Plant Hybridity Before Mendelism: Diversity and Debate in British Botany 1837 - 1899

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Acknowledgements

This thesis was inspired by Max Walter's *Wild and Garden Plants* (1993), a manifesto that the study of nature, horticulture and agriculture inform each other, both in the past and today. Walter's core message for historians of science was to pay attention to intersectional spaces between ways of knowing about plants.

To many botanists, that may seem too obvious to require a mention, never mind an entire Ph.D. thesis. It will resonate, however, for Eric Clement and the late Franklyn Perring. Franklyn first taught me how to identify plants on a residential course at the FSC's Juniper Hall in the mid-1990s. To Eric, I owe the revelation that to be a taxonomic botanist you need to grow plants, as well as observe them in the field; and that 'aliens' are as interesting as rare orchids. Eric really ought to have a Ph.D. of his own by now, for unrivalled contributions to our knowledge of the British non-native flora.

This thesis research was funded by an Arts & Humanities Research Council Studentship Collaborative Award and with funding from the National Institute of Agricultural Botany (NIAB). The project is, in effect, NIAB's back story – where was plant hybridising in Britain situated before 1900? I hope that the story comes as both surprising and encouraging; that agriculture, horticulture and natural history might have a closer future in the celebration of all plant life, whether cultivated or wild.

I have given six years to this thesis, and during the last two years I have worked teaching academic skills four days a week at the Lifelong Learning Centre, University of Leeds. I want to thank my line manager there, Helen Bowman, for her support and for generously allowing me to take unpaid leave in 2020-21. I am also grateful for the support of colleagues on the Chronic Pain & Fatigue Staff and PGR Network, and the staff of the Counselling team at the University of Leeds. I hope this encourages Ph.D. students to acknowledge that the postgraduate process has impacted their mental wellbeing and to seek support: this thesis is the hardest thing that I have ever done, surpassing corporate law, establishing a business, and writing a science textbook.

I am grateful to the staff of herbaria, libraries and archives, listed in the bibliography of this thesis; and to the contributors to online archives, also set out in the bibliography, which proved essential for completing this thesis in a time of pandemic. I would like to acknowledge the amazing PGR community in PRHS at Leeds from 2013-14 to 2019-20; it was fun and you know who you are. But extra special thanks to Becky Bowd and Caz Avery for reading my draft chapters.

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Finally, the thesis would not have been completed without Paul's puns and Tiffy's purrups.

Abstract

This thesis offers a new account of why plant hybridity was controversial in Victorian Britain. Against the received historiography, which depicts a generalised, religiously motivated opposition among botanists to the idea of natural plant hybrids, and treats hybridising—the artificial sexual crossing of species—as a practice used only by horticulturalists and plant breeders, the thesis draws a complex map of interacting but distinctive botanical communities that were all involved in using hybridising to contribute to science. At the centre of the thesis are three episodes focusing on plants—oxlips, willows, and ferns—which employ an object-orientated historiography to highlight for the first time how the transfer of hybridising between these diverse plant knowledge communities eroded earlier attempts to demark socially the science of 'botany' from horticulture and plant breeding. Religion featured in these controversies, but in complex and sometimes surprising ways; in the 1840s, religiously motivated practitioners appealed to hybridisation as a conservative alternative to radical Lamarckian transmutation. After 1859, the botanical practices of a new Darwinian biology included hybridising, which impacted the paper practice of plant taxonomy in relation to hybridity. Local botanist-cultivators used their 'special knowledge' of hybridity to become authoritative taxonomists. Corroboration and collaboration between plant knowledge communities in the closing decades of the century is a corrective to standard histories which emphasise a growing distance between natural history and biologist communities after 1890. Finally, the oft-cited notion of widespread religious hostility to plant hybridity was itself a product of this long history of controversy, originating in the hagiographic historiography launched at the Royal Horticultural Society's 1899 Hybridization Conference. Overall, the thesis reveals a hidden biography of the practice of plant hybridising in Victorian botanical science and how it connected knowledge-making among farmers, gardeners and local botanists with philosophical and academic practitioners, producing diversity and debate.

Table of Contents

Table of Figures

List of Abbreviations

BAAS – British Association for the Advancement of Science

BEC – Botanical Exchange Club

BHL – British Heritage Library

BPS – British Pteridological Society

BSL – Botanical Society of London

CD – Charles Darwin

CUL – Cambridge University Library

DCP – Darwin Correspondence Project

FLS – Fellow of the Linnean Society of London

FRS – Fellow of the Royal Society

Gardeners' Chronicle, the – The various iterations of the title of the *Gardeners' Chronicle and Agricultural Gazette*

NHM – Natural History Museum (formerly the British Museum (Natural History))

OED – Oxford English Dictionary Online

RAS – Royal Agricultural Society

RHS – Royal Horticultural Society

Abbreviations for the Authority for Scientific Plant Names

In the text of the thesis, the scientific plant names given are those currently set out in the *New Flora of the British Isles* (fourth edition).[1] The justification for this approach is that it enables a contemporary reader anywhere in the world to identify the plant being discussed. The authorities for scientific plant names follow the standardised abbreviations set out in the *International Plant Names Index*.[2] For example, in *Primula* L. the abbreviation 'L.' stands for Carl Linnaeus as the botanist who named the genus Primula. The scientific plant names are in italics with only a capital letter for the genus, to conform with modern day conventions. An exception is made for a quotation from a primary source: if this did not use italics, or used a capitalized epithet, then the original source format is retained. Authorities for the nomenclature also follow that cited in the primary source quotation, if any.

[1] Stace 2019.
[2] I.P.N.I. 2020.

Introduction
Understanding Plant Hybridity in Victorian Britain

After nine years he was forced to the reluctant conclusion that he was dealing with hybrids … an awkward propensity in the plant world that most nineteenth-century botanists had determinedly refused to accept.

David Allen, historian of natural history, 2010.[1]

Nobody, in fact, can possibly doubt that wild hybrids exist, are common, and, perhaps, are much more frequent than we think.

John Lindley, taxonomic botanist,
in an editorial in the *Gardeners' Chronicle,* 1844.[2]

During the early twentieth century, hybridising became the defining experimental tool of the emerging science of genetics. Both biologists and historians of science alike have assumed that, before the advent of Mendelism in 1900, hybridising was of interest only to horticulturalists and plant breeders, who were instrumental in bringing this practice to the attention of academic science. Further, historians claim that belief in the existence of plant hybrids in nature, and the practice of hybridising artificially, were controversial in nineteenth century Britain, with opposition largely prompted by religious concerns. Yet, as the quotation from John Lindley above illustrates, some botanists were certain that plant hybrids occurred in nature and promoted the practice of hybridising beyond plant breeding communities. Beginning with evidence of hybridising practice in the 1830s, this thesis explores the complex intellectual, social and cultural topography of Victorian science in which the study of plant hybridity was embedded, demonstrating that diverse botanical communities utilised the practice of hybridising to contribute to the sciences of taxonomy and physiology between 1837 and 1899.

The thesis asks two distinct, but fundamentally interlinked, questions about Victorian knowledge-making about plants: To whom did the study of plant hybridity

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[1] Allen 2010: 349.
[2] Anon. [Lindley, J.] 1844: 443.

and the practice of plant hybridising matter? And what motives underlay those committed to plant hybridity and what motivated their opponents? In answering these questions, the thesis offers two core arguments. Firstly, it claims that diverse interconnected scientific communities used the practice of hybridising for varied purposes including to inform the sciences of taxonomy and physiology. There was no generalised opposition, from botanists or wider publics, to hybridising and to the plant hybrid, as commonly supposed by most historians. Secondly, the thesis offers a new account of why plant hybridity was controversial in Victorian Britain: the diversity and debate around hybridity was more a product of the varied cultural contexts of knowledge-making in nineteenth-century British science, than any conceptual barriers derived from religious beliefs.

Within the first core argument, the thesis corrects three strands within the current historiography of the practices of Victorian botany. Firstly, it shows that plant hybridising was a natural history practice. Secondly, it reveals that the botanical practices of a new Darwinian biology changed plant taxonomic practice. The transfer of hybridising between plant knowledge communities provoked debate and eroded earlier attempts to demark socially ‘botany’ from horticulture and gardening. Thirdly, plant hybridising as experimentation emerged far earlier than historians have believed. It was not especially connected to the re-discovery moment of Gregor Mendel’s pea experiments in 1900.

Turning to the second argument, the thesis corrects the widespread simplistic explanation for the controversial character of plant hybridity. It shows that, while some cultural context of knowledge-making about plant hybrids related to religious belief, this was not necessarily in the way that we might expect, given standard stories polarising religion versus science. Far from opposing hybrids, some religious practitioners embraced hybridisation, preferring this explanation of botanists’ observations to the more radical alternative, the transmutation of species. The thesis shows that another key cultural context was mid-Victorian epistemological morals: for some botanists, making claims about plant hybridisation in nature was too close to ‘unphilosophical’ speculation. However, many cultivators and local botanists were absolutely convinced that hybridising might make new species. Conversely, a Christian-inspired aesthetics led some horticulturalists to reject man-made plant hybrids, yet their stance has been incorrectly interpreted as an objection to artificial hybridising as an impiety. Finally, the thesis argues that the often-cited conflict thesis—that Victorian botanists opposed plant hybridity—arose from the hagiography of hybridity launched at the Royal Horticultural Society's international conference on hybridisation held in 1899.[3]

This introduction outlines the historical framework in which this thesis is situated, including an overview of the sources and methodology. Section one introduces the three overarching communities featured throughout the thesis: philosophical botanists, local botanists, and cultivators, and discusses the historiography of scientific communities. Section two situates the thesis's arguments within the existing historiography of the practices of plant breeding and natural history. The focus here is largely (though not exclusively) on Britain, while acknowledging the wider context of continental European and American histories, where relevant. Section three considers the historiography of Victorian science and religion, in relation to what this thesis claims about attitudes to the practice of hybridising and the plant hybrid in nature, and in relation to the historiography of evolution. Section four provides an overview of the sources and methodology, including consideration of why the thesis includes three central plant-focused chapters on oxlips, willows and ferns. Section five historicises hybridity, discussing the meanings that 'hybrid', 'hybridising' and 'hybridisation' held in the eighteenth and nineteenth centuries and introduces the key terminology used throughout the thesis. Finally, section six provides an overview of the arguments contained in each of the thesis' chapters, and how these come together to deliver the core arguments of the thesis.

Today, wild and cultivated plants are seen as different, even diametrically opposed, biological entities: many conservationists, ecologists and policy-makers consider cultivated plants—still often labelled as 'aliens' when they 'escape' out of the farm or garden—as a potential threat to natural biodiversity.[4] Plant hybrids, and the

[3] This conflict thesis is discussed in chapter five of the thesis. It was persuasively consolidated in Stace 1975 and appears in scholarly accounts, e.g. Elliott 2004, Allen 2010, Meikle 2015, and Preston and Pearman 2015, and in popular garden histories e.g. Martin 2017, Kingsbury 2009, Brown 2004, Shephard 2003, and Campbell-Culver 2001.
[4] Stace and Crawley 2015.

practice of hybridising, are more often associated with agriculture and horticulture than with biology. And 'extinction-by-hybridisation' between cultivated and native plants is a plant conservation concern.[5] Yet in Britain, 908 plant hybrids are listed as occurring in nature, and about half of the world's plant species arose by hybridisation.[6] A theme throughout this thesis resolves this paradox: nineteenth-century plant knowledge communities were intimately entwined. Our contemporary dichotomy between wild and cultivated, natural and artificial, plants was only just developing.[7] Therefore, hybridising as artisanal making-as-knowing is revealed as more important for understanding Victorian science than has been previously acknowledged.

I. Plant Knowledge Communities in Victorian Britain

This thesis draws on three key recent insights from historians about science and communities during the nineteenth century: Firstly, Lorraine Daston and Elizabeth Lunbeck's view that science was collective, even while conducted by heterogenous, unexpected, or dispersed communities over space and time.[8] Secondly, Ruth Barton demonstrates that 'the' scientific community in mid-late nineteenth-century Britain, as 'men of science', existed as a rhetorical grouping obscuring the more diverse actuality of those practising science.[9] Thirdly, the 'Constructing Scientific Communities' project uncovers the complex epistemic topography of nineteenth-century natural history communities. The project shows how the Victorian periodical defined and managed distinctive scientific communities, some of which 'were in tension over what science should look like.'[10] These combined insights encourage historians of science to look to communal ways of knowing.

This thesis uses botanical communities, broadly defined as plant knowledge communities, to address the first thesis question, to whom did the study of plant hybridity matter in Victorian Britain? We have mentioned how there were multiple

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[5] Huxel 1999; Wolf *et al*. 2001; for a more balanced view, see Kohn *et al*. 2019.
[6] Stace and Crawley 2015: 293; Soltis *et al*. 2015.
[7] On 'wild' versus 'domesticated' in the Victorian period see Holmes 2015 and 2017.
[8] Daston and Lunbeck 2011 and for early nineteenth-century botany, Secord 2011a.
[9] Barton 2003.
[10] Dawson and Topham 2020: 15.

overlapping communities engaged in knowledge-making about plants, and that this thesis aims to navigate the intellectual and social landscape that these practitioners engaged in. It is possible to do so by characterising these groupings as botanical communities, even while many practitioners involved would not have recognised each other as 'botanists' at the time. Indeed, taking such a social epistemological approach is only one way of understanding the diversity of Victorian science, but as the Scientific Communities project demonstrates, this is shown to be fruitful in the case of the history of natural history topics, such as astronomy or geology, and can be expected to be so for botany.

As a corrective to the historical presumption that the study of plant hybridity was of interest solely to plant breeders—specifically, to horticulturalists—this thesis shows that, throughout the Victorian period, the following three overarching communities engaged with plant hybridity for different, yet sometimes intersecting, reasons: philosophical botanists, local botanists, and cultivators. [11] This section first identifies the salient features of these botanical communities, featuring their social mix, societies, sites and epistemologies, while acknowledging that they overlapped, with some individuals present in multiple groups.[12] The section then discusses how historians have approached the study of the interactions between these communities.

'Philosophical botanists' have received the most attention from historians. These elite practitioners were concerned with combining natural philosophy with natural history, to reveal causal laws and establish inductive theories or generalisations based on those laws.[13] Their well-established key site of knowledge exchange and dissemination was the Linnean Society of London, originally presided over by physician, botanist and leading advocate of the traditional Linnaean system of classifying plants, Sir James Edward Smith (1757-1828).[14] The Linnean Society was more important for the scientific study of plants during the period of this thesis than

[11] This assumption is made by Olby 2000 and 2000a, possibly drawing on Hurst 1949, who is followed by Elliot 2004.
[12] This historiographical approach is mindful of John Pickstone's 'Ways of Knowing' as a characterisation of the compounds of practices within science (Pickstone 2000 and 2011).
[13] 'Philosophical botanist' is from Anon. [Brewster, D.] 1833: 39. Sloan 2009 provides a more useful recent portrayal of philosophical naturalists than Rehbock 1983.
[14] For biographical details, see Boulger and Walker 2004. 'Linnean Society' is spelt differently to 'Linnaean System' (Endersby 2008: 329 fn. 16).

the Royal Society.[15] The British Association for the Advancement of Science (BAAS), formed in 1831, created a public-facing respectable image for British science, in which, as we will see in chapter one of this thesis, botany underwent a revitalising make-over.[16] The BAAS emphasised a 'reformist ideal' of gentlemanly virtues, practical utility, and religious moderation (Figure 0.1).[17] The philosophical botanists primarily of interest for this thesis were a subset of philosophical naturalists who discussed, debated and/or published on the British flora (for example, John S. Henslow, Hewett C. Watson, Charles C. Babington and Joseph D. Hooker) and therefore were defined by their shared interest geographically.

Figure 0.1: Illustration of the British Association for the Advancement of Science Meeting at Cambridge, 28 June 1845. From: The *London Illustrated News* v.6 165 p.404. Accessed from the Gale News Vault (reproduced under the Creative Commons Attribution License).

Local botanists focused on finding, identifying, and collecting plants, the classic practices of natural history. These communities formed two intersecting societies in 1836, the more high-brow Botanical Society of Edinburgh and the Botanical Society of London (morphing into the Botanical Exchange Club (1856-1947)). Both societies were

[15] Allen 1988.
[16] The seminal study on the BAAS is Morrell and Thackray 1981. For a gender history perspective, see Ellis 2017. On the status of botany in the 1830s, see Endersby 2005.
[17] Bellon 2011: 417 and 2015.

orientated around the exchange of pressed plant specimens and the observation of living plants in what became known increasingly as 'the field', as distinct from 'the museum herbarium' or, after 1870, 'the laboratory'.[18] Many practitioners were physicians or clergymen initially exposed to botany during their university education; although members of the London society were, for the period, 'curious and diverse' and included middle-class women (including George Eliot), shopkeepers, gardeners, and an umbrella repairer. [19] Even at 1899, these local botanists were still sometimes referred to as 'collectors' (Figure 0.2).

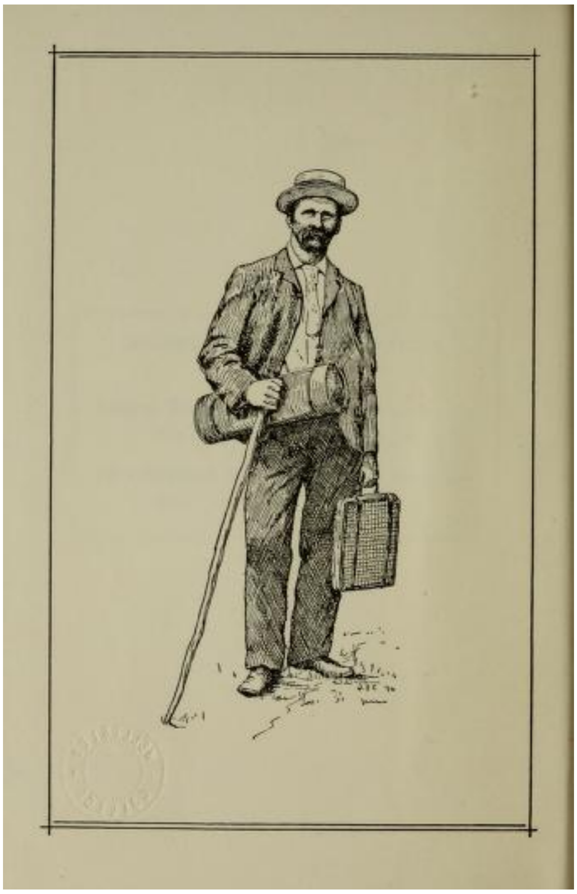

Figure 0.2: 'The Collector': Illustration from a book on botanising, showing a local botanist carrying a cylindrical metal vasculum for plant specimens. From: Bailey 1899: front piece (Author's collection).

Nurserymen, plant traders, farmers and gardeners were often collectively referred to in nineteenth century sources as 'cultivators.'[20] These practitioners understood breeding systems and the effects of climate, topography and soils on plant growth and variation of form, sometimes conducting acclimatization or other physiological experiments.[21] The small but select Horticultural Society of London (later the Royal Horticultural Society) was founded in 1804 by aristocrats together with their head gardeners.[22] At the other end of the social spectrum, 'florists' were artisan plant breeders (not cut-flower sellers in today's sense of floristry), who met in pubs to compete for prizes and enjoy beer with a hearty meal.[23]

[18] Allen 1986. Although there is a need to historicize the 'laboratory' which might include the domestic garden (see Gooday 2008). Another key site for artisans was the pub (Secord 1994 and 1994a).
[19] Allen 1986: 14.
[20] E.g. by Herbert 1837.
[21] Easterby-Smith 2018; Lidwell-Durnin 2019.
[22] Elliott 2004; Fletcher 1969.
[23] On the history of florists' clubs see Duthie 1988; Elliott 2001; and a popular history in Willes 2014.

Increasingly by mid-century, town horticultural societies and shows attracted middle-class gardeners and commercial nurserymen. These communities particularly developed from the 1820s due to the upsurge in gardening periodicals with a social reformist agenda to promote a 'scientific' horticulture, and which gave a voice to artisan and women practitioners otherwise excluded from scientific discussions.[24] The social hierarchy of agricultural societies was similar, the Royal Agricultural Society of England founded in 1838 with the motto 'practice with science', and local societies developing from mid-century.[25] However, in Britain, there was no division between plant 'hybridists' as a coherent community (of philosophical researchers or otherwise), and others, as sometimes portrayed in relation to continental botany.[26]

Figure 0.3: Photograph of gardeners c.1880. From: The Garden Museum, London, collection no. 2014.042 (©The Garden Museum 2021 reproduced with permission for non-commercial research use).

[24] Dawson and Topham 2020; Lustig 1997 and 2000.
[25] See Goddard 1988 and Watson 1939 for broad histories of the society.
[26] E.g.as portrayed by Müller-Wille and Orel 2007; Olby 1985.

How philosophical practitioners transitioned into what became the scientist—a process of professionalization—has been the dominant meta-narrative shaping how we see nineteenth-century science.[27] For botany in Britain, two first-hand histories, Joseph Reynolds Green's *A History of Botany* (1909), and Frederick Orpen Bower's *Sixty Years of Botany in Britain* (1938), presented a narrative of 'science' as an exclusive community increasingly characterised by institutionalisation, laboratory experimentation and specialism.[28] By contrast, Jim Endersby's portrayal of mid-Victorian botany shows that professionalization was neither desired, nor viewed as inevitable, and that gentlemanly behaviours characterised elite scientific communities for much longer than has been assumed.[29]

This thesis takes forward this revisionist historiographical approach to prioritise the intersectional sites or spaces in which botanical communities engaged and demonstrates how these interactions were important for knowledge-making in the study of plant hybridity. These conflicts, corroborations and collaborations provide a richer framework for considering Victorian scientific knowledge-making than a division between amateur and professional. Ruth Barton suggests that, in place of professionalization, we can pay attention to 'scientific achievement' mediated through 'the intertwined themes of hierarchy, class and social status'.[30] This thesis takes Barton's prompt, and asks: achievement in whose eyes? From the perspective of the philosophical botanist, for example, the thesis draws on Richard Bellon's view of the importance to practitioners of epistemological virtues in science, to show that plant hybrids in nature were avoided as too speculative.[31] Alistair Sponsel has recently highlighted Darwin's fear of his long argument falling short of inductive method, by committing the 'sin of speculation', a serious charge of unphilosophical behaviour.[32] Sponsel's focus on speculation in philosophical science is at times over-stated (it seems more likely that the critics reception of *On the Origin of Species* prompted Darwin's

[27] Turner 1978. Allen 1976 and 1998 (later amended in Allen 2009).
[28] This narrative is found in general surveys of nineteenth century science (e.g. Morrell 1990) and science education (e.g. Brock 1990), in reviews of nineteenth-century botany (e.g. Cittadino 2009) and in biographies (e.g. Cock and Forsdyke 2008: xix).
[29] Endersby 2008.
[30] Barton 2018: 24.
[31] Bellon 2015. The canonical text on moral virtues in science is Shapin 2009.
[32] Sponsel 2018.

concerns about speculation, rather than his early geological publications).[33] However, Sponsel's thesis provides support to a claim in this thesis, that plant hybridisation in nature was just too speculative to be acceptable within philosophical science, at least, until the 1860s, when Darwin's botanical studies re-framed respectable knowledge-making practices to include hybridising.

The intersection between local botanists with philosophical botanists has been highlighted by several studies in the 'geographical turn' in the history of science.[34] Philosophical botanists and local practitioners intersected surrounding 'the laws of vegetation', known as 'phytogeography', 'topographical botany', or 'botanico-geography', the belief that there were natural laws which determined and gave 'a peculiar and decided character to the landscape, but have also a powerful influence on the conditions and character of men.'[35] These historians tend to emphasise the autonomy of provincial communities, in the case of Scottish naturalists this also had a strong Highland nationalistic motivation.[36] In general, however, these studies argue that local botanists were often subservient to metropolitan elites, and excluded from science by 1890.[37] This thesis revises these portrayals for botany, as the physiological study of plants did involve local botanists and cultivators. During the 1880s and 1890s, some university biologists sought out the resources of the garden, a point made by Reynolds Green, in a first-hand account that otherwise privileges the professionalization narrative. Therefore this thesis extends arguments made by Sam Alberti, that naturalists' work provided the foundation for an emerging academic biology, while the academics tried to structure the naturalists' observations.[38] This thesis goes further, as in some instances, closer to Anne Secord's more recent studies, local practitioners contributed directly to late-century plant physiology or were

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[33] Endersby 2018a.
[34] E.g. Adelman 2009; Alberti 2000 and 2003; Finnegan 2009; Naylor 2010; Secord 1994, 1996, 1998 and 2002a.
[35] Jones and Kingston 1829: 208.
[36] Finnegan 2009.
[37] Farber 2000 has physiology excluding naturalists. Naylor 2010 has subservient local botanists. Johnson 2016; Holmes 2017a; Wale 2018: 24 (citing Clark 2009: 105-131) each still follow the professionalization trope, especially after 1890.
[38] See Alberti 2000, 2001, 2003, 2005. A similar point is made for German biology by Nyhart 2009.

regarded as taxonomic authorities.[39] In addition, these local practitioners often did so by combining local field botany practices with cultivation practices.

Turning, then, to the interactions between all three botanical communities. This thesis takes a more fluid approach than most studies of Victorian natural history, agriculture or horticulture, which tend to keep to within their respective communities.[40] The thesis reveals that one botanical community, the fernists, combined the interests and practices of the horticulturalist with the local botanist and the hobby gardener, and then also interacted with academic practitioners. The fernists also encompassed a commercial community (nurserymen specialising in native British ferns) contributing to knowledge-making, supporting historians of sciences' recent reappraisal of the role of commerce in nineteenth-century science. Plant breeders used their occupational skill of hybridising in a direct way in their practice of natural history, in the same way that Diarmid Finnegan shows engineers utilised their cartography skills in their hobby practice of geology.[41] In America, by 1870 geologists were able to earn a living using their natural history knowledge as industrial consultants and expert witnesses on patent court cases.[42] In Britain, the inclusion of such commercial practitioners in science was not on the scale experienced in America, in part due to Thomas Henry Huxley's (1825-1895) polemics challenging the autonomy of British industrialists and his vision of biology excluding utilitarian practices such as plant breeding.[43] The British context remained more socially-sensitive to the end of the century, yet at the same time, as has been shown for American late-century plant breeders, the thesis shows that in Britain the growth of university biology did not eliminate from science the experiential knowledge of non-academic practitioners.[44] Instead, the thesis demonstrates how local botanists and cultivators, including commercial nurserymen, negotiated with philosophical and academic communities, who shared an interest in the taxonomy and physiology of plant hybrids and hybridising to 1899.

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[39] Secord 2011a.
[40] E.g. Charnley 2013; Wilkinson 2002.
[41] Finnegan 2009.
[42] Lucier 2009 and 2016.
[43] On Huxley, see Gooday 2012: 549-551.
[44] Pandora 2001.

This shared interest in hybridity among botanical communities might suggest that the plant hybrid functioned as a boundary object, in the sense of Susan Leigh Star and James R. Griesemer's sociological model, as updated by Etienne Wenger's *Communities of Practice: Learning, Meaning and Identity*.[45] Some historians of science see the plant hybrid as a boundary object in the relations between German research institutions and commercial plant breeders 1880-1900. It could be suggested that hybridising might function as a shared boundary practice inhabiting multiple intersecting 'social worlds'.[46] However, as Helen Curry remarks, plant breeding practices were often locally applied in distinct ways without a central project.[47] A shared goal, as envisaged by the sociological model, might be perceived by historians looking back, yet simply was not present at the time. Extending Curry's interpretation back to the Victorian period, we see that unlike Curry's communities, some plant knowledge communities (the fernists in chapter four of this thesis), were corroborating and collaborating with academic communities, but conflict was also present. In addition, *within* each botanical community, as we see in chapters one, two and three, individual practitioners sometimes held conflicting views about plant hybridity. Therefore, this thesis shows that, within the nineteenth century, hybridising was as much a source of debate as a stabilising practice. The thesis contributes towards an improved sociological understanding of how tensions are managed within and between scientific communities; a widespread and adaptable practice like hybridising provides evidence towards the interdependence of scientific communities and a refined version of the Star and Griesemer model.

Having considered how this thesis is situated within one major theme in the history of nineteenth-century science, the historiography surrounding scientific communities and professionalization, we turn next to how historians have often presented plant hybridising narrowly, as either an artisanal practice beyond science, or as connected to a science of heredity. Instead, this thesis shows plant hybridising was a widespread practice within natural history.

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[45] Star and Griesemer 1989; Wenger 1998.
[46] Wieland 2006.
[47] Curry 2010: 262 fn. 11.

II. Hybridising as a Knowledge-making Practice

This thesis is a synthesising praxiographic history, drawing together strands of the historiographies of botanical taxonomy and natural history with those of plant breeding (taken as horticulture and agriculture). The thesis argues that hybridising was not only a cultivators' practice, but also an artisanal form of making-as-knowing, contributing to the sciences of taxonomy and physiology. The thesis therefore recovers hybridising as a fundamental practice of Victorian natural history.

Historians of botanical taxonomy and natural history, including David Allen, Frank Egerton, Peter Stevens, Diarmid Finnegan, Anne Secord and, for America, Robert Kohler, have much to say about fieldwork, but are largely silent on the study of the natural world using any practices of the garden or farm.[48] Jim Endersby, in his biography of Joseph Dalton Hooker (1817-1911), remarks on 'the uncomfortably close relationship between the botanist's and the gardener's skill'.[49] Indeed, Endersby points out that the skills and equipment utilised for gardening were repurposed for botanical study. Yet, Endersby's chapter headings featuring the practices of natural history, like 'collecting' and 'corresponding', do not include 'cultivating'. This exclusion of gardening practices from natural history reflects Hooker's social perspective, as an elite practitioner who, unlike his best friend Charles Darwin, did not conduct hybridising or other cultivation practices.[50]

More recently, historians have begun to diversify the practices of natural history, regarding its practice as 'closely entangled with other enterprises' including agriculture and horticulture. [51] Mark Laird notices a culture among late eighteenth-century aristocratic women of combining botanical knowledge with an aesthetic appreciation of ornamental garden and wild plants.[52] John Lidwell-Durnin sees cultivation as experimentation, connecting natural history practices with food politics (but does not discus plant breeding as a natural history practice).[53] Yvonne Gaspar

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[48] Allen 1976, 1986; Stevens 1997; Egerton 2003; Endersby 2005, 2008; Finnegan 2009; Secord 1994, 1994a, 1996, 2002, 2002a, 2011 and 2011a; Kohler 2013.
[49] Endersby 2008: 62-3 and 82.
[50] As discussed and evidenced in chapter 3 of this thesis.
[51] Jardine and Spary 2018: 3.
[52] Laird 2015.
[53] Lidwell-Durnin 2020.

argues that, while 1820s British nurserymen did not see themselves as erecting new knowledge, some plant breeders produced their own classifications that did not align with philosophical science.[54] Sarah Easterby-Smith disagrees, as her nurserymen-botanists are sources of new knowledge. Their commercial trade in plants, collected overseas and cultivated in Britain, was important for the development of a British science of botany. However, these nurserymen did not breed plants, and Easterby-Smith argues that those that did, would not have considered their man-made productions as within the study of natural history. [55] For Easterby-Smith, this subculture of horticultural interest in botany is wholly distinct from the scientific practices of taxonomy and physiology. Gardening and plant breeding—floriculture—might be useful for understanding plants, but it was socially distinct from the science of botany. This thesis extends these studies into the Victorian period and in doing so, we see that by 1837, these distinctions were far more fluid. Some botanists no longer saw man-made ornamental plants as outside of the Order of Nature.[56] Increasingly, garden-made hybrids and other bred plants becoming established in the wild had to be distinguished from naturally-occurring species and hybrids. Chapter two of the thesis shows how this tension played out in debates over the identity of the 'mock' or common oxlip, which resembled a familiar garden hybrid, the polyanthus, yet grew wild all over Britain.

One way in which the thesis demonstrates how hybridising was important in natural history relates to the practices of a new Darwinian biology emerging in the 1860s. Orthodox historiographies emphasise that Darwin's theory of evolution by natural selection—Darwinism as it became known—did not affect taxonomic practice.[57] More instructive for this thesis is Richard Bellon's focus on Darwin's botanical practice. Darwin was an experimenter and a hybridist.[58] And it was these physiological experimental practices, Bellon argues, which persuaded philosophical

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[54] Gaspar 2017.

[55] Easterby-Smith 2015 and 2018. Elliott 1986: 16 highlights the dearth of historical studies of the Victorian plant breeder contributing to scientific knowledge-making.

[56] A significant gap in the history of plant knowledge-making from a history of science perspective remains between 1815 (when Easterby-Smith ends her book), and 1837, when this thesis commences.

[57] Historians see the immediate effect of Darwinian evolution on existing practical work in science as negligible (Allen 1976: 185-6; Bowler 2009), and as having no impact at all in relation to Joseph Hooker's taxonomic practice (Bellon 2003; Endersby 2008: 327).

[58] On Darwin's practice of hybridising, see O'Reilly 2014.

practitioners to recognise evolution by 1868.[59] Jim Endersby notices that Darwinian practitioners realised that taxonomy might be prescriptive (and not just descriptive). By revealing natural relationships, 'classification could become a powerful tool to understand evolution'.[60] This thesis develops Endersby's comment, together with Bellon's insight, and argues that, after Darwin's botanical physiological practice, plant hybridising became more socially acceptable and was formally acknowledged as an experimental tool to inform botanical taxonomy. For example, in chapter two we see that Darwin's experimental hybridising in the 1860s addressed the puzzle over the mock oxlip and legitimated what gardeners had been saying over twenty years earlier. By 1870, even Hooker's own paper practice shifted to include plant hybrids, where those hybrids had been re-made by experimental hybridising. Hybridising, as we will see in chapter one of this thesis, already had been used within philosophical taxonomy, just for a different end, to reduce the number of species, rather than to recognise hybrids as biological entities in their own right. The inclusion of hybridising within a new Darwinian biology had social implications in Britain for who was able to become a taxonomic authority. Therefore, in contrast to the existing historiography, this thesis argues that Darwinian biology affected botanical taxonomy in two ways: it expanded on the entities regularly included in philosophical paper taxonomies, and diversified whom might practice it.[61]

We turn now to the practice of hybridising within the historiography of plant breeding. There are three trends in how historians approach hybridising: firstly, hybridising in the history of Mendelism and genetics; secondly, hybridising in conceptual and cultural histories of heredity; and thirdly, accounts of the cultural use of plant breeding techniques as knowledge-making in the twentieth century. These first two sets of histories are unhelpful for this thesis, given that they limit hybridising to a practice conducted in Victorian Britain solely by plant breeders, or as of interest to

[59] Bellon 2011.
[60] Endersby 2018: 449.
[61] Peter Stevens insists that Darwinian biology did not affect taxonomic practice in relation to hybrids because botanists did not believe that species could hybridise (Stevens 1997: 359). This is incorrect. He cites the American Professor Asa Gray (1810-88), and the Swiss taxonomist Alphonse Louis Pierre Pyramus de Candolle (1806-93). Gray believed that plant hybrids in nature were rare and generally sterile (Gray 1862: 357) then later discussed examples of natural hybridisation (Gray 1879: 321-2). On Alphonse de Candolle, see chapter three of this thesis.

science only in relation to heredity. We outline these two groups of histories first, before examining how the third, alternative approach, supports that taken in this thesis.

The hybridising of plants appears in presentist, and often hagiographic, historiographies of genetics, with authors seeing a linear progress towards what was known of inheritance at the date of their own writings. The standard, still much-cited account, is Herbert F. Roberts' *Plant Hybridization Before Mendel* (1929).[62] This anachronistic approach continues today in many histories of Darwin's botany, and in broad survey histories of nineteenth-century horticulture, which often see hybridising as significant for science largely to the extent that its practice led to twentieth-century genetics.[63] The intention of this thesis to provide an alternative, more nuanced, account is implicit in its title echoing that of Roberts' book.

Hybridising remains connected to Mendelism in more recent studies. For example, reviewing accounts of late-nineteenth century plant cross-breeding and hybridising in Britain, America, and continental Europe, Staffan Müller-Wille and Marsha Richmond conclude that the rise of Mendelism was 'symptomatic' of the late century rise of experimental systems with potential for industrial applications, including plant breeding.[64] For Britain, Robert Olby portrays hybridising as re-invigorated during 1890s by the horticultural trade, which provided the 'baptismal font' for Mendelism.[65] Phillip Thurtle takes another approach, seeing hybridising primarily as a form of experimental record keeping, developed from the early 1880s by American plant breeders, especially Luther Burbank (1849-1926). Breeders' practices produced large-scale datasets, and therefore enabled early genetics to develop as a statistical science.[66] Jonathan Harwood easily debunks that perspective, given that plant breeders kept systematic records in the eighteenth century.[67]

[62] Roberts 1929 (covering 1763-1900). Still cited regularly by botanists, e.g. Matthews *et al*. 2015.
[63] E.g. on Darwin's botany Thompson 2018, and a scholarly account in Roberts 1919 echoed by Fay, Christenhusz and Chase 2010. For broad presentist histories of horticultural plant breeding, see: Gorer 1970; Kingsbury 2009; and Thoday 2013.
[64] Müller-Wille and Brandt 2016 drawing on Charnley 2011 and 2013; Müller-Wille and Orel 2007; Müller-Wille 2007; Thurtle 2007; Wieland 2006; Bonneuil 2006; Gayon and Zallen 1998; and Palladino 1993 and 1994.
[65] Olby 2000 and 2000a.
[66] Thurtle 2007.
[67] Harwood 2010 and 2015.

Underlying these histories is the implicit view that, from around 1880, plant hybridising was somehow newly 'scientific'. This entailed novel or reinvigorated practices, or at least a new site of practice (a government institutional research station instead of a breeder's nursery), or something else—and as Berris Charnley wryly comments—on which historians cannot agree.[68] Historians of biology have compounded the problem for Britain, traditionally identifying a shift from the laboratory-aided anatomical observation of the 1870s 'new botany' to laboratory-based 'experimentalism' towards the end of the nineteenth century.[69] Yet, chapter one of this thesis demonstrates that, during the 1830s, hybridising might be a form of experimentation for practitioners in certain contexts, and in chapter three we see how hybridising became important in the practices of a new Darwinian biology. In this story, however, the thesis is also mindful of how, from around 1820, the rhetorical discourse around 'experimentation' presented this as a scientific ideal; the label 'experiment' was an appeal for a given observation-and-documentation practice to be seen as within science.[70]

Turning now to how hybridising is presented within cultural histories of heredity, Ohad Parnes argues that, by 1860, experimental hybridising enabled botanists to think genealogically.[71] John Lidwell-Durnin presents a cultural history of the famous graft hybrid *Cytisus adami,* and a study of President of the Horticultural Society of London Thomas Andrew Knight's (1759-1838) plant breeding and beliefs; both of these contributions are primarily in connection to knowledge-making about heredity.[72] Staffan Müller-Wille concludes that 'hybridisation' was 'one of the central concerns of nineteenth-century biology'.[73] However, here hybridisation means any form of sexual (and asexual?) reproduction, and 'theories of generation'. While rich

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[68] Charnley 2011.
[69] Cittadino 2009: 236-7.
[70] Daston and Lunbeck 2011: 3. Historians have recently expanded experimentation to encompass wider practices such as expeditions (e.g. Klemun and Spring 2016). For an overview of the (scant) historiography on nineteenth-century horticultural experiment, see Smith 2016. By contrast, histories of agricultural experiment in the twentieth century are well-developed and reviewed by Parolini 2015 and Harwood 2015a. For the early Victorian period, Lidwell-Durnin 2020 discusses experimentation in potato cultivation by diverse publics, 1795-1848.
[71] Parnes 2007. On the epistemic concept of heredity and its histories, see Müller-Wille and Brandt 2016; Müller-Wille and Rheinburger 2012; and Müller-Wille and Rheinburger 2007.
[72] Lidwell-Durnin 2018 and 2019.
[73] Müller-Wille 2007: 796.

and contextualising, these histories again restrict historians' view of plant hybridity, this time to the history of the concept of heredity.

We have seen how two strands in the historiography on plant breeding emphasise the connection between hybridising, Mendelism and heredity, an approach that continues today.[74] By contrast, the thesis demonstrates that hybridising was a natural history practice that took place throughout the Victorian period, as a tool informing plant taxonomy, and other aspects of physiology beyond heredity. We need to turn to twentieth-century histories of plant breeding to find a more pluralistic historiographic approach, closer to how this thesis sees the history of plant hybridising.

Helen Curry's collection of studies explore the intersections between gardening, industrial-scale plant breeding, and biology. While such a body of twentieth-century studies might seem irrelevant for a thesis about Victorian botany, Curry has several important lessons that are instructive when considering the thesis research question, to whom did the study of plant hybridity and the practice of plant hybridising matter? She does not see any institutional or disciplinary divide between plant breeding and biology, and the latter is diverse, including not only plant physiology and genetics, but also plant taxonomy, pathology, ecology, natural history, and biodiversity conservation. She explores the study and manipulation of plants via intersecting practices such as seed banking, used both by agricultural breeders and nature conservationists. Curry's approach throughout her oeuvre emphasises the interconnectedness of genetics research and wider publics, often local communities interested in how they might make use of its technologies. Curry shows how this interconnectedness is a fruitful space for historians of science and biotechnology to explore.[75]

Helen Curry's approach also sits well with recent historiographical work in relation to material cultures and artisanal epistemologies. The notion that a plant hybrid was re-made by hybridising specifically to re-create the putative hybrid entity observed in wild nature, and therefore to inform taxonomy, can be construed as 'making-as-knowing'. Similarly, the breeder's hybrid plant was also a domestic object

[74] E.g. the ARTEFACT-Hybrid project at the University of Cambridge connecting theories of heredity to the history of hybridising, especially graft hybrids.

[75] E.g. Curry 2012, 2016, 2018 and 2019. For a historiographical review of studies on the relations between agriculture and biology in the twentieth century, see Philips and Kingsland 2015.

in itself, and made using the tools of the garden.[76] Early modern historian Pamela Smith makes an epistemological case for regarding artisanal knowledge that was generalized, replicable and transmissible, as knowledge-production about nature, even where that 'knowing' did not coincide with contemporary knowledge claims from elite practitioners.[77] While this thesis is not a study in material cultures, these perspectives provide important insights to the way in which knowledge-making based on a craft skill like hybridising to produce a domestic object was approached within and between Victorian plant knowledge communities. The differing viewpoints of philosophical botanists who questioned the existence of the plant hybrid in wild nature, and gardeners who were absolutely convinced that their hybridising practice made new species, did not prevent hybridising from being a useful scientific tool. This historiography on artisan epistemologies, and Helen Curry's studies, provide an important lesson for this thesis: to bear in mind that hybridising might be regarded as experimentation at certain times or places, but not in others.[78] That insight leads us to the second research question of this thesis: What motives underlay those committed to plant hybridity and what motivated their opponents?

III. Religion and Science

Religion and social class have long been presented as the warp and weft of the cultural tapestry of Victorian Britain. A standard story is that Christian belief explains Victorian attitudes to biology, both before and after Darwin, due to a timeless conflict between science and religion. However, historians have thoroughly debunked this myth, showing that conflict was never inevitable, and providing examples of where religious practitioners regarded their practice of science as enhancing their faith.[79] Nature and revelation were generally not seen as in opposition; or at least while the actual situation was more complex, in the 1830s the sciences were presented as consistent

[76] Werrett 2019 convincingly situates domestic objects centrally within early modern science, although this thesis qualifies his claim that mid-Victorian men of science's promotion of the laboratory successfully excluded the domestic household from science. On Victorian domestic experimentation, and the nuanced nature of the 'laboratory', which support the approach in this thesis, see Gooday 1991 and 2008a.

[77] Smith 2014.

[78] A point also made for agricultural experiment in Harwood 2015a.

[79] Brooke 1991; Brooke and Cantor 1998; Harrison 2015.

with, and supportive of, orthodox Protestant Christianity and this is an important context for the starting point of this thesis.[80] This aligned relationship between Christian belief and scientific enquiry began to shift mid-century, as Frank Turner's seminal 1978 paper argues, with the so-called professionalization of science, which Turner sees as a political challenge to the Anglican church's domination of Oxbridge over whom had authority to speak about the laws of Nature.[81] James Ungureanu argues that late Victorian Huxleyan-inspired historians who popularized the conflict narrative were attempting to promote religious belief in a liberal theology, by placing conservative Christianity in opposition to scientific progress.[82] This narrative was later appropriated by secularists keen to portray religion as inherently hostile to science. However, these revisionist accounts from historians of science and religion have scarcely impacted views today. Many commentators still insist that science and religion were in conflict, especially after 1859.[83] Therefore, it is perhaps unsurprising that some historians still insist on explaining Victorian attitudes to plant hybridity using a religion versus science conflict narrative.

