the purest of pure sciences.) And a propos the chromosome theory, Weldon's own belief in it suggests how much scope there is for prizing apart the now taken-for-granted association between the theory and Mendel's paper – an association which, to this day, keeps the Mendel brand vital.

We should be more curious about that continued vitality, its sources and its significance. In looking at what the marketing and management of "Mendelian" wheat breeds in Biffen's Britain did for Mendelism, this paper has aimed to stimulate that curiosity. But it is only a beginning. By way of illustration of the puzzles remaining, consider, for example, the following passage, from Paul and Kimmelman, summarizing the appeal of the Mendelian program for American breeders. Paul and Kimmelman emphasize that Mendelism did *not* give the breeders new manipulative powers. So, they go on to ask,

> what was [Mendel's] appeal to the seedsmen? The answer is partly that Mendelism offered a plausible explanation for the extreme difficulty in obtaining varieties that would "breed true." Specific results that had long puzzled practical breeders included the "reversions on crossing" discussed by Darwin, the greater variability of new types, and the problem of fixing hybrids. It was doubtless interesting to know why some varieties could apparently not be fixed, despite repeated selection, and why success was so long coming with others.[51]

One reads this list and imagines Weldon in despair; for surely, he would have said, these were precisely the problems that concerned him, and Laxton before him, but that Mendelian principles would never lead one to expect, let alone understand. From Weldon's point of view, it was the law of ancestral heredity, not Mendelian patterns, that rendered predictable the persistent backsliding of the breeders' "true-breeding" creations, or the bringing on of a reversion after the physiological disruption of crossbreeding. Indeed, in an unpublished manuscript, Weldon instanced a previously dominant character's going recessive as just the sort of thing that crossbreeding was known to stimulate – a double surprise for the Mendelian.[52]

Or consider the remarkable fact of Mendelism's *teachability*. It may be that, because the main technique of Mendelian analysis *was* a breeding technology – hybridization – the technology became newly endowed with Mendelian heuristic power. We have in mind here the possibility that, for some breeders, Mendelism brought new clarity to the tactical use of hybridization, if only by foregrounding the technique in the way Mendelism did. Mendelian triumphalism still has it, for example, that before the likes of Bateson and his students, breeders confronted with a field full of plants lacking a trait of interest would have burned the lot; whereas the Mendelian breeder, alert to the phenomenon of recessiveness, would have considered breeding from the apparently unpromising plants in order to expose valuable but hidden variation.[53] But of course, as noted above, the notion of latent traits was a feature of the Galtonian perspective, indeed, with its emphasis on atavisms, a well-known feature. Furthermore, one of the most successful American plant breeders of the era, Luther Burbank, was no Mendelian, yet clearly was under no great handicap in his understanding of hybridization.[54] A related, more modest possibility was that, given the close resemblance between what breeders already knew about, together with the general simplicity of Mendelian concepts, Mendelism was just easier to assimilate than what someone like Weldon had to offer.

So there is much work, clarifying and complexifying, ahead. Two papers at the end of this set explore some of the territory mapped here a little further, with one on Bateson's efforts as marketer of Mendelism to the breeders, and the other on

Biffen's experimental wheat fields in Cambridge as a hub of imperial, international activity. Between them, these three papers offer the start, we hope, of an account of how the Mendelians dealt with rogues, intellectually and institutionally. But even on this restricted topic more needs to be done, not least in seeing how far the thesis ventured here – roughly, that Mendelism succeeded at least in part because Mendelians succeeded in changing how farming worked – generalizes to other nations (and empires). In the offing is a revised picture not just of the making of Mendelian genetics but of how, in taking ownership of the practices of breeding, Mendelians remade the world.[55]