This thesis shows how this commonplace historiography is incorrect, and goes further to show how this conflict narrative about plant hybrids and religion was produced for a particular reason in a particular time and place. There are two broad historiographies that present plant hybridity within a religion versus science story, that of historian of natural history David Elliston Allen, and from horticultural and garden historians. David Allen has single-handedly created a social history of British botany, enriched, but also coloured by, his own practice as a batologist and local flora author. His oeuvre includes one of the early accounts challenging the nineteenth-century professionalization narrative as in any way desired or inevitable.[84] However, Allen has also created a widely adopted view of Victorian attitudes to plant hybridity. He argues that botanists rejected fertile hybrids, and that sterile hybrids were overlooked or named as species, because of the prevailing belief in a Creator God requiring a

[80] Topham 2022 (forthcoming).
[81] Turner 1978.
[82] Ungureanu 2019.
[83] Hardin, Numbers and Binzley 2018.
[84] Endersby 2004.

commitment to species fixism.[85] Allen describes the 'handicaps' that a leading botanist studying brambles (*Rubus* L.) grappled with: 'That generation began studying brambles, we have to remember, during the era when virtually everyone still subscribed to the idea that species were the handiwork of the Divine Artificer and fixed for all time.' [86] For Allen, a belief in species fixism meant that botanists' passively interpreted observations to fit their surrounding Christian culture. Indeed, for many throughout the period of this thesis, science was not in conflict with their faith; their version of science *assumed* the existence of a Creator God.[87] The trope that all plant hybrids, and the practice of hybridising, were actively opposed as 'impious' or 'sacrilegious' originates elsewhere, with a body of literature produced by horticulturalists and garden historians, which we now examine.

Historians of horticulture and garden history interpret some nineteenth century primary evidence to present plant hybridising as impious and sacrilegious. This is a view presented most extensively by Noel Kingsbury's book *Hybrid* (2009). Kingsbury draws on the American botanist-historian Conway Zirkle's opening comment, that a hybrid was an 'outrage on nature' and the following passage:

> even as late as the eighteenth century, hybridization was not altogether reputable and a number of the early plant breeders felt called upon to justify their attempts at crossing different species. There seems to have been a widespread belief that sexual intercourse between diverse types was an immoral perversion and that the production of new forms of life was an impious affront to the Deity, a tacit criticism of the original work of Creation.[88]

Later Zirkle cites the chapter of Leviticus in the Old Testament of the Bible, which prohibited the Israelites from mixing 'two kinds' of cattle and sowing separate 'kinds' of crops together, interpreting this to mean the Jewish tradition saw hybridisation as a 'form of bestiality'.[89] This interpretation is surprisingly unaware of the standard explanation in the Pauline gospels of the New Testament, and among Christian scholars, that along with the Old Testament's dietary prohibitions, these rules were to protect the Israelites during the dessert Exodus, given that hybrids were usually

[85] Allen 2010: 137 and 349.
[86] Allen 1999:8.
[87] A point made by Desmond and Moore 2009: 53 in relation to the 1830s.
[88] Zirkle 1935: 1.
[89] Zirkle 1935: 10.

sterile.[90] Zirkle was particularly hostile to ideologies (religious and Marxist) which he regarded as hindering positive progress in plant breeding and the possible sexual connotations here seem to have distracted him from investigating further. Although Zirkle limited his comments to the eighteenth century, Kingsbury is persuaded, and apparently expects to uncover such evidence from the nineteenth century:

> As [the practice of] hybridization between species began to spread during the nineteenth century, it appears that there was a rumble of concern about this too, again largely from religious sources. Documentary evidence for this is hard to find, and evidence for it is largely in the form of gardeners defending themselves against accusations of sacrilege.[91]

Kingsbury cites two groups of primary sources in support: Firstly, editorials in the *Gardeners' Chronicle* on the history of hybridising dating from 1880 onwards and papers at the 1899 hybridization conference, discussed in chapter five of this thesis; secondly, *Gardeners' Chronicle* editorials, from 1843 and 1844, discussed in chapter one. In both cases, the evidence has been misconstrued to fit broad assumptions about Victorian religious views, to the exclusion of other potential factors. For example, the 'gardener' defending himself against sacrilege was a nurseryman who passed off artificial plant hybrids as imported rare species, not because hybrids were considered impious (as an 1880s source assumes) but as the imported, natural species would command a much higher price (as an 1840s source reveals).

This is not to say that every Christian accepted plant hybridising as a practice, or the existence of plant hybrids in nature, but the idea of widespread religious opposition to plant hybridity in Victorian Britain is untenable. In fact, in chapter five the thesis argues that the origin of the idea of religious opposition to hybridising began with those *Gardeners' Chronicle* editorials in the 1880s, to promote another agenda altogether. By uncritically accepting the science-religion conflict thesis, historians have curtailed their exploration of the communities interested in plant hybridity. As Ian Hesketh emphasises, by historicizing interpretations of science and religion, we can reveal alternative interpretations that have been overlooked, or even supressed, in the historical record. Similarly, this thesis reveals some of the varied cultural contexts of knowledge-making in nineteenth-century British science which better explain the

[90] Watts 2007.
[91] Kingsbury 2009: 93.

varied attitudes to plant hybridity, such as epistemological morals in philosophical science, horticultural aesthetics, and shifts in the social status of experimental gardening.

In addition, the final chapter of this thesis provides a case study in what Ian Hesketh recently highlights as a central task for historians of science and religion: to trace the history of 'particular interpretations' about the relationship between science and religion.[92] Kingsbury's historiography follows his late Victorian sources uncritically to repeat *their* interpretation of the history of horticulture. The interpretation of religion as hostile to hybridising was prescriptive to serve an agenda in a given time and place, in this case, the rhetoric of the Royal Horticultural Society seeking to retain its place in science during the closing decades of the nineteenth century.

We have seen how this thesis corrects the simplistic explanation for the opposition to plant hybridity in Victorian Britain as due to religious beliefs, whether a belief in species fixism, or that the hybrid was impious, or prohibited by scripture. However, the thesis also shows that one cultural context of knowledge-making about plant hybrids did relate to religious belief, but not in the ways that we might expect.

Chapter two of the thesis demonstrates that far from opposing hybrids, some religious practitioners embraced hybridisation, preferring this explanation of botanists' observations to the more radical alternative, Lamarckian transmutation of species. This is a significant qualification to the relations between religious belief and science during the 1840s, extending the presentation of responses to the publication of the *Vestiges of Creation* in Jim Secord's seminal book history account, *Victorian Sensation* (2000). Historians tend to associate the recognition of hybridity with (rational) non-believers, as in the case of historian Frank Egerton's portrayal of Hewett Cottrell Watson (1804-1881). By contrast, opposition to hybridity is linked with (irrational) believers, largely because today we know hybrids exist and therefore the Victorian actors who got the 'correct' answer must be the rational agnostics.[93] Egerton's portrayal of Watson is refuted in chapter two of the thesis, and Watson's stance on the oxlip hybrid is explained by his display of philosophical caution over the hypothesis of hybridity.

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[92] Hesketh 2019: 191.
[93] Egerton 2003: 152, refuted by chapter two of this thesis.

We turn now to how the thesis offers a new insight into the relationship between plant hybridity and the history of evolutionary theorising. Robert Olby's now-classic *Mendel No Mendelian* (1979) and *The Origins of Mendelism* (1966 revised 1985) argued that Gregor Mendel's (1822-1884) plant cross-breeding and hybridising experiments were conducted to investigate the origin of species by hybridisation.[94] While historians generally agree that Olby went too far, there has been little follow-up by historians of science on Olby's point that hybridisation was important within nineteenth-century botanists' ideas about species formation.[95] This thesis demonstrates that, for some Victorian cultivators, local botanists, and later, also academic botanists, hybridisation in nature was believed to play a role in the origin of species. The thesis claims that, both before and after Darwin, some of those who observed plant hybrids and conducted hybridising were convinced that hybridisation was involved in explaining the origin of species variability, even, in horticulturalist and Anglican vicar William Herbert's case, of species formation.

After 1859, many of these practitioners saw themselves as Darwinian, yet held a version of evolution that was saltationist and therefore contrary to gradual, orthodox Darwinism. This belief, among cultivators who also studied hybrid plants in wild nature, or local botanists who conducted hybridising, is not discussed by historians of evolution. A pro-NeoDarwinian historian, Ernst Mayr, regards species originating by hybridisation as Linnaeus' eighteenth-century idea, which was discredited by 1850. However, Mayr does not distinguish this older Linnaean 'hybrid theory' from later, post-1859 versions of evolutionary theory incorporating hybridisation.[96] Conversely, Peter Bowler correctly states that hybridisation was an alternative account of species origins throughout the nineteenth century, but then says no more.[97] Biologist-historians consider the history of hybridisation in evolutionary studies as only

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[94] Callender 1988 went further, with Mendel working within Linnaean hybrid theory. For recent re-appraisals of Mendel, see Gliboff 2013 and Müller-Wille 2018.
[95] On Olby, pers. comm. Gregory Radick 2018.
[96] Mayr 1982.
[97] Bowler 1983 and 2009. On the history of hybridisation within evolutionary theorising in the twentieth century, see Sapp 2003 and 2009.

commencing after 1917, once scientists understood *how* a plant hybrid could become fertile by polyploidy, and then evolve.[98]

Yet versions of hybrid theory—whether claimed as an element of a heterodox Darwinism or as a stand-alone explanation of what otherwise might appear to be evidence of species transmutation—endured because these were a useful way in which religious practitioners might understand the origin of the diversity of species. Indeed, by 1899, this thesis demonstrates that one of the exciting topics discussed by biologists and plant breeders alike was the view that hybridisation might be far more important in species formation and evolution than had been previously believed. The thesis therefore illuminates an important and novel aspect of the history of relations between religion and science in the history of evolution. Today we see cartoons like this of Noah's Ark (Figure 0.4):

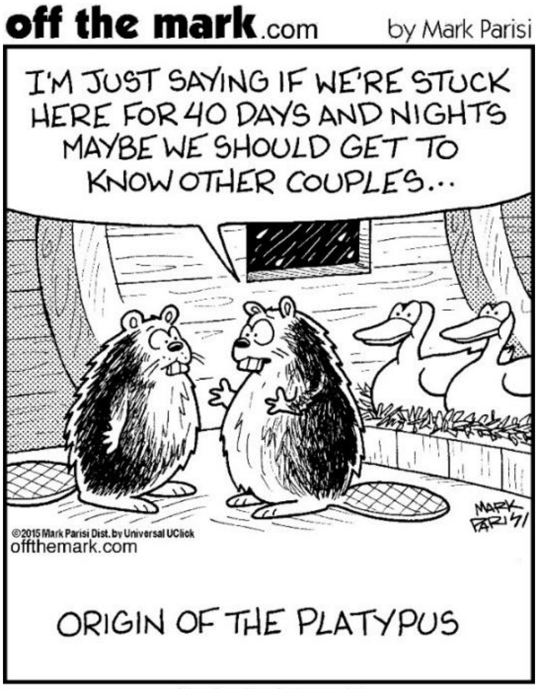

Figure 0.4: Cartoon appearing in the American comic *Off the Mark*, February 2015, titled 'Origin of the Platypus' (©Mark Parisi 2015 with paid permission for non-commercial educational and research use).

The joke in this cartoon only makes sense if we realise that the idea of the formation of species by hybridisation has long history, especially among Christian commentators: as we have seen, hybridisation was used in the 1840s, to counter Lamarckian transmutation and, in the twenty-first century, it is still presented as an alternative to NeoDarwinian evolution on some creationist websites. This cartoon requires its audience to know that the animals are on board Noah's Ark, and it lampoons popular Christian ideas today that hybridisation explains the origin of species (not to mention satirising religious prohibitions against extra-marital relations!).

[98] E.g. Stebbins 1959; Grant 1981; Briggs and Walters 1998; Arnold 2006.

IV. Research Scope, Methodology & Sources

This section considers the reasons for focusing this study on Britain, why the central chapters focus on certain plant groups, and why the thesis centres on the hybridising of ornamental and wild plants, rather than cereal crops, before then discussing the rationale for the sources used.

This thesis comprises a particularly instructive and non-parochial case study. There is an obvious question about what was peculiarly British about how plant hybridity was studied. Contemporary commentators saw the practice of plant hybridising as a distinctly British activity: 'Englishmen have been the most active in hybridising, and the Continental botanists the most acute in detecting wild hybrids.'[99] In Britain, by the 1850s, ways of seeing garden versus wild plants were already ambivalent and nuanced. For example, imported or man-made plants found growing outside of gardens were sometimes incorporated into floras as within 'wild nature' and sometimes excluded.[100] This tension had been formalised in 1847, when a British philosophical botanist introduced the categorising of plants as 'indigenous' or 'alien'.[101] Therefore, there are several ways in which the British experience of plant hybridity differed from that of practitioners elsewhere.

In this thesis, a named plant group provides a central organising thread in the narrative for each central chapter: chapter two looks at oxlips in the 1840s; chapter three on willows and the 1860s; and chapter four on ferns in the 1880s. This object-focused approach conveys several historiographical advantages. Firstly, and most important for this thesis, it enables us to identify botanical communities. Interest in oxlips produced a community cutting across two periodicals, one on gardening and the other on botany. A small but nonetheless distinct group of practitioners (known as salicologists) worked on willow classification and hybridising. One distinctively British botanical community hybridised ferns and founded the British Pteridological Society. Secondly, focusing on the object-as-subject enables the narrative profitably to cut across well-worn paths and sites within the histories of natural history and of late-

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[99] Anon. [Masters, M.T.] 1881: 48.
[100] Walters 1993: 164-66.
[101] Stace and Crawley 2015: 75 citing Watson 1847.

century biology as too often studied separately from the histories of agriculture and horticulture. The result produces a broader, more authentic, history of botanical science better reflecting the interests and lived experience of its actors.[102]

In these plant-focused episodes, the thesis mentions farmers interested in wild plant hybrids, or practising hybridising, but only in one instance (in chapter three) does this encompass hybridising of an agricultural crop (wheat). Agriculturalists looked back to the 1870s as marking the pan-European development of wheat varietal crossing, but not hybridising.[103] In Britain, wheat breeding was little developed by 1885: 'at present the cross-fertilization of cereals, and the subsequent selection of the varieties, has been but slightly attended to, and we must wait for results.'[104] Historian Robert Olby argues that, in Britain, cereal hybridising was ignored, as it only produced sterile progeny, providing no grain to store or sell. Olby states that a chance polyploidy event in Germany in 1882 led to the production of the first fertile cereal hybrid, the wheat-rye cross which then formed the basis of twentieth-century triticale breeding programmes in Europe.[105] In addition to Olby's view, at least some of Britain's agriculturalists retained a traditional belief that wheat did not reproduce sexually (therefore cross-breeding or hybridising was impossible). A well-known horticultural journalist and hybridist, the 'cock-sure' Donald Beaton (1802-63) argued with Charles Darwin over this point.[106] Beaton was convinced that 'no kind of wheat has ever been naturally crossed and never can be.' Beaton's explanation was that wheat selfed 'to prevent famine for lack of wheat', and this was 'one of the most beautiful contrivances in Nature as means to an end, a departure from the law of Nature as it were, to preserve food for man.'[107] Therefore, overall, hybridising as a practice was conducted predominantly among Victorian horticultural and gardening communities, rather than

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[102] E.g. Endersby 2007; Klein and Lefèvre 2007.
[103] Raynbird 1851. Caird 1890. In France Charles Henry Philippe Lévêque de Vilmorin (1843-99) began experimenting with cereal varietal hybridising in 1873 (Vilmorin 1880); in Germany, Wilhelm Rimpau (1843-1903) began crossing of wheat varieties in 1875 (Thiel 1904 cited by Harwood 2000 fn. 17).
[104] Evershed 1884: 91.
[105] Olby 2000a. Marxist historians see this control over farmers underlying why hybridising was popularised by twentieth century agri-industries (Kloppenberg 1988).
[106] C.D. to J.D. Hooker, 14 May [1861], DCP Letter 3149. For biographical details on Beaton, see chapter one of this thesis, p. 75.
[107] Beaton 1861: 113.

by agriculturalists. Having discussed the scope of the thesis, we turn now to the selection of the sources.

The main types of primary source chosen for this thesis include botanical books and periodicals, proceedings of learned and local societies, the emerging genre of the university textbook, and archive materials including private correspondence, and herbarium specimens. To select from these potential sources, the approach taken targeted intersecting spaces between the three botanical communities featured in the thesis.

Given the close relationship between botany, physiology and the developing British nineteenth-century medical curriculum, sources for philosophical discussion of plant hybridity included medical journals and textbooks, as well as the proceedings of the British Association for the Advancement of Science (BAAS). Recovering evidence from an intersectional space between philosophical and future local botanists, led to archives with botany lecture notes for medical students. Finally, for British practitioners, the only nineteenth-century book length philosophical account of plant hybridity was William Herbert's (1778-1847) treatise on the daffodil family *Amarydillaceae* (1837). This text remained an authoritative philosophical account in English (as testified by Charles Darwin) until the publication of American botanist Liberty Hyde Bailey's (1858-1954) *Cross-Breeding and Hybridization* (1892).[108]

Subscribing to periodicals was an essential part of the experience of doing natural history in Victorian Britain.[109] Further, the need to target intersecting spaces where diverse practitioners interested in plant hybridity might communicate, points to the less elite gardening and natural history periodicals with a more diverse readership. The Botanical Society of Edinburgh is another example of an intersecting space, encouraging 'the interchange of botanical information' regarding 'any branch of Botanical knowledge, practical, physiological, or geographical – and the application of such knowledge to Agriculture or the Arts'.[110] Some horticultural and agricultural

[108] Replying to an enquiry about hybridism, Darwin remembered that 'Kölreuter, Gärtner, and Herbert are certainly far the most trustworthy authorities.' (C.D. to George John Romanes, 14 November 1880 at American Philosophical Society, Mss.B.D25.574). The hybridists were German botanists Joseph Gottlieb Kölreuter (1733-1806) and Carl Friedrich von Gärtner (1772-1850).
[109] Dawson and Topham 2020; Wale 2018.
[110] Anon. 1836 cited in Anon. 1875.

books were read widely. For example, William Burbidge's *Cultivated Plants* (1877), the leading manual for gardeners in the final quarter of the nineteenth century, which was also read by botanists at Kew and the British Museum. However, more than any other source, the *Gardeners' Chronicle* (1841-onwards), in its various title iterations, created an imagined scientific community that embodied the intersection between the botanical communities interested in plant hybridity throughout the thesis' period.

The thesis also uses two sources unique to botany: the herbarium specimen and the flora. These two sources are important for this study because they were often produced collaboratively, and then circulated among, and were amended or added to by, a wider community of practitioners. The pressed plant became a biological specimen once labelled with the details of a scientific plant name, who collected it, and with whom, where and when, and who determined the plant's identification; a dried plant on its own, without accompanying details had little or no scientific value.[111] The herbarium collection, an *hortus siccus,* literally a 'dried garden', functioned as a scientific instrument, as its owner used the pressed plants to identify new finds by comparison to those in their collection. Therefore, a private herbarium was an essential part of a Victorian botanist's practice; it was not a mere collection for collecting's sake.[112] It was also a record of the individual practitioners who had collectively observed that plant. Jim Endersby provides an historiographical analysis of the types of information that an herbarium specimen incorporated.[113] He reminds us that biological specimens were not natural objects but instead artefacts produced to set specifications, in the case of his study, those standards of Kew botanists. However, Anne Secord's examination of early nineteenth century bryology shows that the plant specimen might create other botanical communities beyond those controlled by Kew. Specimens pressed between the pages of botanical guidebooks comprised 'textual spaces [creating] a wide and diverse community of botanical observers.'[114] Herbarium specimens also circulated between practitioners, and sometimes carried notes or attached letters providing a valuable source for historians interested in the interactions between practitioners, including those more usually located in distinct

[111] Müller-Wille 2007.
[112] Sunderland 2016.
[113] Endersby 2008: 55-7.
[114] Secord 2011: 284 and 286.

botanical communities. Therefore, herbarium specimens of putative plant hybrids provide a novel way to illuminate the history of the botanical communities studying hybridity.

The observational practice of collecting data on what grows where, the subject of the flora, is known today as making biological records. The flora evolved from the sixteenth century *materia medica*, a basic botanical guide to plant identification and classification, combined with a listing of which plant species were found growing where.[115] By the nineteenth century, a flora listing of the 'native' plants of a country was referred to as a 'general flora' and that of a smaller area, such as a city or county, as a 'partial or local flora'. [116] David Allen traces the historical development of local flora style, content and production during the nineteenth and twentieth centuries.[117] Anne Secord pays close attention to the paper-based practices involved in the use of general floras in the period 1790-1820.[118] By contrast, this thesis uses both local and general floras as a core source for the interaction between plant knowledge communities in Victorian Britain.

The thesis also draws on obituaries and autobiographies, which require careful interpretation. Obituaries provide evidence of the virtues which the practitioner within a scientific community might aspire to, more than evidence of the actual lived detail of individuals, although obituaries are the only source available on some of the local botanists in the thesis. Science autobiography (as distinct from the classic 'life and letters' style biography) was rare in the nineteenth century, but in chapters three and four autobiography provides a source for an experimenter's motivations, which as historian Jutta Schickore notes, is unusual in the history of science.[119] The handling of this type of source is further discussed in the relevant chapters of the thesis. Finally, these core sources are supplemented by newspaper reports, accounts in periodicals like the *London Quarterly Review,* and encyclopaedias, to place the botanical studies

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[115] Allen 2000, 2003 and 2010.
[116] Smith 1824: xviii and xxviii from his preface, and see Allen 2003: 272.
[117] Allen 2003 and 2010.
[118] Secord 2002 and 2011a.
[119] Schickore 2017.

into a wider scientific and social context. Each of these sources poses methodological challenges, which are addressed in the body of the thesis.

Before ending with an overview of the thesis chapters, we next outline the terminology used in the thesis, employed to navigate the diverse interests in plant hybridity found in the nineteenth century. This section also explains why 'hybridising' in this thesis excludes cross-breeding.

V. Historicizing the Hybrid & the Terminology Used in the Thesis

In a 1902, an up-and-coming plant physiologist Vernon Herbert Blackman (1872-1970) deliberately defined 'hybridising' as any sexual cross pollination, whether between species or within a species, and whether conducted by a plant breeder, or occurring without human intervention. The term 'hybrid' was 'incapable of exact definition' because 'it used to be applied only to the result of a cross between different species, but is now more generally used … for the result of a cross between forms sufficiently dissimilar to be considered as belonging to distinct species, races, varieties, etc.'[120] As Blackman realised, the terms 'hybrid', 'hybridising' and 'hybridisation' must be historicized, because these held different meanings before 1900 and beyond his own community of Cambridge plant physiologists.[121] Indeed, in 1899, the title of the RHS' conference on 'hybridization (the crossing breeding of species) and on the cross-breeding of varieties' made the distinction between hybridising and other forms of crossing explicit.[122]

The etymology of the Latin *hibrida* is derived from the Greek concept of hubris which today means arrogant pride, but originally meant 'wanton violence arising from passion or licentiousness.' [123] The Romans applied 'hybrid' to a child born from rape of an alien mother (a foreigner, a freedwoman or a slave) by a Roman father. The Elder Pliny's *Natural History* defined a hybrid more precisely as a 'half wild' cross between a domestic pig and a wild boar and that definition persisted for much of the medieval

[120] Blackman 1902 and 1902a.
[121] See Grubb, Stow and Walters 2004: 16 on Blackman's context in plant physiology.
[122] Wilks 1900: 1.
[123] Potter and Sargent 1973: 175-6 probably drawing on Zirkle 1935.

period.[124] These disturbing cultural meanings co-existed by the mid-eighteenth century alongside the biological terminology developed by the Swedish botanist and founder of nomenclature, Carolus Linnaeus (1707-1778). Linnaeus defined 'hybrid' in both plants and animals as a sexual cross between different species and used hybridising to investigate whether plants had sex in around 1758-9. He noticed a plant growing in his garden that was intermediate in appearance between two *Tragapogon* (goat's-beard) species (Figure 0.5). As he could find no seeds to grow on, he re-made the plant by crossing the putative parent species. Linnaeus concluded 'I doubt whether any experiment demonstrates the generation of plants more certainly than this.'[125]

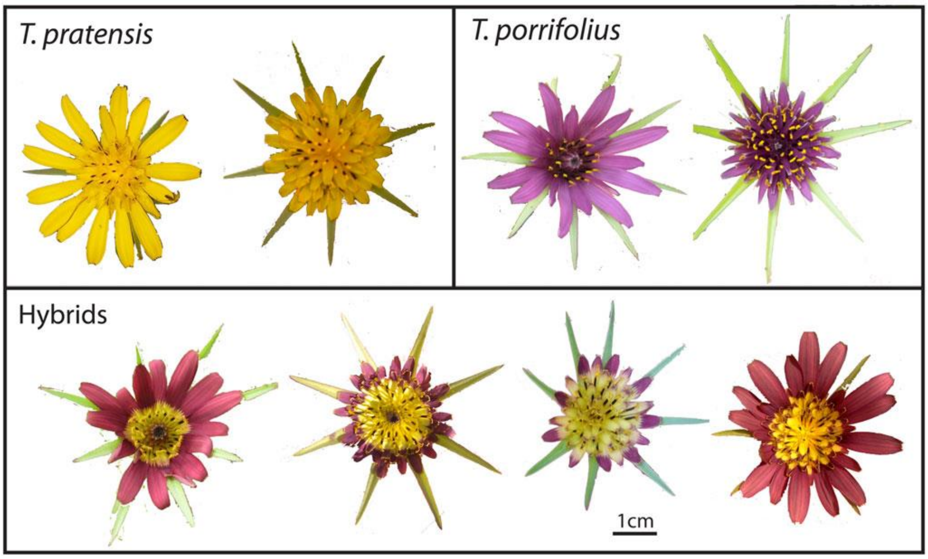

Figure 0.5: Illustration of the range of forms of the *Tragapogon* hybrid between *T. porrifolius* and *T. pratensis,* from a molecular study on speciation in this hybrid which used Linnaeus' cross as a starting point. From: Matthews *et al.* 2015 (reproduced under the Creative Commons Attribution License).

It is easy to see in Figure 0.5 above why cultivators believed that crossing species might make new species. Some of the variable hybrid forms would persist in the next generation of seedlings and look just like something permanent and completely new. Linnaeus believed spontaneous hybridisation between divinely created original forms

[124] Zirkle 1935: 24-5. See also Gienapp 1970 for a descriptive survey of the study of animal hybrids from the Ancient Greeks to Darwin.
[125] Linné 1786: 54 citied by Föcke 1881: 221-222 and Roberts 1929: 22.

produced new species.[126] In fact, this Linnaean 'hybrid theory', as it became known, had a much longer history, for example, appearing in Aristotle's writings.[127]

Eighteenth-century plant breeders also regarded the hybrid as a cross between species, although unlike Linnaeus, their writings betray a view of hybrid plants as an illegitimate union, and outside of the natural order of Creation. For example, in 1788, a high-end Hammersmith nurseryman who also authored a botany textbook, defined 'hybrida' as 'a Bastard, a monstrous Production of two Plants of different Species.'[128] Hybridisation in Nature required not just that plants had sex, but illicit sex. Therefore, the thought of libertine vegetables encouraged some early nineteenth-century philosophical naturalists to restrict plant hybrids to man-made 'curiosities' of the garden and farm, retaining an eighteenth-century treatment of hybrids as monsters and one-off terrata not found in the Creation.[129]

However, by the 1830s, as we will see in chapter one of this thesis, new university botany textbooks and encyclopaedias of gardening alike defined the hybrid as a sexual cross between species, whether produced artificially by animal or plant breeders, or spontaneously in nature, with the most familiar example being the sterile mule (the cross between a horse and a donkey). As Thomas Henry Huxley explained to an audience of working men in 1861: 'There is a great difference between 'Mongrels' which are crosses between distinct races and 'Hybrids' which are crosses between distinct species.'[130] Darwin himself was often less exact, reflecting his conversations with breeders, whose vernacular terminology was not always the same as that used by elite practitioners.[131]

While this section does not aim to provide a history of the different techniques used within plant breeding, mid-Victorian practitioners were clear that cross-breeding was a distinct practice from hybridising, which brought with it specific considerations

[126] Müller-Wille and Orel 2007.
[127] Zirkle 1959.
[128] Lee 1788: 418. One of the early uses of 'bastard' for a plant hybrid in Britain (Zirkle 1935: 64). On James Lee (1715-95) as a leading intermediary between gardeners, plant traders and gentlemen of science, see Easterby-Smith 2018.
[129] Daston and Park 2001; Easterby-Smith 2018.
[130] Huxley 1862: 111.
[131] For example: 'The hybrids or mongrels from between all the breeds of the pigeon are perfectly fertile.' (Darwin 1859: 26).

and concerns. Edinburgh solicitor, Isaac Anderson-Henry (1800-84) learnt hybridising from his gardener between 1836 and 1840 (Figure 0.6).[132] John Lindley in his *Theory and Practice of Horticulture* (1855) turned to Anderson-Henry for an account of 'Anderson's Practice', which he began with the distinction between hybridising (or 'muling') and cross-breeding.[133] Both cross-breeding and hybridising were contrasted with 'accident', in other words, waiting for the spontaneous production of different-looking seedlings, or novel shoots on a plant, or 'sports', which the breeder then selected from.[134] Hybridising was sexual crossing, and therefore also distinct from a form of asexual crossing well-known among those cultivating fruit trees, later known as graft hybridising.[135]

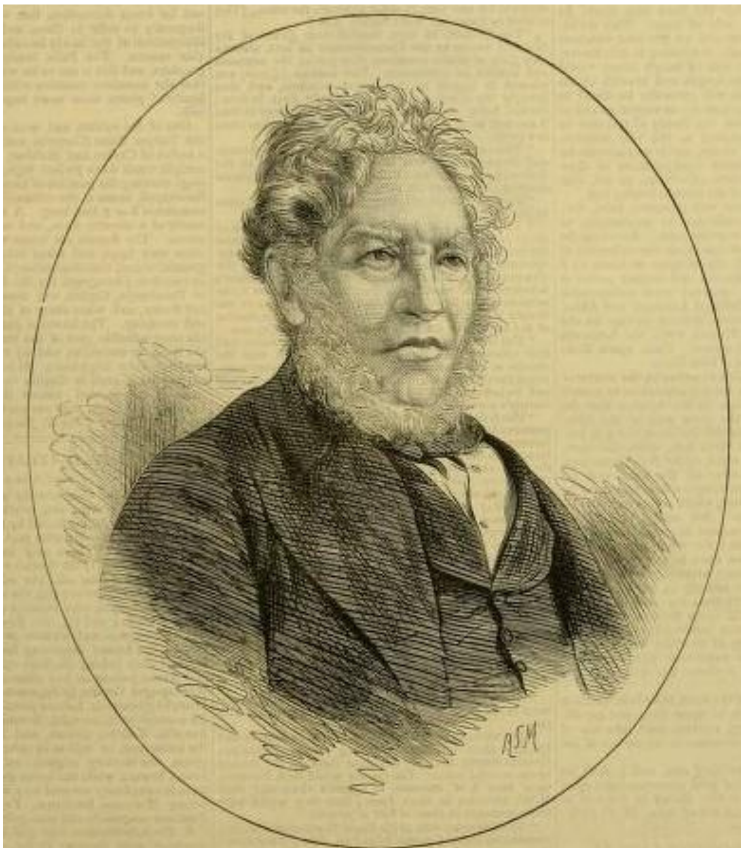

Figure 0.6: Portrait of Isaac Anderson-Henry (1800-84). From: the *Gardeners' Chronicle,* 22 March 1873, p.399. (Reproduced from the BHL Archive under the Creative Commons Attribution License).

The actual technique of cross-breeding or hybridising was simple, Anderson-Henry emphasised, and the equipment list modest: a pocket lens, a pair of wire pincers or camel-hair brush (to transfer the pollen), various coloured silk threads (to mark the two plants crossed) and a note-book for record-keeping.[136] Although cross-breeding involved the same procedure of transferring pollen from one plant to the stigma of

[132] Anon. 1873. Anderson-Henry was President of the Botanical Society of Edinburgh 1867-1868 (Desmond 1994: 16 and a biography in Anon. 1873). Anderson-Henry's account also appeared in Charles McIntosh's *Book of the Garden* (McIntosh 1855: 313-322).
[133] Lindley 1855: 491-94.
[134] Lindley 1855: 491.
[135] On graft hybridising and its history, see Holmes 2017 and Lidwell-Durnin 2018.
[136] McIntosh 1855: 321.

another, Anderson-Henry stressed that there were distinct practices and knowledge required for successful hybridising. The skill lay in judging when the plant parts were suitably receptive, or 'in a fit state'.[137] The weather conditions were crucial, especially for hybridising:

> When the weather is genial, not so much from sun heat as at times occurs from the atmosphere being moderately charged with electricity, when there is an elasticity, so to speak, in the balmy air, and all nature seems joyous and instinct with life, this, of all others, is the season which the hybridist should improve, and above all if he attempt muling.[138]

Anderson later advocated 'violent' crossing, by which he meant hybridising remotely related species, which gave several advantages, including earlier and longer flowering, and more floriferous results.[139] Plant breeding included a range of techniques, and many gardeners crossed by companion planting and allowing insects to cross-pollinate, while florists traditionally only practiced selection. However, by the 1840s, the most common approach, even among florists, was combining crossing with subsequent selection. Horticulturalists, in common with animal breeders, warned against breeding 'in and in' and to guard against contamination from unwanted pollen by emasculating the receptor flower:

> In breeding hybrids, if possible breed many families, and occasionally cross the one with the other; but on no account breed in and in. In this manner, very interesting races may be produced, choosing at all times the most improved and perfect from your seedlings, and rejecting all others afterwards; for the nearer we approach perfection, the more difficult becomes selection, and the greater the danger of retrograding.[140]

Selection was essential and the whole process might take many years (a *Clematis* breeder finally marketed a new hybrid in 1883, after twenty-five years of careful selection!)[141] Above all, Anderson-Henry announced, an 'operator' must have 'indomitable patience, watchfulness and perseverance.'[142] The difficulties of hybridising placed a premium on the knowledge it produced; in Victorian Britain, the

[137] McIntosh 1855: 321.
[138] McIntosh 1855: 321.
[139] Anderson-Henry 1867: 228.
[140] Gordon 1847: 763.
[141] Noble 1888: 152 discussed in chapter five of this thesis.
[142] McIntosh 1855: 320 (cited by Lindley 1855: 490).

care, labour and patience involved in hybridising as making-as-knowing legitimised its practice as scientific.[143]

In summary, we have seen how the study of plant hybrids encompassed a range of botanical interests: the practice of hybridising to make novel plants for pleasure or profit; the naturalist's practices of collecting, identifying and classifying; and theorising about the physiological processes involved in generation, or whether hybrids had a role in the origin of species. This range of interests meant that plant hybrids were simultaneously many things during the nineteenth century: monstrous curiosities, biological entities, anthropomorphic creations, objects of aesthetic appreciation, components of industrial systems, commodities, and objects of manipulation and experiment. To navigate this multiplicity, it is helpful to distinguish the plant hybrid ontologically, as an entity, from the practice of hybridising conducted artificially, and the biological process of hybridisation occurring spontaneously. In this thesis, the following terminology is used:

> 'hybrid' is used in the nineteenth-century sense of a sexual cross between two or more plants at the time recognised as distinct species (and is therefore distinguishable from 'cross-breed' or 'mongrel');
>
> 'hybridising' is the practice of hand pollinating two or more plants of different species, and is distinct from other procedures of plant breeding, with which it may be combined, including selection and/or in-breeding;
>
> 'hybridisation' is sometimes used by historical actors to mean the practice of hybridising, but in the thesis this term is restricted to the biological process of spontaneous species crossing to form a hybrid; and
>
> 'hybridity' is an umbrella term, to mean all or any of the above.

VI. Overview of the Thesis

This thesis approaches the practice of hybridising, and the cultural contexts explaining attitudes to plant hybridity, broadly chronologically. Chapter one, and in part each of

[143] Drawing on, and supporting, the thesis in Bellon 2015.

the subsequent chapters, cover the first thesis question, to whom did the study of plant hybridity and the practice of plant hybridising matter? Chapters two, three and four explore the varied motives underlaying those committed to, and those who opposed, plant hybridity. Chapter five then traces the origin of the conflict historiography about the study of plant hybridity in Victorian Britain. The central plant-focused chapters (on oxlips, willows and ferns) overlap in time and themes, but each can stand alone. The broader argument therefore builds cumulatively and thematically.

The first Chapter focuses around 1837, to establish the variability of the science of 'botany' in the 1830s, in answer to the thesis question, to whom did the study of plant hybridity and the practice of hybridising matter? Despite the views of historians to the contrary, philosophical botanists, local botanists and cultivators all engaged with hybridising as a natural history practice, to inform the science of taxonomy. A tension arose between philosophical practitioners committed to reducing the number of plant species, and, firstly, local botanists finding and growing wild plant hybrids, and secondly, cultivators absolutely convinced that hybridising made new species. These three botanical communities had overlapping yet different views of hybridity, but none held the generalised opposition to plant hybrids commonly supposed by most historians. Indeed, each of the philosophical botanists in the chapter held a distinct approach to plant hybrids and diverse attitudes to the importance of hybridising. However, while the chapter does not aim to explain these differences, one hostile view of hybridity stemmed from a philosophical practitioner's religiously inspired abolitionist politics.

The second Chapter features a debate in the 1840s over the hybrid or 'mock' oxlip in the genus *Primula.* Historians who present the primula puzzle as solved by Charles Darwin in the 1860s obscure the complexity of attitudes to plant hybridity. This Chapter shows that plant hybrids mattered for diverse botanical communities formed as intersecting subscriber communities of two periodicals, the *Phytologist* and the *Gardeners' Chronicle*. Examining the second thesis question, the motives underlaying those committed to the hybrid oxlip, reveals a neglected episode in the history of biology and evolutionary theorising. Some religiously motivated practitioners appealed to hybridisation as a conservative alternative to radical Lamarckian transmutation. However, philosophical practitioners Hewett Watson and Charles Darwin hesitated, and examined the hypothesis of hybridisation carefully, as at stake were their ideas about transmutation.

In the third chapter, the opposition from some philosophical botanists, especially Joseph Hooker, to willow hybrids around the time of the publication of the *Origin of Species* might seem to support the existing historical narrative. Historians suggest that philosophical botanists rejected hybrids due to taxonomic commitments to 'lumping' (versus 'splitting'). Yet this divide between taxonomists did not cut the intellectual territory at its joints. We see how philosophical botanists' views on hybrids related to the epistemological morals expected of a philosophical practitioner. This chapter also considers how Darwin's botanical science affected attitudes to plant hybridity. Several events coalesced during the 1860s raising the profile of plant hybridity, and the social acceptability of hybridising, within the practice of an emerging new Darwinian biology. By 1870 Joseph Hooker had changed his taxonomic practice to include hybrids, while local botanists' 'special knowledge' of hybridity in turn allowed them to become taxonomic authorities, at a time when standard histories portray a widespread decline of local practitioners in science.

The fourth chapter examines one botanical community conducting hybridising and the controversy it provoked during the 1880s. The fernists combined the local botanists' collecting and cultivating of native British fern plants with plant breeding and other research-enabling practices borrowed from commercial nurserymen. Fern hybridising, as making-as-knowing, was a core element of their natural history practice. The fernists presented hybridising as an experiment verifying the existence of fern hybrids in nature, but this was opposed. The evidence suggests that fern hybridising did not satisfy the epistemic requirements for experimental method set by Kew's philosophical botanists. However, the fernists persisted and communicated their studies to biologists, using patronage and a mutual shared interest in ferns among university cytologists. Interactions between cultivators and academic botanists took place in the closing decades of the nineteenth century, a corrective to standard histories presenting a growing divide by 1890 between the communities interested in natural history as distinct from biology; and histories presenting horticulturalists as the only community interested in hybridising around the re-discovery moment of Mendelism.

With the chapter on the fernists, the sequence of episodes in the history of plant hybridising engaged in this thesis concludes. But the thesis itself does not. Throughout this thesis, opposition to plant hybrids, to the practice of hybridising, and to the existence of plant hybridisation in nature has been discussed, but at no point has religious belief emerged as the dominant theme in explaining this hostility. Therefore, the final chapter considers the origin of this religion versus science narrative about plant hybridity in Victorian Britain.

The fifth chapter presents the first historical analysis of the inaugural history of plant hybridity in Britain established at the 1899 RHS hybridization conference by Maxwell Masters and his colleague at Kew, Robert Allen Rolfe. Masters' historiography was part of his on-going rhetorical campaign to present horticultural hybridising as a contribution to scientific knowledge-making, and to counter what he saw as the unjust neglect of horticulture, especially after a debacle between the Royal Horticultural Society and the Royal Society in 1887. He situated hostility to plant hybrids and hybridising within the late-century Huxleyan narrative of a conflict between science and religion. Rolfe followed the approach taken by his colleague, but also had deep-seated personal reasons for amplifying hostility to hybrids in his historiography, and back shadowed his contemporary conflicts across much of the nineteenth century. Yet Rolfe's own account provides evidence to support a core claim of this thesis, that there were diverse views about plant hybridity throughout the Victorian period.

Collectively, these chapters reveal how Victorian practitioners conducting plant hybridising contributed to knowledge-making in plant taxonomy and physiology. Plant hybridising was a natural history practice, and connected knowledge-making among farmers, gardeners and local botanists with philosophical and academic practitioners, producing diversity and debate.

Chapter 1
Taking Stock: Diversity and Debate among British Botanical Communities

I. Introduction

1837 marked the commencement of everything Victorian in Britain. 'Vegetable wonders' made headline news alongside reports of technological shifts in communications—the first electric telegraph—and sobering stories of economic crisis, social unrest and riot.[1] The botanical sensation of the year was an enormous tropical waterlily, leaves six feet in diameter, claimed as an imperial possession by naming after the young Queen; its leaf venation later inspired the architecture of a great symbol of Victorian culture, the Crystal Palace (Figures 1.1 and 1.2).[2] Plants were intrinsically tied to British Imperialism, progress and prosperity: the vegetable world prompted innovation (the lawnmower was first patented in 1830) and entertainment for the swelling middle-classes.[3]

Figure 1.1: The giant Amazonian waterlily, *Victoria regia* Lindl. Coloured lithograph by W. Fitch, c. 1845. From: The Welcome Collection, no. 26758i. (Reproduced under the Creative Commons attribution license).

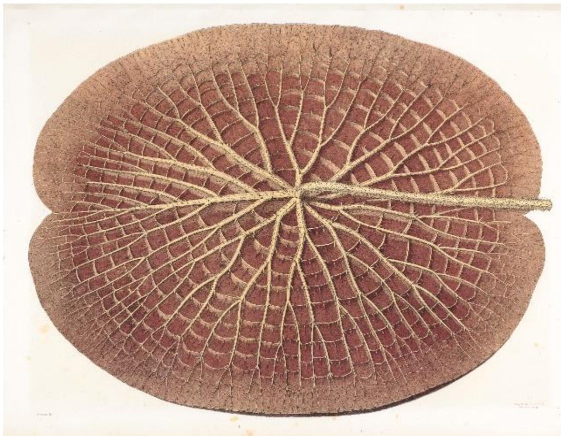

Figure 1.2: *Victoria Regia* Lindl. Underside of leaf showing radiating cantilevers. From: *Victoria Regia*, or *The Great Water Lily of America* by John Fisk Allen, 1854. Reproduced with paid license for non-commercial research use © Royal Botanic Gardens Kew.

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[1] On the 1830s wider context, British reformist politics, American economic crisis, and technological developments, see Secord 2014.
[2] Opitz 2014: 92.
[3] On the imperial symbolism of plants in the 1830s, see Drayton 2000; on the waterlily, Opitz 2014 and with reference to architecture, Nielsen 2010. On the lawnmower, see Elliott 1986: 16.