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Figure 1

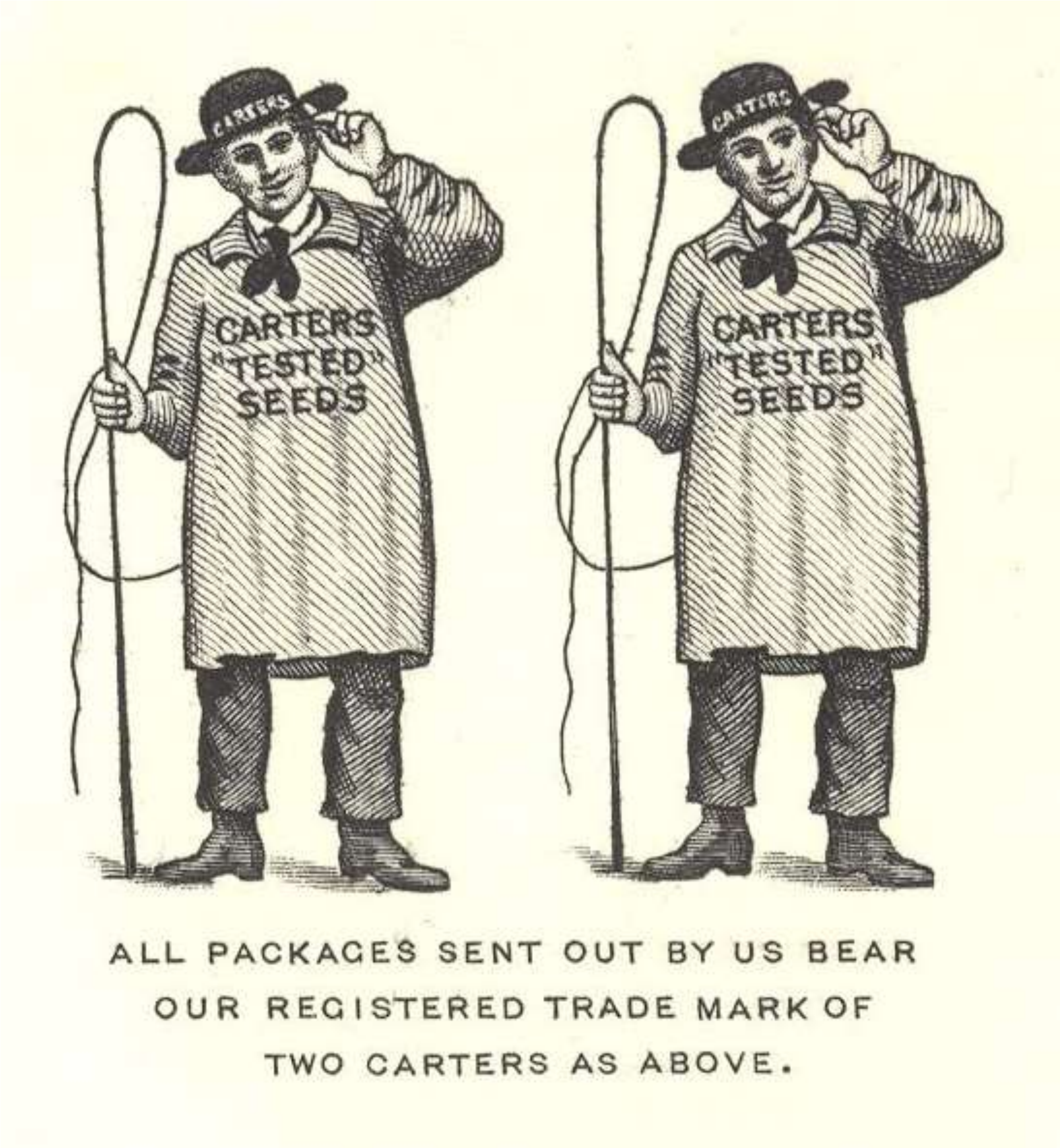

CARTERS
CARTERS "TESTED" SEEDS
CARTERS
CARTERS "TESTED" SEEDS
ALL PACKAGES SENT OUT BY US BEAR
OUR REGISTERED TRADE MARK OF
TWO CARTERS AS ABOVE.

Figure 2

24

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 4 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.

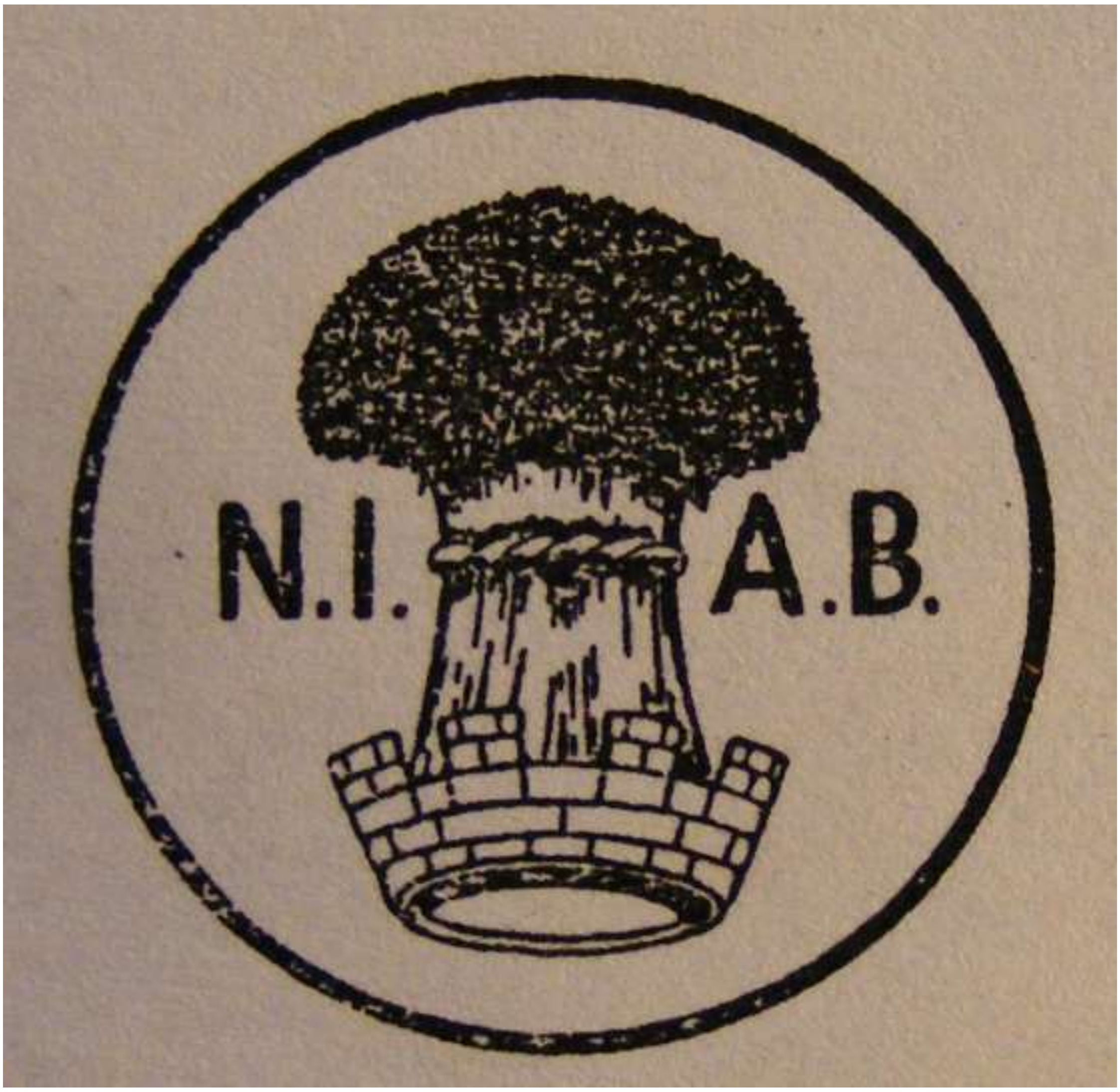
Price, in sealed packets, per pint, 2s. 6d.

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



Captions

Figure 1. Seal for seed sacks used by Carter's, c.1900. Weldon Papers, University College London. Image reproduced courtesy of UCL.

Figure 2. 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.

Figure 3. The seal used by NIAB to close bags of the new Yeoman II wheat. Image reproduced from Rowland Biffen, "The new wheat Yeoman II" *Journal of the Ministry of Agriculture*, 31 (1924), pp. 509-512.

ENDNOTES

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[1] Present address: Law School, Nathan Campus, Griffith University, Brisbane 4111, Australia. Email: Berris.Charnley@gmail.com.