1837 was also an important year for those with an interest in plant hybridity. Britain's best-known horticultural hybridist, the Honorary and Reverend William Herbert (1778-1847) published a new classification system for the plant kingdom, accompanying his treatise on the amaryllis, daffodil and other bulb plants, *Amaryllidaceae*, and an account of his hybridising practice.[4] And a young Charles Darwin (1809-82) opened his theorising notebooks. Meanwhile, two newly formed botanical societies, in Edinburgh and in London, debated an alleged fern hybrid reported from a botanic garden in Belgium. The *Gardener's Magazine* enthusiastically predicted that the 'art of hybridising' might produce a multitude of novel ferns.[5] By contrast, the Cambridge University botanist, Reverend Professor John Stevens Henslow (1796-1861), cautioned an over-excited Darwin that an experienced gardener had 'never met with anything of the sort.'[6]

These events illustrate a diverse cultural milieu in which the three botanical communities featured throughout this thesis engaged with plants, science and society. Philosophical botanists, local botanists, and cultivators all investigated plant hybridity and practised hybridising during the 1830s. These three communities had overlapping yet different views of plant hybridity, yet none held the generalised opposition to plant hybrids supposed by most historians. Philosophical practitioners sought to revitalise botany with experimental methods to precisely define the species, just as chemists had set out a law of crystallography to circumscribe the species of rocks.[7] Philosophical botanists held diverse perspectives on plant hybridity, but some did hold hybridising as a form of botanical experimentation that might inform science. A tension arose between philosophical practitioners committed to reducing the number of plant species, and, firstly, local botanists reporting wild plant hybrids, and secondly, gardeners absolutely convinced that hybridising made new species. While this chapter does not aim to explain the differences between these botanical communities over

[4] Herbert 1837.
[5] Anon. 1837: 373.
[6] J.S. Henslow to C.D., [c. 14 April 1839], DCP Letter 505.
[7] Henslow 1837.

hybridity, it does suggest that, for some philosophical botanists, at stake underlying these debates over hybridity was their religiously motivated abolitionist politics.[8]

Using previously overlooked material from archives at Cambridge University Library, together with attention to floras and gardening periodicals, two sources still largely unexplored by historians of science, the chapter presents the widespread interest in plant hybridity and the shared practice of hybridising: In section one, we consider how philosophical botanists were interested in hybridity and why they conducted hybridising. Two prominent figures in 1830s BAAS science approached plant hybrids differently: John Henslow, and John Lindley (1799-1865). The section offers a new insight into the inclusion of hybridity within the botany curriculum at Cambridge University. In section two, we see how William Jackson Hooker's (1785-1865) *British Flora* (1830) created a dispersed community of practice, and as the book developed over subsequent editions, he negotiated a tension between the observations of hybrids from local practitioners and the philosophical pressure to reduce the number of species. In section three, we examine the reception of horticultural hybridist William Herbert's book. A gardener-reader of Herbert's treatise claimed hybridising gave gardeners a role in scientific knowledge-making. Another reader of Herbert's book, William B. Carpenter (1813-85), rejected its claims about plant hybrids because of his abolitionist politics. Finally, of wider importance for historians of science, the chapter concludes that the story of hybridising enhances our understanding of the variability of the science of 'botany' in the 1830s and gives new insights into its intimate relations with horticulture, showing how local botanists conducted gardening, cultivators engaged with taxonomy, and philosophical botanists practised hybridising.

[8] As shown by Desmond and Moore 2009.

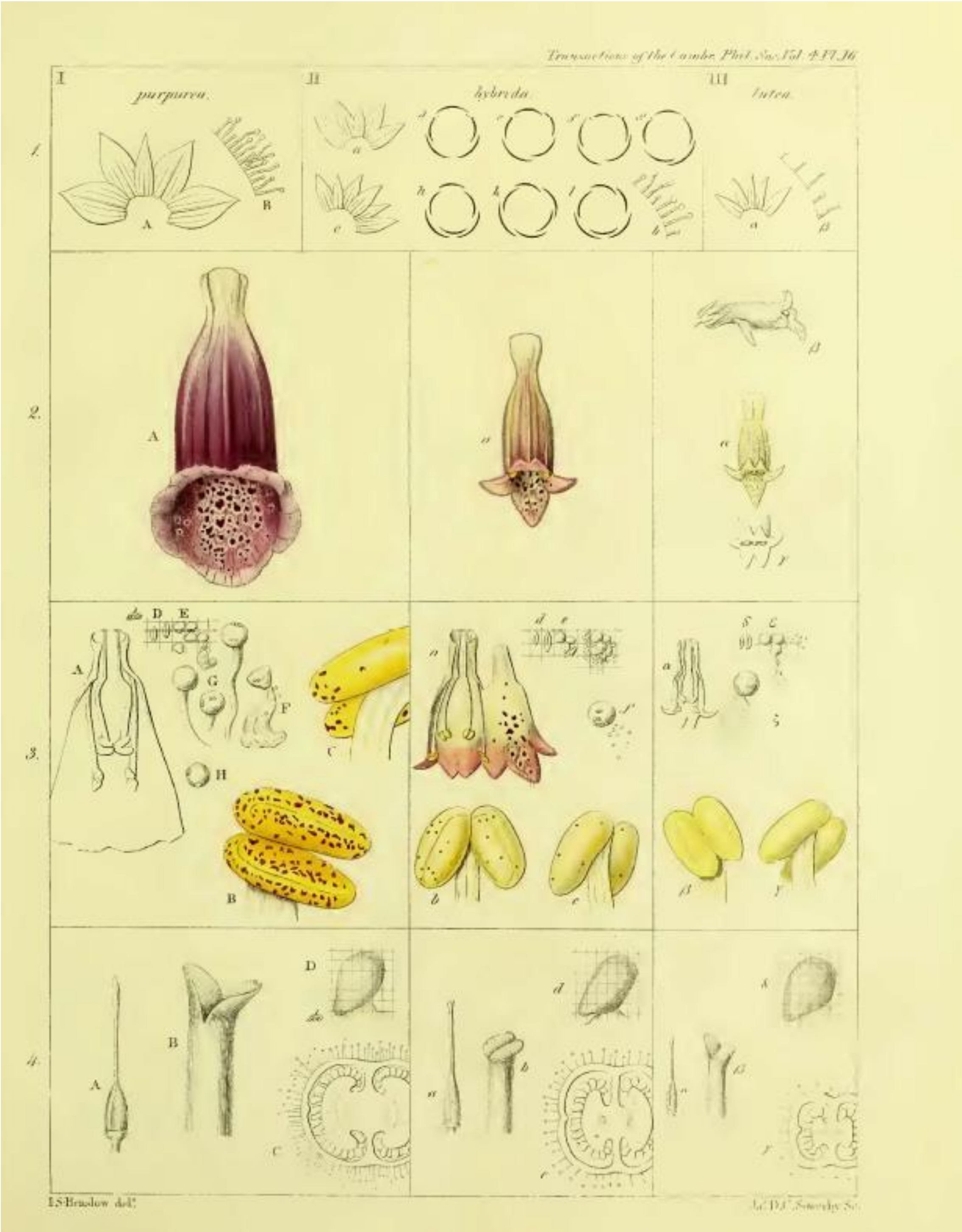

Figure 1.3: Illustrations from John S. Henslow's paper on a hybrid *Digitalis L.* (foxglove). From: Henslow 1831, plate 16, p.27. (Reproduced from the BHL under the Creative Commons Attribution License).

II. Philosophical Botanists Practise Cultivation

Professor Henslow's Hybridising

In 1831, 35-year-old professor of botany, Reverend John Stevens Henslow, planned to accompany his friend Charles Darwin on a trip to the Canaries. [9] Henslow's wife, however, was due to give birth, and he reluctantly stayed at home. Instead of exploring his longed-for Africa, Henslow spent the summer hybridising plants in his garden. In June or July, Henslow noticed an intermediate form of foxglove, with yellow-streaked purplish flowers, growing among his garden-made hybrid plants (Figure 1.3). The foxglove was 'a decided hybrid' as it had 'most of its characters exactly intermediate', observed by his 'rigorous examination' with 34 daily observations using a microscope.[10] Henslow's foxglove study attempted to elucidate the cause of hybrid sterility, in particular, which organs of a hybrid plant were defective and how, or whether an external cause was involved. He speculated that pollen size may determine which species might cross to produce hybrids but could add nothing definite.[11]

Henslow had been hybridising in his garden for about two years 'on a considerable scale'.[12] The most recent edition of John Claudius Loudon's (1783-1843) *Encyclopaedia of Gardening* (1827) discussed 'vegetable crossing'. This account represented an earlier view of hybridity, focused on 'the practicability of improving the species', especially directed by the-then President of the Horticultural Society of London Thomas Andrew Knight's (1759-1838) experimentation with fruit trees and his belief that these varieties might degenerate without a crossing programme.[13] Hybridising was, up until around 1830, the *Encyclopaedia* demonstrates, for

[9] Henslow's biographies reflect what historians felt was important for their science at the time, so initially Henslow was primarily an educationalist (Jenyns 1862); dismissed as a systematist during the 1970s growth of plant physiology (Morton 1981: 442); and after the development of molecular phylogenetics, as directing the evolutionary thought of Charles Darwin (Walters and Stow 2001).
[10] Henslow 1833: 257-58 [read 14 November 1831]. Walters and Stow stress that Henslow's hybrid studies 'foreshadowed' twentieth century experimental taxonomy (Walters and Stow 2001: 163-169).
[11] Henslow 1833: 257 and 275-6.
[12] Walters and Stow 2001: 165.
[13] On Knight, see Lidwell-Durnin 2019.

'amusement or improvement', and not considered in relation to the study of plant physiology or taxonomy. [14]

British botanists at this time were concerned to improve the status of their science. Compared to astronomy, physics and chemistry, natural history was particularly dismissed as trivial collecting; while botany lacked the exciting controversies that geology provoked, and worse, was associated with women. A sustained campaign in the 1830s to promote botany as a philosophical science suggests the actual low status of this subject.[15] The way forward for British botany, Henslow believed, was through physiological studies, a science 'still so far in its infancy' in Britain.[16] His colleague in the British Association for the Advancement of Science (BAAS) and administrator at the Horticultural Society of London, John Lindley, called for a 'philosophical' science combining physiology, anatomy and morphology to order plants according to their natural relationships, the 'Natural System' devised by the French botanist Antoine-Laurent de Jussieu (1748-1836). Both men also emphasised that the importance of botany lay in its utility.[17] By the 1830s, Lindley was 'one of the pillars of botany' and *the* public face of 'oeconomic botany' in which the laws of physiology were put into practice in the arts of medicine, horticulture and agriculture.[18] As we will see, both men acknowledged the importance of the practice of hybridising, but in distinct ways, and had different approaches to the plant hybrid in nature.

The following year after his foxglove hybridising, in 1832, Henslow reviewed Augustin-Pyramus De Candolle's (1778-1841) *Physiologie Végétale,* the first complete treatise on vegetable physiology, produced by one of Europe's leading natural philosophers.[19] 'One enquiry of the vegetable physiologist', Henslow explained in his review, was 'to ascertain the limits within which the characters of a given species may

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[14] Loudon 1827: 182-3.
[15] Endersby 2008: 34-42 and 211 on geology.
[16] Anon. [Henslow, J.S.] 1833a: 334.
[17] Lindley 1833. The most astute source on Lindley is Drayton 2000 and 2009. 'Unsung hero' style biographies portray Lindley ahead of his day in taxonomy (Stearn 1999) and paleobotany (Chaloner and Pearson 2005).
[18] Watson 1837: xxi on Lindley. On 'oeconomy', see Roberts 2014.
[19] Anon. [Henslow, J.S.] 1833a. Attributed to Henslow in 1832 in Walters and Stow 2001: 311.

vary'. He continued: Botanists have hitherto sadly neglected the only sure means of bringing this question to a satisfactory issue, namely, the test of careful experiment.'[20] Henslow explained that a vegetable physiologist might study the range of variability within the species. Henslow pointed out that there were several theories surrounding how 'various modifications' of a species—he listed as varieties, races, variations, deformations, monstrosities, and hybrids—were formed.[21] Some of these modifications arose from external causes alone, climatic conditions or soils, others were 'connected with the fecundating process' of flowering and fruiting. However, Henslow was clear that 'the production of hybrids, also, and of such varieties as constitute distinct races, never introduces any entirely new form, but merely modifies those which are already in existence.' [22]

Henslow's perspective on hybrids contrasted to the view of many horticulturalists, who believed that their hybridising made permanent forms, often called 'hybrid species'. This was the view of one of the most prominent nurserymen-hybridists Robert Sweet (1783-1835). [23] As Loudon's *Encyclopaedia* noted, this was a widespread belief among horticulturalists and nurserymen, based on their hybridising practice: 'Salisbury is of a different opinion [to that of Knight], and considers that new species may be created both by bees and the agency of man; and the recent experiments of Herbert, Sweet, and others seem to confirm this opinion.'[24]

While Henslow disagreed about the nature of the forms that Sweet and other nurserymen made, in 1832, while he was reading De Candolle's new textbook, he was hybridising flowers. He attempted to show that two pimpernel plants named as separate species, widespread weeds of cultivation, ought to be regarded as a single species. If the scarlet pimpernel (*Anagallis arvensis* L.) crossed with the blue pimpernel (then called *A. caerulea* L.) produced a fertile hybrid then, Henslow concluded, these must have arisen from the same original stock and therefore could be regarded as the

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[20] Anon. [Henslow, J.S.] 1833a: 359.
[21] De Candolle believed hybrids should be given a species epithet (De Candolle 1832: 720-21 and Oghina-Pavie 2015: 61).
[22] Anon. [Henslow, J.S.] 1833a: 361.
[23] Sweet 1824: folio 241. Sweet worked as a gardener from the age of sixteen. In 1812 he joined Colvills, a famous Chelsea nursery, and was elected a fellow of the Linnaean Society. By 1818 he was publishing horticultural and botanical works (Woodward and Goldbloom 2010).
[24] Loudon 1827: 183 and 1102 on hybrid species.

same species (despite the two plants having such strikingly different flower colours). A sterile hybrid (or no hybrid resulting at all from the attempted cross) might be interpreted as meaning that the parent plants were distinct species. He experienced limited success in that regard, as it was 'so very difficult to avoid the introduction of error', by, for example, sowing the wrong seeds, or foreign pollen contaminating an 'experiment'.[25]

Henslow emphasised two things: Firstly, that the philosophical taxonomist must be guided by 'the multiplied results of direct and accurate experiment' and he used the term 'experiment' referring to his own hand pollination during hybridising Primulas. Secondly, a need to reduce the number of species: the 'closer botanists observed', 'the more inclined we seem to be to multiply species.' [26] In combining these two practices, Henslow distinguished himself from an older generation of British botanists. He also arranged his herbarium specimens in a different format to other British botanists, which he called 'collation': displaying a range of variation between two extreme forms on a single herbarium sheet. Therefore, his herbarium practices also reflected his commitment to reduce two species to a single species by providing evidence of linking in-between forms. [27] Even a respected taxonomist might be reprimanded as unphilosophical for naming new species too readily (Lindley was scolded by the President of the Botanical Society of Edinburgh for flouting his own rules).[28]

In April 1833, a prominent attack on 'mere descriptive' botany appeared in the highbrow *Edinburgh Philosophical Review*.[29] The reviewer of the life and letters volume produced for the late Sir James E. Smith presented a double-edged portrayal of the life's work of Britain's greatest botanist.[30] This wider context must have abruptly served as a reminder to Henslow that traditional descriptive botany fell short of philosophical practice, at least in the eyes of many intellectuals. Therefore, reading De Candolle's textbook, alongside the first issue of Lindley's *Introduction to Botany* (1832),

[25] Henslow 1832: 493.
[26] Henslow 1830: 406.
[27] Kohn *et al.* 2005 but they do not ask why Henslow did so.
[28] Graham 1838: 35.
[29] Anon. [Brewster, D.] 1833.
[30] Endersby 2008: 308-9.

while also conducting his own garden hybridising experiments, Henslow decided to amend his botany curriculum at Cambridge University.

Henslow had devised his first syllabus in botany in 1828 based on Smith's *Introduction to Physiological and Systematical Botany* sixth edition (1827).[31] Smith's textbook omitted mules or hybrids in the taxonomic section and hybridisation in the physiological part. By contrast, Lindley's new textbook included a dedicated chapter on hybridisation. Henslow produced a *Sketch of a Course of Lectures on Botany,* replacing Smith's textbook with Lindley's and De Candolle's, and including more physiology. He demonstrated on British plant 'organography, phytography, and taxonomy on Tuesdays and Thursdays; and physiology on Mondays, Wednesdays and Fridays', with a separate session on botanical geography. In physiology lectures on 'Vegetable Functions', Henslow taught on 'reproduction' (5 periods), comprising:

(1.) flowering (its periodicity)
(2.) fertilization (of ovule)
Hybrids
(3.) maturation (of fruit and seeds)
(4.) dissemination (of seeds)
(5.) germination (of embryo)
Propagation by subdivision

Various modes of reproduction.[32]

Henslow taught about hybrids and gardening propagation techniques as part of plant physiology, directly drawing on Lindley's textbook (with, as we will see, its chapter on hybridisation in nature and the use of hybridising for improvement).[33] This amendment to his teaching, combined with his own garden hybridising, suggests how important Henslow believed that hybridising was, as an observation investigating the physiological limit of the species. He later made this point in his first botany textbook:

[31] Walters and Stow 2001: 57-62. J.S. Henslow, *Syllabus of a Course of Botanical Lectures 1828* (CUL Special Collections JSH Archive CCC.47.181).
[32] J.S. Henslow, *Sketch of a Course of Lectures on Botany for 1833* p.6-7 (CUL Special Collections JSH Archive CCD.47.386).
[33] Walters and Stow regard Henslow as heavily influenced by De Candolle's textbook, but only mention Lindley's textbook in passing (p.65) and not Henslow's addition of hybrids in 1833.

While descriptive botany dealt with naming and arranging ‘pieces of machinery’, he explained, physiology considered ‘these machines, as it were, in action.’ Physiological botany ‘possesses a more general interest, owing to the numerous and striking phenomena, of practical and economical importance, which it enables us to explain.’[34] Plant physiology was therefore also likely to appeal to Henslow’s students. The attendees at his lectures were predominately medical students, along with some reading for a theology degree (most famously, of course, Charles Darwin). Like other natural history topics, botany remained an extracurricular option and there were no science degrees.[35] The aim of teaching physicians plant classification was not, as is often assumed, so that medical men might identify herbal plants; by the 1830s, that was the task of a lowly apothecary. Instead, taxonomic botany was believed to develop rational observation, and the skill of ‘diagnosis’. To identify a plant species, or an illness, a practitioner made careful observations and then followed a process of ruling out various possibilities according to the combination of, either the characters of the plant, or the symptoms of a patient:

> Affinities of plants … depend on more or less intricate combinations [of characters], the power of judging of which is the same test of a skilful botanist, as the appreciation of symptoms is that of a physician.[36]

Further, by the mid-nineteenth century, diagnostic procedures in both medicine and botany also increasingly required the use of a microscope. Botany, therefore, developed vital skills for the task of medical diagnosis. Or at least, that was the argument that Lindley repeatedly advocated at this time.[37]

Plant hybridity and hybrids remained in Henslow’s published syllabus of the new Science Tripos in 1853 and in his handwritten lecture notes on botany delivered in 1860, the year before he died.[38] Therefore, Henslow taught about hybridity and propagation (including hybridising) as an integral part of physiological botany for over thirty years. Nonetheless Henslow, still presented a conservative view of plant hybrids

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[34] Henslow 1835a: 2-3.
[35] On botany education in the early nineteenth century, see Secord 2011 and Allen 2000.
[36] Lindley 1830a: iv cited by Stevens 1994: 123.
[37] E.g. In his address at the commencement of the Medical Session, 1834-5, University of London (Lindley 1834).
[38] CUL Special Collections JSH Archive MS.Add.8178: 1 envelope.

in nature in his first botany textbook, *Principles of Descriptive and Physiological Botany* (1835). In the next subsection, we compare Henslow's approach to plant hybridisation to that of his BAAS colleague, John Lindley.

Lindley and Henslow's Textbooks

In 1835, alongside Henslow's first foray into the new genre of the botany textbook, John Lindley brought out the second edition of his *An Introduction to Botany*.[39] Lindley was well known as a botany lecturer at University College, London, arriving for morning lectures

> a fresh, ruddy, hale-looking man, and after his morning's ride, in the midst of large bundles of fresh-plucked plants, presents an appearance entirely different from that of the "pale students", who have generally been but a few minutes before roused from their too scanty slumbers.[40]

Figure 1.4: Portrait of John Lindley as a young man. From: The *Naturalist,* 1839 (Anon. 1839: front piece). (Reproduced from the BHL Archive under the Creative Commons Attribution License).

Lindley probably lectured about hybridisation; he certainly covered it in detail in an expanded chapter in his textbook. Lindley began by presenting plant hybridisation in relation to the known position in animals, remarking that 'the power of hybridising appears to be far more common in plants than in animals.' [41] Manmade plant hybrids were not always sterile and could be fertile for up to four generations. A sterile hybrid could become fertile by crossing with the pollen from either of its parent species. Lindley wrote acutely aware of political importance of hybridising. Food and its supply was one of the most pressing political issues at the time, a fear of failure of supply and what this might mean (riots, even revolution) provoked much debate.[42] Hybridising was a potentially powerful and far-reaching tool to improve crops: 'there is scarcely a genus of

[39] Henslow 1835a; Lindley 1835.
[40] Anon. 1839: 442.
[41] Lindley 1835: 286.
[42] Cunningham 2001: 15.

domesticated plants in which this effect cannot be produced by the assistance of man' but in general between only 'nearly allied species' within the same genus such as strawberry species, or nurseryman Robert Sweet's famed Pelargoniums.[43] He considered the 'few well attested instances' of wild plant hybrids and added: 'It is difficult not to believe that a great number of the reputed species of *Salix, Rosa, Rubus*, [willows, roses, brambles] and other intricate genera, have also had a hybrid origin; but I am not aware that there is at present any positive proof of this.'[44]

Lindley dealt with these issues in 'my special study', classifying the Michaelmas daises (*Aster* L.), a genus of straggly, dull purple flowers native to North America.[45] In 1830 he suggested that he could help out with producing a section on *Aster* for De Candolle's unfinished Latin taxonomic tome, known as the *Prodromus,* claiming that he had 'more Asters perhaps than any other person.'[46] *Aster* was proving particularly fiendish to sort out. To an unpractised eye, the specimens all appeared either all the same, or all completely different. By 1836, De Candolle's latest newly published volume of the *Prodromus* spurred Lindley to take up the genus again: the book 'has set me a-Compostizing' and he asked De Candolle for his specimens back from six years previously as 'the study of Compositae has now <u>bitten</u> me completely.'[47] Revisiting some North American specimens, Lindley concluded: 'I hardly doubt that very many of the reputed species are wild hybrids.'[48] Lindley was happy to recognise intermediate forms as hybrids, and in order to resolve classifying a difficult genus.

By contrast to Lindley's approach to hybridity, in his textbook, Henslow more closely followed the example set by zoologists. Henslow acknowledged that hybrids were 'readily produced by art' but that the plant hybrid was 'manifestly analogous to mules in the animal kingdom'. Hybrid plants in wild nature were all sterile, and very rare (under fifty were then known, the same point that Lindley cited to suggest wild plant hybrids might be more numerous). Henslow argued that flowers normally self-

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[43] Lindley 1835: 301-2.
[44] Lindley 1835: 304.
[45] John Lindley to A.P. De Candolle, 26 November 1830 (Hamilton 1998: 5).
[46] J. Lindley to A.P. De Candolle, 26 November 1830 (Hamilton 1998: 5).
[47] J. Lindley to W. Hooker, 4 November 1836 (Hamilton 1998: 43); J. Lindley to A.P. De Candolle, 4 October 1836 (Hamilton 1998: 43).
[48] J. Lindley to John Torrey, 26 October 1836 (Hamilton 1997: 31).

pollinated, therefore the chance of another species being involved in pollination was remote. Further, hybrids did not persist in nature: although a hybrid might backcross with one of its parent species, the progeny would then revert to one of the parental forms, and a hybrid of 'the second degree' was always sterile; further, fertile hybrids were restricted to, and a product of, the conditions of cultivation.[49]

Yet behind the reassuringly straight-forward account in his textbook, Henslow was intrigued by hybrids. In 1833, Henslow had published the description of a hybrid he had made a couple of summers before between *Potentilla* species. He concluded that: 'Notwithstanding the theoretical position taken by some botanists, we believe, doubtlessly, that hybrid plants sometimes become established, and hold a permanent place in the vegetable kingdom.'[50] He was interested by hybridity in animals too, also publishing an 1834 paper on pheasant hybrids and a dove hybrid, bred by his father, who presented the stuffed birds to the Cambridge Philosophical Society.[51]

How do we explain Henslow's apparently inconsistent approach to hybrids? One factor is that Henslow addressed distinct audiences in different ways: While the Cambridge Philosophical Society and the *Magazine of Natural History* heard about his pimpernel hybrids, as an example of how hybridising might potentially reduce the number of recognised species, Henslow only presented his *Potentilla* hybrid to a gardening audience in a 'humble' magazine whose editor had nagged him for contributions.[52] An unphilosophical gardener's paper might include a report on fertile hybrids occurring spontaneously in the garden. Whereas in a textbook aimed at the 'educated general reader', Henslow insisted that spontaneous hybrids were temporary forms and always sterile. [53] While the university textbook was still developing into what would become a central mode of transmitting the core conservative values of science, Henslow must have decided to produce an inherently moderate text.[54] He did hint at an explanation. In his textbook, despite his tentative claims elsewhere, he

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[49] Henslow 1835a: 287-88.
[50] Henslow 1834a: folio 385 [unpaginated].
[51] Henslow 1834.
[52] J. S Henslow to W. Hooker, 17 May 1837, Kew Archives: Director's Correspondence 9: folio 128. The editor was pharmacist and bookseller Benjamin Maund (1790–1863).
[53] The quote was from a review of Henslow's textbook (Anon. [Carpenter, W.B.] 1837: 3).
[54] Brock 1990.

dismissed outright *fertile* hybrids: these might never spontaneously occur in nature, because otherwise 'it would seem to be impossible for us to draw any distinction between true species and hybrids.'[55] Henslow voiced an underlying fear that the order of Creation might descend into chaos if fertile hybrids might persist outside of the conditions of cultivation. Nonetheless, Henslow encouraged gardeners and horticulturalists to consider hybridising, to provide experimental evidence to reduce the number of recognised plant species.

A Community of Hybridists

In 1837, Henslow took another opportunity to encourage practitioners to conduct hybridising experiments on the launch of a new periodical, the *Magazine of Zoology and Botany.*[56] He authored the key-note article in the first volume of the journal, and opened the piece by pointing to contemporary concerns about the decline of British science. While there were individual British botanists of note, the country lacked a scientific community of local practitioners, which he attributed to the dominance of horticultural periodicals over more 'strictly scientific' publications. British botanical journals stood at a 'low ebb' and most subscribers were horticulturalists therefore the editors 'must sacrifice the character of strictly scientific publications to suit the taste of their horticultural readers.'[57]

However, botanists should turn this situation to their science's advantage. Britain might become the leading nation in botanical science if every horticulturalist ceased 'loitering about the threshold of science' and instead 'increased their stock of intellectual enjoyment by conducting his pursuits in connection with scientific enquiry.' There were 'plenty of willing workmen' to observe individual specimens in nature (local botanists) but a 'want of scientific experimenters.' Henslow had been hosting soirées at Cambridge to discuss how to experimentally define the species using gardening practices.[58] The experiments he had most in mind would be those to help establish botany as a precise science: 'There is, in short, no law whatever hitherto

[55] Henslow 1835a: 287-89.
[56] This article is discussed in Kohn *et al*. 2005 (but not in Walters and Stow 2001).
[57] Henslow 1837: 113-14.
[58] Henslow 1835.

established, by which the limits of variation to a given species can be satisfactorily assigned, and until such law be discovered, we cannot expect precision in the details of systematic botany'.[59] Henslow summed up:

> We have dealt somewhat longer on this topic than to many would seem advisable, but we feel so thoroughly persuaded of the very great importance of some exertions being made for the purpose of obtaining a better criterion for the discrimination of species than the mere empirical rules at present practised, that we trust to be excused for attempting to direct the attention of all those who have it in their power to follow up the subject experimentally, to this very desirable object.[60]

In this public appeal to the subscribers of a new periodical, Henslow had explicitly connected hybridists' multiplied observations with making botany a philosophical science.

We have seen how Henslow and Lindley approached the plant hybrid, and hybridising, differently. Lindley emphasised the practical importance of hybridising in vegetable and crop improvement and saw the plant hybrid as a potential commodity. He also used the hybrid as a place marker within his taxonomic practice, to resolve his classification of problematic groups. For Henslow, a community of practitioners hybridising might establish a physiological approach to taxonomy, and even a precise definition of the species, whereas the hybrid itself was a problematic entity. Henslow was unsure of how to present the possibility of fertile plant hybrids and chose to deal with these differently according to his audience.

Henslow and Lindley gave prominence to the practice of hybridising. However, both men were clear that horticultural practices, including hybridising, did not equate to the science of botany. Lindley was especially sensitive about the overlap of horticulture with classification and physiological botany, which we will see in his response to claims from horticulturalists to have made new species in the final part of this chapter.[61] For Henslow, the collective results of a multitude of experiments might amount to 'philosophical' science, not the individual's practice of gardening. Historians have identified a division of labour in British science in the 1830s, inspired by

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[59] Henslow 1837: 116.
[60] Henslow 1837: 125.
[61] Lindley 1829. On Lindley's role in the politics between horticulture and 'scientific botany' over control of the Royal Gardens Kew, see Drayton 2000: 146-169.

eighteenth century economics, in which science functioned to discipline the lower-classes, who would generate the observations and 'facts' needed for the philosophical practitioner to then interpret using inductive method. Henslow's approach, given his reformist politics, saw science functioning to manage the collective body of unphilosophical observers. [62]

Similarly, in the next section, we see how philosophical flora authors collated reports from the communities of practice around their books. While philosophical practitioners needed both local botanists to find species, and cultivator-hybridists to delimit species, the practice of hybridising and the finding of hybrids caused tension between these communities.

III. Local Botanists Debate Hybrids

Making-as-Knowing Mullein Mules

In 1830, John Lindley's close friend, Professor William Jackson Hooker (1785-1865) published *The British Flora,* a text that would reconfigure the practice of British local botany. [63] Hooker was 'erect, slim, muscular' and his energetic style of teaching involved 'practical field trips' combined with a devotion to his subject.[64] Although these biographical details from his son perhaps say more about the ideal Victorian gentleman of science, Hooker senior was clearly obsessed with plants in a way that must have been infectious to enthuse even the most indifferent medical student. The inclusion of practical field work was also important; 'practical' was something of a buzz word during the 1830s, denoting a move away from scholarly book learning, and what was slightly disparagingly termed 'closet botany', towards a more progressive and muscular view of knowledge-making by observing and doing.[65]

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[62] Alborn 1996.
[63] From a family of Norwich wool traders, Professor of Botany at Glasgow from 1820, and knighted in 1836 for services to botanical education. For biographical accounts, see Drayton 2000 and Barton 2018.
[64] Hooker 1901: 551 and Hooker 1902: lxxxv.
[65] Allen 2000. Drayton 2000 first made the point that the 1830s campaign by Lindley, Henslow and Hooker to revitalise British botany had a strong gendered element towards masculinising the science.

One of the best-known plant hybrids, originally named by Carl Linnaeus, came from a genus of yellow and white flowered, four-foot-high, candelabra-like plants, the mulleins (*Verbascum* L.). The unmissable plant was listed in William Hudson's *Flora Anglica* (1778) as *V. thapsoides*, the 'bastard mullein'.[66]

Figure 1.5: Example of a mullein (*Verbascum thapsus* L.), one of the parent species of Edward Robson's mullein mule. From: Smith and Sowerby's English Botany (1799) v.VIII t.549 (Author's collection).

In 1789, a County Durham botanist Edward Robson (1763-1813) planted two mulleins side by side, which spontaneously produced a mullein hybrid or mule, which he called 'Verbascum thapso-nigrum'. This practice, of deliberate juxtaposed planting, was considered a valid way of hybridising, allowing insects to conduct the cross-pollination (and is still used by some plant breeders today who market the approach as more 'natural').[67] Robson reported his findings to William Withering (1741-99) who included this hybrid in the third edition of the first general flora in English, his *Arrangement of British Plants* (1796). Robson was a Fellow of the Linnean Society and his mullein mules apparently caught the attention of its President, (the then Dr) James Edward Smith. Until now, Smith had maintained the traditional view that plant hybrids were 'accidental and artificial' and not part of the order of Nature. The plant hybrid was included in his teaching in a 1795 lecture at Guy's hospital, London, as a familiar manmade production, an example of how moral

[66] Hudson 1778: 90.
[67] Elliott 2014 on horticultural breeders doing so in the 1880s and Hardy 2021 for this practice today.

and material improvement—a public good in today's terminology—might be derived from the science of botany.[68]

Then, in 1797, Smith found a hybrid mullein near his home in Norwich. He reported both his own hybrid find, and Robson's mule, in the first edition of his 36-volume *English Botany* (1799), commenting 'no genus is more apt to engender such [i.e. hybridity] than this.'[69] That comment was intimately entwined with local botanists' reports from their gardens. Smith's Norwich circle included a bank clerk and former shoe-maker, Lilly Wigg (1749-1829), admitted to this elite circle of botanists because of his skill in identifying seaweeds and other algae. Wigg 'cultivated for many years in his garden Verbascum nigrum, pulverulentum, and Blattaria, and the seminal varieties which rose in almost every possible state of intermediate graduation between them were highly curious and beautiful.'[70] Similarly, the resident botanical authority of Denbighshire, a Mr Griffiths, around 1800, reported that Linnaeus' hybrid mullein had reproduced itself in his garden: V. thapsioides 'has frequently been produced in my garden by the farina of V. Thapsus falling upon the stigma of V. lychnitis. This plant is strictly an hybrid, the seeds, though good in appearance, never vegetating.'[71] By 1824, Smith summarised that the mullein species were 'extremely variable, subject to cross impregnation'.[72] However, of Linnaeus' alleged hybrid, Smith remarked: 'the mule variety I have never seen wild'.[73] Smith doubted the occurrence of hybrids beyond the conditions of cultivation; and even there he refused to accept plant hybrids among the willows (*Salix* L.).[74] Yet the observations from his own circle of practitioners suggested that hybridity might be more widespread if mules formed so readily in gardens. At play here was the philosophical belief that variation arose from the conditions of cultivation somehow disrupting the plant's constitution, so that it might interbreed with another

[68] Smith 1795: 35-6. Easterby-Smith 2018 emphasises that late eighteenth-century gentlemen of science in Britain did not agree with Linnaeus and regarded hybrids as unnatural aberrations.
[69] Smith and Sowerby 1799 v.8 t.549 and 1798 v.7 t.487.
[70] Turner and Dillwyn 1805: 426. On Wigg, see Allen 2010: 139 and 144; Secord 2007.
[71] Turner and Dillwyn 1805: 168-9.
[72] Smith 1824 v.1: 308.
[73] Smith 1824 v.1: 310.
[74] Meikle 1975 and Allen 2010.

plant of a distinct species in cultivation, an event which would rarely, if at all, occur in wild Nature.[75]

This tension, between knowledge from cultivators' observations and knowledge from philosophical paper taxonomies, had existed at least since the sixteenth century, when British botanists began attempts to classify. The idea of 'making-as-knowing' was one of the canonical scientific styles of Enlightenment thought, particularly associated with Francis Bacon's philosophy of science.[76] In the seventeenth century, natural philosophers revised the Aristotelian knowledge of causes and held that a made thing was a known thing and, by extension, that the harnessing of natural processes to produce objects was to know those processes and objects. So artificially reproducing a plant by crossing that looked the same as a plant found in Nature provided sufficient grounds for accepting the premise that the naturally-occurring specimen was a hybrid, and no further evidence was required. [77] As a way of knowing about plants, plant breeding including hybridising, might be employed systematically 'to extend, categorize, innovate, and accumulate new knowledge about nature'.[78] Historians of science and sociologists have long argued that the rise of commercial technologies and tacit knowledge systems from the seventeenth century led to developments in empirical knowledge-making.[79] However, by 1830, in Britain the social stratification developing between 'botany' and any form of horticulture, gardening or nurseryman's practice led to the dismissal of these earlier reports of plant hybrids. As we have seen, while hybridising might be put to use within taxonomy to reduce species, both Henslow and Lindley were clear that gardening was not botany. Likewise, when William Hooker came to compile his new flora for a new decade, he did not mention the mullein mules reported in older publications.

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[75] From Pallas 1780. Pytor Simon Pallas (1741-1811) was a German philosophical naturalist at the St Petersburg Academy of Sciences, who dominated Russian natural history for over forty years (Gienapp 1970). British philosophical naturalists followed Pallas according to Darwin (Darwin 1859: 253-4).

[76] Crombie 1994; Pérez-Ramos 1996. Francis Bacon's famous principle was that 'The artificial does not differ from the natural in form or essence, but only in the efficient.' (Bacon 1863: 410).

[77] Holmes [T.] 2015 and 2017.

[78] Smith 2018: [unpaginated].

[79] E.g. Merton 1938 and the trajectory of such historiography is reviewed by Smith 2018.

Hooker's *British Flora* as a Community of Practice

Hooker's *British Flora*, aimed at medical students, was portable and affordable and both the Botanical Society of London and the Botanical Society of Edinburgh recommended the guide to their members. [80] In a major break with earlier general floras, Hooker's primary aim, he explained in his Preface, was to provide 'a manual useful in the field as well as in the closet'.[81] The flora was, above all, about *communally* observing plants in nature:

> The collecting of materials, indeed, in their native hills and vallies [sic.], upon the sea shore, in the woods, and among the majestic alpine scenery with which the northern parts of our island, eminently, abound, generally in the society of friends of a congenial taste, or students full of ardour and enthusiasm, has been a very delightful occupation, especially when taken in conjunction with "anticipations of the pleasure we may have to bestow on kindred minds with our own, when sharing with them our discoveries and our acquisitions."[82]

This reframing of the observations of plants from 'wild nature' and the accompanying emphasis on group muscular mountain walks, was a distinctive feature of 1830s BAAS science.[83] The *British Flora* also appealed to those who wished to identify their plants according to the philosophical principles of diagnosis, especially given its accounts of difficult plant groups often omitted from the growing number of what were increasingly known as 'popular' flower guides.[84] Unlike any botany textbook, Hooker's flora was an interactive work-in-progress, with taxonomic and nomenclatural amendments and newly found plants added to each edition. A community of practice developed among readers using the book. Therefore, tracking how Hooker's flora's various editions approached plant hybrids indicates how the interests of philosophical and local botanists overlapped, and demonstrates how frictions between these communities may have arisen, and how these were addressed.

Hooker consulted Smith's *English Botany,* re-examined herbarium specimens, and collated the views of his immediate circle of philosophical botanist-friends. He did not reject outright reports of plant hybrids beyond cultivation, but took a cautious

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[80] Allen 1976: 109.
[81] Hooker 1830: vii.
[82] Hooker 1830: ix-x.
[83] Ellis 2017.
[84] Allen 2010: 140. On the 1820s novelty of the growth of 'popular' science, see Topham 2016.

approach grounded on first-hand observational reports. Hooker omitted all the various mule mulleins in older floras.[85] Hooker similarly interrogated other reports of hybrids. For example, for two plants originally named by Linnaeus as hybrids, Hooker argued that these forms were neither species nor hybrids:

> [Veronica] V. hybrida, Linn.—E. Bot. t. 673. E. Fl. v. i. p. 17. Rare. In dry chalky pastures about Newmarket and Bury.— in Lancashire, and in Wales, where, in addition to the station discovered for it in Ray's time, Mr. Wilson finds it at Ormeshead, and at Gloddaeth near Conway. Fl. July, Aug. The V. hybrida seems indeed scarcely deserving of being commemorated as a var., for it differs only in its more luxuriant growth, depending probably upon soil.[86]
>
> [Pyrus] P. pinnatifida, Ehrh. (Bastard Mountain-ash); leaves entire pinnatifid and pinnated white and downy beneath, flowers corymbose, fruit globose. E. Bot. t. 2331. E. Fl. v. ii. p. 365. —Sorbus hybrida, Linn. Isle of Arran, the northern part; first found by Mr. J. T. Mackay. In Derenth wood, near Dartford; Rev. Prof. Henslow. Fl. May. Some of the leaves of this plant so nearly resemble the following [S. aria], that I fear (and Prof. Henslow is of the same opinion), it can only be considered a variety.[87]

Mr Wilson was probably William Wilson (1799-1871), a Warrington former solicitor turned botanist of independent means, already known as a fastidious collector and Hooker's bryological protégé. [88] Hooker collaborated in producing his flora with Henslow, Wilson, and two other friends, William Borrer (1781-1862) and Reverend Miles Joseph Berkeley (1803-89). [89] Hooker felt that Wilson's speedwell might perhaps be a temporary form caused by changes in soils. Another alleged hybrid was the bastard mountain-ash or rowan, a tree found by Mr James Townsend Mackay (c.1775-1862), was a gardener, later a curator of the botanic garden at Trinity College, Dublin. Mackay claimed the *Sorbus* as a species-hybrid new to Britain. Henslow and Hooker agreed the plant was neither a species nor a hybrid, instead a variety of the common species of mountain-ash. As earlier practitioners had done with the mullein mules, some local botanists were also comparing their wild finds to cultivated forms. A gardener-hybridist, William T. Bree (1787-1863) believed that he had made a hybrid

[85] Hooker 1830: 94-6.
[86] Hooker 1830: 3-4.
[87] Hooker 1830: 222.
[88] For biographical details on Wilson, see Anne Secord's publicaitons.
[89] Hooker 1830: x. Berkeley was a well-known horticulturalist, and close friend of Henslow (Price 2004). Borrer was a wealthy farmer and FRS who had collaborated with Sir James Edward Smith, growing over 6000 plants in his garden including many British species (Kell 2004).

between two saxifrages, and that the progeny were identical to a plant that Linnaeus had named as a species, *Saxifraga hirsuta* L. (hairy oval-leaved saxifrage): [90]

> W. T. Bree, who has cultivated and studied the Saxifrages very assiduously, says that it is certainly a hybrid between the preceding [*Saxifraga Geum*] and the following [*S. umbrosa*].[91]

Reverend Bree had also purchased a figwort plant which he was sure was a hybrid:

> Mr. Bree has sent me a plant which he considers a hybrid between *S. [crophularia] Scorodonia* and *S. aquatica*, brought from St. Ives, and cultivated in his garden.[92]

Bree's contributions were duly acknowledged in footnotes, but Hooker did not accept these plants by giving them a name, as a hybrid-species or otherwise. On the other hand, in the case of perhaps the best known, and most morphologically obvious, plant hybrids (often given its own species name as *Geum intermedium* Ehrh.), Hooker decided the plant 'seems to be a hybrid' but still listed it as a variety under one of its parents.[93]

These examples show that a friction between Hooker, and even his friends, was particularly likely to arise over the question of whether an intermediate form was produced by spontaneous hybridisation or by the action of climate and soils. The effect of climate and soils might explain a form as being part of the variation within a species; whereas claiming a specimen to be a hybrid was more problematic. However, by tracking hybrids in Hooker's later editions of his Flora (it was selling successfully and he produced four editions in the next eight years), we can see how he was increasingly accommodating the reports of hybrid plants coming from philosophical and local botanists.

Hooker produced a second edition of the *British Flora* the following year, in 1831. He added a report of a hybrid mullein, a single plant, observed by his school friend John Lindley, growing 'in wild nature' on a roadside in Kent. This plant was referrable to Linnaeus' celebrated mullein hybrid, which Hooker gave a species name in inverted commas: '"V. thapsiforme Schrad." (Thapsus-like Mullein) synonym V.

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[90] Bree was an Anglican vicar, lepidopterist, local flora author, and gardener-hybridist (Britten, Boulger and Rendle 1931: 44).
[91] Hooker 1830: 192.
[92] Hooker 1830: 289.
[93] Hooker 1830: 254.

thapsoides Willd.'[94] Hooker also added a thistle, *Cirsium Forsteri* as another plant believed to be a hybrid; then in the fourth edition of 1838 he added a hybrid *Polygonum* and an *Erica.* The first two plants illustrate the problem faced by a philosophical practitioner attempting to collate reports of plant hybrids:

> *Cirsium Forsteri* Mr. T. F. Forster. Foot of St. George's Hill, Weybridge, J.S. Mill, Esq. Fl. July, Aug. 2. - "The fructification most accords with that of the last two sp., while the herbage and habit approach some of the following, or rather the exotic Cn. rivularis, Willd." Sm. - Mr. Borrer suspects it to be a hybrid production between C. pratensis and C. palustris.[95]
>
> Mr Borrer suggests that there should be inserted between P. Persicaria and P. lapathifolium, as uniting these two, if not itself a species, P. laxum (Reich.) [description from Reichenbach's Iconogr. Bot.] [96]

Borrer suggested that the thistle might be a hybrid; yet he also felt that an intermediate form of between *Polygonum Persicaria* L. and *P. lapathifolium* L. should be interpreted as meaning these two species were conspecific. Borrer had devoted more time to travelling around Britain than most other botanists of the period and corresponded profusely with many continental taxonomists. He had worked with Smith on naming a multitude of willow species (*Salix* L.), but was clearly now feeling the pressure to reduce species. Hooker was unsure and included the form as a possible hybrid anyway. Hooker's fourth edition of 1838 also included a putative heath (*Erica* L.) hybrid found by another philosophical botanist.