[2] Bateson (1902), p. 208.

[3] Biffen (1904), p. 345.

[4] Davenport (1910), p. 66.

[5] Castle (1951), p. 60. Castle's remarks are preserved in a volume commemorating a "golden jubilee" Mendel celebration meeting in Ohio in September 1950. On the meeting and its Cold War context, see Wolfe (2012). From that era, see also a classic history of genetics by the volume's editor, L. C. Dunn: Dunn (1965), p. 85.

[6] Paul & Kimmelman (1988); Olby (1990), (2000a) and (2000b); Kim (1994). Complexities surrounding Bateson's and Biffen's positions as champions of applied Mendelism are dealt with in more detail in the papers elsewhere in this set by Radick and Charnley respectively. For Olby on the limitations of the (narrowly and broadly) Marxian historiography discussed in Radick's paper – a historiography indifferent, as Olby's is not, to Bateson's efforts in agriculture and horticulture – see Olby (1989a).

[7] Davenport (1910), p. 66.

[8] Lewontin & Berlan (1986); Palladino (1990), (1993) and (1994); Harwood (1997) and (2005); Bonneuil (2006); Theunissen (2008).

[9] The best general introduction to the Mendelian revolution remains Bowler (1989).

[10] On the contemporary American plant IP situation, see Kevles (2007).

[11] Percival (1900), pp. 306-307. The passage discussed and quoted from here remained unchanged through the many editions that followed. Later based at the University of Reading, Percival in 1900 was teaching at the recently founded Agricultural College at Wye. Of his textbook, Sir E. John Russell observed in 1955 that "many of the applied biologists of to-day were brought up on it"; Russell (1955), p. 9. On Percival's career, see Palladino (1993), esp. pp. 312-318.

[12] Percival (1910), pp. 289-299, quotations on pp. 298-299.

[13] Cf. Percival (1900), ch. 23, and Percival (1949), ch. 23. For an account of Percival's attitude to Mendelism which, however, depicts him as more sceptical than seems to us accurate, see Palladino (1993), pp. 316-317.

[14] Palladino (2002), p. 42.

[15] See, e.g., Weaver (1925).

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[16] Biffen (1924a), p. 2.

[17] Biffen (1903), p. 337.

[18] On the life of Mendel's paper from 1866 to 1900, see Brannigan (1981), pp. 89-119. The most detailed analysis of citations before 1900 can be found in Olby (1985), pp. 219-234. All of the existing translations into English of Mendel's 1866 paper are problematic; a standard translation is freely available at http://www.mendelweb.org/MWpaptoc.html

[19] Correns (1966), esp. pp. 119-120, 132.

[20] Campbell (1993), p. 267.

[21] Correns (1966), pp. 120-122, 131-132. In a postscript added in proof (132), Correns wrote: "I must emphasize again: 1. *that in many pairs of traits there is no dominating member* (p. 121); 2. *that Mendel's Law of segregation cannot be applied universally*" (emphases in original).

[22] Brannigan (1981), pp. 89-119, esp. pp. 90, 94-95; see also Olby (1989b). For Correns on the "re-discovery" and the "strange coincidence", see Correns (1966), pp. 120, 121 respectively.

[23] The term "gene", as a label for the factors or elements underlying Mendelian characters, was a back-formation from "genetics": see, e.g. Keller (2000), pp. 2-3.

[24] For a superb recent biography of Galton, with excellent coverage of his scientific work, see Gillham (2001). That work can be accessed at www.galton.org. On Bateson's change of mind about Galton's law, see Olby (1987).

[25] Punnett (1909), pp. 70-89, quotation on p. 70.

[26] Punnett (1909), p. 89. On Biffen's work with wheat rust, see the discussions later in this paper and also in the paper by Charnley in this issue.

[27] Galton (1909), pp. 33-34. Note the beginning of his title: The possible improvement of the human breed.

[28] The counterfactual question posed here receives fuller treatment in Radick (2005).

[29] Weldon (1902), esp. pp. 245-247. On the biometrician-Mendelian controversy, see Provine (2001); for the role of Weldon's paper, see esp. p. 70.

[30] For an accessible explanation in molecular terms of why wrinkledness in peas tends to be correlated with sweetness, see Guilfoile (1997), esp. pp. 92-93.

[31] On the *Telephone* controversy see Charnley (in press a) and, for further detail, Anonymous (1879 a), (1879b), (1879c) and (1879d).

[32] The classic statements are Galton (1897) and (1898). For historical commentary see, e.g., Provine (2001), pp. 179-187, and Radick (2005), pp. 35-36. On Weldon's enthusiasm for Galton's physiological theory as well as his quantitative law, see Radick (2011), esp. pp. 132-133.

[33] Weldon (1902), pp. 246-250.

[34] Percival (1910), p. 299, emphasis added.

[35] Weldon (1902), p. 251.

[36] University College London [UCL], Special Collections, Papers of Karl Pearson, 891/1: W. F. R. Weldon to Karl Pearson, 21 Nov 1901; emphases in original.

[37] On Laxton, see Roberts (1965), pp. 104-110; also Anonymous (1930). For Darwin and Laxton, see Darwin (1998), 1, pp. 400, 428-429; 2, pp. 42-43, 152; also Burkhardt

et al. (2004), pp. 337, 365-366, 374-375. Thomas Laxton was the father of the Laxton who appears in Charnley's paper in this issue.

[38] Laxton (1866), quoted in Weldon (1902), p. 251.

[39] Weldon (1902), p. 251, quoting from Laxton (1890), p. 34. This paper is a fascinating survey of the pea-breeding world of the nineteenth century, with remarks on all the varieties discussed in this volume, but also on more general techniques, problems and so on.

[40] In brief, when it came to breeding new varieties, Percival (1902), ch. 23, advised as follows. One could watch out for the appearance of an individual plant that unexpectedly departs in a big way from the old type, then take cuttings from it. One could watch out for several individuals departing from the old in a big way and cross them, doing the same with their offspring while selectively weeding out the rest, until, generations down the line (and provided the departing variations are hereditary), "the new characters become constant in all the offspring, after which the variety is said to be '*fixed*' and 'comes true' from seed" (p. 299, emphasis in original). Or one could use this same process of selection and fixation gradually to improve an existing type – to the point, at the limit, of creating a new type. Then too, and preparatory to any of the above, one could try to induce the usefully type-departing variations, by two means: altering soil and other external condition; and crossing or hybridizing with other types of plants – a technique also useful, Percival noted, for bringing together attractive characters presently not found in a single type.

[41] Bateson (1902), esp. pp. 178-183; also pp. 160-168.

[42] See Radick's paper in this issue.

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[43] On Biffen, see Engledow (1950); Palladino (1993).

[44] Biffen (1904), p. 340.

[45] Biffen (1904), p. 344.

[46] Punnett (1909), pp. 81-82.

[47] Biffen (1922); Biffen & Engledow (1926).

[48] Biffen (1922), p. 45.

[49] Anonymous (1923), p. 734.

[50] Biffen (1924b), p. 512.

[51] Paul & Kimmelman (1988), p. 295.

[52] "I see no escape from the conclusion, drawn by Professor Tschermak from these results, that the power to produce wrinkled seeds is latent in these smooth-seeded races of *P. arvence*, and that it becomes active as a result of crossing. … We must I think adopt Tschermak's view, that one or two allelomorphic characters may remain associated with another, but in a latent or recessive condition, for many successive generations, but that the stimulus afforded by cross-breeding is sufficient in some races to *reverse* these conditions, and to render dominant the character that was previously recessive." UCL, Pearson Papers, 264/1, clip no. 12, quotation on pp. 10-11: Weldon, 11-page MS on Mendel and Galton. See too note 60 below.

[53] We owe this point to Mike Buttolph.

[54] On Burbank, see Palladino (1994), esp. pp. 413-414 for Burbank's Darwin-inspired conception of cross-fertilization as a means for disturbing heredity-environment

equilibrium and so inducing new, valuable variation (a familiar practice in his day among British, but not American, breeders).

[55] Thomas Hughes' writings on technological systems are proving inspirational in developing this point; see Charnley (in press b) and, more generally, Charnley (2011).