Figure 1.6: Portrait of Hewett Cottrell Watson (1804-1881)aged 35 in 1839. From: M. Haghe, drawing, 1839, reproduced in the *Naturalist*, facing p. 26. (BHL Archive reproduced under the Creative Commons Attribution License).

In the summer of 1837 Hewett Cottrell Watson (1804-1881) found an intermediate-looking *Erica* in Cornwall. Physically striking with an intense brow, black hair and long limbs, Watson had studied medicine alongside Hooker at Edinburgh. Watson

[94] Hooker 1831: 109-10; Hooker 1835: 111-12; Hooker 1838: 98; and in Lindley 1829: 181 but without inverted commas around the species name.
[95] Hooker 1831: 351.
[96] Hooker 1838: 165.

reported his *Erica* find to Hooker, who decided that the plant was 'probably a hybrid'.[97] Therefore, despite Hooker's initial reluctance to include the mullein mules in his flora, eight years later he was prepared to explicitly consider that a new plant was a hybrid. Watson's putative hybrid was just one among a plethora of observations. Just as Henslow had talked of harnessing horticulturalists, Hooker and Watson spoke of the need to organise local botanists. The final part of this section demonstrates how philosophical botanists attempted to direct local botanists, who, in fact, organised themselves and sometimes differed with philosophical authors over plant hybrids.

Local Floras as Dispersed Communities

In 1831, a short-lived BAAS Botanical Committee focussed botanists on the production of 'County or other Local Floras', with the aim of a complete survey akin to the British Geological Survey.[98] The BAAS meeting 'threw out to the consideration of naturalists' the proposal for 'catalogues of county or other local Floras' with

> such remarks as may be useful towards determining the connexion which there may be between the *habitats* of particular plants and the nature of the soils or strata upon which they grow, with statements of the mean winter and summer temperature of the air and water at the highest as well as the lowest elevation at which species occur, the hygrometrical condition of the air, and any other information of an historical, economical, and philosophical nature.[99]

'Habitats' were localities broadly conceived in relation to climatic situation (and so not today's 'habitat' as understood in the science of ecology).[100] The meeting concluded: 'If a complete botanical survey of the British Isles could be obtained, the results would be important, when the Flora in the aggregate came to be compared with its relations to soil, climate, elevation, &c.'[101]

Local floras were, as we discussed in the introduction to this thesis, a seventeenth century innovation, in Britain dating back to John Ray's catalogue of the

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[97] Hooker 1838: 158. George Bentham named the plant and cited Hooker's view of its hybrid nature (Bentham 1838: 665) (on Bentham, see chapter 3 of this thesis, p. 138).
[98] Anon. 1835: 54.
[99] Anon. 1834: 54-5.
[100] E.g. as deployed in Watson 1835 and 1837.
[101] Anon. 1834: 55.

plants growing around Cambridge.[102] The Humboltian-inspired focus on plant distribution was not new either; however, the BAAS proposal envisaged from the outset that a community of organised practitioners would produce these local floras: for example, the 1833 *Flora of Oxfordshire* included a quotation of the BAAS proposals and an 'Index of plant habitats', devised so that 'Oxford Botanists, if they please, to co-operate with myself [Dr Daubeny], and with each other, in collecting the data necessary'.[103]

Although a BAAS national local flora project was quickly dropped, local floras continued to be produced as a communal project of an open collective of observers.[104] For example, a cultivator-reader of Hooker's *British Flora*, gardener Henry Baines (1793-1878), *Flora of Yorkshire* (1840).[105] Henry Baines local flora was collaboratively produced, and he thanked the 'friends of science' who had contributed towards a catalogue over 1000 species. Baines included his own observations of plants, made decisions about the inclusion or otherwise of herbarium specimens, and at least thirty-three botanists provided plant lists. Many of the contributing botanists were visitors to Yorkshire, so while its focus was provincial, the project's social reach was far wider. [106] One of them took up the challenge to find more plants, and his copy of Baine's book held interleaved sheets, where the owner inserted additions and annotations, and in this case, also corrections of his own, earlier contributions.[107] This interactive practice was especially a feature of mid-nineteenth century floras, for example, a few preparatory copies of Mary Kirby's *Flora of Leicestershire* (1850) were printed with 'alternate blank pages and forwarded to local botanists with an appeal for assistance.'[108] Baines saw his printed publication as a work-in-progress and his book demonstrates that the local flora embodied a diverse, dispersed community; and that this community was to a degree autonomous, producing what they considered as useful and relevant knowledge about plants. BAAS direction was arguably significant,

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[102] Allen 2003.
[103] Walker 1833: preface [unpaginated]. Daubeny was Professor of Chemistry at Oxford.
[104] Allen 1986: 187 fn. 25 although it is unclear why, as Henslow was 'ready to do all he can' (Morrell and Thackray 1984: 116).
[105] Baines referred to Hooker's *British Flora*, p.39, and to *English Botany* p.122. On Baines, see Coles 2011: 36-8.
[106] All quotations are from the preface (unpaginated) to Baines 1840.
[107] Coles 2011: 38.
[108] Kirby 1850: vii.

however, in encouraging the development of the local flora as an active scientific endeavour specifically for a community of botanists, rather than a single or pair of philosophical authors passively collating reports from field observers and herbaria. This all tied in with the BAAS wider shaping of gentlemanly science, and in particular philosophical botanists' drive to improve the status of their practice during the 1830s. To that end, Hooker felt that a philosophical practitioner should collate and verify the reports of plant species found in local floras, and suggested this idea to Hewett Watson.[109]

This philosophical project became a county-by-county listing in Watson's *The New Botanists' Guide* (1835-37). His editorial work was of the 'dull plodding kind' but the *Guide* was important, he felt, for 'farmers and gardeners' who might take part in his project to develop 'a scientific knowledge of the laws of vegetation' which 'must place a future race of cultivators as much above the present workmen, in skill and power, as the scientific chemist of to-day is superior to the cooks and the drug vendors, who were the chemists, empirically, centuries ago.' [110] Despite Watson's efforts to present the project's utility and as work towards establishing the laws of vegetation, some reviewers saw the exercise as unproductive. The *Athenaeum* unenthusiastically asked: 'can a list be science'? [111] Lists were an integral element of natural history practice, but by 1830, rather too closely associated with collecting, for the *Athenaeum*'s readers at least. Other, less prestigious, publications with a social improvement agenda, saw the merit in directing farmers and gardeners to pursue botanical list-making. For example, the *Gardener's Magazine* commended the *New Botanist's Guide* to its readers.[112] These contemporary tensions notwithstanding, the local flora was important, as a site of negotiation between its community and that of philosophical botanists.

A 'prickly schoolmaster' and bookseller Alexander Irvine's (1792-1873) *Flora of London* (1838) shows that local practitioners were prepared to challenge philosophical classifications with comments in a local flora.[113] In 1836, Irvine proposed that

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[109] Watson 1835 frontis [unpaginated] dedication states Hooker suggested the idea for the book.
[110] Watson 1837: preface, p.vi and xix.
[111] Anon. 1837b: 909.
[112] Anon. 1837a: 597. On the prohibitive cost of local floras, see Allen 1996a.
[113] Allen 2010: 295-6.

members of the Botanical Society of London might produce a collaborative flora of the London area. The initiative did not take off (probably due to Irvine not being well liked) and Irvine published his own flora two years later, but still with support from half a dozen or so men and women sending in their lists. [114] Irvine explained that he was following Lindley's over-arching taxonomy for genera and Hooker's general flora as 'the most useful and popular work on this subject'.[115] However, he made suggestions for some changes in species epithet names as 'productive of greater simplicity and consistency'. He approved of Dr Lindley transferring *Verbascum* to Schrophulariaceae from Solanaceae, but was also quick to point out where he disagreed. In particular, he claimed priority over Lindley in reporting the hybrid mullein as 'the author recognised this plant as different from V. Thapsus and V. nigrum, above ten years ago.'[116] Irvine also defined 'hybrid' in his glossary as 'partaking of the nature of two species', an explicit acknowledgement of the existence of plant hybrids outside of gardens. Local floras up until this point either did not mention plant hybrids, or regarded hybrids as restricted to gardens.[117] Irvine provides a good example of a local practitioner who used the local flora as a way of communicating with philosophical botanists over hybrids, classification and nomenclature. [118]

British general flora authors, like Hooker, and Watson in his Guide, faced how to reconcile the philosophical pressure to reduce the number of species, with a need to acknowledge reports of plant hybrids from local practitioners. Hooker's responses over time in the pages of his flora is example of how a scientific community approached uncertainty in its paper taxonomies; and how that uncertainty generated diverse views and debate about how to treat reports of hybrids. A general flora like Hooker's was not inherently accepted as authoritative; a local flora even less so. But both publications were a site of negotiation over plant identification, classification and nomenclature between the author-compiler, contributing botanists, reviewers and reader-users. The ensuing feeling among philosophical botanists that local botanists

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[114] Allen 1986: 13; Allen 2018; Pamplin 1873.
[115] Irvine 1838: vii.
[116] Irvine 1838: 128.
[117] E.g. John Jacob's *Flora of West Devon and Cornwall* issued in parts 1832-8 included the oxlip *Primula elatior* as a garden hybrid: 'I have never found where I should think it decidedly wild.' (Jacob 1836: unpaginated comment under *P. veris*).
[118] Anon. 1873b: 1018.

should be directed was unsurprising given what we have seen of John Lindley's obsessing over the low status of botany in the 1830s and Henslow's urge to organise gardener-hybridists. The following section explores this interacting relationship between philosophical botanists and cultivators surrounding the practice of hybridising and what it might mean for science.

IV. Cultivators Practise Taxonomy

Horticulturalists Make Hybrids

In 1835, the fifty-seven-year old Honorary Reverend William Herbert was a silvery-grey haired and surprisingly short man, yet his stature among horticulturalists had never been greater.[119] Herbert grew up at a famous estate, Highclere in Hampshire, and together with his brother, father and uncle, founded the Horticultural Society of London.[120] From about 1808, hybridising began at Highclere, specifically to extend the flowering season of the newly constructed 'shrubberies' from April into the summer. William's brother, Lord Caernarvon, and his personal secretary Robert J. Gowen Esquire, were some of the 'gentlemen propagators' whose 'extensive experiments' developed novel forms in place of purchasing imported plants, most famously among the Rhododendrons and Azaleas. [121] Herbert's own hybridising focused on the ornamental bulb families of plants among the lilies, Amaryllis and daffodils. In preparation of the accounts in his forthcoming monograph, *Amaryllidaceae,* Herbert wanted to compare the fruit of the various species and genera of daffodil. He received herbarium specimens from Hooker, and fresh plants from several botanic gardens, a nurseryman from Hackney, and seeds and information from the head gardener of the Horticultural Society's Chelsea Garden. He was surprised to find that most of his extensive contacts in the nursery trade could not provide him with seeds: many of the

[119] Guimond 1966: 19 (from an account of Herbert in 1840 from the *Manchester Guardian*). Herbert studied law, then spent a year as a Tory MP, before ordination in the Anglican church and moving to Spofforth near Harrogate, Yorkshire (his only biography is an unpublished Ph.D., Guimond 1966).
[120] Amherst 1896. See the description of Highclere in Loudon 1834 and Cobbett 1830.
[121] Loudon 1834: 251.

different groups of daffodil were propagated by division of the bulbs and the flowers were sterile. [122]

Herbert set about investigating the idea that these plants might, in fact, be hybrids. One plant, *Narcissus incomparibilis* Mill. was known to grow wild in France, and Herbert successfully re-made several forms of this plant by hybridising the wild British daffodil with a southern European species*, N. poeticus* L. Herbert's hybridising produced a shocking result. The daffodil monographer, succulent specialist and entomologist Adrian Hardy Haworth (1768-1833) placed *Narcissus incomparibilis* separately into its own genus, *Queltia*. One of Haworth's other daffodil genera, *Philogyne*, turned out to be produced by hybridising a wild daffodil with the Iberian species *N. jonquilla* L. . Herbert's hybridising undermined Haworth's classification. [123] A few years later, John Lindley as editor of *Edward's Botanical Register*, invited Herbert to produce a plate together with an account of his re-made daffodil hybrids.[124] Lindley praised Herbert's contribution to taxonomy as philosophically reducing the number of daffodil species, rather than agreeing with Herbert's claims that he had proved how species might be made by hybridising. Unlike Lindley and most philosophical botanists, but in common with nurserymen, Herbert was absolutely convinced that the hybridist could make new species.

[122] Herbert 1837: 7.
[123] Anon. 1833. Haworth was well-connected, including with the Norfolk botanists surrounding Sir James Edward Smith (Crompton and Nelson 2000). Biographical details in Boulger and Gross 2004.
[124] Herbert 1843. Herbert challenged the recent classification of daffodils in Haworth 1831.

Figure 1.7: The Hon. and Very Rev. William Herbert's Hybrid Narcissi (Herbert produced this illustration). From: Herbert 1843: Plate 38. (Reproduced from the BHL Archive under the Creative Commons Attribution License).

Herbert gathered together his array of experimental evidence for 'hybrid species' in his book (and Alice Guimond's biography shows that he maintained this view to the end of his life).[125] Herbert first announced that he had 'created a new species' in 1818 by hybridising 'in the same manner in which the distinct species of many genera of plants have been produced in the course of time, by the accidental operation of nature'.[126] By acknowledging that gardeners might make species, Herbert believed that he had also removed the problem among botanists over what counted as the one or more structural differences needed to recognise a plant as a species, rather than as a mere variety: 'All divisions, except that of generic identity are artificial, and rest on the supposed agreement of different individuals in one or more essential points of structure.'[127] Herbert then argued that species and varieties were both formed by crossing and by the actions of soils and climate. The species we see were produced, in the past, by the crossing of the aboriginal created forms. Herbert was reiterating, as we saw in the introduction to this thesis, an eighteenth-century idea. God created the original progenitor forms—the major kinds or types known as genera, as represented by an original progenitor species—but all other species produced by crossing over the ages. This view neatly reconciled the biblical account of Creation in Genesis with the latest evidence from geology, and contemporary empirical beliefs about how species might be formed among horticultural communities. Herbert's Christian beliefs positively elevated hybridity as part of the created order of Nature. A species to Herbert was simply a permanent variety: any difference between a species and a variety was 'artificial, capricious and insignificant'.[128]

Herbert's book included an autobiographical account of the antagonism he experienced to his views about plant hybrids and the role of hybridisation in the Creation:

> Soon after the publication of that communication to the Society [his paper read in 1818 claiming to have made a hybrid-species], I was accosted by more than one botanist in the words, "I do not thank you for your mules," and other expressions of like import, under an impression that the intermixture of species which had been commenced, and was earnestly recommended to cultivators, would confuse the

[125] Guimond 1966.
[126] Herbert 1822: 190.
[127] Herbert 1837: 16.
[128] Herbert 1837: 346.

> labours of botanists, and force them to work their way through a wilderness of uncertainty.[129]

In his book, Herbert argued that instead of causing confusion, botanists could make use of the gardener's art of hybridising, as it would:

> afford a test whereby the accuracy of their [ie. botanists'] distinctions might be more satisfactorily investigated, many of the errors of their system eradicated, and its details established upon a more solid foundation, and less upon the judgment or caprice of individuals.[130]

This statement touched on an extremely sensitive issue. Jim Endersby shown how botany in 1830s Britain had a major image problem due to the disagreement between individual taxonomists over their classifications.[131] Hybridising and cross-breeding, Herbert believed, provided a test of these classifications. In any event, by 1837, Herbert noted that this hostility from botanists had dissipated:

> The alarm, which some botanists had taken inconsiderately, appears to have subsided, and admissions have been already made by some of the most distinguished, which, if the consequences that flow from them are considered without prejudice, must lead to much more extensive avowals, and a final assent to the principle of my statements concerning specific and generic distinctions. [132]

Herbert concluded:

> The effect, therefore, of the system of crossing, as pursued by the cultivator, instead of confusing the labours of the botanist, will be to force him to study the truth, and take care that his arrangement and subdivisions are conformable to the secret laws of nature.[133]

Who were the botanists opposing Herbert over his mules, and what, exactly were they objecting to? Herbert exchanged 'much difference of opinion' on plant hybridity at the meetings of the Horticultural Society of London and in the premier science publication, the *Philosophical Transactions of the Royal Society*, with Thomas Andrew Knight.[134] Knight maintained the orthodoxy that hybrids—'mules' as he called them—were invariably sterile. It is unclear what exactly Knight and Herbert debated, as Knight sometimes used the term 'mule' for a cross between species of different genera and Herbert did claim to have made bigeneric hybrids. Knight was certainly Herbert's main

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[129] Herbert 1837: 336.
[130] Herbert 1837: 336.
[131] Endersby 2005.
[132] Herbert 1837: 336.
[133] Herbert 1837: 336.
[134] Knight 1822: 367.

adversary at the time the comment ‘I do not thank you for your mules’ was made. However, Herbert later explained in 1843 that the quip originated from another philosophical practitioner, Haworth, whose daffodil classification Herbert’s hybridising had demolished. Herbert concluded ‘the public will however perceive, that, instead of confounding the Botanist as he [i.e. Haworth] fancied, while they embellish the garden, they offer the surest test of the accuracy of scientific divisions.’ [135] Haworth had died in 1833, so any comments made, about hybrids confusing classifications, date to between 1818 and 1833, and by 1837 this concern had subsided.

Turning next to what these objections were about. Herbert in the above quotation refers to distinguished botanists now finally assenting to his ‘statements concerning specific and generic distinctions.’ The debate was apparently over Herbert’s new classification system, which he claimed as a ‘natural’ system, constructed using hybridising, and which demoted the species. This context is important when we come to interpret two oft-cited articles on hybridising from 1843 and 1844 in the *Gardeners’ Chronicle,* which we mentioned in the historiographical introduction to the thesis. We discuss these here, as they relate to William Herbert’s book.

The first article from 1843, authored by a gardener-journalist, W. P. Ayres, provides evidence of the increased popularity of hybridising, in part in response to Herbert’s book.[136] Indeed, by 1837 hybridising was commonplace in Britain, a country ‘where the passion for horticulture is great, and the attempts to produce hybrid intermixtures have been very extensive in the last fifteen years...’[137] The 1844 editorial was John Lindley’s account to promote hybridising, which began with his view that plant hybrids were proven in nature, cited at the start of the introduction to this thesis. Lindley then added:

> We mention these things by way of vindicating the hybridizers, who have been accused of attempting to subvert the whole Order of Nature by monstrous practices. It is clear that they only imitate the practices of Nature. [138]

[135] In the case of daffodils, these tensions between horticulturalists and biologists, over ways of knowing about plant hybrids, persist today (Könyves, David and Culham 2019).
[136] Ayres 1843: 444.
[137] Herbert 1837: 349.
[138] Anon [Lindley, J.] 1844: 443.

Lindley was writing with Herbert's book in mind, and Herbert was the 'hybridizer' facing such accusations. Lindley had just explained how the main worry was that indiscriminate crossing between species would result in all species 'in the lapse of ages, be confounded in one inextricable chaos.' Lindley continued by re-assuring readers that plant hybrids would not confuse the recognition of the species, as hybrid forms that might survive under the conditions of domestication were unlikely to persist in nature:

> Although we conceive that the *production* of hybrid plants naturally is of more common occurrence than may be supposed, it must be remembered that the *preservation* of them is quite an artificial process … it is not, therefore, likely that natural hybrids will often be long perpetuated, although they may be frequently produced.[139]

Lindley concluded that 'Botanists afflicted with the *hemionophobia'* should ignore cultivated plants.[140] In considering Lindley's words, it is crucial to remember that in this period, botanists sought to order wild and cultivated plants together in a single classification; no distinction was necessarily made between garden productions and naturally occurring hybrids. Therefore, it is easy to understand why the task of a taxonomist might become overwhelming if hybrids were included in classifications. Lindley played this worry down: he had successfully tackled the issue in his monograph of the roses (1820), and reminded readers that since the introduction to England of the China Rose in 1789, garden roses were almost all likely to be of hybrid origin.[141] Lindley's far greater concern was over the indiscriminate naming of plant forms as new hybrid species. He complained that 'people are continually fancying they have obtained hybrids when they have only gained natural seedlings.'[142] He drew the reader's attention to how to hybridise taking the correct precautions and cited William Herbert's practices, as 'the greatest of all authorities in this matter.'[143]

Some historians interpret Lindley as speaking of widespread religious opposition from botanists to hybridising as contrary to the divine order of Creation,

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[139] Anon [Lindley, J.] 1844: 443.
[140] Anon [Lindley, J.] 1844a: 459.
[141] Anon [Lindley, J.] 1844: 443 citing Lindley 1820.
[142] Anon [Lindley, J.] 1844a: 459.
[143] Anon [Lindley, J.] 1844a: 459.

and then citing Herbert as a clergyman whose practices might reassure them.[144] This is misleading. Lindley was discussing the comment in Herbert's book, that botanists had accused Herbert of confusing their classifications. Herbert, as we have seen, stated that the opposition he faced was to his system of classification using hybridising; a natural system, albeit different to that promoted by Lindley. Henslow, as we saw earlier in this chapter, was concerned about fertile hybrids (what nurserymen called 'hybrid-species') causing confusion in classifications. There were clearly multiple issues at play surrounding the production and naming of cultivated plant hybrids, some intellectual, some social, some religious; Herbert was, as Darwin later quipped, 'heterodox on species'.[145]

In sum, garden historians who see this hostility towards hybridising and hybrids as being wholly religiously motivated ignore the wider context of Lindley's and Herbert's writing. The standard historiography from historians of natural history is correct: opposition from botanists to hybridising in the period 1818 to around the mid-1830s arose from concern that plant hybrids—both manmade and spontaneous—would confuse nomenclatural systems and paper taxonomies. Underlying this opposition was a practical appeal to insulate philosophical taxonomy from disruption by the rapid increase of new forms made by gardeners and nurserymen. Hybridising was, as all historians agree, a practise that rapidly popularised, and its reception was most likely similar to that of other new technology. We will see how in chapter two, a philosophical botanist partly resolved this tension by drawing a division between unruly garden plant productions and uncertain putative wild hybrids on one hand, and 'indigenous' plants on the other; local botanists would concentrate on studying indigenous native plants. We will also see, later in this chapter, how a worry about hybrids causing chaos in classifications may, for some practitioners, have been underpinned by an anxiety over the classification of Man.

Returning to Herbert's book, historians tend to focus on his most famous reader, Charles Darwin, who seized on Herbert's view, of species and varieties as a continuum, to support his own theorising.[146] Other readers of Herbert's book were

[144] Kingsbury 2009.
[145] C.D. to J.D. Hooker, 28 October [1845], DCP letter No. 922.
[146] E.g. Darlington 1937; Stearn 1952; Rix 2014.

interested in his claim that hybridising might be conducted to correct classifications. A head gardener, Donald S. Beaton (1802-63) was enthralled and responded to Herbert's message that gardening might contribute to science.

Gardeners as Taxonomic Experimenters

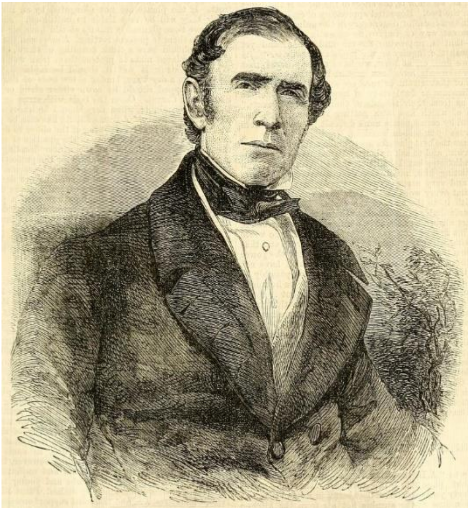

Figure 1.8: Portrait of Donald Beaton (1802-63). From: the *Gardeners' Chronicle,* 24 November 1863, p.415. (Reproduced from the BHL Archive under the Creative Commons Attribution License).

Donald Beaton was a Scottish gardener who, as a teenager journeyman, taught himself not only botany but also English (he spoke only Gaelic), and then 'how to hybridise'.[147] He was 'initiated into the mystery of crossing flowers' in 1816 crossing two Primula species.'[148] Beaton reviewed Herbert's book for the *Gardener's Magazine*, presenting himself as the leading authority on hybridising.[149] Beaton's review tells us most about the sort of gardener-botanist that he himself aspired to be. He lavished praise on Herbert's book, proclaiming it as the most scientific work ever produced on what Beaton termed 'practical botany', comprising 'the labours of the cultivator, blended with the researches of the botanist.'[150] Beaton continued, gardeners should set aside the enjoyments of natural history, and become serious botanists: 'The truth is, that much progress will not be made in the application of the science of botany to vegetable culture, till such a state of society occurs as will call forth a race of gardeners, any one of whom could produce a work of similar nature to this of Mr Herbert.'[151] Each gardener, he explained, would take on a plant genus or order and 'experiment and

[147] Beaton 1854: 153. There is no biography of Beaton but he authored several autobiographical accounts, which were attempts to use his science to elevate gardeners' social respectability.
[148] Beaton 1861: 112.
[149] Beaton 1837: 280.
[150] Beaton 1837: 270.
[151] Beaton 1837: 272.

treat upon it as Mr Herbert has done with Amaryllidaceae'; in other words, conduct hybridising to address taxonomic questions.[152]

While allowing for Beaton's exaggerated self-aggrandisement, his view was in keeping with how gardeners were encouraged to see their practice by gardening periodicals. Loudon aimed his magazines at a broad (and therefore potentially financially viable) audience of both gardeners and naturalists, like Herbert's book, seeking to bridge a rhetorical gap between 'the scientific botanist' and the 'unlearned cultivator'.[153] The 1820s and 1830s upsurge of botanical communities created around inexpensive gardening and natural history periodicals, often reprinting the content from more exclusive scholarly publications, meant more voices were heard on botanical matters than ever before.[154] And Beaton's was one of the more vociferous of those voices.

Beaton's review of Herbert's book, and the book itself, encouraged gardeners to seek botanical knowledge. In 1837, the *Gardener's Magazine* ran a leading article authored by another head gardener, Robert Fish (1808-1873), who extolled gardeners to obtain a scientific education.[155] This was part of the magazine's progressive social agenda since its inaugural issue over a decade earlier.[156] Head gardeners like Fish and Beaton directed gardeners to make specific experiments. For example, on reading Herbert's tome, Beaton decided that the large number of examples adduced showed that hybrid fertility or sterility depended on the constitutional similarity of the parents. This 'opens a wide field for the ingenuity of the cultivator in devising certain modifications of culture, with a view of changing or counteracting constitutional habits in the subjects of his experiments.'[157] In other words, applying certain treatments, such as transplanting when a plant was on the point of opening its flowers, was believed to affect the plant's constitution and so enable crossing to take place. Beaton mined Herbert's book for other examples of the hybridist's art that might be tested scientifically, namely, ways of altering a plant's constitution or manipulating its

[152] Beaton 1837: 272.
[153] Announcement of Herbert's treatise in the *Gardener's Magazine* (Anon. 1836a).
[154] Dewis 2014; Topham 2013.
[155] Fish 1837. On Fish, see Desmond 1994: 273.
[156] Lustig 1997 and 2000.
[157] Beaton 1837: 275.

functions. It was this knowledge, Beaton stressed, that a botanist-cultivator brought to hybridising; whereas the act itself of transferring pollen was ‘the simplest part of the whole process, and might be taught to a child in two minutes.’ Although even such a child also needed knowledge of the salient anatomical structures of plants to facilitate crossing.[158] In all this, the studies of a botanist coupled with ‘eye of the hybridiser’ was central to Beaton’s claim that gardeners might erect scientific knowledge, rather than merely consume such knowledge. Above all, such scientific knowledge-making was exclusive to a gardener-hybridist.

Historians of science have established that in the 1820s horticulturalists’ rhetoric often presented their practices as ‘scientific’, which in general terms encompassed rational observation and manipulation, conducted with the Baconian aim of improving fruit, vegetables and crop plants for human benefit.[159] Historians also highlight how ‘scientific’ was a frequently used tag to denote serious study and self-improvement among horticultural and agricultural communities from 1820 onwards in Britain and France.[160] These vociferous claims to be ‘scientific’ suggest that most considered horticulture to fall outside of science, and were an appeal to make gardening socially acceptable.[161] Therefore, Beaton’s aspirational vision, that hybridising might correct taxonomists’ classifications, was socially ambitious. Yet it was grounded in his belief that taxonomists needed the knowledge of gardeners, and his own experiences, illustrated by a dispute over Fuschias.

In 1835, John Lindley named a new species of *Fuchsia* from Port Famine, in the Falkland Islands, as *F. discolour*.[162] Lindley admitted ‘if we are asked to state in what respect this differs botanically from *F. gracilis* and *tenella*, we should find it very difficult to answer the question. The botanical difference, if any, of all the Chilian fuchsias is very trifling.’ The actual difference, Lindley continued, was that this new form was more hardy than any other *Fuchsia* introduced to British gardens. But that was not, in itself, grounds for recognising a plant as a new species. He acknowledged

[158] Beaton 1837: 276.
[159] For Britain, see Lidwell-Durnin 2018; for France, see Oghina-Pavie 2015.
[160] Lustig 1997, 2000; Wilkinson 2002.
[161] Even a head gardener was socially unacceptable in 1840 (Enderby 2016).
[162] Lindley 1836a.

that some botanists considered that *all* of the Chilian Fuchsias were mere varieties of a single species.[163] Donald Beaton seized on Lindley's apparent self-contradiction and wrote into the *Gardener's Magazine*, mockingly paraphrasing the botanist: "It is difficult to distinguish it botanically from grácilis and tenélla; yet it is decidedly different." —Lindley.[164] Beaton disagreed with both Lindley and with the botanists who believed that these Fuchsias were 'mere varieties' of a single species. He announced that 'their origin is still more singular in a botanical point of view.' By this he meant that these 'species' named by Lindley had all arisen by hybridisation, either in Chile, or in European nurseries. He had proved this by producing some of the various forms himself, including the Port Famine Fuchsia, by hybridising.[165]

Beaton was convinced he was right, as a practical man 'at the potting bench', and corrected the botanists' classifications. The hybrid Fuschias were species, albeit 'home-made species':

> Now, the query is, are these fuchsias species, or varieties? The exact limits of species and varieties are so imperfectly understood, and so difficult to be defined, that many botanists throw such as are produced by artificial means into varieties. Should any of them be capable of reproducing themselves, they are said to revert to either of their parents at the third or fourth generation, or become sterile altogether. This is plausible enough, and may be found convenient in the closet, but it will not do at the potting-bench. That plants can be originated artificially which will be found capable of reproducing themselves from seeds, ad infinitum, with as little variation as is to be found in any natural species, is as obvious to gardeners as the sun at noon-day. To distinguish such home-made species from mere varieties, we ought to have some peculiar term.[166]

Made in 1835 (while Herbert was still drafting his book), Beaton's exclamation here reveals two interrelated points at issue between horticulturalists and philosophical botanists. Firstly, Beaton believed that artificially created hybrid plants could be fertile and permanent. Secondly, such 'home-made species' deserved naming. Philosophical botanists, as we have seen, were under considerable pressure to reduce the numbers of named species. Beaton did not believe that he held a different concept of the

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[163] Today Lindley's species are all sunk into *F. magellanica* Lam. (I.P.N.E. 2020).
[164] Beaton 1835: 580. The intonation marks assisted readers with their pronunciation of the Latin plant names.
[165] Beaton 1835.
[166] Beaton 1835: 581.

species to Lindley; both men followed similar criteria, which Beaton listed. A species was a stable form; therefore for Beaton, a permanent form that reproduced itself, with some variation but within acceptable limits, could be legitimately called a species. Beaton's request for 'some peculiar term' to distinguish 'hybrid species' was taken up by Herbert in his book. Herbert proposed a system for naming hybrids, partly following the existing practice in Britain, but to distinguish garden crosses from hybrids found in the wild:

> Hybrid plants which are found of spontaneous growth in the wild abodes of their parents should rank as species marked Hyb. Sp. or spontaneous hybrid; those of complicated or uncertain intermixtures in our gardens should be marked as variety garden hybrid.[167]

Those garden varieties should be

> specified as belonging to one of four characters, viz. 1. local; 2. accidental; 3. cultivated; 4. hybrid. 1. Varietas, i. e. var. loci; 2. var. fortuita; 3. var. hortensis; 4. var. hybrida …[168]

This role the *Gardener's Magazine* in the reception of Herbert's book, via Beaton's review, combined with Henslow's call to horticultural communities to support botanical science by hybridising, provide a new insight in support of entwined relations between gardening and botany in 1837. Herbert's book identified and attempted to reconcile some of the tensions between gardeners and philosophical botanists over the naming of new hybrid species (yet ironically it is more often cited by historians as evidence of Victorian botanists' hostility to hybrids). However, that is not to say that all philosophical practitioners were willing to acknowledge Herbert's hybridising. One of Herbert's readers would reject plant hybrids outright, a philosophical botanist who was later known as a leading physician and physiologist, William Benjamin Carpenter's (1813-1885).

A Philosophical Rejection of Herbert's Hybrids

William Carpenter was, in his university teacher's words, 'a young and eager gentleman' and 'intelligent member' of the Botanical Society of Edinburgh.[169] His early reputation was built on the physiology of vegetables. He wrote a prize-winning essay

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[167] Herbert 1837: 33.
[168] Herbert 1837: 31.
[169] Graham 1838: 37.

on plant physiology, then a textbook, both while still a medical student.[170] Carpenter believed that physiology embodied the most progressive and scientific element of botany; and that the study of vegetables might underpin the whole of the science of animal and human physiology:

> The physiologist may advantageously resort to the study of vegetable life for the explanation of many of the proximate causes of those phenomena which are complicated in the higher forms of organized beings by so great a variety of secondary influences.[171]

Therefore, his textbook, *Principles of General and Comparative Physiology* (1839) would the first attempt in English to unify human, animal and plant physiology.

In 1837, Carpenter prepared for his textbook by reviewing the leading plant physiology texts. He used his review to set out his approach to the species. He argued that the concept of the species should be explicable by subordinate laws regulating the exercise of the reproductive function. The philosophic naturalist', he explained, 'aims to reduce the number of species' and should do so by multiple observations and experiment, almost echoing Henslow's words.[172] However, Carpenter felt that was difficult using only structural characters (or morphology) as these varied so much: 'Amidst all these difficulties attending the discrimination of species from structural characters alone, it is not unreasonable to enquire if there are any other means of effecting the object with greater certainty.'[173] He continued that 'the laws regulating the intermixture of species if stated in a sufficiently general form' were equally applicable to both the Animal and Vegetable kingdoms and therefore 'may be regarded as one of the most valuable tests which naturalists possess.' In other words, hybridising was a test of species discrimination. The general law appeared to be that crossing distinct species produced either no hybrid progeny at all, or a hybrid which

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[170] Carpenter was a student at Edinburgh Medical School 1835-39, then Professor of Physiology at the Royal Institution from 1845. He was later well-known for his popular science books, theory of mind, and stance against Mesmerism (see Lidwell-Durnin 2019a; Delorme 2016).
[171] Carpenter 1837: 5. This review of the year's books on vegetable physiology was attributed to Carpenter in his collected writings (Carpenter 1889).
[172] Carpenter 1837: 414.
[173] Carpenter 1837: 414-5.

was sterile. Carpenter had read about Herbert's experimental crossing studies and he rejected Herbert's 'unphilosophical' view that hybrids might sometimes be fertile.[174]

Carpenter went beyond the orthodoxy presented in both Lindley and Henslow's plant physiology textbooks. These botanists declined to determine any laws governing the breeding of species or varieties, whereas Carpenter, in his textbook, set up a law of hybridity, or general principle, that:

> beings of distinct species, or descents from stocks originally different, cannot produce a mixed race which shall possess the capability of continuing itself; whilst the union of varieties has a tendency to produce a race superior in energy and fertility to its parents.[175]

He had admitted in his earlier review that plant hybrids were different to animal hybrids:

> the limits of hybridity are more narrow [in animals], since the hybrid is totally unable to continue its race with one of its own kind; and although it may be fertile with one of its parent species, the progeny will of course be nearer in character to the pure blood, and the race will ultimately merge into it.

but then, in his textbook, did not consider this difference among plants to detract from the general position. His law of hybridity rejected Herbert's claims that gardeners might make species by hybridising:

> Many plants, which have been described as distinct species, are either accidental varieties or mules between two varieties; and this fact leads us to doubt whether mules … are ever produced between two really distinct species. We are aware that the gardener would bring forward many instances to the contrary….[176]

As historian John Lidwell-Durnin notices, Carpenter often appealed to the common-sense observations of plant and animal breeders in discussing heredity. He accepted some of Herbert's experiments with Primulas, where the results supported his view of heredity.[177] And, in this instance of intermediates between the various forms of Primulas, where the result also supported his view of the species. However, Carpenter rejected Herbert's hybrids. In the final part of this section, we explore why.

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[174] Carpenter 1837: 417. 'Unphilosophical' here denotes the Victorians' special sense of this word.
[175] Carpenter 1839: 417.
[176] Carpenter 1837: 29.
[177] Lidwell-Durnin 2019a.

Carpenter had been thinking about species and hybridity for some time. A couple of years before, at the BAAS meeting at Bristol in summer 1836, he presented a paper on 'the criteria by which species are to be distinguished'.[178] Carpenter's paper was followed by a heated debate over pig hybrids. Thomas Campbell Eyton (1809-80) a gentleman farmer who went to Cambridge with Darwin, spoke about his successful hybridising of Chinese, African and English pig species. He concluded that either species might produce fertile hybrids, or that the species could not be defined by its inability to interbred with other species. BAAS members, however, vigorously rejected Eyton's argument. Eyton's view of pig hybrids dangerously opened the way for the Chinese, African and English human races to be different, but interbreeding, species. Evidence usually presented for distinguishing species included failure to interbreed at all, or producing a sterile hybrid. It was well-known that the different human races did interbreed, therefore it became important for proslavery naturalists to find fertile hybrids, so that the 'mulatto' (the term used for a mixed race child) might be classified as a hybrid and, by inference, its parents as different species. A major sub-thesis of Adrian Desmond and James Moore's biography of Charles Darwin is the political significance of animal breeding in the 1830s and 1840s. This discussion took place among American and British botanists too. The BAAS farmyard debate rumbled on in the pages of Loudon's 'informal and chatty' *Magazine of Natural History*, encouraging plant breeders and gardeners to report their observations about hybridity. [179] Another encouragement to those who might wish to see the human races as distinct species, and discourage miscegenation, came from plant hybridising.

Already by the mid-1830s, the idea that a hybrid was aesthetically inferior to the parent species, and unhealthy and degenerate, was gaining purchase. John Lindley, ever the advocate for hybridising, nonetheless warmed against the 'unskilfulness of gardeners' creating ugly diseased productions:

> In their haste to improve the works of nature, these gentlemen have converted some of the fairest races in the Vegetable world, into forms in no case more beautiful than the original, and in the majority of instances unhealthy, mongrel, and debased. We strongly recommend all those who value this really beautiful and most singular genus,

[178] Anon. 1836b: 99.
[179] On the *Magazine of Natural History,* see Allen 1996: 113. On Eyton 1837 and subsequent articles, see Desmond and Moore 2009: 213-17.

> to abandon a pursuit which has as yet led to few results which good taste can approve, and to apply the same skill which they have used in spoiling Calceolarias to recovering the pure original races, to preserving them uncontaminated.[180]

Already by 1836, Lindley was lamenting the fact that hybridising was apparently getting rather too popular. Purveyors of horticultural aesthetics regarded plant hybrids unfavourably, as spoiling the pure original races, is a point that we will return to in the final chapter of this thesis. An analogy to the races of Man was unavoidable.

The debate over human hybrids, and the unity or plurality of the human species, would come to dominate the BAAS meetings in 1839 and 1841.[181] Historian John Lidwell-Durnin points out that, in America, plants too featured in these debates.[182] In Britain, it was William Carpenter's reading of William Herbert's book that brought plants squarely into this debate about the classification of Man. Carpenter grew up in a Unitarian family, a church in which the Christian was encouraged to question intellectual orthodoxy and called to act for social justice. Historian John Lidwell-Durnin has recently shown how Carpenter's religious commitments to temperance and the abolition of slavery underlay his understanding of heredity.[183] Similarly, his abolitionist beliefs affected his approach to plant hybridity. Around the time that he was drawing together his account of species and hybrids in his textbook, and read Herbert's book, Carpenter also read Alexander Walker's *Intermarriage* (1838) on the vexed question of the human hybrid. [184]

Carpenter opened his textbook with a survey of the 'hypothetical' definitions of the species used by leading philosophical naturalists. He concluded that none were satisfactory. For example, how much difference between the skulls of dogs, wolves and other mammals justified naming these as different species? He concluded that 'two races may rank as either distinct species or as varieties of the same species.' He then analysed the actual criteria employed by naturalists, in practice, for distinguishing species among animals and plants. This included the view that two races were

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[180] Lindley 1836: folium 1743 [unpaginated].
[181] Desmond and Moore 2009.
[182] Lidwell-Durnin 2019a.
[183] Lidwell-Durnin 2019a. Carpenter used his science to shore up his social reputation, given his radical beliefs. On Unitarians and abolition, the classic text is Stange 1984.
[184] Carpenter 1839: preface, p.v.

'undoubtedly specifically identical' where these were connected by a series of 'intermediate graduations' of forms, citing Herbert's Primula experiments.[185] Carpenter's survey showed that animals and plants grouped within a single species 'bred freely with each other and the offspring are fertile.'[186] This was his fundamental point. Plants were relegated to an exception in a footnote: that in plants 'the limits are wider' but that 'there is obviously a want of fertility, and a consequent tendency to extinction, in all hybrid races whose parents are specifically different.'[187] Carpenter later made the connection between hybridity and ethnology explicit in his 1849 account of the races of Man. The Caucasian and the Negro might interbreed and produce fertile offspring because they were both of common stock and blood, and therefore a physiologically-informed classification upheld the Unity of Mankind.[188] In sum, Carpenter's reaction to Herbert's book revealed exactly what the cultivators' view of hybridity threatened: the unity of Mankind.

V. Conclusion

The story of hybridising enhances our understanding of the variability of the science of 'botany' in the 1830s, and its entwinement with horticulture. Local botanists cultivated wild and garden plants, cultivators engaged with taxonomy, and philosophical botanists practised hybridising. These three botanical communities had overlapping yet different views of hybridity in the 1830s, and none held the generalised opposition to plant hybrids more commonly supposed by most historians. Further, within the philosophical community, botanists held diverse perspectives on plant hybridity. John Lindley recognised hybrids as an entity in his textbook and in his own classifications, whereas Henslow was more cautious, at least in his textbook. Henslow's approach in his textbook versus his gardening periodical piece revealed a tension between how gardeners and philosophical botanists interpreted hybrids. What was acceptable for Maund's 'humble' gardening journal might not be for wider circulation among philosophical practitioners. Local botanist-gardeners sometimes challenged

[185] Carpenter 1839: 415.
[186] Carpenter 1839: 414-417.
[187] Carpenter 1839: 416-17.
[188] Carpenter 1849 (and see Lidwell-Durnin 2019a).

philosophical practitioners over hybrids in floras, and philosophical botanists made space in their paper taxonomies for their observations. Hooker increasingly included reports of plant hybrids in the subsequent editions of his general flora. Philosophical practitioners also faced a paradox: to reduce species, they needed local botanists and cultivators to make observations and conduct hybridising; yet cultivators were often convinced that hybridising made additional permanent forms, and even new species.

This Chapter set out to demonstrate the divergent attitudes to plant hybridity in and around 1837. In doing so, it also reveals one cultural factor in explaining the much-cited opposition to plant hybrids. Plant hybrids confused classifications for some botanists, as the standard historiography correctly claims, but more precisely, the hybrid challenged the classification of Man. William Carpenter provides the clearest example, but it is plausible that abolitionist politics underlay the taxonomic approaches of other philosophical botanists in this period. For example, historians agree that a youthful Charles Darwin was impressed more by Henslow's moral courage and abolitionism than by his science.[189]

We also explored hybridising as experimentation. As we discussed in the thesis' introduction, historians traditionally see an experimental tradition entering natural history practices with the growth of British physiology from around 1890.[190] Similarly historians of plant breeding see an emergence of experimental hybridising among horticulturalists from 1890, setting the scene for the rediscovery moment of Mendelism in 1900.[191] Conversely, we have seen how, during the 1830s, some philosophical practitioners conducted hybridising to inform taxonomy, and saw hybridising as a physiological contribution towards erecting laws of nature, which in turn would improve the status of botany as a science. Investigating plant hybridity involved harnessing and directing the activities of lower-class practitioners. A head gardener was certain that his hybridising contributed to knowledge-making; his practice was an experiment and therefore 'scientific' (even if this claim was socially over-stated). Although historians remind us that we cannot get a unified answer to the

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[189] Desmond and Moore 2009: 51.
[190] Farber 2000; Stevens 1994; Schiller 1980.
[191] Olby 2000 and 2000a in Britain and Thurtle 2007 in America.

question what a ‘scientific’ experiment might be, the chapter concludes that hybridising was seen as experimentation when instrumentalised, in other words, put to use within taxonomy or physiology.

While philosophical botanists attempted to establish philosophical approaches to plant hybridity via university textbooks, concurrently we have seen how rapidly developing communications led to debate. Wider voices were heard through communities formed around early Victorian botanical societies, gardening periodicals and floras. One such debate arose in the 1840s over oxlips, conducted among natural history and gardening periodical communities, and is the focus of the next Chapter.

Chapter 2
‘It really is an important case’: Mock Oxlip, Hybridisation & Transmutation

I. Introduction

In March 1842, Charles Darwin jotted in his ‘Questions & Experiments’ book:

> 5. 7(a) Experimentise on Primrose seeds—it really is an important case—cross with cowslip pollen. —as these are wild varieties. Is any intermediate form found wild [1]

British botanists had been puzzling over the primulas for many years. In his *Zoonomia* Notebook in 1839, Darwin worried: ‘I am bound to insist honestly that the *sudden* change from Primrose to Cowslip is great difficulty.’[2] Darwin’s theory of transmutation by natural selection—later known as evolution—needed gradual changes over eons of geological time. Nonetheless, in the 1842 sketch of his theory, the first example he noted was primrose and cowslip, which experiments had shown were ‘strongly marked races’ of a single, yet very variable species. Wild intermediate forms between primrose and cowslip were crucial evidence of variation occurring in nature, to show that there was no hard and fast distinction between varieties and species, and support his inference that the cowslip and all the intermediate forms had ‘descended from a common stock’, the primrose.[3]

However, many local botanists and gardeners were absolutely convinced that the primrose and the cowslip were not only different species, but also that the widespread in-between form, sometimes known as the common oxlip, was in fact a hybrid. This oxlip resembled the florist’s flower, the garden polyanthus. The primrose, cowslip and polyanthus were three of the most familiar flowers in Britain. [4] In 1833, a Wardian case arrived in Sydney, Australia housing a primrose in bloom, and reminding

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[1] CUL-DAR 206.1 in Barrett *et al.* 1987: 495.
[2] DAR-CUL 124.113 Notebook E in Barrett *et al.* 1987: 428 and on Darwin’s Notebook theorising in relation to Primula see Barrett *et al.* 1987: 401.
[3] Darwin 1909: 4 and 82.
[4] Jacob 1836: unpaginated entry *Primula vulgaris*, Hudson – Common Primrose.

Figure 2.1: Plates of the cowslip (left) common primrose (right), and common oxlip or 'primrose-cowslip' (centre). From: Smith and Sowerby's *English Botany* 3rd ed. v. VII 1867, plates 1129, 1130, and 1131, pp.131 - 133. (Author's collection).

the colonists of home, caused such a sensation that it had to be placed under armed guard.[5] Primroses and cowslips were widely sold in city markets and the polyanthus was 'a good old-fashioned, old English flower'.[6] In the early Victorian imagination, these familiar primula plants powerfully signified Englishness, and an intimate association between Nature and the garden.[7] So Darwin knew that whatever was going on among the Primulas might be an important case to illustrate his theory.

One version of the history of primula studies has the puzzle resolved in progressive stages.[8] First, from 1841-44, plants in a field in the village of Bardfield, Essex, were identified as a species distinct from the more widespread and variable oxlips. The Bardfield oxlip was the 'true' oxlip, the *Primula elatior* Jacquin found on the continent.[9] That prompted discussion over 'mock' or 'false' or 'common' oxlips found elsewhere in Britain, whether these forms may be hybrids. Then, in 1868, primrose-cowslip hybrids were re-made. The experimental hybridist was Charles Darwin, who 'conclusively established' that mock oxlips were hybrids, imposing order on the earlier

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[5] Ward 1854: 17.
[6] Horner 1842: 21; Hall 1839: 71.
[7] A point made by botanists in historical accounts, Richards 2003 and Walters 1993.
[8] Three progressive stages are identified by Preston 1993: 30 probably derived from Robert Miller Christy's (1861-1928) history of primula studies (Miller Christy 1897).
[9] Described in 1778 by Nikolaus Joseph Freiherr von Jacquin (1727-1817), a Dutch Professor of Botany and Chemistry and director of the botanical gardens of the University of Vienna.

'confused' view of primulas.[10] Even historians who have examined the debate over primulas from beyond Darwin's perspective, notably those considering the philosophical botanist Hewett C. Watson's role, neglect to examine the 1840s primula debate independently of what we now know came later.[11]

This Chapter offers a historical revision of the 1840s 'mock' oxlip debate. It argues that this episode illustrates how plant hybrids mattered for diverse and intersecting scientific communities formed around two botanical periodicals. The 'mock' oxlip was not a misunderstood entity waiting for someone to discover its true identity, but instead the plant form was interpreted differently according to what was at stake in calling it a hybrid or not. Examining debates around the mock oxlip also reveals a neglected episode in the history of biology and evolutionary theorising. Religiously motivated practitioners, rather than opposing the plant hybrid, appealed to hybridisation as a conservative alternative to radical Lamarckian development theory. Meanwhile, Darwin and Watson both remained unconvinced about the hybrid oxlip in the 1840s, because at stake were their ideas about transmutation. Far from being responsible for resolving the primula puzzle, Darwin was a rather late convert to the view of farmers, gardeners and local botanists, once he had conducted the hybridising experiments in the 1860s that he had originally planned to do over twenty years earlier.

This chapter traces the mock oxlip through the following three sections: The first section shows how the debate over oxlips connected gardeners, local botanists and philosophical botanists as subscribers of the *Phytologist* and the *Gardeners' Chronicle*. The second section shows how the anonymous publication in 1844 of *Vestiges of Creation,* and the sensation it caused over transmutation, brought out what was normally taken for granted, that hybridisation might explain intermediate forms. Then the third section considers the tension over oxlip hybrids between philosophical botanists on the one hand, and horticulturalists and local botanists on the other.

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[10] Darwin 1868a and Darwin 1876: 55-71. The quote is Miller Christy 1883: 174-5. Most historians discuss *Primula* because their topic is Darwin e.g. Bellon 2012; Stamos 2011 and 2013. For the wider cultural impact of Darwin's Primula studies, see Smith 2006.
[11] The only extended discussion of the 1840s oxlip debate is in Egerton 2003.

Philosophical botanists responded to the Primula debate by seeking to organise and discipline the observations made by farmers, gardeners and local botanists, and local botanists turned to hybridising to investigate their conviction that these intermediate forms must be hybrids. The chapter concludes with some observations relating to the broader aims of this thesis illustrated by the 'mock' oxlip story: about hybridisation and transmutation, and between hybridising and early Victorian science.

II. The 1840s Oxlip Debate

Periodicals Create Intersecting Botanical Communities

West of Cambridge a band of boulder clay drift overlies the chalk, unmarked on geological maps, yet of great significance to the botanist. The calcareous clay, combined with coppicing, created a woodland in which the familiar primrose was almost replaced by another Primula plant, 'the Great Cowslip' in John Ray's 1660 *Catalogus* of Cambridge plants, found 'in Kingston and Madingly woods abundantly and elsewhere' (and still is today).[12] However, cowslips were a familiar wild flower found in livestock pastures, not woods. Even the plant's name dictated its place in nature: Cowslip is a polite form of the Old English cū-sloppe, cū-slyppe or cow muck, as folklorist Geoffrey Grigson put it 'obviously from the conception that the plant sprung up from wherever a cow lifted its tail.'[13] Ray's plant sometimes grew in damp meadows too, and to distinguish it from the similar cowslips, became an oxlip, *Primula elatior* L. Oxlips are a leggy version of the squat primrose (*elatior* means taller) but with flowers closer to the cowslip's umbel. However, oxlip-like plants were also found in many other parts of Britain, local people noticed, where primroses and cowslips met, along woodland edges and field boundaries, and called these *Primula elatior* too.[14]

[12] Ewen and Prime 1975: 98. Babington 1860: 188 and Crompton 2001.
[13] Grigson 1955: 265.
[14] Miller Christy 1897: 172.

Figure 2.2: Herbarium specimen of the Claygate Oxlip from Hewett C. Watson's Garden, 1847, labelled as *Primula vulgaris* var. *intermedia* L.C. (=London Catalogue). From: British and Irish Herbarium, the collections of the NHM, London. (Author's photograph, ©the author, on the advice of the IP Rights Officer at the NHM).

In April 1841, in the first pages of a new botanical monthly the *Phytologist,* Hewett Watson recounted finding an unusual intermediate-looking primula plant in an old orchard at Claygate, Surrey, growing between hoards of primroses and cowslips. He did not draw any definite conclusions and transplanted the primula into his garden (Figure 2.2).[15] Like his fellow philosophical botanists, Watson believed that plant variation was largely induced by climate and soils. If the oxlip still looked the same in his garden, then it might be a permanent form. However, whereas botanists might see this odd-looking, stable form of an oxlip as worthy of a scientific varietal name, Watson suspected that this variation might somehow accumulate to produce a new species.[16]

Watson wrote to Charles Cardale Babington (1808-95) at Cambridge about the Claygate oxlip.[17] Both energetic men in their thirties, Watson and Babington dominated British philosophical botany in the early 1840s. [18] A few weeks later, at a meeting of the Botanical Society of Edinburgh, Babington reported on 'experiments' conducted by 'a highly intelligent Nurseryman and Botanist of Bristol', which appeared to corroborate Watson's observation of this particular oxlip form. Several members commented that the variously named sorts of the primrose and the oxlip—including the intermediate plant described by Watson—were in fact all versions of the primrose.[19] Yet some gardeners disagreed. 'Might not the oxlip (P. elatior) be a hybrid between the Cowslip and Primrose?' asked J.B. Whiting, head gardener at the Earl of Tyrconnel's estate in Yorkshire, in response to reading a report of the Edinburgh meeting in *the Gardeners' Chronicle*.[20] The debate in the *Chronicle* was reprinted in the *Phytologist*, whose editor added 'this is also the opinion of some good botanists.'[21]

The publication in the early 1840s of two new periodicals, the *Gardeners' Chronicle* (1841-) and the *Phytologist* (1841-63), facilitated this debate over the forms

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[15] Watson 1841: 9.
[16] Bellon 2003a explains how philosophical practitioners in the first half of the nineteenth century saw variation around a fixed 'type' of the species.
[17] Babington attended the University of Cambridge with Charles Darwin. For biographical details, see Allen 1998 and 2004' on relations with Watson, which soured by the late 1840s, Egerton 2003: 128-133.
[18] Allen 2004.
[19] Anon. 1841: 645.
[20] Whiting 1841: 205. Whiting was Head Gardener at Kiplin Hall, North Yorkshire from 1835-40. Whiting wrote regularly for the *Gardener's Magazine* until its demise in 1843.
[21] Edward Newman, commenting in Whiting 1842: 205.

of *Primula.* The *Phytologist's* founder and funder, Edward Newman (1801-1876), promoted the paper as 'the medium' for communications between 'field-botanists and the botanical public'.[22] These contributions were, rather unfairly Newman felt, regarded as 'too trifling' for publications with a 'high scientific pretension' yet he believed they were of much interest and valuable, as was shown by the contributions to the *Phytologist* in its inaugural year from leading botanists.[23] The choice of name was also indicative of Newman's vision of local practitioners contributing knowledge to science in their own right, as 'phytologist' was an eighteenth century word for plant taxonomist.[24] While the *Phytologist* sales were initially very low, its ambitions and reach was wider than that limited circulation might suggest.[25] In particular, its readership included several philosophical botanists.

Founded the same year, in 1841, by John Lindley and the famous head gardener and architect at Chatsworth House, Derbyshire, Joseph Paxton (1803-65), the weekly *Gardeners' Chronicle* covered national news as well as articles on 'the science of cultivation'; and at a relatively pricey 6d, engaged with a broad, and not necessarily botanical, audience.[26] Lindley thought that the *Phytologist* might be 'hardly scientific enough for the botanist, nor gossiping enough for the general reader.' Conscious of a highly competitive market, Lindley distinguished the *Gardeners' Chronicle* by its scientific content while emphasising that the *Phytologist's* editor must not encourage

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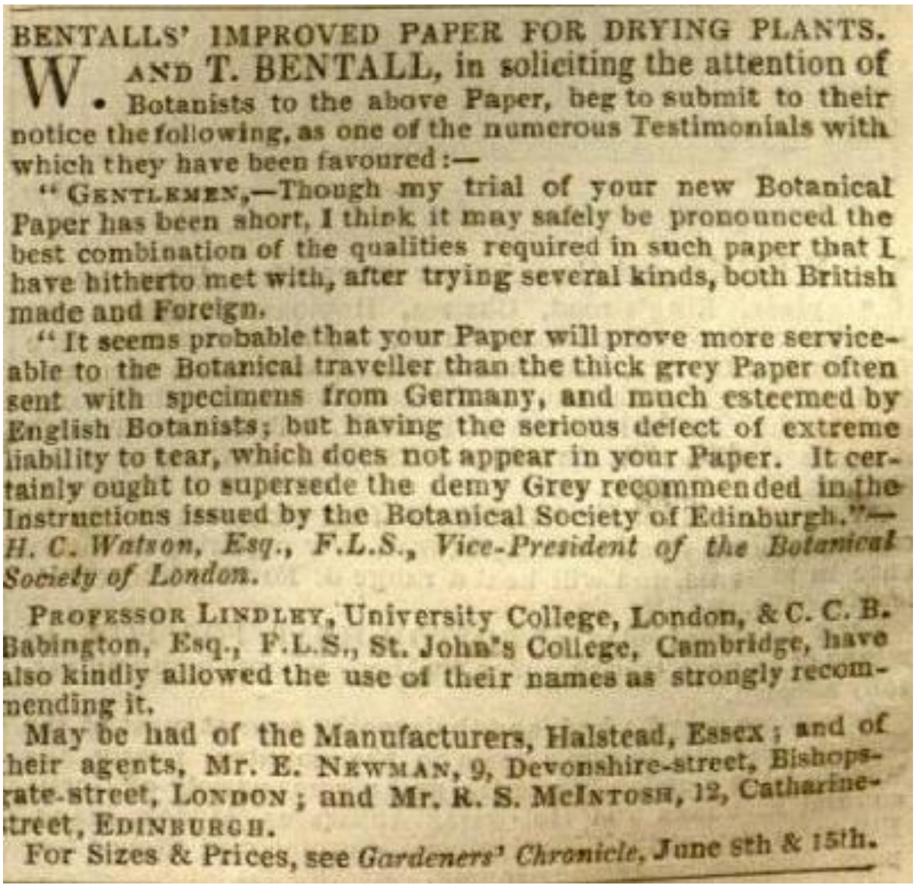
[22] Newman was partner in a printing company, an entomologist, editor of the *Zoologist*, and a natural history author, including of the first best-selling guide to British ferns. On the *Phytologist* see Allen 1996a; Sheets-Pyenson 1981; Wale 2018.
[23] Newman 1844: unpaginated.
[24] According to Easterby-Smith 2018: 111 fn. 135.
[25] Allen 1996a: 115-8.
[26] J.L. [Lindley] 1841: 1. On Paxton, see Kenworthy-Browne 2004.

'the very injudicious practice of multiplying species'.[27] The *Chronicle* carried advertising which revealed the overlap in the readership of both papers (see Figure 2.3).



BENTALLS' IMPROVED PAPER FOR DRYING PLANTS.

W. AND T. BENTALL, in soliciting the attention of Botanists to the above Paper, beg to submit to their notice the following, as one of the numerous Testimonials with which they have been favoured:—

"GENTLEMEN,—Though my trial of your new Botanical Paper has been short, I think it may safely be pronounced the best combination of the qualities required in such paper that I have hitherto met with, after trying several kinds, both British made and Foreign.

"It seems probable that your Paper will prove more serviceable to the Botanical traveller than the thick grey Paper often sent with specimens from Germany, and much esteemed by English Botanists; but having the serious defect of extreme liability to tear, which does not appear in your Paper. It certainly ought to supersede the demy Grey recommended in the Instructions issued by the Botanical Society of Edinburgh."—*H. C. Watson, Esq., F.L.S., Vice-President of the Botanical Society of London.*

PROFESSOR LINDLEY, University College, London, & C. C. B. Babington, Esq., F.L.S., St. John's College, Cambridge, have also kindly allowed the use of their names as strongly recommending it.

May be had of the Manufacturers, Halstead, Essex; and of their agents, Mr. E. NEWMAN, 9, Devonshire-street, Bishopsgate-street, LONDON; and Mr. R. S. McINTOSH, 12, Catharine-street, EDINBURGH.

For Sizes & Prices, see *Gardeners' Chronicle*, June 8th & 15th.

Figure 2.3: Front page of *The Gardeners' Chronicle,* 20 July 1844, p.473, showing advert for botanical drying paper recommended by Watson, Lindley and Babington, indicating an overlapping practice between gardeners and botanists of making herbarium specimens. (Reproduced from the BHL Archive under the Creative Commons Attribution License).

The *Gardeners' Chronicle* also included agriculture from its inception. Indeed, many head gardeners managed estates with farming as the major income source; gardening, the *Chronicle's* prospectus explained, was 'the parent of farming'. It also addressed the forester and the florist, while contributions had been promised by a long list of professors of botany and other philosophical practitioners. [28] This variety of subject matter and sympathies perhaps explained its success in a highly competitive periodical market. The growing popularity of gardening from the 1830s enabled botanical periodicals to persist where other natural history topics failed.[29] Both the *Chronicle* and the *Phytologist* gave cultivators a voice in debating natural history, and a

[27] Anon. [Lindley, J.] 1841: 488.
[28] J.L. [Lindley] 1841: 1. (on agriculture, p.814, p.831; and an example of a review of a Local Flora p.56).
[29] Belknap 2020.

medium for botanists to address cultivators. Further, some subscribers to both these periodicals were also members of the Botanical Society of London or the Botanical Society of Edinburgh, or, indeed, of both societies. Unable to finance their own *Proceedings,* Botanical Society of London members communicated through the pages of the *Phytologist*, their community both shaping, and being shaped by, the periodical's debates.[30]

Hewett Watson, who we met in chapter one, was the driving force behind the Botanical Society of London. He was also aware of the importance of gardening to local botany.[31] Watson built his network of local botanists by distributing his *New Botanist's Guide* to any *Gardeners' Magazine* correspondent known to one of botanists who had supplied plant locality records for the work. [32] Indeed, gardening was how he himself had come to be a botanist: Watson escaped his authoritarian father, and physical pain from a smashed kneecap, with the family's gardener, developing a 'boyish fancy for plants and floriculture'.[33] However, Watson apparently did not manually conduct hybridising himself. Watson's cultivation of his primula plant consisted of what was known as an 'acclimatisation experiment' combined with allowing hybridisation by bees.[34] It seems that hybridisation among garden primulas was so familiar that it was taken for granted that it would take place: 'It is well known that *P. vulgaris* will form hybrids with *P. veris* when they grow intermixed'; in fact, 'the peasantry' planted cowslips with primroses in gardens deliberately to obtain intermediate forms from crossing which 'they cherish as objects of great curiosity from the novel way in which they go to work to procure them.'[35]

The *Primula* puzzle highlighted the knowledge-making overlap between botany and gardening. In the Spring of 1842, once the primroses and cowslips were flowering, the oxlip issue bloomed into intense debate on the pages of the *Phytologist*.

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[30] Allen 1986.
[31] Allen 1965 established Watson's pivotal role.
[32] Anon. 1837a: 597.
[33] Watson 1883: 10 and cited in Egerton 2003: 11.
[34] On the nineteenth-century acclimatisation experiment, see Holmes 2017a.
[35] Bentall 1846: 516.

The Bardfield Oxlip

On 10 February 1842, the meeting of the Botanical Society of Edinburgh heard papers from Watson and Babington, alongside a contribution from the Reverend John Ewbank Leefe (c.1817-89) of Audley End, Essex.[36] The Anglican vicar had found an oxlip-type of plant growing intermixed with primroses and cowslips and could not match it to any illustrations in floras, either in the most detailed British flora, Smith and Sowerby's *English Botany,* or the leading German flora by Wilhelm Daniel Joseph Koch (1771-1849).[37] Perhaps many of the oxlips were local temporary varieties, formed due to local conditions, like the precisely localised circle of fertilizer provided by a cowpat? Professor Henslow commented:

> Let a cowslip be highly manured, and its seeds sown in a shady, moist aspect, and I suspect the chances are in favour of some of them coming up as primroses, or, at least as oxlips. I have had several independent testimonies to the fact of cowslip roots *changing* to primroses; and until proof, by direct experiment, contradict the experiments of Mr Herbert and myself, I cannot help believing that the three species (as they are thought) and the polyanthus are merely races of one species.[38]

William Herbert, the vicar-hybridist we met in chapter one, had made something like garden polyanthus plants by crossing a primrose with a cowslip in 1818. The resulting progeny included an array of forms: primroses with pure white and rhubarb and custard flowers; primroses with flowers growing as umbels on long stalks; cowslips with pale flat flowers; and the bronze tones of polyanthus edging their way into the mix.[39] Henslow admitted that he had 'given very little credit' to this report, until he observed intermediate plants, similar to Herbert's man-made forms, in a Cambridge wood in 1826. A large number of these plants were fertile, therefore Henslow explained these intermediates by holding that the primrose and cowslip were

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[36] Desmond 1994: 422. Leefe collaborated with Watson on his *New Botanists' Guide* (1835-7), providing the section on his speciality, *Salix* L. the willows.

[37] Leefe is one of many examples of a British practitioner reading German botanical monographs. Thomas Henry Huxley (1825-95) knew how well a classical university education, and especially Latin, Greek, French and German, prepared clergymen for scientific work, hence his programme to translate German botanical texts into English (see further chapter three of this thesis).

[38] Henslow 1842: 191. This was still the leading philosophical view of the Primulas, adopted by Charles Lyell in his *Principles of Geology* (1830-32). Lyell cited Henslow and Herbert's experiments with Primulas as evidence of variation, 'dependent on soil and situation', and as evidence contrary to the transmutation of species (Lyell 1833: 34-5).

[39] Herbert 1822: 19 (read 21 December 1819).

conspecific (although he concluded that further experiments were needed).[40] Henslow believed these plants were all of the same species, so the in-between form of the oxlip might be produced somehow *from* the primrose or cowslip or both. Add cow muck, Henslow suggested, and you might get a cowslip, or an oxlip, from primrose seed, or vice versa.

Robert S. Hill (1817-72), a physician from Basingstoke and member of the Botanical Society of London, read the *Gardeners' Chronicle's* report on oxlips.[41] Hill was compiling a local flora of Hampshire and on 21 February asked the *Phytologist's* readers for views on the so-called oxlip called *P. elatior*. Contrary to Henslow, he believed the plant was 'a hybrid production'.[42] On 10 March, an overseas corresponding fellow of the Botanical Society of Edinburgh objected to the opinion that the oxlip was a hybrid.[43] In Switzerland, the primrose did not grow anywhere in the vicinity of stands of oxlip—hybrids were in part identified by their physical position, growing between the putative parent species—and the three plants had a distinct distribution occurring at different elevations. The 'hypothesis of hybridity' meant that a plant's place in the field was important to determining its identity.[44] This also meant that museum-based herbarium taxonomists relied on observations made by local botanists. Contributing to the problem was the fact that the diagnostic characters distinguishing these primulas were largely obliterated in the process of drying.[45] Botanists needed more than the herbarium specimen to study hybridity, notably observations from the field and, as has been largely unacknowledged by historians of science, from the garden. Cultivating was as much a part of botany as travelling, collecting, or classifying.[46]

Then at the Botanical Society of London meeting on 18 April 1842, member Edward Doubleday (1810-49) exhibited a specimen of an oxlip found by his brother,

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[40] Henslow 1830.
[41] On Hill, see Desmond 1994: 342 and Townsend 1883. His letters could not be located at the British Museum (Natural History) archives.
[42] Hill 1842: 188.
[43] Colonel P.J. Brown (1785-1842), of Thun, Switzerland. Brown was author of a local Swiss flora. See brief biographical details in Britten and Boulger 1893: 24.
[44] Hill 1842: 187.
[45] Miller Christy 1897: 199.
[46] A point made by some historians, but excluding plant breeding (see the introduction to this thesis).

Henry, and 'expressed his opinion very decidedly' that the common oxlip was 'nothing more than a hybrid' and that this specimen, an oxlip found in a damp meadow at Bardfield, was 'the true oxlip' species and 'claimed for his brother the credit of first detecting the distinction.'[47]

Where did the Doubleday brothers get their notion that the Bardfield plant was something special? Henry Doubleday (1808-75) and his brother were Quaker businessmen and known in the 1840s as entomologists rather than as botanists, although Henry later would become an inventive experimental agriculturalist.[48] Henry subscribed to the *Gardeners' Chronicle*. He explained that he had read a contribution about oxlips by 'S' in the *Chronicle* on 12 March. 'S' was probably the outspoken head gardener Donald S. Beaton (who we met in the last chapter), already by the mid-1840s a well-known writer on hybridising. Beaton commented on reports of the primula discussions at the Botanical Society of Edinburgh. Beaton agreed with Whiting that the widespread 'English oxlip' was probably a hybrid and that it was the same as the garden polyanthus, because 'we look in vain for a specific character'- in other words, the oxlips were just too variable to be a good species, and for a gardener, that suggested hybridity. The *Primula elatior* of 'the German botanists' was something different and, he thought, not found in Britain.[49] The Doubledays were quick to argue that Beaton was wrong: the Bardfield plant was the *Primula elatior* named by Jacquin. But they did not comment on the identity of the other look-alike oxlips found elsewhere, including Watson's Claygate plant.[50]

The *Phytologist* carried the Botanical Society of Edinburgh's March meeting report alongside reprinting Whiting and Beaton's articles from the *Gardeners' Chronicle.* Watson responded by announcing that the Claygate Oxlip had flowered, 'and now is decidedly a primrose.'[51] However, oxlips elsewhere might not be the same as his plant, so every few weeks additional observations appeared, either for or against

[47] Anon. 1842: 240. For biographical details on Edward see Mays 2008.
[48]For biographical details on Henry, see Mays 2004 and Hills 1976.
[49] 'S' [Beaton, D.] 1842: 171-2.
[50] Doubleday cited in BSL Meeting Report, 18 April 1842 (Anon. 1842: 240).
[51] Watson 1842g: 232.

the hybridity of these oxlip look-alikes.[52] Robert Hill had wanted to provoke exactly that response:

> "The Phytologist" appears to me to offer to botanists, particularly those residing in the country, great facilities for making known any observations they may make relative to the science, and especially with regard to our indigenous plants; and to afford such persons a valuable medium through which they may communicate…[53]

One function of the community this periodical created, then, was to provide multiple witness' testimony. It was 'unsafe to draw conclusions from a solitary instance' Hill explained, asking other correspondents 'to detail any circumstances under which the plant may have occurred to them, with a view to the settlement of the question.'[54]

In May 1844, Babington and Henslow made a pilgrimage to see the Bardfield Oxlip and, happy that it had maintained its form and so was a good species, Babington suspected that the other 'spurious' oxlips elsewhere were 'probable' hybrids.[55] The mild-mannered and exacting Babington was well-known for his careful attention to the latest developments in continental plant taxonomy and he most likely drew on the French Professor of Natural History Dominique Alexandre Godron's (1807-80) reports of a similar *Primula* from Lorraine.[56] Elsewhere on the continent, Babington explained, the hybrid was 'almost unknown' because the primrose and cowslip grew in separate 'districts', not coming into contact like they did in Britain.[57] By summer 1844, Newman boldly announced to the *Phytologist's* readers that the identity of the Bardfield Oxlip was settled, it was a continental species, and the other oxlips elsewhere were 'nothing more than a hybrid, or a casual variety.'[58]

Gardeners in Tension with Botanists over the Mock Oxlip

The oxlip story demonstrates the interaction between the botanical communities formed by two participatory periodicals, contrary to historical accounts which

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[52] E.g. 'S' [Beaton, D.] 1842 was in favour; and Moxon 1842 was against.
[53] Hill 1842: 188.
[54] Hill 1842: 188.
[55] Babington 1844: 1018.
[56] On Babington and continental taxonomy, see Allen 1998. Godron believed that all intermediate Primula forms were hybrids (Godron 1844: 21). For biographical details on Godron, see Roberts 1929 and Stevens 1994.
[57] Babington 1844: 1018 and Babington 1843: 241-42.
[58] Newman 1844: 997.

traditionally do not see much overlap between gardeners and local botanists, or portray a unidirectional influence from science on to gardening and plant breeding.[59] David Allen has gardeners and naturalists as 'largely separate communities' to explain why many natural history periodicals failed 1830-60 while gardeners' journals flourished.[60] However, that angle obscures the fact that naturalists like the Doubledays were reading the gardeners' papers. Conversely, gardeners like Whiting felt they had contributions to make to natural history. The disagreement over Primulas and hybridity helped generate a community, and served to promote the *Phytologist* to the readers of the *Gardeners' Chronicle*.[61] The *Phytologist* endured because it gave voice to the intersection of the farmer, gardener and local botanist communities and its format encouraged controversy, even where opinions conflicted with the editors' own views (as a Quaker, Edward Newman believed in encouraging reasoned debate).[62]

This episode in primula studies from 1841-44 also shows that the interactions between those interested in hybrid plants were complex and reciprocal. Philosophical botanists held different views about plant hybrids. While Watson was sceptical, and Darwin intrigued, others like Henslow were certain that intermediate forms were caused by the effects of soils and climate and might be evidence that a pair of species like the primrose and cowslip were conspecific. By contrast, many gardeners and plant breeders were convinced that intermediate forms were hybrids.

In many ways, the identity of the Bardfield Oxlip was a side issue. The main debate remained over the widespread common or 'mock' oxlips, the putative primrose-cowslip hybrids. This debate could not have taken place without the interaction between botanical communities, facilitated by the diverse and intersecting readership of the *Phytologist* and the *Gardeners' Chronicle*. Then, just as the matter seemed settled, a new situation gave impetus to the primula puzzle, the uncertainty

[59] Horticultural periodicals were of 'considerable significance in natural history' (Dawson and Topham 2020: 17). However, animal-centred historiographies tend to emphasise the conflict between breeders and science, e.g. Harriet Ritvo has animal breeders emulating scientific nomenclatural practices, but sees little cooperation between zoologists and breeders (Ritvo 1987, 1997, 1997a). Instead, Jim Secord's knowledge exchange approach is closer to that taken in this thesis (Secord 1981 and 1985).
[60] Allen 2001: 186. See also Allen 1996, 2010 and Belknap 2020.
[61] The *Phytologist* survived through another series and was essentially replaced by the *Journal of Botany: British and Foreign* in 1863 (see Allen 1986: 77-9).
[62] Secord 2000: 454-5. On Quakers and science, see Cantor 2005.

surrounding the publication of a sensational anonymous book claiming that species transitioned from one form to another, *Vestiges of Creation.*

III. The *Vestiges of Creation* and Plant Hybridity, 1844

Hewett Watson, *Vestiges* & *Primula* Transmutation

Watson had, until now, remained silent about hybridity. The irritation in his note of 25 May 1844 was palpable: 'The correspondents of "The Phytologist" appear to be still feeling an interest in the subject of oxlips' he began, so here was an opportunity to remind everyone of his view, and to announce his own experiment, 'which so far tends to negative the supposition of hybridity', but the results would have to wait.[63] In November 1844, thrust into these discussions about Primulas, came the publication of *Vestiges of Creation*.

It was rumoured that the author of *Vestiges* was Watson himself (along with many other high-profile men of science). He was unimpressed, as although the book was a good read, 'the botanical part is quite a failure'.[64] He reviewed it for the *Phytologist* in a series of four articles, setting out the detailed botanical evidence that the author should have marshalled to support the case for transmutation of species.[65] By April 1845, the *Annals and Magazine of Natural History* (co-edited by Babington) had not published a review, nor had several of the major Quarterlies. Given this situation, the *Phytologist* was not an obvious choice for a highly detailed scientific critique.[66] Therefore it seems likely that Watson intended to communicate to those readers who he wished to influence, the local botanists reading the *Phytologist* and contributing specimens to the Botanical Society of London's annual 'distribution' (in 1838-9 already 18,592 specimens exchanged between members). The distribution

[63] Watson 1844g: 1001-2. This thesis uses the same date-lettering as Egerton 2003, for ease of comparison.
[64] H.C. Watson to Prof. Balfour, 9 March 1845 (cited by Egerton 2003: 154). This account of Watson's reaction to the *Vestiges* largely follows the detail in Egerton's biography (Egerton 2003: 150-157).
[65] Watson 1845.
[66] A point made by Secord 2000 but without elaborating on why Watson chose the *Phytologist*.

directly supplied Watson's own project collecting 'the complete series of forms' between pairs of species.[67]

Watson's review considered the botanical and geological (including paleobotanical) evidence. He argued that there were pairs of species (cowslip and primrose were among many others) which had an array of intermediate, graduated forms, suggesting that the species were unstable and one form might turn into the other when the environmental conditions altered. He concluded: 'These and other facts, point towards the conclusion that varieties may gradually become species.'[68] He believed that cultivation experiments might demonstrate that a variety could transition into a species within a few generations. Unlike Darwin, and more like the French Jean-Baptiste Pierre Antoine de Monet, Chevalier de Lamarck's (1744-1829) theory of species progressive development, Watson focused on showing that there was botanical evidence at least suggesting irreversible change in species morphologies, growing in a state of nature. Calling cowslips and primroses the same species provided an unsatisfactory taxonomic solution to what Watson saw as a physiological problem.[69]

However, overall, Watson concluded his review of *Vestiges* cautiously, stating that more experiments were needed. Straight after his lengthy review of *Vestiges* concluded in print, in April 1845, Watson was back in his garden with his primulas.

Watson had grown on 88 oxlips from seed, raised from the original single plant collected four years earlier. He had produced true cowslips, cowslips 'passing to' oxlips, oxlips, stalked primroses, true primroses and a few non-flowering specimens. Nothing looked anything like the Bardfield Oxlip, so at least it was clear that the Essex plant was a separate species altogether. Watson felt his results suggested that cowslips and primroses were the same species, but that many botanists would reject this given that these plants looked so dissimilar. Yet he argued that was the preferred

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[67] Distribution figures from *Proceedings of the Botanical Society of London* 1839.
[68] Watson 1845: 228.
[69] Egerton 2003: 151.

option and suggested that other 'frivolous attempts at species-making' in the brambles (Rubi) might need reconsidering.[70] However, it was problematic because:

> If we allow the cowslip and primrose to be two species, and yet allow that one can pass into the other, either directly or through the intermediate oxlip, we abandon the definition of species, as usually given, and fall into the transition-of-species theory, advocated in the 'Vestiges'.[71]

Watson apparently rejected the step-wise theory in *Vestiges* that smacked of French radical Lamarckism, although given that he had publicly avowed a pro-transmutationist view in 1836, it is likely that he was most affronted by *Vestiges* 'unphilosophical' presentation of the topic.[72] However, he also stressed that:

> I do not see that we get more clear of the difficulty by assuming, without proof thereof, that the 'Claygate oxlip' is a true example of hybridity. Do hybrids, if fertile, produce at once their own like, the like of each parent, and a progeny of intermediate likeness also? At best, the hybrid is only half of either species, and can the half produce the whole? Such an event would assuredly not be 'like producing like' through an endless succession of descents? [73]

Watson distanced himself from the 'unphilosophical' *Vestiges*, but equally so from hybridity.[74]

Watson's Primula paper was reprinted in the *Gardeners' Chronicle*. One florist and naturalist responded, William Marshall (1815-90), a solicitor from Ely, prize-winning breeder of carnations and later a contributor to Babington's 1860 *Flora of Cambridgeshire*.[75] Marshall repeated Watson's experiment with a cowslip and an oxlip producing 60 seedlings which remained like the parent plants. As an experienced florist, he knew how to take precautions against crossing. 'Would not this experiment, which can be attested by others as well as myself, lead to the conclusion, that there

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[70] Watson 1845h: 219.
[71] Watson 1845h: 219.
[72] Watson 1836.
[73] Watson 1845h: 219.
[74] See Secord 2000 and Egerton 2003 on Watson and his 1836 pro-transmutationist views. Jim Secord shows how, at a time when texts like George Combe's 1828 *The Constitution of Man* might be read as supporting transmutation, authors were explicitly hostile to any reading going so far as to favour the ideas of 'French revolutionary ruffians.' (Secord 2000: 74). Watson carefully navigated the topic by defending Combe's book as not denying a role for a Creator while also claiming that science might show that species were not fixed (Watson 1836).
[75] On Marshall, see Desmond 1994: 470 and Anon. 1842a. Marshall contributed 'many localities of plants in the Isle of Ely' to Babington's Flora of Cambridgeshire (Babington 1860: iv-v).

must have been a 'hitch' somewhere in the recorded experiments of Professor Henslow and Mr Watson? I do not pretend to suggest where.'[76] Marshall believed that Watson's plants were hybrids. Watson retorted that this experiment meant nothing. Marshall could have sown one of the Bardfield oxlips, which comes true from seed; he would have known about this plant, Watson sniped, if he were a member of the Botanical Society of London.[77]

This thesis argues that Watson remained unconvinced about primula hybrids, contrary to historian Frank Egerton's view. Egerton states that Watson eventually 'found what he had suspected all along – that Primrose and Cowslip produce a hybrid similar to, but distinguishable from, Oxlip.'[78] However, Watson never stated that he was convinced that the Claygate plant, or the common oxlips found elsewhere, were hybrids; he was open to the possibility, but repeatedly refuted this claim when it was made by others, as unphilosophical without further proof. We will return to this point, that the expected epistemological behaviour of a philosophical practitioner demanded caution about plant hybridity, in chapter three of this thesis.

In 1848, Watson concluded that the forms produced by his experiments were 'oxlips' but this might include varieties or hybrids: 'I employ the term *oxlip* to designate the intermediate form, because it is undoubtedly to that variety of primrose or cowslip, or hybrid of both, that rustics apply the name.'[79] The 'hybridization hypothesis' he continued, might still be 'negatived' by further experiment.[80] Second, the same experimental results could support either a hypothesis of hybridity <u>or</u> of transmutation. Watson was committed to a form of transmutation from at least 1836.[81] He also presented the Primulas as a key example supporting transmutation in his review of the *Vestiges.* Therefore, Watson had two reasons to remain unconvinced about primula hybrids, especially fertile hybrids. In short, some historians have been too quick to assume that Watson, a non-believer, must therefore have accepted plant

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[76] Marshall 1845: 285.
[77] Watson 1845h.
[78] Egerton 2003: 152 citing Watson 1845h and 1848n.
[79] Watson 1848n: 148.
[80] Watson 1848n: 149.
[81] Egerton 2003.

hybridity.[82] This illustrates Ian Hesketh's point in the introduction to this thesis, that religious explanations for the Victorian period can unhelpfully distract historians and obscure a more complex reality.

Several botanists reading the *Phytologist* also realised that hybridity might counter Watson's claim that intermediate forms comprised evidence for transmutation. Surprisingly, therefore, given the traditional historiography portraying British botanists as hostile to hybrids, several religiously motivated botanists advocated hybridisation. In the next subsection we see how both local and philosophical botanists appealed to hybridity, to 'defend' botany from the 'trash' of *Vestiges* and Lamarckism.[83]

Hybridisation Versus 'Other Hypotheses of a More Startling and Improbable Aspect'

In the spring of 1845, cotton mill owner Edward Wilson (d. 1846), from Congleton, a Pennine town near Manchester, read about primulas alongside Watson's review of the *Vestiges* in the *Phytologist*.[84] Six months later he noted that no one else had taken up the primula question. He pointed out that Watsons' primula results could equally be explained by his plant being a hybrid. He did not experiment himself, not being a gardener, but was surprised other botanists did not, as hybridising would sort out the primula problem. The oxlip from his own neighbourhood, he believed, was 'definitely a hybrid.'[85] This was more of a personal challenge than Wilson perhaps realised, as Watson had grown up in Congleton. Wilson was convinced that the existence of hybrids (in animals as well as in plants), including fertile hybrids, was consistent with 'the received view of species': that species are fixed and do not transmutate, but instead vary within certain limits:

> a hybrid is not a species, nor is it, properly speaking, a variety of any one species. It is a variety compounded of two species, from neither of which is it separated by more than the usual range of variation. Now, if we were to speculate, a priori, on the probable offspring of such a being (having respect to the two grand laws of

[82] Egerton is probably swayed by David Allen's historiography on plant hybridity. Interestingly, botanist-historian Clive Stace had already noticed that Watson was unconvinced about hybrids (Stace 1975: 20).
[83] Newman 1845: 149. On Newman's high-profile opposition to *Vestiges* on religious grounds, see Secord 2000: 453-5.
[84] Biographical details in Britten, Boulger and Rendle 1931: 330.
[85] Wilson 1845: 377.

> generation,-that like produces like, within certain limits,-and that varieties tend to revert to their original types; assuming also that the parental influence might be unequally distributed through the reproductive system of the hybrid), should we not say that it would consist of precisely such a series of forms as that obtained by Mr Watson?

Surely, Wilson thought, Watson's plants were hybrids.

> …Now I admit that all of this is purely hypothetical. Still, if not be contradicted by facts, I think it may fairly be weighed against other hypotheses of a more startling and improbable aspect.[86]

The other 'startling' hypotheses were 'either that one species may generate another', or that 'the primrose and the cowslip are of one and the same species.' These two alternative hypotheses were equally problematic: the first, transmutation, 'implying that there is no such thing as a species, in the strict sense of the term'; the second, making two forms that look so different conspecific, 'that if there is such a thing as species, no assignable amount of outward character will suffice for its determination.'[87] At this time Wilson was thinking of Lamarckism and the transmutation of the *Vestiges*, distinct from the later gradualist alternative still confined to Darwin's notebooks and private essays. Wilson argued that accepting the hypothesis of hybridity was the rational (and therefore scientific) way to resolve this dilemma.

Other *Phytologist* correspondents responded to the view that Watson's oxlips were hybrids. William Wilson (1799-1871) read Edward Wilson's comments and, he explained, these prompted him to write in with a comment 'On the Progressive Development of Species'—drafted before he had even read *Vestiges*—because 'too much has been conceded to the transitionists.'[88] Wilson was, like his namesake, a devout Congregationalist; he was also a close friend and collaborator of William Jackson Hooker, the Director of the Royal Botanic Gardens, Kew.[89] Echoing Edward Wilson, he felt that transmutation would mean surrendering two fundamental principles: that species can be determined by permanent characters; and that the variability and changes in plants were simply the natural result of climatic and other

[86] Wilson 1845: 378-9.
[87] Wilson 1845: 378-9.
[88] Wilson 1846: 447. William Wilson was not related to Edward Wilson.
[89] On Wilson, see chapter one of this thesis, p. 60.

conditions. Discarding these two principles, he argued, was irrational because it inevitably led to sweeping scepticism about the basis of taxonomy, and by extension, all knowledge:

> It is surely the most rational course to assume that an intelligent, creative power, in benevolence to rational creatures, would not so constitute existing races of animals and plants as to elude recognition after a certain lapse of time, and thus impress a character of fluctuation and uncertainty upon all the works of Creation, even upon man himself as a *species*, and by lamentable consequence, upon his intellect also, rendering it (I may say) impossible to know anything, according to the doctrine of the ancient sceptics, whose tenets seem only to be revived in another shape by the transitionists.[90]

William Wilson extended the primula debate to encompass the intellectual nature of Man himself and of the moral nature of God. In common with many other readers, Wilson's response to *Vestiges* was centred on its affront to human and divine dignity, rather than an insistence on special Creation and species fixism.[91] Wilson was also an example of a religiously motivated critic of transmutation who pointed to hybridisation as an alternative, naturalistic account of intermediate forms. He was also protecting botanical science, he explained, from what he regarded as morally dangerous scepticism and a radical French theory.[92]

William Wilson's religious views led him to rational arguments in favour of hybrids. For both Wilsons, as evangelical Christians, hybridisation was preferable to transmutation. William Wilson was concerned about the philosophical basis of the science of taxonomy, but even among the religiously motivated, this objection did not arise from a naïve need to see species as fixed; rather from a sophisticated understanding of the purpose of taxonomy. Classification might reconcile developments in science with scripture, 'a theology of nature': species could vary, but as Edward Wilson highlighted, species had to be definable entities by which the Creation had been ordered by God.[93] This was a rational attempt to retain an order in

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[90] Wilson 1846: 446.
[91] A point made in Secord 2000.
[92] Wilson 1846: 447.
[93] On 'a theology of nature' in the 1830s and 1840s and its wider role in mediating science to wider publics, see Topham 2022 (forthcoming).

Nature. The threat of developmental transmutation was, for William Wilson, as much a threat to the reputation of a science of botany, as to his faith.[94]

Other botanists had also made a connection between hybridisation and transmutation and its investigation by garden hybridising: For example, Isaac Anderson-Henry, the Scottish horticulturalist we met in the introduction to this thesis, later explained that his hybridising was partly to make new garden plants, and partly motivated by scientific theorising: looking back to the 1840s, he later claimed that he was 'convinced of the truth of the Lamarckian doctrine of development' (he was later an ardent Darwinian supporter).[95] However, while Lamarckism had some support from horticulturalists, there was apparently little interest in William Herbert's so-called hybrid theory: that hybridisation was the agent of species formation. This hybrid theory was dismissed in several general horticultural texts, and during the 1840s and 1850s, largely ignored elsewhere.[96] For example, G. W. Johnson's *The Principles of Practical Gardening* (1845) commented that if hybrids had been formed as much as Linnaeus (and Herbert) intimated, the continual blending of offspring would have meant that we 'lost all traces of genus and species long ago.'[97] This logic drew on the prevalent view of heredity of the day, that offspring exhibited a blended mix of both parents' characteristics.

We have seen how the anonymous publication in 1844 of *Vestiges of Creation,* and the sensation it caused over transmutation, brought out what was normally taken for granted, that hybridisation might explain intermediate forms. Religiously motivated practitioners, both philosophical and local botanists, appealed to hybridisation as a conservative alternative to transmutation. By contrast, in the next subsection we see how Watson remained sceptical about hybridity and was determined that the readers of the *Phytologist* should not be misled into thinking that his oxlip was undoubtedly a hybrid.

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[94] A point supported by Secord 2014.
[95] Anderson-Henry 1867: 123 and Gorer 1970: 185.
[96] While Ernst Mayr was grossly biased against hybridisation as a process in evolutionary thought (O'Malley 2010), his observation that Herbert's version of hybrid theory was largely ignored stands supported. By 1859, Charles Darwin, could dismiss Herbert's theory as old fashioned (Mayr 1982). However, Mayr failed to distinguish the Linnaean hybrid theory from later versions that were incorporated into a heterodox Darwinism by some local practitioners (see chapter three of this thesis).
[97] Johnson 1845: 203.

Experiment Appears to Confirm the Progressive Development Theory

In 1847, Watson responded to the mounting number of claims that the Claygate oxlip must be a hybrid. Watson countered that his plant could not be a hybrid form because any form of crossing produced blended progeny, so hybridisation would never generate a pure primrose or a pure cowslip form, like those Watson found growing among the offspring of his cultivation experiment.[98] Darwin also shared exactly this line of reasoning when discussing the primulas in his 1844 essay.[99] Hybridity could explain many of the facts of the case, but, crucially, not all. Above all, Watson stressed that extensive and careful crossing and cultivation experiments were needed, to address this and other 'great problems of science' in descriptive botany and in physiology: 'In physiology, there are unanswered questions of much interest and real importance, connected with the laws of hybridity, hereditary transmission of peculiarities, and transmission of forms and qualities'.[100] Interestingly, this was the only explicit mention of heredity in all the discussions about primula hybrids; the focus instead was on variation and transmutation, and the implications for the science of taxonomic botany. Watson concluded that it was 'inevitable' that primrose and cowslip comprised variable forms within the same species, no matter how much botanists might dislike that verdict.[101]

A few weeks later in June 1847 another local botanist objected to Watson's verdict. A friend of Edward Wilson's (they visited the Bardfield oxlip together in 1843), Joseph Sidebotham (1824-85) was a Manchester textile magnate and a prominent naturalist in his city. [102] Sidebotham cautioned that the idea that primroses and cowslips were the same species, 'gives a good handle to the supporters of the progressive development theory.' Surely, Sidebotham argued, these other oxlips and the intermediate forms were hybrids?[103]

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[98] Watson 1847c.
[99] Darwin 1844: 129.
[100] Watson 1847c: 854.
[101] Watson 1847c: 854.
[102] The evidence that Sidebotham and Edward Wilson were friends is from an herbarium specimen of the Bardfield Oxlip in MANCH which lists them as collecting the specimen together. On Sidebotham, see Cook 2015.
[103] Sidebotham 1846: 887.

Watson responded by again supplying primula herbarium specimens to the Botanical Society of London, this time for its distribution among members in January 1848. Watson was attempting to quash the primula dissenters: these specimens were not hybrids but instead 'sufficient to show that there is a gradual transition from genuine primroses to genuine cowslips. Even those botanists who refuse faith in the carefulness or exactness of the experiments on record, may see with their own eyes that the intermediate links do exist.'[104] There were no gaps between the forms suggesting two distinct species, so that meant that the intermediate forms could not be hybrids. However, Watson now back tracked and was 'reluctant' make primroses and cowslips conspecific, as including so much variation within a single species would throw into doubt the 'thousands of supposed species as they now stand.'[105]

The closest Watson came to recognising a hybrid was in the toadflaxes (*Linaria* L.), a genus in which he had first observed a series of intermediate forms running from *L. repens* (L.) Mill. to *L. vulgaris* Mill. in October 1841. Watson did not suggest hybridity as an explanation, instead mentioning this as an example where the two species might be sunk into one and described himself as 'a lumper of species'.[106] Later in 1848, Watson considered the possibility of the Linarias producing hybrids. [107] Yet Watson also used the Linarias as a prime example of the botanical evidence for transmutation in his review of *Vestiges*. There was a reason for him to leave the matter open. Meanwhile, Babington repeated his comment about the spurious oxlips being probable hybrids in the second edition of his *Manual* of 1847: 'probable that hybrids are formed between this [cowslip] and *P. vulgaris* [primrose], in England, which are mistaken for *P. elatior* [Bardfield Oxlip].' [108] Babington's 'probable' here shows that hybridity was still a hypothesis, as philosophical botanists—and indeed, as we have seen, the *Phytologist's* community—agreed that experiments were needed to verify a plant as a hybrid. However, he now added that he believed that most of the reported Primula hybrids, comprising what looked like 'the flower of a primrose placed on the

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[104] Watson 1847a.
[105] Watson 1848d: 44-5.
[106] Cited in Egerton 2003: 131. Possibly the first use of 'lumper' (see further chapter three of this thesis).
[107] Watson wrote: 'your hybrid Linaria is in all likelihood the same as my L. Bauhinii; and if hybrid, we should expect some differences, as in fact do exist in the Cornish, Hants, Cork and Swiss specimens.' (Bromfield 1849: 625).
[108] Babington 1847: 257.

stalk of a cowslip', were umbellate varieties of the primrose.[109] At one extreme, William Arnold Bromfield (1801-51) used his manuscript of a flora of Hampshire (written between 1845 and 1849) to announce the Primulas 'a stumbling-block and a warning' to those who would name species on 'minute characters' when plants which looked so different as the primrose and cowslip were shown, by 'applying philosophical principles', to be the same species.[110] Yet Watson could not go so far, given that he needed primroses and cowslips to be maintained as distinct species if these might provide evidence of transmutation. In 1848, he decided that 'the hybridization hypothesis' in the Primulas was an open question but might be negated by one crucial experiment showing that a primrose could produce a cowslip, or vice versa, without the intermediate oxlip stage; in addition, hybridity could not account for all of the results of his experiments.[111]

In 1849, Joseph Sidebotham published results of his own primula experiments in the *Phytologist* which were 'in imitation of those published by Mr Watson and others, but on a much larger scale, and with such precautions that the result might be relied on.' Sidebotham favoured hybridity as an alternative to the unwanted progressive development theory, but his results 'disappointed' him, as 'however unwillingly acknowledged, such transition appears really to take place.'[112] Watson responded to this public criticism of his methods in his compendium of geographical records of British wild plants, the *Cybele Britannica* (1852), rejecting Sidebotham's experiments as unreliable.[113] Waston held that the Primulas were *either* hybrids or varieties, which at most suggested the primrose and cowslip were conspecific.[114] The philosophical scientific community later would reject Watson for such displays of ungentlemanly rudeness, but in the meantime Watson apparently ostracised Sidebotham from science, and closed down the oxlip debate.[115]

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[109] Babington 1847: 257; Coleman and Webb 1849: 234 citing Babington's guidance in their Preface, p.ii.
[110] Bromfield 1849: 399.
[111] Watson 1848n: 148-9
[112] Sidebotham 1849: 705.
[113] Watson 1852: 488.
[114] Watson 1852: 488.
[115] Babington credited Watson with resolving the primula debate (Babington 1864) whereas some historians credit this to Babington himself (e.g. Preston 1993). Sidebotham was refused membership of the Manchester Philosophical Society and instead set up the Manchester Field Club in 1860. Jim

Some historians suggest that Sidebotham's experiments were disregarded—there were even allegations of fraud relating to his entomological work—because he was an amateur in the increasingly professional scientific world.[116] However, as we discussed in the introduction to this thesis, these professionalization pressures have been over-emphasised. Personal animosity almost certainly played a part (Sidebotham had publicly rowed with Watson the year before). Watson's status was grounded on his philosophical reputation and his independent means, which he cherished to the point of refusing paid employment despite his gentlemanly poverty.[117] By contrast, Sidebotham was a wealthy all-round naturalist and collector, whom Watson regarded as an impertinent dabbler in botany.[118] Although in shaping science, bourgeoise industrialists did provoke snobbism from scientific elites, in this case part of Sidebotham's problem was that he was not known as a dedicated, and therefore competent, botanist.[119] Newman had also faced similar hostility on publishing his fern book in 1840. No amount of scaling Scottish peaks in the hunt for ferns would make Newman a botanist, philosophical or otherwise; 'the entomologist Newman', the President of the Botanical Society of Edinburgh remarked, 'cannot be expected to have botanists follow him.'[120] In the developing socio-scientific hierarchies among naturalists of the mid-century, the emerging botanical community in Britain was ordering itself around a division of labour reflecting a dedication to plants, and shunning those with even a hint of the collector or all-round hack.[121] Sidebotham had also betrayed his motivations rather too clearly. He had stated that he was looking to confirm his view that the plants were hybrids, and that lack of humility marked him down in the eyes of even those philosophical practitioners who wanted to believe the results of his work.[122]

Endersby argues that Watson had lost his philosophical reputation by 1860, for persistent ungentlemanly rudeness (Endersby 2008: 260).

[116] Cook 2015 erroneously assumes Sidebotham's primula experiments aimed to investigate inheritance.

[117] Egerton 2003.

[118] See also Egerton's take on the Watson-Sidebotham dispute (Egerton 2003: 142-3).

[119] Kargon 1977. Watson 1852: 489.

[120] Grenville [1840] 1844: xix.

[121] Alborn 1996. Martin Rudwick maps the hierarchies of competence among 1830s geologists (Rudwick 1985) and a similar approach stratified entomologists in the 1840s and 1850s (Wale 2018).

[122] Darwin tabulated Sidebotham's results in his *Natural Selection* M.S. but stated that these were not commenced 'in a sceptical frame of mind' (Stauffer 1975: 131).

The sensation over the *Vestiges* brought out what was normally taken for granted, that hybridisation might provide an alternative explanation of intermediate forms in place of transmutation. However, while the oxlip debate was apparently over by 1852, at least in the pages of the *Phytologist*, it is worth bearing in mind that hybridity remained connected to transmutation. For example, Henslow's lecture notes for the mid-late 1850s included: 'Cross fertilisation and its use. Hybridization. Fertility and sterility of hybrid plants. Supposed change of one species into another.'[123] However, a hypothesis of hybridity could only be tested by facts collected by local botanists and gardeners. The mock oxlip served to emphasise the interdependence, and social tension, between gardening and botany. For Victorian philosophical botanists that was an uncomfortable situation. Indeed, as Jim Secord reminds us, the status of those producing botanical knowledge for science or for commercial gain, or both, was 'at maximum flux in the 1840s…for all groups, there was everything to play for'.[124]

In the next section, we see how some philosophical practitioners responded to this situation by attempting to discipline and organise gardeners and local botanists.[125] Firstly, Henslow tried to direct the 'practical men' away from testimony and towards 'experiments'. Secondly, Watson set a 'structure' or framework for what local botanists should be finding, observing and exchanging, in the process codifying the hybrid as a garden production, and therefore unimportant to science. However, local botanists persisted in calling intermediate forms hybrids and set about testing their taxonomic claims by hybridising.

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[123] Pamphlet titled 'Botany Examination Leaflet' for the Natural Sciences Tripos at Cambridge, pasted into the front flyleaf of Henslow's personal copy of his 1853 published syllabus for 'An Examination in Botany'. It is undated but refers to a 'William Bateson' - probably William Henry Bateson (1812-81), Master of St John's College, Cambridge and the father of William Bateson (1861-1926), the early geneticist. The document probably dates to the mid-1850s and Henslow's teaching for the following reasons: first, its content mirrors that of the 1853 text, font and printing with some additions; and the content mirrors Henslow's handwritten lecture notes for a course of 20 lectures on botany delivered in 1860, the year before he died.

[124] Secord 2000: 405.

[125] Sam Alberti shows that university professionals used this strategy to engage amateur naturalists in Yorkshire from 1875-1899 (Alberti 2000, 2001, 2003). In botany, this approach apparently originated in the 1840s.

IV. Responses to the Primula Puzzle

Henslow's *Cohors Botanicorum*

In 1849, Henslow wrote into the *Phytologist* to suggest a way forward with the primulas. He admitted that 'positive testimony' was needed to prove that the oxlip was a hybrid (reports from gardeners were insufficient) or, alternatively, to affirm his view that oxlips were evidence of primroses and cowslips being conspecific.[126] He suggested something that gardeners frequently practised, a seed exchange, a '*Cohors Botanicorum'*. [127] The seeds would be used for the 'special purpose' of 'a carefully-conducted series of experiments made with reference to specific identity' and an 'accurate record of the results' to be published in the *Phytologist* as this would save 'some ink and no little discussions.'[128] As he had called for a community of horticultural hybridists in 1837, Henslow likewise felt local botanists needed a degree of tactical military organisation.[129] In 1841, Henslow published a paper in a gardener's journal on a new 'hybrid species' made by the curator of a botanic garden, from crossing Mexican climbing snapdragons *Lophospermum scadens* and *L. erubescens* (Figure 2.4, overleaf). Henslow wrote this publication, like his *Potentilla* paper from 1834 (which we discussed in chapter one), aiming to educate gardeners about science:

> At present no one knows with certainty what are the true limits to the variations in forms which any one species may assume; and it is impossible to foresee whether multiplied observations on hybridizing may not lead us to some law of vegetation by which a botanist may be able to pre-determine the possible limits of every species.[130]

He called to gardeners to work with local botanists and to keep records of their hybrids in a tabular format so that we may see at a glance to what extent the hybrid resembles, or differs from, both parents'. Further, if 'we were in possession of some hundred comparisons of this kind (and the more minute the better), we might then, possibly, be able to detect some general law by which the production of hybrids and

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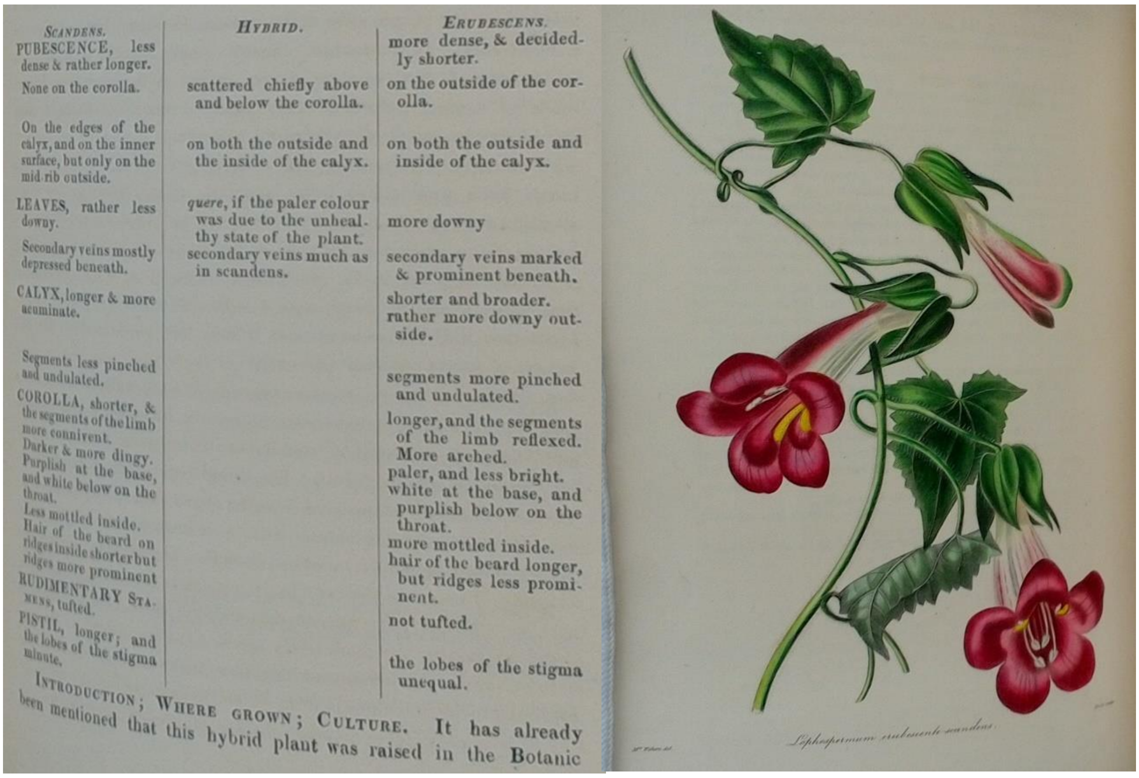
[126] Henslow 1849: 652.
[127] The *cohors* was the basic Roman infantry tactical unit of between 300 and 800 men, depending on the period; a legion comprised 10 *cohortes* (Keppie 1998).
[128] Henslow 1849: 652.
[129] See chapter one, p. 53.
[130] Henslow 1841: folio 242 [unpaginated].

the limitation of species is governed...'[131] Henslow's interest in hybridity was consistently in relation to what it might reveal about taxonomic relationships and the physiology of how the species might be kept distinct.[132]



SCANDENS.	HYBRID.	ERUBESCENS.
PUBESCENCE, less dense & rather longer.		more dense, & decidedly shorter.
None on the corolla.	scattered chiefly above and below the corolla.	on the outside of the corolla.
On the edges of the calyx, and on the inner surface, but only on the mid-rib outside.	on both the outside and the inside of the calyx.	on both the outside and inside of the calyx.
LEAVES, rather less downy.	*quere*, if the paler colour was due to the unhealthy state of the plant.	more downy
Secondary veins mostly depressed beneath.	secondary veins much as in scandens.	secondary veins marked & prominent beneath.
CALYX, longer & more acuminate.		shorter and broader. rather more downy outside.
Segments less pinched and undulated.		segments more pinched and undulated.
COROLLA, shorter, & the segments of the limb more connivent.		longer, and the segments of the limb reflexed. More arched.
Darker & more dingy. Purplish at the base, and white below on the throat.		paler, and less bright. white at the base, and purplish below on the throat.
Less mottled inside.		more mottled inside.
Hair of the beard on ridges inside shorter but ridges more prominent		hair of the beard longer, but ridges less prominent.
RUDIMENTARY STAMENS, tufted.		not tufted.
PISTIL, longer; and the lobes of the stigma minute.		the lobes of the stigma unequal.

INTRODUCTION; WHERE GROWN; CULTURE. It has already been mentioned that this hybrid plant was raised in the Botanic

Figure 2.4: Text table and illustration of *Lophospermum erubescente-scandens* (Hybrid Climbing Lophospermum). From: the *Floral Keepsake,* folio 242 [unpaginated] (Henslow 1841) (Reproduced from the BHL Archive under the Creative Commons Attribution License).

Earlier in 1849, Henslow had written a semi-anonymous, almost satirical, note in the *Gardeners' Chronicle* about plant hybrids. We examine this note next, as it reveals more of Henslow's underlying motives. These included navigating an on-going tension between observations and philosophical interpretation, and an aspect of the wider cultural context of plant hybridity by the 1850s, which will help situate the attitudes of philosophical botanists in the next chapter of this thesis.[133]

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[131] Henslow 1841: folio 242 [unpaginated].

[132] This paper is interpreted as showing Henslow as a liberal Christian not opposing hybridisation (Walters and Stow 2001: 167-8). Henslow's focus on variation supported Darwin's theorising, but some historians go too far in seeing Henslow as open to transmutationist views at this time (e.g. Kohn *et al.* 2005). His interest was in an educational taxonomic project, harnessing the observations of cultivators.

[133] Henslow 1849a. The note was signed 'J.S.H.'

Hybrids made headline news.[134] As we mentioned in the introduction to this thesis, there was a close and long-standing cultural relationship between monsters and hybrids, which eighteenth-century naturalists had tried to extinguish, as monsters did not belong within the Order of Nature.[135] Victorian journalists revived this fascination, and were particularly intrigued by reports of hybrid sea monsters.[136] In the Autumn of 1848 the captain of HMS *Daedalus* logged a sea-serpent's location with the Admiralty, combined with detailed trigonometric records of its dimensions and speed. This sighting was then swiftly reported in London newspapers. Professor Richard Owen (1804-92), Britain's leading authority on vertebrate anatomy, investigated the 'Great Sea-Serpent'. [137] He suspected that the spate of sea monsters were, in fact, whales, but nonetheless took the matter seriously.[138] Many people were amused: further sightings by *Punch* in 1850 were caricatured as the Irish variety, 'a monstrous hybrid' between a lizard, elephant, ape, cockatoo, snail, fish, porpoise, rattlesnake and caterpillar.[139] In March 1849, the *Athenaeum* sent out a correspondent to interview Hebrides fishermen about another sea-serpent. This inquiry from a leading weekly favoured by the intelligentsia was too much for Henslow. He wrote into the *Gardeners' Chronicle* to guide readers away from such 'popular errors' and to distinguish the hybrid—at least, plausible plant hybrids between closely-related species—from 'absurd monsters'.[140]

Henslow opened his letter with an example from 1833 (reported again in 1838), of a gamekeeper who found the following 'monstrosity', and believed that he had a hybrid between a rabbit and an elephant (Figure 2.5):

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[134] Ritvo 1997 chapter 3, especially pp.94-5. Harriet Ritvo shows that there was a consistent fascination in general interest newspapers and magazines with animal hybrids from the 1820s to the 1880s.
[135] Ritvo 1997: 144; Daston and Park 2001: 359-60.
[136] Ritvo 1997: 182-7.
[137] The standard biography of Owen is Rupke 1994. On the sea-serpent debacle, see Regal 2012.
[138] Owen wrote a much-discussed piece in *The Times* (11 November 1848).
[139] Anon. 1850: 141. Such anti-Irish jokes, to ridicule a practice or belief, were commonplace in the nineteenth-century.
[140] J.S.H. [Henslow, J.S.] 1849a: 148

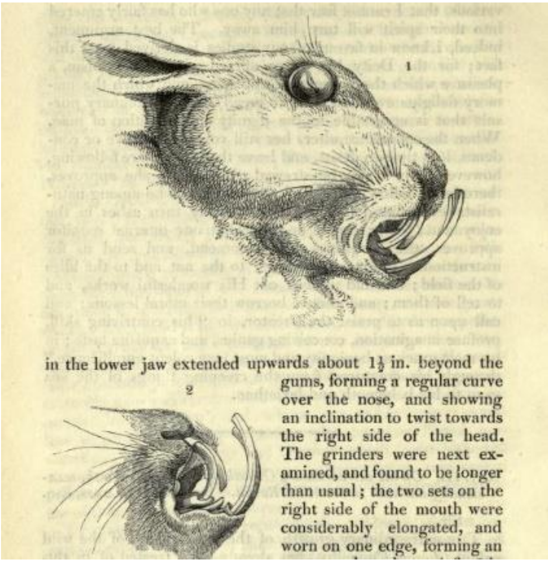

Figure 2.5: Engraving of rabbit's head with over-grown teeth. From the *Magazine of Natural History* v.6 1833 p.22 (cited by Henslow 1849a). (Reproduced from the BHL Archive under the Creative Commons Attribution License).

Seeing something resembling an intermediate form between a rabbit and an elephant did not mean that it was scientific to believe in such hybrids, Henslow explained. Some readers might argue that a cross between animals in different families was plausible as 'have we not heard of hybrids between the species of different families of plants just as little related to each other as the Pachyderms and the Rodents?' Farmers reported hybrids between plants from different Orders: a Swedish Turnip and a Dandelion, and a Turnip and a Lettuce. The 'Dandy-Turnip' was credible, some claimed, because the dandelion has an edible root, just like a turnip. Did the fact that this testimony was from a 'most respectable and intelligent' man mean it was any more believable? A gentleman, Henslow pointed out, might make a claim equally absurd as that of a gamekeeper. Henslow explained: 'at present, we assert, there is no good and sufficient reason for believing that hybrids can be produced between any two species of vegetables or of animals which are not very nearly allied to each other.' 'We are not wishing to dogmatise' he went on, 'by saying such things are impossible', but 'something beyond the mere opinions of practical men' was needed. [141]

Henslow called for 'experiments done more cautiously in order to reveal the probable absurdity of the claims.' Henslow believed that gardeners and local botanists should be encouraged to experiment to see for themselves that these reports of plant hybrids were incorrect. This was an explicit strategy, Henslow explained, to preserve the status of science, as practical men needed direction to produce and collate useable facts. The persistent fascination in the Victorian imagination with monsters meant Henslow had to discipline those observing plant hybrids to keep these within the

[141] J.S.H. [Henslow, J.S.] 1849a: 148

bounds of science. Today we regard the identification of a plant as a hybrid as a display of taxonomic expertise. However, it is important to remember that in the 1840s, a claim of finding or making a hybrid was regarded differently; if anything, it was suggestive of the over-imagination of the collector.

Henslow's *Cohors Botanicorum,* however, emerged in a rather different guise. Rather than forming a seed exchange and a network of experimenters, local botanists (some of whom were also cultivators) developed into a community of practice under Hewett Watson's direction.

Watson's Catalogue Excludes Hybrids from Science

As early as 1832 Watson, like Henslow, realised that science was a community endeavour. Watson argued that

> If we desired to obtain a large picture on the division of labour system, and for this purpose requested a dozen painters each to bring one sheet, the chance of congruity and keeping in the materials so brought together would be slight indeed. Our proper plan would be, to sketch a comprehensive outline, and then leave the individuals to fill up each his own department. [142]

As we discussed in chapter one of this thesis, early Victorian philosophical botanists applied the principle of 'division of labour' to science. Watson here was elaborating on how such a system might work in practice by the use of a 'structure' to attain 'congruity' with a 'comprehensive outline' for local botanists to then contribute to filling in the details of which plants grew where, to paint an overall picture of the distribution of British plants. In 1844, Watson first published such an outline framework, the *London Catalogue of British Plants* (1844).[143]

Watson's list was, he felt, more 'philosophical' than the alternative Edinburgh catalogue, as his version ordered plants taxonomically and aimed to 'clear our overloaded lists of British plants' of 'spuriously wild plants and erroneous names' by defining what counted as indigenous to Britain, and excluding 'aliens, casuals, waifs of cultivation.'[144] Watson later produced a detailed set of defined terms for differing

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[142] Watson 1832: preface vi, cited by Egerton 2003: 34. 'Department' reflected the model provided by floras of French départéments (Allen 2010: 291).
[143] Anon. 1844a (but see fn. 146 below on Watson's authorship).
[144] Watson 1847: preface [unpaginated].

degrees of non-native plant in his four volume *Cybele Britannica* (1847-59). This book acted as a register of all reliable records (personally scrutinized by him) of British plants with their localities, producing a snapshot distribution for each species around the country.[145] Watson thereby introduced the term 'alien', meaning a plant that was not indigenous, and therefore not of interest, to philosophical science.[146] Both the *Cybele* and the *London Catalogue* omitted hybrids for over thirty years. Watson finally relented, adding some hybrids in the seventh edition of the *London Catalogue* of 1877.[147] We will see in chapter three of this thesis why Watson made that shift in the 1870s. However, returning to the 1840s, taken together with Watson's stance on the primulas, the omission of hybrids—not even a mention under his 'waifs and strays'—it is hard to escape the impression that he believed hybrids were likely to be restricted to gardens and not 'indigenous' to Britain. The hybrid plant, like the 'alien' exotic garden plant, lacked the philosophical virtues required to be admitted to Watson's list.

The omission of hybrid plants from these Watsonian publications was significant because these texts codified the entities that local botanists were encouraged to recognise, record and collect as herbarium specimens. Historians see a list as a research technology that can define, or curtail, inquiries and Watson's catalogue provides a nineteenth-century example, that was arguably more effective at imposing such limits than Bentham's *Handbook of the British Flora* (1858).[148] The *London Catalogue* was widely-used, by the most experienced local botanists, even more so than Bentham's handbook, given the latter aimed at ladies and beginners.[149] It was initially controversial (Sidebotham was one of the local botanists who challenged Watson over what had been left out), but successfully supplanted the Edinburgh catalogue by the 1850s: it was Darwin's list of choice in 1855.[150] By 1857, the List 'had long been the text-book' of 'all British botanists' (a laudatory exaggeration) but it did

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[145] On Watson's *Cybele* (1847-59) and his later book replacing it, *Topographical Botany* (1873-4), see Allen 2010: 285-7.
[146] Bentham 1858: xi citing Hewett Watson as responsible for introducing the term 'aliens' to the British flora.
[147] Anon. 1877. Watson had sole credit for editions 1, 6 and 7 although he was a co-editor until his death (Allen 1983). The 7th edition of 1877 includes hybrids but the 6th edition of 1867 did not.
[148] On lists, see Müller-Wille and Charmantier 2012. On Bentham's flora curtailing local botanists' knowledge-making, see Allen 2003a. It was revised by Joseph Hooker in 1887 and as 'Bentham and Hooker' ran to a seventh edition of 1924 (Allen 2003b).
[149] The *London Catalogue* endured until the First World War, with eleven editions.
[150] Allen 1986: 34-5.

replace the Edinburgh version entirely from 1865.[151] Many local botanists followed the London list, those who most needed to have philosophical support for their work, including the only two women to author a British local flora in the nineteenth century.[152] Additional uses for the list developed, including to order one's herbarium and as a pedagogic tool: it provided a clear-cut listing, instead of a beginner having to negotiate the multiple conflicted classifications in botany textbooks and general floras. No other European country had anything like it.[153] However, Watson's list also caused a problem when it came to placing a specimen not easily referrable to a variety on the list, such as a putative hybrid plant, as one local practitioner discovered: 'I feel uncertain how to treat these in the List?' (Figure 2.6).

Watson's list, therefore, effectively established what local botanists should be finding and exchanging, and in the process codified the plant hybrid as unimportant to science. However, Watson was unable to dictate to every local botanist in his emerging *cohors*. Some local botanists remained convinced that cultivators were right: at least some intermediate plant forms were hybrids. By the 1840s, some local botanists set about testing their taxonomic claims by hybridising.

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[151] Anon. [H.B.] 1857: 246 possibly Harriet Beisly (Allen 1986: 188 footnote 1).

[152] Mary Kirby's 1850 *Flora of Leicestershire* followed the *London Catalogue* in preference to Hooker's *British Flora* or Babington's *Manual*. Miss E. A. Warren, author of an 1856 unpublished list for a *Flora of Cornwall,* asked Watson to review her manuscript (Allen 1986: 186 fn13) but Watson found errors of plants highly unlikely to occur in Cornwall (Watson 1883: 561). On Kirby as a successful science popularizer, see Lightman 2007.

[153] Anon. [H.B.] 1857: 246.

Figure 2.6: 'Uncertain' *Primula* herbarium specimen. The comment reads: 'I feel uncertain how to treat these in the List? Will Mr Baker direct me. E.H.' The collector was possibly a Miss Hodgson (according to a pencil note on the sheet), although the handwriting is not a match to specimens collected under that name on herbaria@home, and is older, potentially dating to J.G. Baker's Thirsk Exchange Club from the mid-1850s (see Allen 1986: 69-76) but not earlier, so the 'List' must be the London Catalogue. From: Manchester Museum Herbarium (Author's photograph, © MAN reproduced with permission for non-commercial research use).

Local Botanists Practise Hybridising

In 1844, '*Vidimus*' (literally, 'we have seen' and probably the pen name of a woman gardener) wrote for the *Gardeners' Chronicle* on the 'Nature and Art' of improving plants:

> By far the most important and extensively applicable mode by which we can vary plants is hybridisation … it is not at all necessary to enter into the *modus operandi* of this process—every cultivator of plants knows enough of it, and deems it one of the most delightful and interesting of his occupations in the garden.[154]

In case any readers were unaware of the *modus* involved in crossing, by the mid-1840s, local horticultural societies instructed their members on 'the art of hybridising'.[155] Periodical correspondents wrote in with detailed 'how to' accounts for different genera.[156] The evidence amassed by garden historian Richard Gorer suggests that the 1840s were a pivotal period for the growth in popularity of man-made varieties (today known as cultivars) by hybridising.[157] Later in the nineteenth century, most crossing was cross-breeding (with the exception of orchids, which were late-comers to hybridising; even by 1885, orchid breeding was considered as in its infancy).[158] By contrast, during the 1840s crossing of species dominated specifically because crossing more distantly-related plants, sometimes from different continents, could combine useful features of each parent plant, especially to fuse woody stock with herbaceous plants, or tender exotics with native hardy perennials. Isaac Anderson-Henry, the hybridist we met in the introduction to the thesis, believed that the cheap availability of glass after the revocation of the glass tax in 1845 was the single most significant development to promote cross-breeding and hybridising (glass cloches were needed to exclude insects).[159] John Lindley gave just one example, in the Perlagoniums, comparing the number of bred varieties known in 1827 to those in 1847, that we might find it difficult to believe 'that twenty years have sufficed to produce so great a change

[154] Anon. 1844: 830.
[155] Report of a lecture, 6 December 1846, 'On the Art of Hybridising Flowers' at Stamford Hill, Clapton, and Stoke Newington Gardeners' Society for Mutual Instruction (Anon. 1846: 6).
[156] E.g. Beaton 1849, 1850; Gordon 1847 for Perlagoniums. For biographical details on George Gordon (1806-79), gardener, see Britten, Boulger and Rendle 1931: 126.
[157] Gorer 1970.
[158] Elliott 2010.
[159] Anderson-Henry 1867.

as has really been effected.'[160] The 1830s and 1840s were as much the 'era of the hybrid' as the 1890s.[161]

Donald Beaton, the gardener whom we met in chapter one reading William Herbert's treatise on hybridising, felt that 'naturalists at first were very jealous of the cross-breeder's art as it revealed in some instances the looseness of their classification' but by 1849, he went on, 'all this misunderstanding has happily passed away.' Hybridising was now 'a useful check on the labours of the botanist, by which he may clear doubtful points in his arrangement, or allow the gardeners, or rather THE COTTAGE GARDENER, to do it for him.'[162] However, Watson recounted how he was once told by a gardener that he had 'helped his master's horticultural experiments … so as to produce the results which he supposed would gratify his master.' [163] Gardeners had a poor reputation in the 1840s and the proliferation of 'how to' gardening manuals this decade was to obviate the need for a gentleman to sully himself with employing such rascals.[164] Therefore, against this background, we can understand why local botanists might conduct their own hybridising, even though many middle- or upper-class local botanists had a gardener to conduct it for them. There was also much in common between how a gardener and a local botanist valued their observational skill: Beaton was eulogised for having 'a story about every plant' drawn from his intimacy with vegetables and flowers; he 'saw everything'.[165] The similarity here between how a gardener saw plants, and how a local botanist observed wild plants is palpable. Therefore, it is unsurprising that many local botanists grew plants and many gardeners became field botanists. In sharing these observational practices, some local botanists also shared with cultivators the practice of hybridising, used as a tool to investigate the identity of plants that they had found growing in wild nature.

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[160] Anon. [Lindley, J.] 1847: 763 (Lindley continued to cite Gordon 1847 cited above).
[161] On this claim for the 1890s, see Olby 2000 and 2000a, discussed in the introduction to this thesis.
[162] Beaton 1849: 90.
[163] Watson 1842g: 233.
[164] Endersby 2016: 75. See also Elliott 2014 and Musgrave 2009 on the low social status of gardeners in early Victorian Britain.
[165] Fish 1866: 589.

Dorset physician and author of a pamphlet on the *Flora of Poole,* Thomas Bell Salter (1814-58) conducted hybridising in the 1840s and later his experiments were cited by other local botanists to justify including hybrids in their floras. [166] Bell Salter experimented by hybridising plant species in 1842 and believed that he had proved that wild plant hybrids existed. He hybridised wood avens (*Geum urbanum* L.) and water avens (*G. rivale* L.), and two willowherbs (*Epilobium* species). He chose to attempt to re-create one of the most well-known plant hybrids, often given the species name *Geum intermedium* Ehrh.. More significantly, his hybridising experiments, he claimed, refuted the well-established 'physiological doctrine on hybridity' that hybrids in nature were always sterile, as set out in William Carpenter's *Principles of General and Comparative Physiology* (1838).[167] Bell Salter conducted his hybridising in 1842 to investigate species limits, he explained, but had not realised the significance of his results for the laws of hybridisation at the time. The timing of 1842 suggests that his interest was piqued by the Primula debate that year in the *Phytologist.* Ten years later, Salter delivered a paper to his local Philosophical Society, published an account in the *Phytologist,* and sent seeds to Charles Darwin.[168]

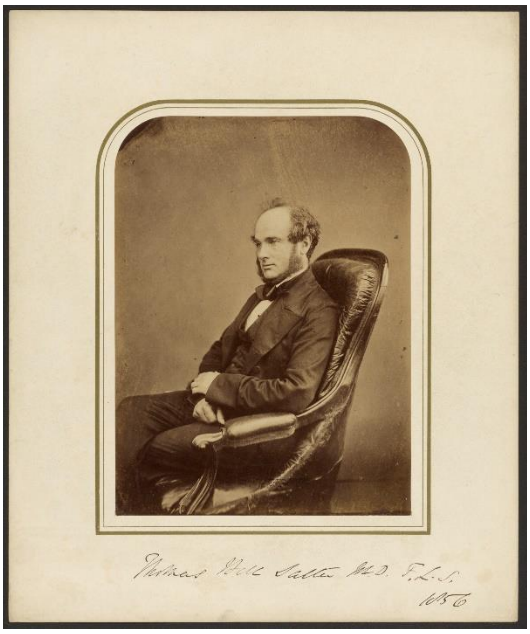

Figure 2.7: Portrait of Thomas Bell Salter (1814-58). From: Getty Art Collection, 185686.XM.742.27. (Digital image courtesy of the Getty's Open Content Program).

Bell Salter's experiments were well-received: Carpenter's next edition in 1854 tentatively reported that he had shown that hybrids, and more strikingly fertile

[166] On Bell Salter, see Desmond 1994: 604. He was on the Council of the BSL (Allen 1986: 14).
[167] Carpenter 1838 Preface p.vi. On this book's importance during the 1840s see Secord 2000:64-5 and 103 and Lidwell-Durnin 2019a. On Bell Salter's reading of Carpenter influencing Darwin, see Delorme 2018: 162-3.
[168] Darwin and Bell Salter exchanged letters about his paper in June 1855 and Darwin's notes on it are in CUL-DAR 73: 88–9. Two days after receiving Bell Salter's letter, Darwin asked Henslow for help on hybrids. After research into continental publications, Darwin concluded that the *Geum* was probably not a hybrid after all. Darwin sent the Epilobium seeds on to Joseph Hooker, who grew them on, reporting in 1856 'It is so clearly E. roseum and nothing else, having no trace of either parent that I have no faith whatever in his experiments.' (J.D. Hooker to C. R. Darwin, 10 July 1856, DCP Letter no. 1923.

hybrids, might persist in a state of nature. At this time philosophical botanists acknowledged the possibility of only one or two other putative wild fertile hybrid plants growing in Britain.[169] However, there was an undercurrent of feeling that this was grossly under-representing the situation. Horticulturalists like Isaac Anderson-Henry writing in 1853, were already convinced:

> Gentlemen eminent as physiologists [he meant Carpenter] have read nature's laws in these matters a little differently from what my own humble experience has taught me … Again, it is asserted that a proper hybrid—i.e., one species which is crossed with another species, which is separate and distinct from it—will produce no fertile seeds. This does not accord with my observations.[170]

However, while gardeners might still be insisting that plant hybrids not only existed in nature, but might also be fertile, one philosophical botanist did not agree, Darwin's best friend Joseph D. Hooker. Darwin sent Bell Salter's *Epilobium* seeds on to Hooker, who grew them on, reporting in 1856: 'It is so clearly E. roseum and nothing else, having no trace of either parent that I have no faith whatever in his experiments.'[171] We will discuss Hooker's attitude to plant hybrids in the next chapter, but of more significance here is the fact that Bell Salter's experiments convinced some local botanists.

John Gilbert Baker (1834-1920) joined the Botanical Society of London aged seventeen, and wrote a local flora aged twenty.[172] In his Yorkshire flora of 1854, he followed Watson's *London Catalogue*, but departed from the *Catalogue* over Primula: Baker was sure that the widespread mock oxlip was a fertile hybrid between primrose and cowslip.[173] The origin of his treatment of primula was, in fact, unremarkable (and not some unique insight on the part of the young man). Baker knew that Bell Salter had shown experimentally that intermediate forms between a pair of species were hybrids. The intermediate avens 'appears to be a fertile hybrid analogous to *P. elatior* rather than a distinct species' decided Baker, and cited Bell Salter's paper in the *Phytologist*.[174] Another local flora author, a cashier at a cotton company, the

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[169] Bell Salter 1852:740-1.
[170] Anderson-Henry cited in Lindley 1855: 493.
[171] J.D. Hooker to C. R. Darwin, 10 July 1856, DCP Letter no. 1923.
[172] Allen 1986: 56 and 72.
[173] Baker 1854: 105.
[174] Baker 1854: 61.

'exuberant and fanciful' Leopold Hartley Grindon (1818-1904), thought that Bell Salter was right about hybrids. Grindon was a close friend of Joseph Sidebotham and took an interest in his experiments and those of Bell Salter. There were 'innumerable' hybrids among *Epilobium* L. (willowherbs), he decided.[175] Grindon felt that following Watson and unifying all three primulas was 'quite an uncalled-for notion'.[176] It was obvious, he explained, that the mock oxlips were hybrids, so primrose and cowslip were good species. Publishing on the primulas in 1859, Grindon espoused the view of many local botanists at a time when Darwin in *The Origin of Species* was about to adjudicate publicly over hybridism in general.

In 1856, Darwin decided that the intermediate oxlips were best viewed as evidence of variation in nature and not as hybrids. In his 'Big Species Book' manuscript section on variation in nature, written in December 1856, Darwin argued that intermediate transition forms, like the mock oxlip, 'have been attempted to be explained away by the supposition of the intercrossing of the several forms.'[177] The intermediate forms of primula were evidence of variation, because although a large part of this variation probably arose due to ancestral crossing, hybridism could not account for *all* the intermediate forms of primula.[178] Darwin discussed how, in most closely-related species pairs, this intermediate variation had long since disappeared, so the species could be defined by a clear morphological gap between them; whereas the primrose and cowslip were two 'close-species' effectively in the final throws of transmutation, and the intermediates would die out or create a third species between them.[179]

Primulas were, in 1856, Darwin's 'most interesting case on record', yet also unfinished business. Once Darwin had published the *Origin of Species*, he soon returned to the mock oxlip as part of his botanical studies in support of natural selection.[180] In Spring 1862, Darwin noticed an odd-looking primula growing with

[175] Grindon 1859: 269 on *Primula* and p.283 on *Epilobium*. For biographical details on Grindon, see Lightman 2007.
[176] Anon. 1905: 30; Anon. 1904a: 373.
[177] Stauffer 1975: 131.
[178] Stauffer 1975: 131-132.
[179] Stauffer 1975: 133.
[180] Stauffer 1975: 128 and 133. Darwin's Experiment Book CUL-DAR157a1-84 folio 55 first mentions oxlips in an entry around April-June 1860.

cowslips and primroses in ‘the Big Wood’, an ancient woodland near his home at Down in Kent:

> April 27 1862: I have just compared the oxlips in corner of big wood whence I got the seeds last year now coming up in the K. garden, with the Bardfield Oxlip or P. elatior - has very different appearance- (my oxlip has yellow marks at mouth of corolla, larger flowers & [?*unclear text*]) There was a group of 5 or 6 plants, long and short-styled slightly different in tint and size; have seedlings from some neighbouring plant – Both cowslips and primroses grow mingled in this open bit of wood.[181]

Alongside this entry in his Experiment Book, Darwin noted that the French botanist Godron believed that the mock oxlip was a hybrid. In 1862, Godron discussed the primrose-cowslip hybrid, reiterating his views from publications in the 1840s, and claimed to have now re-made the hybrid, but published no experimental results.[182] Local botanists were quick to follow continental practice: there are examples of oxlip plants named as ‘Primula hybrida’ in several herbaria (even though this was not the scientific name for the hybrid), including a sheet dated 1864 collected by the suffragette Lydia Becker (1827-90) and from 1867 a specimen collected by a local botanist from Manchester (Figure 2.8).[183] However, for Darwin, the only remaining course of action was to conduct his own Primula hybridising experiments, which he finally published in 1868, announcing that the mock oxlip was, indeed, a hybrid.[184]

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[181] Darwin’s Experiment Book CUL-DAR157a1-84_047 folio 75 unpublished M.S. and my transcription.
[182] Godron 1862.
[183] Primula specimen marked as ‘probably a hybrid’ in Lydia Becker’s handwriting, collected for the 1864 British Botanical Competition run by the RHS, collected 27 April 1864 from a location in Lancashire (Manchester Museum Herbarium, Primula hybrids folder).
[184] Darwin 1868a.

Figure 2.8: Herbarium specimen*,* collected by John Barrow (1822-90), on 22 April 1867 and labelled at the time of collection as '*Primula Hybrida '.* From: Manchester Museum Herbarium (Author's photograph, ©MAN reproduced with permission for non-commercial research use). (On Barrow, see Desmond 1994: 49).

V. Conclusion

The object of this chapter has been to explore the diversity of attitudes to plant hybridity which are obscured by presenting the primula puzzle as solved by Charles Darwin, when in fact, Darwin returned to what many in the *Phytologist*'s and *Gardeners' Chronicle's* communities had been saying over twenty years earlier. The case of the debate over oxlips shows that, in the 1840s, hybridisation was a familiar explanation for the array of intermediate forms between species pairs like the primrose and cowslip. Many farmers, gardeners and local botanists were convinced that these intermediate forms were hybrids. However, Hewett Watson and Charles Darwin examined the hypothesis of hybridisation carefully, as at stake were their ideas about transmutation.

The 'mock' oxlip grew at the intersection between gardening and botany; and between hybridisation and transmutation. Its story illustrates three significant historical points about the relations between these, which relate to the broader aims of this thesis.

Firstly, plant hybridity mattered for diverse intersecting botanical communities. The on-going debate over what to call the 'mock' oxlip resulted from communication between gardeners, local botanists and philosophical botanists, facilitated by the subscriber communities formed around a natural history periodical, the *Phytologist* and a horticultural periodical, the *Gardeners' Chronicle*.

Secondly, the primula puzzle shows how botanical communities interpreted an intermediate form differently according to what was at stake in calling it a hybrid or not. For many practitioners, it seemed obvious that the mock oxlip was a hybrid, especially those who conducted hybridising in their gardens and for whom hybridising, and more generally, cultivation of wild plants, was an aspect of their natural history practice. However, hybrids were associated with unphilosophical 'rustic' beliefs (to quote Watson) and a philosophical practitioner might therefore remain cautious about the hypothesis of hybridisation in *Primula*. Therefore, the oxlip episode also illustrates the on-going social tension between gardening, making-as-knowing, and botany which lies at the centre of this thesis and in part explains the complexity of attitudes toward plant hybrids. Philosophical botanists responded to this debate over hybridity by seeking to organise and discipline the observations made by local botanists, just as they had tried to motivate horticulturalists a decade earlier to use hybridising to reduce the number of species. While those gardeners and local botanists hybridising realised that their experimentation placed them in a knowledge-making position within science, given the philosophical caution over hybridity, their rhetorical claim of taxonomic authority was socially overstated. Quite how these practitioners might develop scientific authority is developed in the next chapter of this thesis.

Thirdly, the case of the hybrid oxlip illustrates a neglected episode in the history of evolutionary theorising. The publication in 1844 of *Vestiges* brought out into the open a long-standing alternative explanation for intermediate forms like the oxlip, that these might be the result of hybridisation. For some philosophical and local botanists, hybridisation defended botany from what was perceived as a dire threat to respectable science, the radical French developmental theory. Religiously motivated practitioners, rather than opposing the plant hybrid, appealed to hybridisation as a conservative alternative to Lamarckism. Conversely, at stake for both Watson and Darwin in calling the oxlip a hybrid, was losing an exemplar of transmutation. This episode in the history of British botany demonstrates the centrality of hybridity in the history of evolutionary theorising before 1859, and how its importance is overlooked if historians portray Darwin as the person who solved the primula puzzle.

The next chapter moves to the 1860s, and explores two mid-century developments: firstly, how the epistemological virtues expected of a ‘philosophical’ practitioner determined attitudes to plant hybridisation in nature, more effectively than species concepts or other concerns, religiously motivated or otherwise. Secondly, how the practice of an emerging new Darwinian biology affected approaches to plant hybridity.

Chapter 3
'The Loves of the Willows': Plant Hybrids and a New Darwinian Biology

I. Introduction

Up to this point, this thesis has explored the three key botanical communities for whom plant hybrids mattered in early Victorian Britain—philosophical botanists, local botanists and cultivators—and how these communities intersected via contributions to floras and debates about hybridity in gardening and natural history periodicals. The practice of hybridising was applied in different contexts by practitioners across botanical communities. There was no demarcation between 'hybridists' and others, as often referred to in relation to German and French early nineteenth-century science.

In chapters one and two, we saw how gardeners and local botanists sometimes realised that their hybridising practice placed them in a knowledge-making position, but their rhetorical claim of scientific authority was socially overstated.

Moving forward chronologically, this chapter argues that the practices of an emerging new Darwinian biology affected the taxonomic treatment of the plant hybrid, and the social relations between philosophical and local botanist-cultivator practitioners. It does so by considering the study of a plant group or genus, in which all three botanical communities engaged, *Salix* L., the willows.

Figure 3.1: Herbarium sheet with material collected by Boswell Syme in 1880 showing Graham's willow labelled as S. Bakeri in Bowell Syme's handwriting. Specimen circulated by the Botanical Exchange Club (BEC logo on the blue label, top right). From: British and Irish Herbarium, the collections of the NHM, London. (Author's photograph, ©the author, on the advice of the IP Rights Officer at the NHM).

In 1868, a former railway engineer from an impressive yet impecunious Scottish family, John Thomas Irvine Boswell Syme (1822–1888) described a willow, *Salix Grahami* Borrer *ex* Baker, or Graham's willow, for the new edition of *English Botany.*[1] He stated 'I suspect it to be a hybrid' but continued for over a decade to use a species epithet, S. Grahami or S. Bakeri, on the tickets he wrote as distributer of London Botanical Exchange Club herbarium sheets (Figure 3.1). [2]

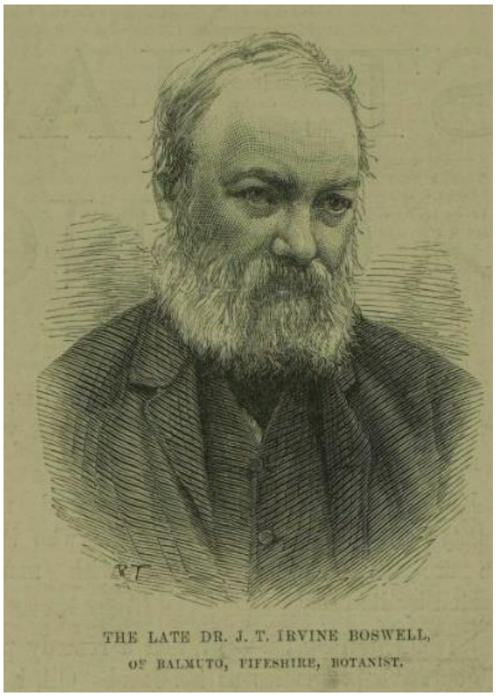

Figure 3.2: Portrait of John Thomas Irvine Boswell Syme (1822–1888) in old age. From: the *London Illustrated News*, 11 February 1888, p.12. Accessed from the Gale News Vault (reproduced under the Creative Commons Attribution License).

Syme's dilemma over the willow's identity reflected a wider anxiety among British botanists. In 1863, Professor Charles Cardale Babington, who we met visiting the mock oxlip in chapter two, complained that 'the definition and classification of Willows has long been a disgrace to systematic botany.'[3] The embarrassing consequence was that no one could agree on how many wild plant species there were in Britain.[4] Yet by 1890, the situation had shifted so that the Director of the Royal Botanic Gardens, Kew, William Turner Thiselton Dyer (1843-1928) could comment:

> I cannot get him [George John Romanes] to face the fact that natural hybrids are being found to be more & more common amongst plants. At the beginning of the century it was supposed that there were some sixty recognisable species of willows in the British Isles. Now they are cut down to about 16 & all the rest are resolved into hybrids.[5]

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[1] For biographical details on Boswell Syme, see Allen 2004a. This willow was named after Professor Robert Graham (1786-1845) of the University of Edinburgh, who found the plant on a Scottish mountain between 1827 and 1833 (Meikle and Tennant 2015: 129). The modern spelling of the epithet is *Grahamii* (Stace 2019: 354).

[2] Boswell Syme 1868: 258.

[3] Babington 1863: 167.

[4] Endersby 2009 and Allen 2001.

[5] Letter from William Thiselton Dyer to Alfred Russel Wallace, 27 September 1893 (1881-1911 MS 46436 folios 300-2, no. 302 in the Wallace Correspondence at the British Library, London). Leading Darwinian zoologist, George John Romanes (1848-1894), wrote the 'hybridism' section of the scholarly ninth edition of *Encyclopaedia Britannica* (Romanes 1882). For biographical details on Dyer, see Thomason 2004 and 1987.

This chapter traces how this taxonomic about-turn took place in Britain. An existing historical narrative holds that leading British taxonomic botanists opposed plant hybrids, and that opposition is explained by their broad species concept, known as 'lumping'.[6] Yet this divide of 'lumping' and 'splitting' of species between taxonomists did not cut the intellectual territory at its joints. Instead, this chapter supports the second thesis argument: during the 1860s, one of the wider cultural contexts explaining botanists' views of hybridity related to the epistemological morals expected of a philosophical practitioner.

Examining the story of willow hybrids also leads to an unexpected angle for a well-known story, the impact of Darwinism—or rather, a new Darwinian way of practising biology—on taxonomy. Historians of science argue that the Darwinian theory of evolution had a conservative impact on taxonomic practice. This chapter looks instead to the effects of a new Darwinian biology focused on botanical experimentation, including hybridising, and what that meant for the practice of plant taxonomy.[7] The theory of evolution by natural selection drew attention to hybridity (or 'hybridism' as Darwin termed it) as an *idea*; but, as historian Richard Bellon demonstrates, it also highlighted the *practice* of physiological botanical studies.[8] Darwin had carefully negotiated with his best friend, the leading botanist Joseph Dalton Hooker (1817-1911), to ensure that Hooker's taxonomic practice might continue unaffected by his theory.[9] Yet unanticipated by both Darwin and Hooker, during the 1860s, a distinctively Darwinian way of practising biology emerged. Darwin, with one eye looking back to his botanical mentor Henslow's practices, demonstrated how botanical physiological experimentation included hybridising.

In addition, several events coalesced during the 1860s, to raise the profile of the practice of plant hybridising linked to this Darwinian way of knowledge-making. In 1865, a German monograph about experimental willow hybridising circulated among British botanists, which included a Darwinian explanation of hybrids in nature.[10] This

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[6] This argument originated with Stevens 1997.
[7] Endersby 2008, Bellon 2003, and Allen 2001 in relation to botany, and Bowler 2009 more generally, argue that the Darwinian theory of evolution had a conservative impact on taxonomic practice.
[8] Bellon 2011.
[9] Darwin 1859: 484; Bellon 2003 and 2006.
[10] Wichura 1865.

coincided two years later with wider discussions between horticulturalists and philosophical botanists over naming hybrids at the International Botanical and Horticultural Congress. A further consequence was that the practice of hybridising received a social boost, marked out as experimentation, instead of mere gardening. By 1870, Joseph Hooker had changed his taxonomic paper practice to include hybrids, while local practitioners' 'special knowledge' of hybridity allowed them to build an identity as taxonomic authorities within late-century Victorian science.

This chapter draws on untapped local botanists' archival sources, and in particular, annotations to herbarium specimen sheets, to show that a new Darwinian biology with its botanical experimentation affected the practice of plant taxonomy. The chapter is divided into three sections: the first section unpacks the attitudes of philosophical botanists to hybridity around the time of Darwin's publication of the *Origin of Species* (1859). It considers how philosophical botanists held differing interpretations of intermediate forms, and argues that the taxonomic approaches of 'lumping' or 'splitting' did not explain attitudes to plant hybridisation in nature. Instead, opposition to hybridisation related to the epistemological morals expected of a philosophical practitioner. The second section shows how attitudes to plant hybridity shifted in the period 1860 to 1870 in response to Darwinian developments. The third section demonstrates how the Darwinian study of plant hybrids and the practice of hybridising enabled local cultivator-botanists to become taxonomic authorities, at a time when standard histories portray a widespread decline of non-academic practitioners in science.

II. Getting Philosophical about Hybrids

Hooker's Lumping Opposes Hybridisation

In 1855, the philosophical botanico-geographer Hewett Watson explained taxonomic approaches in botany to a rather bemused non-botanist, Charles Darwin:

> Taking J. D. Hooker & Jordan as representative men for the opposite factions in botany,—'lumpers & splitters', the former would reduce the species of Vascular plants

> to three score thousand, or perhaps much fewer;—while Jordan would raise them to three hundred thousand.[11]

William Hooker's son, Joseph D. Hooker had just been appointed Assistant Director of the Royal Botanic Gardens, Kew and published the first volume of his taxonomic tome, *Flora Indica*.[12]

Figure 3.3: Portrait of Joseph Dalton Hooker (1817-1911) in 1855, aged 38. From: albumen print, NPG P106(12) (© National Portrait Gallery, London. Reproduced with permission for non-commercial scholarly use under the Creative Commons Attribution License).

Hooker's *Flora Indica* was co-authored by Thomas Thomson (1817-78) but its 'Introductory Essay' was Hooker's work.[13] Hooker used this essay especially to target the French botanist Claude Thomas Alexis Jordan's (1814-97) excessive naming of new species.[14] Hooker criticised the Frenchman's practice:

> M. Jordan has not unfrequently, it would appear, found that seeds collected on particular species have produced a different form, and he has not hesitated to infer that the ovules of the plant had been impregnated by a different species. The contrary inference, that species are subject to a certain amount of variation, does not seem to have occurred to him.[15]

Continental botanists, Hooker felt, 'generally assumed that hybrids do occur in nature'. However, it was 'a singular fact, that these hybrids are vouched for only in genera most notoriously apt to vary, and mainly by hair-splitting botanists'.[16] The willow genus *Salix* was one such example of a genus or group prone to variability. Hooker remarked how 'persons of intelligence' might know our common English trees, such as the iconic weeping willow, 'at first sight', yet be unable to recognise the same tree in exotic countries where 'his

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[11] H.C. Watson to C. D., 13 August 1855, DCP letter 1740. Jim Endersby incorrectly states that Darwin first coined the terms lumper and splitter (Endersby 2007: 101). Watson used these terms from at least 1841 (Egerton 2001: 131). Other botanists used the less loaded terms of 'dividing' and 'combining' (Baker 1867: 158) or 'analysis' and 'synthesis' (Thomson 1865: 236).

[12] Hooker and Thomson 1855. Details here are from Endersby 2004b and Endersby 2008. *Flora Indica* was originally proposed as a 15-volume set.

[13] Endersby 2008: 50 and citing Turrill 1953 (in addition, the views expressed are those of a lumping taxonomist whereas Thomson was more moderate, e.g. in Thomson 1865).

[14] Jordan 1852. For biographical details on Jordan, see Stevens 1997.

[15] Hooker and Thomson 1855: 23-4.

[16] Hooker and Thomson 1855: 23.

preconceived ideas [will] fall to the ground in very many cases' due to the variable forms produced by the differing climatic conditions.[17] Hair-splitters saw hybrids or more species of willow, where Hooker saw climatically-induced variation within a broader, single species.[18]

In his essay's argument about variation in plants, Hooker began with 'the Effects of Hybridization', debunking the 'defective information' in reports of thistle and gentian hybrids.[19] He was convinced that plant hybrids did not exist in India (or elsewhere) because neither he, not other reputable (British) botanists, had seen them growing in the wild.[20] Nor could plant hybrids in the garden be used to support hybridisation in nature. He explained that the topic of plant hybridity had never been scrutinised within philosophical science. Hooker continued:

> It is often argued that hybrids are common in gardens, and that their occurrence in a state of nature cannot be denied; and that if the permanence of one such hybrid be admitted, the whole fabric of species is shaken to its foundation.

Hooker continued:

> Such summary conclusions are however opposed to philosophical caution: the whole subject is one that cannot be cleared up by a consideration of exceptional cases; it must be argued upon broad principles, and unfortunately no argument has ever been adduced that has not been taken in evidence on both sides of the question. This is especially the case with hybridization, which, in so far as it can produce a form distinct from either parent, does, in one sense, create what may temporarily pass for a species; and in so far as the hybrid combines the characters of both parents, it temporarily obliterates the distinctive characters of each.

Hooker then concluded that plant hybrids were 'not proven' in nature:

> All, then, that we could legitimately conclude from these facts is, that were hybrids of universal occurrence, they would have obliterated all traces of species, but that, exceptional in art, and not proven if not almost impossible in nature, they cannot be assumed to have produced any appreciable result.[21]

In this passage, Hooker also showcased his argumentative skills as a mid-Victorian philosophical practitioner. The recognition of hybrids in nature relied on 'summary

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[17] Hooker and Thomson 1855: 35.
[18] An irony for today's botanist is that the widespread weeping willows in Victorian Britain were hybrids rather than the original Chinese species named by Linnaeus *Salix babylonica* L. (Meikle 1975: 308; Stace 2019: 347).
[19] Hooker and Thomson 1855: x and 23.
[20] Hooker and Thomson 1855: 23-4.
[21] Hooker and Thomson 1855: 22-23.

conclusions' which were 'opposed to philosophical caution'. Even if hybrids did exist in nature, they were temporary and rare; they were 'exceptional in art'—in plant breeding—and not so easily produced artificially as horticulturalists claimed. Hooker was adamant that hybrids cannot exist commonly in nature, nor are they permanent forms, as then they would have overwhelmed all species; even if that was the case, what we see as a species would be of hybrid origin, but we would still call these species. Therefore, either way, he argued, hybridisation in nature was unimportant. However, Hooker was rather too vociferous about the topic to be merely disinterested. Indeed, Hooker faced considerable differences of opinion on plant hybridity from other taxonomists, not just from the more extreme practitioners like Jordan. Hooker's collaborator, Vice President of the Linnean Society, and doyen of gentlemanly taxonomy, George Bentham (1800-84) took stock on hybridity in the preface to his new *Handbook of the British Flora* in 1858: 'Frequent as they are in gardens, where they are artificially produced, they are probably rare in nature, although on this subject there is much diversity in opinion, some believing them to be very frequent, others almost denying their existence.'[22] Interestingly, Bentham, perhaps the greatest lumper of them all, continued in his preface to display a more moderate position than Hooker over hybridity, listing the characteristics he felt 'must always co-exist in a wild hybrid', including evidence of sterility. While he would not accept fertile plant hybrids in nature, he was prepared to acknowledge potentially widespread sterile forms.[23]

In 1859, the publication of the *Origin of Species* and Darwin's theory of evolution by natural selection comfortably reinforced Hooker's taxonomic lumping approach.[24] In the first case study supporting a new Darwinian botany, Hooker's *Flora Tasmaniae* (1860), hybrids were 'rarer than generally supposed' and were, in any event, 'invariably barren', and their characters were not those of new varieties or races. Conversely, in exceptional cases, 'that some supposed species may have their origin in hybridization cannot be denied.' However, this was immaterial as the philosophical botanist was concerned only with 'phenomena on a large scale' looking for uniformly acting causes whose effects were 'unmistakeable' and 'traceable

[22] Bentham 1858: 32.
[23] Bentham 1858: 32-3.
[24] Bellon 2006.

throughout the vegetable kingdom'.[25] Hybridisation was not such a cause because there were no accepted hybrids known in the ferns or the lower plants, the mosses, liverworts, algae or lichens. Darwinism rendered an explanation for, and indeed, demanded, the vast variation and intermediate forms observed in plant species. While hybridisation produced variation, Hooker believed that it was one of several causes, and not the most important. Indeed, even in 'gardening operations' while crossing produced a lot of variation, even fertile hybrids were 'doomed' as they would only reproduce with one of their parent species and therefore would eventually revert in form to the parent form.[26] Hooker insisted that gardeners were cross-breeding and that their claims to cross species were mistaken. Cross-breeding might appear the same as hybridisation, but hybridisation was 'a phenomenon of a very different kind, however similar it may appear in operation and analogous in design to crossing within a species'.[27] In sum, hybridisation in nature for Hooker was an unsubstantiated hypothesis; not a high-level law or *vera causa* that the philosophical botanist should be concerned with. He was also confident that a new Darwinian botanical taxonomy might comfortably ignore hybrids.

Then, in 1862, Joseph Hooker had an unwelcome shock. Darwin found a wild hybrid mullein (*Verbascum* L.) growing in a field near his home at Down, Kent. As we saw in chapter one, gardeners and local botanists had observed hybrids in this plant genus since the mid-eighteenth century, but by 1830 philosophical botanists doubted these older reports. According to Darwin, in a letter to his friend the Harvard Professor of Botany, Asa Gray (1810-88), Hooker suffered 'a fit of the horrors' when Darwin mentioned this wild hybrid.[28] Darwin also wrote to Hooker remarking that he was reconsidering his view of hybridity in nature, a view which had previously supported Hooker's own stance: 'I formerly thought with you about rarity of natural hybrids, but I am beginning to change.'[29] Hooker's dismay arose because the possibility of natural hybrids meant that he could no longer assume that the many intermediate forms

[25] Hooker 1860: x.
[26] Hooker 1860: x-xi.
[27] Hooker 1860: x.
[28] C.D. to Asa Gray, 16 October 1862, DCP letter 3766. On Gray, see Dupree 1988.
[29] C.D. to J.D. Hooker, 6 October [1862], DCP letter 3753.

between species pairs justified *de facto* lumping those forms into a single species. Hooker responded to Darwin:

> The dismal fact you quote of hybrid transitions between Verb. Thapsus & nigra (or whichever two it was) & its bearing on my practice of lumping species through intermediate specimens, is a very horrible one; & would open my eyes to my own blindness if nothing else could. I have long been prepared for such a case, though I once wrote much against its probability— I feel tolerably sure I must have encountered many such, but have not the tact to discern them, when under my nose: & I hence feel as if all my vast experience in the field has been thrown away.[30]

Hooker was not interested in which two *Verbascum* species were involved; instead, he focused entirely on the implications for his taxonomic lumping approach. Historians generally agree that underlying Hooker's commitment to lumping was a 'defensive doctrine' at the centre of an imperial project to organise and discipline colonial botanists. From a practical point of view, the botanical resources of the British Empire might only be successfully managed with a limited number of named species.[31] Jim Endersby suggests matters went even deeper for Hooker, beyond practicalities, given the violence with which he enjoyed the 'smash, smash of species' falling to his lumping regime. Hooker's taxonomic views may have originated with his frustration at sorting out the mess that a 'splitter' had made of the Rhododendrons, and the fact that this task had held up his marriage.[32]

Worse than plant hybridity negating his lumping taxonomic approach, Hooker regretted that his own hubris led him to write much against the probability of hybrids occurring in nature. Hooker realised that all his field experience in exotic locations had not enabled him to make the observations his friend—looking at wild flowers a mile or so from his home—had carefully noticed. This episode over the wild hybrid *Verbascum* reveals how Hooker's lumping taxonomic approach directly conflicted with interpreting intermediate forms as plant hybrids. Yet more significantly, despite his commitment to lumping, his moral obligation to philosophical science was greater, and he humbly accepted Darwin's observation. However, Hooker's perspective cannot be

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[30] J.D. Hooker to C.D., 12 October 1862, DCP letter 3757.
[31] Allen 2001:281 interpreting Stevens 1997.
[32] Endersby 2008: 161-2; Jim Endersby pers. comm. 30 June 2017.

used to explain attitudes among all British philosophical botanists. In the next section, we see how some philosophical 'splitters' were also cautious about hybridity and why.

Splitters and the Morals of Hybridity

In the introduction to this chapter, we saw how the leading philosophical botanist working on the taxonomy of British plants, Charles Babington, lamented the confused classification of the willows. He hoped that someone might sort out the *Salices*, laboriously describing the various forms, in the way that he was tackling *Rubus* L., the brambles.[33] Babington was also Britain's best-known splitter, yet declined to name intermediate bramble forms as hybrids.[34] Historian David Allen astutely notices that the division between lumping and splitting taxonomic approaches cannot explain why philosophical practitioners rejected plant hybrids, for 'even an arch-splitter like Babington was a lifelong disbeliever in the existence of hybrids.'[35] Babington quipped late in life: 'I have but little belief in evolution or hybridization, but time will shew'.[36] David Allen argues that this statement, and his treatment of *Rubus*, demonstrate that Babington rejected hybrids due to his devout Christian belief in species fixism.[37]

There are several problems with David Allen's explanation, including that Babington did accept plant hybrids in *Primula* (as we saw in chapter two of this thesis). Instead, this thesis argues that Babington's correspondence suggests that he was concerned to avoid the unphilosophical behaviour of what he saw as careless naming of plant hybrids. Indeed, Babington also would have regarded his own religious belief in fixed, but variable, species as not a legitimate reason to dismiss hybridity, even fertile hybrids, as comingling faith and science was not the behaviour expected of a philosophical practitioner by the 1860s.[38] In interpretating Babington's position, we need to heed his philosophical qualifier: 'but time will shew'.[39]

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[33] Babington 1869.
[34] Allen 2001: 282.
[35] Allen 2010: 236.
[36] Allen 1999: 9 citing Babington 1897: 414.
[37] This thesis argues here against David Allen's interpretation of this quotation in Allen 1998: 8, Allen 2001:282 and Allen 2010: 236.
[38] Lightman and Dawson 2014.
[39] Babington 1897: 414.

After his death, Babington was eulogised in a 'Life and Letters' volume (published in 1897) as the mild-mannered exemplar of a devout Christian who saw no conflict between science and his faith. Several accounts emphasised his 'care and labour' in his taxonomic studies.[40] This might tell us more about the ideal philosophical practitioner, than Babington himself, except that Babington's own correspondence also shows how he lived out these virtues, from his field work in the 1860s to his advice in old age to local botanists in the 1890s. For example, he explained to a correspondent that naming a problematic specimen as a hybrid was 'usually a careless way' of avoiding much labour:

> Are you inclined to believe that hybrids are usual, or not infrequent in Epilobium? If they are, the E. virgatum may be one, and we are relieved from much difficulty. But I certainly tend to join in the opinion of Fries, that the calling a difficult plant a hybrid is usually a careless way of escaping from difficulty.[41]

He spent over 30 years studying examples of the heath *Erica Mackaiana* Babs., and first collected it in 1839, to support his opinion that this was a distinct species. [42] As he commented in 1873:

> The more I see of this heath, the more convinced I am that it is quite distinct from E. Tetralix. I think it is our most beautiful heath. Neither do I in the least believe that it is a hybrid. If E. ciliaris had been very abundant there, and also E. Tetralix (which although there, is very scarce), there might have been some excuse for the idea. But every thing is a " hybrid " now, if it causes any trouble to systematists in their studies and herbaria, and if there is the faintest excuse for considering it as one.[43]

Babington was concerned that botanists were naming forms as hybrids without having put in the observational labour that he believed was required for a correct determination. Further, Babington was also prepared to name a plant as a hybrid, if this was justified by 'proof'. For example, his comments on specimens sent to him from a Surrey vicar and local botanist-hybridist, Edward Sherburn Marshall (1858-1919), were as follows:

> Mentha. Witley. Closely approaches aquatica. Is it a hybrid?

[40] Professor G.D. Liveing in his obituary reprinted from the *Cambridge Review*, 17 October 1895 (Babington 1897: lx).
[41] C.C. Babington to William Borrer, 4 May 1855 (Babington 1897: 327).
[42] C.C. Babington to William Borrer, 12 June 1839 (Babington 1897: 280).
[43] C.C. Babington to Professor J. H. Balfour, 12 Sept 1873 (Babington 1897: 370).

> Luzula. What is your evidence of the hybridity of L. Borreri? I think it is so, but have had no proof.[44]

We will see in the next section of this chapter how the proof that Babington sought was delivered by hybridising experiments. However, it seems that there was a distinction in Babington's approach between individual plant hybrids, for which there might be proof available, and the 'theory of hybridisation'.

In the quotation cited earlier about hybridisation and evolution, written in 1887, Babington went on to discuss evolution and hybridisation: 'Beautiful theories they are, but not proved facts yet, even if they ever are to become such, as I much doubt.'[45] It is important to consider why Babington coupled hybridisation here with evolution. One interpretation is that Babington was speaking about the idea that hybridisation held a role in Darwinian evolution; that a fertile hybrid might form a new species in its own right; an updated version of the hybrid theory promoted by William Herbert in 1837. As we will see in chapter five of this thesis, hybrid evolution reoccurred as a topic of interest among biologists during the 1880s and 1890s. Therefore, it is plausible that here, writing in 1887, Babington was referring to the belief that fertile hybrids might have a role in a version of Darwinian evolution, and not more generally to individual plants named as hybrids, which were usually, in any event, sterile.

However, in following Babington's correspondence to 1890, we have got ahead of ourselves. In the 1860s, Babington was not alone among 'splitters' in expressing concern about plant hybridity in relation to the moral virtues of philosophical practice.

Supposition over Graham's Willow

The practitioner we met in the introduction to this chapter, John Thomas Boswell Syme, was well-known as a 'species-making' splitter.[46] Typical of many mid-century aspiring philosophical botanists, he survived on occasional herbarium and writing work, and by editing a third edition of the leading tome *English Botany,* before

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[44] C.C. Babington to Rev. E.S. Marshall, 14 March 1890 (Babington 1897: 429). On Marshall, see Allen 1986 chapter 8.
[45] C.C. Babington to F. J. Hanbury, 13 September 1887 (Babington 1897: 414).
[46] Allen 2010: 224.

eventually inheriting his Scottish estate in 1868.[47] Well-connected with philosophical practitioners, his role as distributer for the London Botanical Exchange Club (the successor of the Botanical Society of London, then a clique of 37 members), provided him with a rich supply of specimens, despite his relative geographical isolation in Fife.[48] In the same year that he moved up to Scotland, he received the Graham's willow specimens for distribution.

Syme is sometimes portrayed by botanist-historians as hostile to hybridisation in nature.[49] Yet he stated in print that he was 'strongly inclined' to agree with continental taxonomists, that many of the intermediate forms in the willows, like Graham's willow, were hybrids. However, he declined to use hybrid names in *English Botany*:

> I have derived great assistance from Dr Wimmer's 'Salices Europeae' and the admirable 'Monographia Salicium' of Mr Andersson, but although I strongly incline to the conclusion arrived at by these writers, namely, that a great number of the forms are hybrids, I have not ventured to use the hybrid nomenclature until this question shall have been satisfactorily settled.[50]

Willows dominated European taxonomic botany in the form of two monographs, by the German taxonomist Christian Friedrich Heinrich Wimmer (1803-68) and the Swedish botanist Nils Johan Andersson (1821-80).[51] Andersson's approach was considered more moderate, with fewer hybrids, and most of these forms were sterile, in contrast to the fertile hybrids contained in Wimmer's work. On balance, Syme felt hybridisation in nature was a 'supposition':

> The great abundance of these so-called hybrid forms, and the fact that some of them shade imperceptibly into one of the supposed parents but not into the other, are the two chief points which may be urged against the supposition of their hybrid origin; [52]

In chapter two, we saw how the philosophical botanist Hewett Watson referred to hybridisation in nature as a 'mere hypothesis' and it seems likely that Syme's caution

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[47] Biographical details from Allen 1986 and 2004a.
[48] Allen 1986: 77-9.
[49] E.g. Meikle 1975 and Meikle and Tennant 2015.
[50] Boswell Syme 1868: 201.
[51] Wimmer 1866; Andersson 1868.
[52] Syme 1868: 200-201.

may have in part stemmed from Watson's guidance.[53] A 'supposition' was sometimes used interchangeably with an 'hypothesis' and both terms contrasted (unfavourably) to an 'inductive theory'.[54] The British version of science developed in the 1830s by the BAAS drew a division between knowledge-making from the sober, stable accumulation of inductive facts, less desirable deductive reasoning involving hypotheses, and dangerous speculative theorising. An enduring anxiety among British elites was that speculation encouraged atheism, produced radical politics, and led to revolution. [55] These fears had also been recently publicly highlighted by reviews of Darwin's *On the Origin of Species*. Historians recognise that, in 1860, the conservative critique of the book centred on accusing Darwin of unphilosophical speculation. Any argument perceived as falling short of inductive method might be construed as speculation, an extremely serious charge of unphilosophical behaviour. [56] As part of the recovery from that charge, in 1863 as President of the Linnean Society, George Bentham did important philosophical leg work in rehabilitating Darwin's 'hypothesis' as a legitimate position in science, and distinct from speculation.[57] Therefore, it is easy to understand why Syme hesitated over what he saw as the 'supposition' of hybridisation among the willows and connected this to recent sensitivities around speculation.

In sum, Babington's and Boswell Syme's commitment to the epistemological morals of philosophical practice dictated caution about hybridisation. These virtues included grounding taxonomic decision-making on extensive labour and care, and to avoid any possibility of a charge of unphilosophical speculation. This explanation does not deny that Babington's religious belief underpinned his views; or, indeed, that Watson's transmutationism may have guided Syme's approach. However, as we discussed in the introduction to this thesis, a narrow focus only on conceptual barriers from Christian (or other) beliefs about the species can obscure cultural factors in explaining scientific practice. Therefore, during the 1860s, philosophical botanists held

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[53] On Syme as Waston's enthusiastic assistant, see Allen 1986.
[54] In the 1830s and 1840s, leading BAAS gentlemen of science debated what might amount to philosophical method in science and emphasised the centrality of induction to philosophical behaviour. (Hull 2009).
[55] Harrison 2015: 156-7.
[56] For an extended discussion of speculation in mid-century British science supporting this thesis and in relation to Darwin's theorising, see Sponsel 2018.
[57] Bellon 2003.

differing interpretations of intermediate forms. Their reasons for opposing the recognition of plant hybrids related not only to taxonomic approach, species concepts, or religious belief, but more generally, and therefore more persuasively, to the epistemological morals expected of a philosophical practitioner.

III. New Insights from the Continent

Willow Experiments Link Hybridising to a New Darwinian Biology

In 1865, a German-speaking lawyer, Max Ernst Wichura (1817-66), published a series of experiments from breeding willows in his garden in Breslau, an industrial city then in part of the Prussian Empire.[58] Wichura announced that he had found wild willow hybrids and confirmed the identification of these hybrids by hybridising the putative parent species, and demonstrated that the fertile hybrid progeny propagated themselves by selfing. Wichura's most dramatic hybridising experiment produced a willow whose pedigree included no less than six species.[59] His publication on willow hybrids also connected plant hybridisation with a Darwinian explanation, to argue that fertile hybrids might not persist outside of cultivation as they were 'less fit' in the struggle for life.[60] Wichura sent a presentation copy of his book to *the* authority on hybridity, Charles Darwin.[61] Darwin wrote in response: 'The extreme frequency of Hybrid Willows is quite a new fact to me.'[62] The book, Darwin explained to another correspondent, 'quite convinced me that in Europe there is a multitude of spontaneous hybrid willows.'[63]

From 1860 Darwin had embarked on a programme of physiological plant studies. As we saw in chapter two of this thesis, one of the first things he did after the publication of the *Origin*, in the Spring of 1860, was to look back to his botanical mentor Henslow's traditional experimental practice of hybridising. Compared to Henslow, Darwin sought to answer different questions with his *Primula* crossing. Yet,

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[58] On Wichura, see Roberts 1929; Olby 1985; Lorenzano 2012.
[59] Berkeley 1866: 59 commenting on Wichura 1865.
[60] Wichura 1865: 92-4 translated by Berkeley 1866: 81.
[61] Desmond and Moore 2009.
[62] C.D. to M.E. Wichura, 3 February [1865] (DCP 2014 letter 4765A).
[63] C.D. to B.D. Walsh, 27 March [1865] (DCP 2014 letter 4797) cited by my study on Darwin and hybridisation as O'Reilly 2014: 64-65.

in 1868, he also published a paper on the taxonomic identity of these plants, which served to ratify the long-held views of many local botanists and gardeners about the widespread mock oxlip forms. While Darwin did not intend his theory to affect taxonomic practice, his botanical practices during the 1860s stood in contrast to Hooker's practice of taxonomy based solely on dried herbarium material; appearing more like the physiological approach among continental botanists. Plant hybridity was already a central topic of enquiry, especially as in 1860, the Académie des Sciences, Paris, offered the physical science prize for a study on hybridity in plants. The competition produced a prominent discussion of the production of fertile plant hybrids, referred to as 'constant hybrids', by French professor Charles Victor Naudin (1815-99) (although he had first suggested that species arose by crossing in 1856): [64]

> Plusieurs botanistes d'une grande authorite croient que certains hybrids fertiles, sinons tous, peuvent se fixer et passer a l'etat de varieties constants, c'est-a-dire de veritables especes.
>
> Many leading botanists believe that certain fertile hybrids, if not all, may become fixed and pass to a state of constant varieties, that's to say, true species. [65]

Thomas Henry Huxley's (1825-95) evaluation of Darwin's theory also suggested that physiological breeding studies were needed: natural selection could not be considered a *vera causa* for the origin of species until varieties had been produced by artificial selection that were sterile with each other and with their parent forms.[66] While British philosophical practitioners were cautious about continental claims about the laws of hybridism, Wichura's willows book interpreted fertile hybrids in line with Darwin's own view of the overriding importance of adaptation, instead of suggesting these hybrids might become new permanent forms.[67] The book also highlighted how physiological experimentation using hybridising might investigate Darwinian questions. Darwin was delighted.

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[64] Föcke 1881: 505, 509.
[65] Naudin 1863: 196 (thesis author's translation).
[66] See Burkhardt *et al.* 1997: Appendix VI on Huxley and Darwin's exchange over hybridism and this wider context of a heightened interest in hybridity from 1860.
[67] Bentham 1864 summarised the philosophical response in Britain to Godron and Naudin's work, with input from Hooker and Darwin. On Darwin's view of adaptation in his treatment of the species, see Stamos 2007 and 2013.

Darwin's annotated copy of Wichura on willows circulated among several philosophical botanists. Darwin lent Hooker his copy and had to repeatedly plead with him to return the book. [68] Hooker was most likely disinterested at this time, reassured after Bentham's conservative appraisal of the latest position on hybridism delivered at the Linnean the year before.[69] Darwin's copy travelled next to another heavyweight BAAS gentleman of science, the Reverend Miles J. Berkeley.[70] Berkeley had previously translated continental hybridists' studies for the *Journal of the Royal Horticultural Society* and so was an obvious choice to produce an abstract of Wichura's book, which, he explained, drew on Darwin's marginalia.[71] Bernard Lightman has shown how Huxley's strategy to reform British science included a major programme of translation of French and German scientific texts, and the importance of these translations for circulating scientific work with a Darwinian flavour.[72] Berkeley was no Darwinian, but his translation similarly served to promote hybridising as a form of physiological botany associated with Darwin's practices. He cautioned readers that Wichura's claims about fertile willow hybrids might suggest 'the conversion of one species into another', adding dismissively, 'it appears from Wichura's experiment that it requires only four years.'[73] Berkeley concluded that whatever was thought of Darwin's theory, Wichura's experiments followed on from the admirable approach set by Darwin in his *Orchids* book, published three years earlier in 1862. Berkeley's review of *Orchids* had proclaimed Darwin as the premier philosophical botanist of his day: 'What powers of observation, investigation, and experiment—what infinite skill, close reasoning and sound judgment—and, after all, this is only a little episode in his great labours!' [74] Berkeley anticipated here what George Bentham, would announce the following year in his May 1863 Presidential Address at the Linnean Society. Bentham's stance compellingly shows that, whatever was thought of the evolution or of natural selection, it was Darwin's physiological experimentation that set the model for how British philosophical practitioners might conduct biology. For Bentham, Darwin's

[68] C.D. to J. D. Hooker, 22 and 28th [October 1865] in Burkhardt *et al.* (1985-) volume 13 p.279. This was the third request that Darwin had made in six months.
[69] Bentham 1864.
[70] Biographical details in chapter two, p. 60.
[71] Berkeley 1866: 57.
[72] Lightman 2015.
[73] Berkeley 1866: 61.
[74] Berkeley 1862: 554.

botanical project was more important than the theory of evolution.[75] As it turned out, historian Richard Bellon establishes how Darwin's physiological experimentation in his Orchid book, led to the acceptance of evolution.[76] This chapter develops Bellon's insights to consider the impact of Darwin's botanical studies on how practitioners regarded plant hybridity.

Wichura's hybridising experiments, with their Darwinian connection, reached a general intellectual audience via an enthusiastic review in an aspiring 'first-class literary newspaper', the *Reader* (1863-67).[77] This short-lived monthly paper aimed to unite liberal readers with interests in both science and theology. Behind the scenes, however, from 1865, Huxley directed the science section.[78] As an obscure German pamphlet on hybrid willows, Wichura's book was most likely reviewed because it supported Darwinian theory.[79] The willows book was 'teeming with original matter', its reviewer proclaimed.[80] Wichura's microscopic study comparing the pollen of pure and hybrid willows was especially valuable for the clear laws that this produced: that a sterile hybrid might be identified by its malformed pollen grains when viewed under a dissecting low power microscope. The author of this review already accepted that *Salix* 'abounds in natural hybrids', evidenced by observations of a 'confused mass' of forms of willows on the banks of the River Dee, Aberdeenshire. They accepted the Darwinian explanations given, including that many hybrids 'are lost in the Darwinian struggle for life'. Where Wichura speculated about the nature of fertile hybrids, and possible conversion into species, the philosophically minded reviewer suggested that such hypotheses of hybridisation leading to species formation were 'a matter for fresh examination rather than proof'.[81] Darwin praised this abstract as 'a capital resumé' and it was reprinted verbatim in the *Gardener's Chronicle*.[82] It seems likely that Maxwell

[75] Bentham 1863. Bentham believed that the elevated botanical expertise Darwin demonstrated in his *Orchids* book might only be conducted by a philosophical practitioner (Bellon 2003). The interpretation here of the relations between taxonomic practice and Darwin's studies is closer to Bellon 2011.
[76] Bellon 2011.
[77] From the Editor's Statement of Purpose front piece advertisement, in the *Reader: A Journal of Literature, Science, and Art* v.1, 3 January 1863 (unpaginated).
[78] Huxley effectively took over the editorship in autumn 1865 (Gooday 2004).This group behind the science section then founded the journal *Nature* (Baldwin 2015).
[79] On Huxley's control of the editorial policy of periodicals to advocate for Darwinism, see Baldwin 2015 and Barton 2018.
[80] Anon. 1865a: 631.
[81] Anon. 1865a: 631.
[82] C.D. to J.D. Hooker, 17 June 1865, DCP letter 4862. Anon. 1865b: 794-5.

Tylden Masters (1833-1907) was the author of this review. The *Reader's* book reviewers were often drawn from the University of London, where Masters lectured in botany. The article was reprinted verbatim in the *Gardener's Chronicle*, and Masters formally took over the paper's co-editorship a few weeks later.[83] Meanwhile, Darwin's copy of the willows book, reached an Anglican vicar and headmaster, Henslow's son, George Henslow (1835-1925).

George Henslow had asked Darwin for help with writing a *Popular Science Review* article titled 'Hybridism Among the Vegetables' and received Darwin's copy of Wichura's book in around March 1866.[84] Henslow's essay emphasised two things: firstly, a Darwinian take on the laws of hybridism, so far as these were known; secondly, that the British horticultural hybridists (notably William Herbert) already knew much of what continental hybridists claimed as new. A prominent horticultural hybridist had remarked on Wichura's willows book to Darwin: 'Naudin and Wichura's observations are good & interesting, especially the "loves of willows", highly confirmatory, but *new*, no.'[85] Henslow's essay circulated among local botanist subscribers to natural history society journals and was abstracted in the *Transactions of the Edinburgh Botanical Society*. This meant that local botanists heard about Wichura's willows thought the lens of Henslow's account, which emphasised the importance of British horticultural hybridising and connected this practice to Darwinian biology.

In sum, British philosophical botanists interpreted Wichura as confirming the existence of hybridisation in nature. More significantly, his research stood as an example of how an emerging Darwinian biology encouraged the experimental study of plants by hybridising. What those experimental botanical studies might look like had been alluded to by Masters, the likely reviewer of Wichura's book in the *Reader.* In the

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[83] The anonymous author was familiar with the practical process of hybridising and their view of variation in nature fits with Masters' approach in his 1860 lecture, that variability arose when a plant was removed from the position to which it was adapted (Masters 1862). Masters knew he was taking on co-editorship of the *Gardeners' Chronicle* with curator of Chelsea Physic Garden Thomas Moore (1821-87) in July 1865, so shortly before this review appeared in August, and his appointment in October 1865 (DCP letter 4886). On Moore, see chapter four of this thesis, p. 185.
[84] Henslow 1866. George Henslow to C.D., 12 March 1866, DCP letter 5033. Darwin then corrected the proofs of Henslow's paper but asked not to be acknowledged (C.D. to George Henslow, 12 June [1866], DCP letter 5118).
[85] Richard Trevor Clarke to C.D., 6 November [1866], DCC letter 4932.

next subsection, we see how Maxwell Masters was pivotal behind the scenes in promoting hybridising as a Darwinian experimental practice.

Maxwell Masters and the Botanical Congress Endorse Hybridising

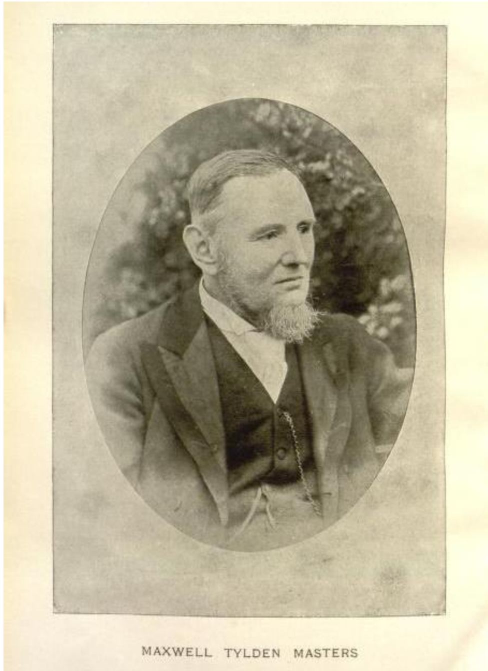

Figure 3.4: Portrait of Maxwell Tylden Masters (1833-1907). From: Britten 1907: 256 (Reproduced from the BHL Archive under the Creative Commons Attribution License).

In 1860, at the age of twenty-seven, Maxwell Masters was an administrator for the Royal Horticultural Society, and an aspiring philosophical botanist. As an early Darwinian enthusiast, Masters swiftly sought to legitimate his own taxonomic project on teratology, the study of what were commonly called 'monstrosities' or abnormal forms.[86] He argued that the study of peloria in different genera might support an evolutionary origin of species, and enable a genealogical classification, although Darwin was unconvinced.[87] Masters was also the son of a nurseryman-hybridist, with first-hand experience of conducting hybridising.[88] He realised that a revitalised Darwinian botanical taxonomy might fuse physiological information with morphological, in a way which continental systematists recognised as the most desirable combination of data, yet which British philosophical botanists had scarcely attempted.[89]

Maxwell Master's ideas about how horticultural hybridising might become a part of a revitalised Darwinian taxonomy, were further developed at successive meetings of the International Horticultural Exhibition and Botanical Congress. Designating himself as 'Honorary Secretary', Maxwell Master's apparently organised much of the first Congress, the London meeting in 1866.[90] The London Congress

[86] Masters 1856. Masters was working on a monograph on plant tetratology (Masters 1869).
[87] C.D. to M. Masters, 25 April [1860], DCP letter 4818.
[88] Boulger and Stearn 2004.
[89] Masters 1863: 6.
[90] Elliott 2004: 27. Masters asked Darwin to join the organising committee (he declined) (Maxwell T. Masters to C.D., March 1866, DCP Letter No. 5022). On the congress generally, see Stafleu 1970.

resolved that the naming of wild and cultivated hybrids was an urgent matter demanding the attention of both horticulturalists and botanists. The 'confused nomenclature' and use of 'fanciful' names by nurserymen for their cross-breeding and hybridising productions must be distinguished from those plant forms of interest to science.[91] Significantly, this topic was proposed jointly by a British horticulturalist and a German taxonomist. [92] As we mentioned in the introduction to this thesis, the British were, by mid-century, renowned as leading the world in horticultural hybridising, as specifically distinct from more regular cross-breeding practice; whereas continental botanists tended to be associated with taxonomy, and field work finding wild plant hybrids.[93]

Maxwell Masters in his editorial in the *Gardener's Chronicle* portrayed the London Congress as one of the most memorable events of the year, alongside 'signs and wonders, wars and rumours of wars, dynasties overthrown, and a new electric cord deposited on the bed of the Atlantic, and, more marvellous still, an old one fished up for repairs.'[94] The London Horticultural and Botanical Congress rivalled the telegraph as a display of progress and improvement, showcasing the 'general superiority of English gardening' whereas the subsequent Paris meeting would deal with 'sundry specialities and matters of detail'. Indeed, at the following Congress in Paris in August 1867, the relations between horticultural hybridising and botanical taxonomy were explicitly set out. The Congress sanctioned the first Laws of Botanical Nomenclature, drafted by the Swiss taxonomist, and Congress President, Alphonse Louis Pierre Pyramus de Candolle (1806-93). Article 12 of the Rules defined a hybrid as a cross between distinct species. These rules distinguished for the first time between hybrids in wild plants and the production of artificial hybrids.[95]

Masters later commended to his readership the new nomenclatural rules agreed by over 100 international botanists at Paris as 'a practical result of a gratifying character' from an otherwise unimpressive meeting.[96] Although Masters apparently

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[91] Anon. 1866: 21.
[92] Shirley Hibberd and Professor Koch (Anon.1866: 21).
[93] Meikle 1975.
[94] Anon. [Masters, M.T.] 1867: 5.
[95] De Candolle 1868.
[96] Anon. [Masters, M.T.] 1867a: 900; and Anon. [Masters, M.T.] 1867b: 6.

did not attend the Paris Congress, he received regular reports from the *Chronicle's* correspondent who sat on the subcommittee discussing De Candolle's proposals. The Paris Congress agreed to name both artificial and naturally occurring plant hybrids using a double-barrelled epithet, but this was only to be used where the form was 'experimentally demonstrated':

> the combination of the two names shall be only employed when the origin of *the hybrid has been experimentally demonstrated;* that is to say, when both parents are known. In all other cases, and these are undoubtedly the most numerous, the name must be analogous to ordinary specific names… In another point of view the motive which prompted this decision is an excellent one: too much cannot be done to oblige authors to be accurate; now, to assert that an offspring is of such and such a parentage, when no evidence can be produced, is anything but accuracy. [emphasis in the original] [97]

De Candolle later emphasised that the experimental evidence 'must be unimpeachable' before a hybrid could receive a name.[98] Wichura's hybridising experiments provided the requisite evidence of the parentage of some willow hybrids in Wimmer's classification, but not all. However, some readers of the *Gardeners' Chronicle* were unconvinced by the new rules. Several correspondents objected to botanists telling horticulturalists what to call their plants. They cited the example of a new *Coleus* hybrid and complained that new rules had no basis in the realities of hybridising; clearly these botanists did not realise that the same cross between two species might produce very different-looking plants.[99] Maxwell Masters responded in an editorial comment that it was up to the RHS' new Scientific Committee to verify and publish all the evidence for a hybrid, as 'the Society should have no trade secrets', thereby hinting at what underlay some of these objections. Further, the matter of the *Coleus* should be 'cleared up, as the plants have been publicly distributed and names given to them with its [the RHS] sanction and authority.'[100] The RHS Scientific committee had ambitiously set itself up as the body to regulate the naming of *all* plant hybrids, whether wild or cultivated.

[97] De Candolle 1868: 48-9.
[98] De Candolle 1868a: 460.
[99] ['Mistus'] Anon. 1868: 407; ['Registrar'] Anon. 1868: 406; and [T.M.] Anon. 1868a: 434 (correspondence from 18 and 25 April 1868 and referred to in De Candolle 1868a).
[100] Anon. [Masters, M.T.] 1868a: 460.

The RHS Scientific Committee and Darwinian Hybridising

In announcing the new Scientific Committee to the *Gardeners' Chronicle* readers in 1868, Masters noted that the committee's founding 'main object' was (he quoted from the proposals): "to promote and encourage the application of physiology and botany to the purposes of practical culture, and to originate experiments which may assist in the elucidation of horticultural subjects."[101] The RHS committee combined leading philosophical botanists with horticultural hybridists, including Major Trevor Clarke (1818-97) (who had spoken on wild and cultivated hybrids at the London congress); Isaac Anderson-Henry (in-coming President of the Botanical Society of Edinburgh); and Charles Darwin, who was explicitly invited by Masters on the basis of his experimental reputation. [102] Masters envisaged that the new Committee would act like a learned society, with an intentionally broad remit, so 'communications from physicists, from chemists, from botanists—in a word, from experimenters and observers in all and any department of knowledge that has a bearing on plant life, should be solicited.'[103] The Scientific Committee would oversee experimenters, like the hybridists who he believed had made major contributions to plant physiology:

> It may be said that there is a dearth of physiological botanists—that is true now, but it need not always be so; and amongst our amateurs and professional horticulturalists are there not such observers and experimenters as DARWIN, as DOMINY, as HENRY, as PAUL, as CLARKE, as RIVERS,—but to mention other names would be invidious.[104]

The RHS Scientific Committee, therefore, publicly claimed Darwin as a horticulturalist, and therefore his hybridising as science. Such a prominent claim was a significant risk for Masters. Gardening was still socially uncomfortable and hybridising associated with the nursery trade. Dominy, Rivers and Paul were public figures among horticultural communities yet did not have the social advantage of Clarke or Henry as gentlemen enjoying an inherited private income. RHS Historian Brent Elliott emphasises how

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[101] Anon. [Masters, M.T.] 1868: 235 citing the agenda for the RHS Council Minutes for 21 April 1868, (hence this quotation is in double quotation marks).
[102] On the RHS Scientific Committee see, Elliott 2004 and Elliott 2010a with a list of the founding members. On Clarke, see Elliott 2014: 42-44.
[103] Anon. [Masters, M.T.] 1868: 236.
[104] Anon. [Masters, M.T.] 1868: 236. John Dominy (1816-91) of Veitch & Son was Britain's top orchid hybridist (Elliott 2010). On Anderson-Henry, see the introduction to this thesis, p. 33-4. William Paul (1822-1905) nurseryman hybridist, co-editor of the *Florist and Pomologist* and horticultural author, sat on the Executive Committee of the 1866 Botanical Congress (Desmond 1994: 540; Anon. [Masters, M.T.] 1905). T. F. Rivers (1831-99) was a nurseryman at Sawbridgeworth, Hertfordshire (Desmond 1994: 786).

these appointments of nurserymen to the Scientific Committee were socially controversial in 1868. For example, Paul's solid middle-class background saw him eventually made a Fellow of the Linnean Society in 1875, yet even by 1899, as we will see in chapter five, a gardener-hybridist making major contributions to taxonomy was only accepted as an Associate Fellow. Therefore, these appointments to the Scientific Committee represent the degree to which Masters was prepared to push the standing of horticulture, and reflects the confidence felt by Darwinian botanists in 1868, the year that Hooker took the Presidency of the BAAS and announced in his address the philosophical approval of evolution.[105] This also illustrates how an improved social status for hybridists stemmed from Charles Darwin and the botanical experimental practices of a new Darwinian biology.

Finally, even a philosophical unbeliever in hybridisation, Joseph Hooker, had to accept that Darwin's observations, and Wichura's willow experiments, made hybrids more significant than he had previously acknowledged. Hooker delivered his verdict on Wichura's willows in 1870 in the *Student's Flora*. Jim Endersby shows how this textbook epitomised Hooker's philosophical science.[106] It also marks a formal recognition of the emerging move to associate local botanists with a site of practice rhetorically distinct from the museum or laboratory, as 'field' botanists. Hooker aimed 'to supply students and field-botanists with a fuller account of the Plants of the British Isles than the manuals hitherto in use aim at giving'. [107]

The *Student's Flora* was produced with Hooker's lumping approach, but incorporated twenty-three hybrids. He had shifted, from in 1855 almost portraying hybrids in nature as a figment of the splitters' imagination, to including a careful evaluation of their reported forms in 1870. Of the twenty-three hybrids mentioned in his textbook, nine were willows, more hybrids than in any other genus. He pronounced unambiguously that a willow, re-made by Wichura, 'is a hybrid' and said plants were 'probably' or 'supposed' hybrids elsewhere.[108] Hooker did not accept all of Wichura's hybrids, and did not follow Andersson's accounts slavishly, for example, he criticised

[105] The context of the formation of the RHS Scientific Committee here follows Bellon 2011.
[106] Endersby 2008: 13-15.
[107] Hooker 1870: preface, p.i.
[108] *Salix purpurea* L.× *S. viminalis* L. (Hooker 1870: 343).

the Swede's comment on the flowering time for goat willow *Salix caprea* L. as it was wrong for England.[109]

Hooker was likely persuaded most of all by Andersson's traditional herbarium practice. He chose to follow Andersson rather than Wimmer, because 'he knows the willows thoroughly & has monographed all the species most carefully & ably after studying all the European Herbaria'.[110] However, Hooker also gave more weight to experimental hybridising than he had done previously. A common British basket willow, which Wichura had experimentally re-made, he accepted outright. Andersson relied on some of Wichura's experiments, which Hooker then followed with the 'probable' qualification, while he rejected the triple hybrids and greater combinations that Wichura had made and Wimmer had included. Further, beyond the willows, he now stated that the well-known intermediate avens *Geum intermedium* Ehrh. was a hybrid, based on the experimental hybridising conducted by Bell Salter, noting that Salter's hybrid plant was fertile, despite having previously dismissed Salter's hybridising as unreliable: he had previously stated that the *Geum* was 'explained away by the assumption of hybridism'.[111] In the second and third editions of the *Student's Flora* of 1877 and 1884, the total number of willow and other hybrids mentioned steadily increased.[112]

Hooker would also have seen Wichura's willow hybrids as potentially of economic value. An image of Arcadian waterways lined with weeping willow trees evoke a picturesque image (well-known from George Eliot's Midlands novels like *The Mill on the Floss* (1860)) that can distract us from the reality of botany in mid-Victorian Britain. The willows were, above all, a crop. The fast-growing hybrids that Wichura had observed on riverbanks might prove to be too lucrative a resource for Hooker to

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[109] Hooker 1870: 337.
[110] J.D. Hooker to C.D., [before 6 May 1858], DCP Letter 2277. Andersson had started work on his monograph in 1857, the same year that a Prussian field botanist Wilhelm Lasch (1786-1863) reported extensive observations of wild willow hybrids (Lasch 1857; Meikle 1975).
[111] Hooker 1870: 114; Hooker 1877: 119 and Hooker 1884: 122. Jos D Hooker to C.D., 10 July 1856, DCP letter 1923. See Chapter Two of this thesis on Bell Salter's hybridising experiments.
[112] Hooker 1877 and 1884. The Kew herbarium taxonomist who named Graham's Willow, John G. Baker (1834-1920) contributed genera to, or even produced, these editions (Allen 1986: 88; Allen 2010: 238 but without citing his primary source). Hooker nonetheless must have sanctioned the contents given the philosophical importance he placed on the book (see Endersby 2008: 13-15).

dismiss.[113] Hooker was developing Kew's economic botany collections and the new RHS Scientific Committee provided a respectable front for these fiscal interests to be developed with Kew in a controlling position. This economic motive was left unmentioned, not least because, as we have seen, some commentators complained about the inclusion in a learned committee of businessmen lacking the independence of a gentleman of science.[114] The RHS Scientific Committee included several hybridists with commercial interests: Major Trevor Clarke was collaborating with British plantation owners in developing cotton plant hybrids; and by the late 1870s, pharmaceutical chemist John Eliot Howard (1807-83) was hybridising species of *Cinchona*, or 'fever' tree, in his garden glasshouses, to develop improved yields of quinine to treat malaria. Howard took 'great interest in the question of hybridization' especially regarding whether hybrids might produce a stable product.[115] Despite some complaints about the RHS Scientific Committee involving businessmen, by 1874 Howard was elected a Fellow of the Royal Society, symbolising his acceptance within philosophical science.[116]

While one underlying driver for Hooker's reconsideration of plant hybrids may have been economic, like Maxwell Masters and others involved in hybridising, he was keen to align hybridity with botany, rather than commerce. One member of the new RHS scientific committee, Anderson-Henry, whose hybridising we heard about in the introduction to this thesis, decided that hybridists might be about to have their moment in science.

Anderson-Henry responded to his reading of *On the Origin of Species* by writing to Darwin about his own hybridising practice, advertising his experimental services, specifically, his skill of hybridising *species* (not of cross-breeding varieties).[117] In his

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[113] Stott 1992 (includes a historical review).
[114] Elliott 2010.
[115] Anon 1883: 36.
[116] Boulger and Satchell 2010. A material cultures study on malaria includes Howard's 1880s liaison with Kew over plantation activities in India (Deb Roy 2017). Kim Walker is completing a PhD at London Royal Holloway including Howard titled: 'Biocultural Collections and Networks of Knowledge in the Nineteenth Century: A Quest for Quinine'.
[117] Anderson-Henry read papers on plant hybridising at the Botanical Society of Edinburgh on 14 March and 14 November 1867 (Anderson-Henry 1867 and 1867a).

1867 Presidential Address after attending the London Congress, Anderson-Henry modelled his own experience hybridising on Wichura's book:

> Among those now in the field I would especially instance Naudin and Wichura, whose published researches in this department have stamped them as physiologists of no mean order, as close and discriminating observers, and generally just and sound in their conclusions. I may perhaps take occasion, in giving some of my own experiences, to point out how far they harmonise or conflict with theirs.[118]

He boldly promoted experimental hybridising to local botanists, giving a detailed 'how to' account (in part to elevate his own horticultural practice among a natural history-orientated audience). Anderson-Henry realised that hybridising provided an opportunity for farmers, gardeners and local cultivator-botanists to participate in scientific knowledge-making, both by making discoveries about plant physiology, but also by their moral behaviour as an 'industrious' and 'zealous' hybridist who might also improve the human condition:

> There is romance in the pursuit, and laurels to be gathered by every acute, industrious observer. If he make no grand discovery, he may zealously endeavour, and assuredly he will succeed in improving our common flowers, fruits, and vegetables, and, what is still more important, our cereals and grasses.[119]

Anderson-Henry gave this encouragement to cultivators in November 1867, a few months before the publication in February 1868 of Darwin's *The Variation of Animals and Plants under Domestication*. Other than the enthusiastic review written by Hooker in the *Gardeners' Chronicle*, the immediate wider reaction to the *Variation* was, according to RHS historian Brent Elliott, 'muted'. Yet Elliott also states that the *Variation* drove the establishment of the RHS Scientific Committee a few weeks later. It seems more likely that the call from the Botanical Congress for experimental hybridising (via Maxwell Masters) the previous year was instrumental in the establishment of the Scientific Committee.[120] The *Variation* was, however, added reason for Hooker to accept the importance of hybridising to botanical science. Either way, irrespective of the role of Darwin's book, the co-incidence of its publication with wider interest in hybridity, including Darwin's botanical papers on *Primula* and

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[118] Anderson-Henry 1867a: 208.
[119] Anderson-Henry 1867a: 208.
[120] Elliott 2010a: 44. On the horticultural reception of Darwin's *Variation*, see Elliott: 43-46. Harriet Ritvo imagines the response from animal breeders might have been largely indifferent due to preferring 'time-hallowed practices and beliefs.' (Ritvo 1997: 69).

Verbascum hybrids published later the same year, was an opportunity for Masters to promote a Darwin-inspired, experimental form of horticultural practice within science.

Therefore, between 1860 and 1870, we have seen how botanical experimental practices of an emerging new Darwinian biology included hybridising. Darwin's observations persuaded Joseph Hooker that hybrids existed in nature. The circulation of a book about willows, with a Darwinian explanation of fertile and sterile hybrids, encouraged Masters to promote hybridising as a form of physiological experiment that might contribute to plant physiology and taxonomy. The Botanical Congress set out hybridising as the scientific test for a plant to be named as a hybrid in both nature and the garden, and the RHS Scientific Committee under the control of key Darwinian botanists took on the role of co-ordinating hybridising experimentation.

This left the issue of *who* might conduct this hybridising. Hooker was not a fan of botanical experiments. He had previously grumbled to Darwin: 'This is just the way, whenever I do make an experiment it is sure to end either in smoke, or disappointment, or in a disgusting opposition to some preconceived theory of my own.'[121] He admitted in private to Darwin that he had barely attempted experimental crossing, and 'all my little attempts at it have failed'.[122] Yet after 1860, the need for trustworthy plant experimenters—specifically, Darwinian hybridists—became increasingly important.[123] For example, in 1878, Darwin was asked to find a 'young botanist capable of experimentation' on Russian wheat varieties. Darwin encouraged a 'peculiarly shy and reserved' Aberdeenshire farmer Alexander Stephen Wilson (1827-93), to take up the task.[124] Wilson was also a subscriber-member of the Botanical Society of Edinburgh. He must have read about President Anderson-Henry's call to hybridise cereals with interest, and he also read about wheat cross-breeding in Darwin's *Variation.* Wilson began hybridising wheat with other cereal species, from

[121] J.D. Hooker to C.D., 10 July 1856, DCP Letter 1923.
[122] J.D. Hooker to C.D., 20 April 1864, DCP Letter 4469.
[123] One example was John Scott (1836-80), a foreman gardener at Edinburgh Botanic Garden who exchanged at least 89 letters with Darwin (Grout 2004). Scott is included in a current PhD thesis on artisan practitioners in mid-century science by Laura Brassington. On trust as a core motif in British botany around 1800, see Easterby-Smith 2015.
[124] Anon. 1893: 665. C.D. to A. Stephen Wilson, 23rd February 1878, DAR 148:361 (unpublished transcription by the DCP).

around 1870.[125] He conducted 'between 400 and 500' hybridisings between wheat, spelt, rye, barley and oats. All failed except two cases of rye crossed with wheat. Following Wichura's example study of hybrid pollen, he described the 'arrested' growth of the pollen grains using a microscope.[126] His cereal hybrid was sterile, and therefore of little commercial value to him. Wilson's frustration with wheat hybridising was palpable. He had little to show for his 'scientific spirit':

> When an experimenter goes through a good deal of labour, perhaps he may be pardoned for expecting positive results. Undoubtedly the proper scientific spirit to cultivate is not to anticipate results at all. The results of the present experiments were mostly negative. [127]

These difficulties in part explain why no other farmers in Britain or elsewhere had attempted wheat hybridising (as distinct from wheat cross-breeding). In Wilson's case, while he aligned his natural history pursuits with his economic interest in producing an improved form of wheat, he was also directly responding to the promotion of hybridising within Darwinian science.[128] He spoke enthusiastically of Darwin's theory ('Though a believer in Evolution myself, I have always doubted the rapid transformations accepted by some') and offered to experiment more.[129] Darwin complained to his Russian contact about the dearth of such willing botanical experimenters. To Wilson, he encouraged: 'I have read several of your papers with much interest and hope that you will continue your experiments, as there are so few in Britain who experimentize on plants.'[130]

This chapter demonstrates that the botanical practices of a new Darwinian biology (and Maxwell Masters' rhetoric in the *Gardeners' Chronicle*) helped circumvent the social stigma that gardeners and nurserymen faced from the philosophical

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[125] A gentleman farmer-naturalist who initially worked as a civil engineer. He published 10 papers agricultural botany and mycology and five books, including two on the intersection between natural history and his Christian faith (Desmond 1994: 752).
[126] Wilson 1875: 288. Wilson corresponded extensively with Miles J. Berkeley who had reviewed and translated Wichura's book. Wichura's account in Berkeley's translation is apparently one of the first descriptions widely disseminated in English of how to observe and distinguish sterile from fertile pollen under a microscope, which Wilson did for his cereal productions.
[127] Wilson 1875: 496.
[128] In the second half of the nineteenth-century, farmers often aligned their economic and natural history interests (Holmes 2017).
[129] A. S. Wilson to C.D., 27 February 1879, DAR 181: 114; 5 January 1880, DAR 181: 115 (thesis author's transcription).
[130] C.D. to A. Stephen Wilson, 23rd February 1878, DAR 148:361 (unpublished transcriptions by the DCP).

botanists' community. In turn, this promoted their knowledge-making claim, which we heard from horticulturalists and gardeners in chapters one and two of this thesis, that hybridising might be knowledge-making contributing to science. Further, historian Jim Endersby has made the point that gardeners could now test Darwin's theory and relates this to the development of middle-class gardening and a new culture of participatory science.[131] However, Endersby's comment does not connect botanical experimentation to the practice of taxonomy. In botanical taxonomy at least, what has been little appreciated is that this participation by cultivators was now highly desirable. Hooker and Boswell Syme did not strictly follow the Botanical Congress' dictate over hybridising, still listing hybrids as 'probable' or 'supposed' in the absence of experimental evidence. The lack of experimenters within the philosophical community created an opportunity for the local botanist who could also cultivate and hybridise plants. In the final part of this chapter, we explore how Darwinian-inspired local botanist-cultivators took on resolving the classification of willows (and other plant groups with putative hybrids) by establishing their 'special knowledge' of hybridity.

IV. Hybridising as Darwinian Participatory Science

A Special Knowledge of Hybridity

In the introduction to this chapter, we saw how a philosophical botanist hesitated over the identity of Graham's Willow. This willow was published as a species in 1867, despite several botanists suspecting that it was, in fact, a hybrid.[132] At this time, the most likely combination, Boswell Syme believed, was a cross between two upland species, *Salix herbacea* L. and either *S. nigricans* Sm. or *S. phyllicifolia* L.; combinations that Wichura had not tested by hybridising.[133] Hooker, however, in the second edition of his *Student's Flora* in 1878, insisted that the plant was a variety of another willow species altogether (*S. myrsinites* L.), despite accepting other willow hybrids.[134] Neither

[131] Endersby 2016: 93-4.
[132] Baker 1867.
[133] Wichura 1865. By 1880, various combinations of alpine species of willow were reported from Scandinavia and one from Scotland (Föcke 1881: 366-7).
[134] Hooker 1878: 360-1.

man's verdict, we shall see, settled the question as far as local botanists were concerned.

This part of the chapter shows how local practitioners studied hybrids to develop their identity as taxonomic authorities within Victorian science. They also claimed their work to be within a Darwinian framework, even if their understanding of evolution did not resemble orthodox Darwinism. While historians recognise the emergence of 'specialism' as providing significant opportunities for local practitioners, and their adaptability and endurance, the study of a 'critical' genus as a distinct taxonomic practice has not been examined. For example, Sam Alberti has naturalists as expert collectors, but not as authorities in erecting scientific taxonomies.[135] British provincial practitioners were far less deferential, at least after 1870, to Kew, than some colonial practitioners were regarding their own form of local knowledge.[136] We consider the 'special' and 'critical' practice of three local botanist-cultivators, a former physician in Perthshire, Francis Buchanan White (1842-94); a market gardener in the Cambridgeshire fens, Alfred Fryer (1826-1912); and a vicar from Bournemouth, Edward Francis Linton (1848-1928). All cultivated and studied plant hybrids to develop their scientific authority, and Linton conducted hybridising to test his taxonomic claims.

Francis Buchanan White took a medical degree at Edinburgh in 1864 but never practised, instead enjoying a gentlemanly living from his family farms.[137] He began studying willows around 1880, he explained, as they were 'neglected' because they were 'difficult'.[138]

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[135] Alberti 2000, 2001, 2003; Finnegan 2009:3; Knell 2000.
[136] While colonial botanists might disagree with Hooker, Jim Endersby portrays them overall as in a far weaker social position: 'Colonial botanists clung to endemic species that validated their prized local knowledge.' (Endersby 2008: 149 and 143-50).
[137] Founder member and President of Perthshire Society of Natural Science and author of *Flora of Perthshire* (published posthumously 1898). For a useful descriptive biography drawing on archive material, see Taylor 1986.
[138] F. Buchanan White to Charles McIntosh, 9 November 1880 (Perth Museum Archives 1109/13).

Figure 3.5: Portrait of Francis Buchanan White (1842-94). From: Front piece in *Flora of Perthshire* 1898 (Author's collection).

Figure 3.6: Photograph of Perthshire Society of Natural Science excursion to Methven Wood, 30 June 1883, with Francis Buchanan White (1842-94) in centre. ©Perth Museum and Art Gallery. Reproduced with permission for non-commercial research use.

Buchanan White rejected the authoritative listing in the seventh edition of the *London Catalogue,* the final edition produced by Hewett Watson, and re-issued in 1883.[139] The catalogue had a bewildering array of 63 willow forms under 30 species and no hybrids.[140] Historian Simon Naylor, in his study of mid-late nineteenth-century regional science in Cornwall, emphasises that local botanists conformed to the *London Catalogue*.[141] Yet looking at a controversial group like willows, provides a corrective to the view that provincial practitioners were invariably subservient to metropolitan botanical elites. White studied this group because he felt Watson's official listing in the *London Catalogue* was simply wrong. Willows and their hybrids were notoriously variable so that White could claim that 'a practised eye is more to be relied on than the characters found in books'.[142] Further, Wichura's 'hybridisings', White explained, 'prove, by experiment, the truth of what had before been only—though on good grounds—suspected', that many of the intermediate forms were hybrids. White

[139] Allen 1983.
[140] Anon. 1877.
[141] Naylor 2010.
[142] Buchanan White 1891: 346.

eventually published a monograph on *Salix* based on 10 years of observations from field work and plants cultivated in his rockery.[143]

In the last quarter of the nineteenth century, local naturalists' communities emphasised those qualities that supported their identity within science.[144] Historians have, for example, highlighted naturalists' appeals to their 'accurate' observations, and naturalists' clubs emulating museum and university departments' organisational structures.[145] One such quality was Buchanan White's knowledge of place (coupled with, as Diarmid Finnegan shows, unabashed nationalism). [146] White bluntly corrected Hooker on the distributional details of some 41 Scottish plants in the *Student's Flora*.[147] However, another important way in which local botanists asserted their knowledge-making within Victorian science related to plant hybridity. Buchanan White chose a 'special' group requiring 'a practised eye' to develop his identity as a taxonomic authority.[148] Administrative historians regard 'special knowledge' as a term used for government inspectors in the emerging technocratic state of late Victorian Britain; and historians of science traditionally see 'specialism' as characterising late-century professional-institutional science.[149] Sam Alberti shows how local natural history societies emulated university departments to establish sections for different broad groups of study, such as zoology, botany and geology. A 'special knowledge' among local botanists, however, was something more fine-grained than a 'specialism' in bryophytes, or butterflies, or higher plants.

By the mid-1870s, local botanists significantly increased their efforts to find putative plant hybrids in the field. Botanist-historians David Pearman and Chris Preston have shown how the numbers of local botanists reporting plant hybrids in nature escalated from around 1875. They argue that this abrupt increase is unlikely to be attributed solely to the commensurable growth of local natural history societies in the

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[143] Buchanan White 1889: 77; Buchanan White 1891: 341.
[144] Finnegan 2009.
[145] Alberti 2000, 2001, 2003.
[146] Finnegan 2009.
[147] Memorandum from F. Buchanan White to J. D. Hooker undated [post 1870] (Kew Archives Director's Correspondence, folio 127). List of amendments and annotations to Hooker's *Student Flora* in White's handwriting (Kew Archives Director's Correspondence, folios 123-126).
[148] Buchanan White 1891: 346.
[149] See Woods 2013 and the classic account in MacLeod 1988.

last quarter of the nineteenth century.[150] Historians sometimes attribute this interest in plant hybrids among local botanists to the influence of individual 'great men' who took up the study of these forms.[151] Instead, this chapter argues that this interest in plant hybrids more likely stemmed from local botanists seeing the study of hybridity as one way in which they might build an identity for themselves and their communities within late-Victorian science.

These communities, the dispersed postal community of the Botanical Exchange Club, and the botany groups meeting in local naturalists' societies, established their knowledge-making by appeals to the moral qualities involved in the practice of naming plants as hybrids. While many naturalists' societies appealed to civic virtues, more dispersed botanical communities emphasised the patience, labour and care involved in taxonomic practice for difficult or 'critical' plant groups.[152] Their knowledge-making was, like Darwin's example in *Orchids*, morally legitimated by the patience, labour and care involved in their practice of studying a difficult group involving hybridisation. As so much remained down to the individual judgement of each botanist, what mattered was not so much the truth of White's system or determinations, but rather the 'care' and 'minute study' that went into the 'special' work of a local taxonomic practitioner, *entitling* him to make such judgements:

> On such differences of opinion only those are entitled to express their views who have given the same minute study to the group as both these botanists. Of the care and judgement manifested in the execution of the work there is no question, even though further investigation may modify his [White's] conclusions in a few cases.[153]

Buchanan White was praised for his 'unwearied patience' in the study of 'special groups' including the willows. This eulogy was a heavily biased account, yet it reveals exactly the qualities that its botanical audience (readers of White's *Flora of Perthshire*) aspired to, 'special' knowledge of a plant group like the willows.[154] White emphasised

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[150] Preston and Pearman 2015.

[151] E.g. Stevens 1997 on George Claridge Druce (1850-1932) as the great man directing BEC field work towards recording hybrids. As David Allen points out, Druce was not as respected as we might assume, in part due to his 'imperfect education' and unrespectable reliance on trade (he ran a pharmacy) (Allen 1986: 92 citing a letter from Edward Marshall).

[152] On these civic virtues of masculine muscularity, a wide range of literary interests, sociability, and public spiritedness, see Finnegan 2009, who does not identify the values of patience, care and labour in scientific work found by Bellon 2011 and 2018.

[153] Trail 1898: xxxviii.

[154] Trail 1898: xxxvii and on the 'special' knowledge a naturalist sought to develop, p.xxxvi.

the same moral qualities during his own account of his taxonomic work: In order to name hybrids and resolve the willows, he examined ‘in a living condition several thousand examples’.[155] White described his system for not mixing up material in his vasculum by fixing labels to each twig; he laboriously tied coloured threads to mark male and female willows so he could return to collect material from the same tree later in the year (reminiscent of the hybridist using coloured silks to mark his parent plants).[156] The ‘special’ knowledge that White displayed as characteristic of the salicologist, was also claimed by local practitioners studying hybrids in other plant groups.

Our next local botanist to emphasise his ‘special’ knowledge in connection with his study of plant hybrids was Alfred Fryer. Fryer had solid connections with the philosophical naturalist community: he turned down exploring the Amazon with school friend Henry Walter Bates (1825-92), and instead socialised with London literati, before inheriting and moving to rural Cambridgeshire.[157] Despite having to market garden to supplement his low income, Fryer saw the advantage of this experience for pursuing botany, which he took up as a hobby in the 1870s. His developing authority as a taxonomist was a product of his cultivation practices. His ‘continuous observation by artificial cultivation’ involved growing ‘countless thousands’ of pondweeds in tanks for over thirty years.[158] He felt strongly that only a local practitioner combining fresh and cultivated material might have unique insights that were unavailable to a philosophical museum or herbarium-based botanist; his ‘special’ herbarium practices preserved the crucial hybrid characteristics of the soggy uninspiring material: ‘No one save a special Pot man can know.’[159]

The special knowledge of determining plant hybrids was reserved for a few within the local botanist community: ‘This hybrid would almost certainly prove to be of frequent occurrence, if searched for carefully by competent observers; but its accurate determination requires a special knowledge possessed by a few.’[160] As

[155] Buchanan White 1891:336.
[156] Buchanan White 1891: 345.
[157] For biographical details see Evans and Britten 1912 and on Fryer’s taxonomic studies, see Preston 1988, 1988a and 1995.
[158] Fryer and Bennett 1915: vii and 61.
[159] A. Fryer to James Britten, 13 December 1889, Botany Library Archives, BM (cited in Evans and Britten 1912: 109).
[160] Hanbury and Marshall 1899: 46.

historian Anne Secord notes for another ‘special’ group earlier in the century, seaweeds, this had a democratising effect as philosophical practitioners turned to local botanists for their taxonomic knowledge.[161] Buchanan White and Fryer’s obituarists prioritised taxonomic authority as an idealised attribute of the highest achieving Victorian naturalist.[162] Local practitioners sought to reinforce their claim to authority by emphasising the care and patience required to determine putative hybrids, thereby adopting a strategy successfully employed by the BAAS to shape science into a morally acceptable mid-Victorian pursuit.[163] These moral qualities echoed those held by eminent Victorians more generally, not only in science, but also literary, religious and political elites.[164]

In the language local botanist’s communities used to describe their knowledge-making, their special knowledge and critical practice was aligned to that within a university degree. In 1871, William Carpenter, as Registrar of the University of London, explained to the Royal Commission on Scientific Instruction and the Advancement of Science: ‘A very special knowledge’ was the hallmark of a doctoral degree, whereas a bachelor’s degree in biology included ‘special knowledge to a certain degree as a critical knowledge of the genera and species of some particular group’.[165] Therefore, local practitioners claiming ‘a special knowledge’ appealed to a form of knowledge equivalent to that associated with not only a bachelor’s degree, but research, the core element of a higher science degree. This topic, of what attributes ought to be exhibited by a university professor of Natural History, was an extremely sensitive one for Buchannan White: in the 1870s he repeatedly attempted (unsuccessfully) to obtain a teaching post at a Scottish university.[166] For the first issue of his new journal, The *Scottish Naturalist* White commissioned Dr William Lauder Lindsay (1829-80), an Edinburgh physician and leading lichenologist to publish an essay on the knowledge-making that a local naturalist might contribute to science.[167] The wide-ranging ‘extensive’ scientific knowledge of a science professor stood in ‘strong

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[161] Secord 2011a.
[162] Trail 1895; Bennett 1912. A point not made by Finnegan 2009.
[163] Bellon 2011: 397.
[164] Bellon 2014.
[165] Anon. 1872: 540 (entry 7882, 2 May 1871).
[166] Trail 1898.
[167] Lindsay 1871 and 1871a. For biographical details on Lindsay, see Hawksworth and Seaward 1977.

contrast' to 'the knowledge of a man who makes himself the master of a limited subject… Such knowledge is *critical* and *profound'* (emphasis in original).[168] We need to be wary of over-stating the Huxleyan-inspired epistemic contrast between knowledgeable naturalists and ignorant Anglican professors, but nonetheless, clearly the local botanist was free to develop ways of knowing that an institutional role was perceived to curtail. Among local botanists, and especially those involved with the Botanical Exchange Club, such 'critical' knowledge-making relating to hybridity had become, as we will see in relation to the study of willows, a focal point of their taxonomic practice.

Local Botanists, Hybridity and a Darwinian Framework

Local botanists also made an explicit connection between their taxonomic work, hybrids, and Darwinian evolution. Several obituaries stressed that Buchanan White was 'a thorough-going evolutionist'. His 'unobtrusive but devout' Christian belief did not curtail his science; although the apologist author did add: 'he yet knew that, after all, evolution is but creation under a new name'.[169] While allowing for the panegyric exaggeration in these memorials, White placed hybridisation squarely in the context of the study of evolutionary relationships. Wimmer's work, he explained, was preferable as he gave hybrids a 'compound name', whereas Andersson used species names 'which do not in any way indicate the real or supposed parentage'; in other words, the hybrid's 'pedigree'.[170] Naming was, as part of taxonomy, to be on evolutionary principles.

As Buchanan White was a keen gardener and knew Wichura's work, it seems plausible that he might also have been using Wichura's technique to assess willow pollen sterility or fertility. Indeed, Wichura's book contained much of practical value to propagators, not least the pollen study that Maxwell Masters' review had praised. For example, *Cultivated Plants; their Propagation & Improvement* (1877) by Frederick William Thomas Burbidge (1847-1905), which rapidly became the standard gardening manual on hybridising. Its author, a former Kew gardener, included a chapter on 'Natural Hybridism', heavily cited Darwin's books, and presented details on hybridising

[168] Lindsay 1871a: 66.
[169] Coates 1895 cited (uncritically) by Taylor 1986. See also Trail 1898: xl.
[170] Buchanan White 1891: 342.

directly from Wichura's book, remarking on his technique to preserve willow pollen in honey.[171] Burbidge explicitly tied the practice of hybridising to Darwinian biology. Burbidge quoted Darwin under his 'Hybridising' chapter's heading:

> "One new variety raised by man will be a more important and interesting subject for study than one more species added to the infinitude of already recorded species."— Darwin: Origin of Species.[172]

Burbidge intended his book as a 'stepping stone to works of a higher scientific character, and more especially to those of Darwin.'[173] In 1874, the *Spectator* disparagingly referred to gardeners as 'labourers', uneducated and uncivilised. Burbidge in the editorial of the *Garden* (the cheapest of the weekly horticultural papers with a readership predominantly of working men) rebuked the *Spectator* for being out of date. Gardeners were now doing valuable scientific work, and above all, cited the example of hybridising.[174] This scientific work, for Burbidge, was contributing to taxonomy, by investigating relationships and variation: 'By hybridisation we may prove the natural affinity of plants far better than by poring over herbarium specimens.'[175] Burbidge cited Darwin's proof of the hybrid origin of the common oxlip by hybridising and also believed that the vast number of wild orchid species arose by natural hybridisation.[176] Therefore, from the late 1870s, many horticulturalists and gardeners interested in plant taxonomy most likely worked within a Darwinian context, guided by Burbidge's manual.

Buchanan White and Burbidge were not alone in placing plant hybridising within a form of Darwinian biology, and plant hybridisation within an evolutionary framework. Alfred Fryer is remembered by botanists because he was the first person to recognise naturally-occurring hybrids among pondweeds—ahead of any continental or American workers.[177] However, of more interest to historians is the fact that Fryer documented the thinking behind his taxonomic choices. Fryer explained that he was

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[171] On the manual's status, see Elliott 2010: 57. Burbidge wrote for the *Garden* 1870-77 in his mid-twenties, before Hooker engineered his appointment as Curator of Trinity College Botanic Garden, Dublin from 1879 (Desmond 1994: 118 and Boulger and Goldbloom 2010).
[172] Burbidge 1877: 87.
[173] Burbidge 1877: preface, unpaginated.
[174] Anon [signed 'A Gardener'] 1874: 495.
[175] Burbidge 1874: 45.
[176] Burbidge 1874: 7-8.
[177] Preston 2015: 319.

troubled because some pondweed hybrids were apparently fertile. Fertile hybrids conflicted with 'the first theoretical demand of a species being sterile with all other species of the genus.' He changed his mind 'not hastily' due to 'the local facts' and the 'direct proof' he obtained from observations of plants in situ in the fens and under cultivation.[178] That finding led him then to consider the question of the hybrids' 'origin, or the relationship they bore to one another'. Fryer then went further to explicitly explain hybridity within an evolutionary framework. He cited the 'Hybridism' chapter of *On the Origin of Species* when justifying a plant as a hybrid and elsewhere concluded: 'We may safely assume that crossing of "species" has been, and still is, one of the methods by which other species are fashioned.'[179] Fryer regarded this view of hybrid evolution as within 'the demands of the Darwinian postulate' because he believed that the hybrids gradually developed fertility 'under the influence of extended time and favourable conditions.'[180] Fryer's own testimony shows that working within a Darwinian biology—or, at least his own version of evolutionary theory regarding hybridisation—allowed him to contemplate the existence of not only sterile, but also fertile, hybrids occurring in nature.[181]

Re-making Graham's Willow

The study of Graham's Willow was adopted by a stern yet avuncular vicar, Edward Francis Linton.[182] In 1890, *Nature* remarked on Linton as 'one of our most painstaking British botanists', who acted as distributer of specimens for the Botanical Exchange Club, then with 'about 50 members' who were paying 'much attention' to 'hybrid willows, hybrid Epilobia and Potamogetons'.[183] Reverend Linton enjoyed what had enabled Anglican country clergymen to contribute so much to natural history in the nineteenth century: a classical university education, a comfortable living, and plenty of spare time: 'comfortable in a steady, ugly, respectable way' as Margaret Oliphant

[178] Fryer 1890: 173. Although we do not know if Fryer attempted to hybridise pondweeds.
[179] Fryer 1892 citing Darwin; Fryer 1890: 173-179.
[180] Fryer and Bennett 1915: 60 (this monograph was published from Fryer's notes posthumously).
[181] Preston 1988 notes Fryer's Darwinian approach as unusual, although this thesis sees this as likely widespread among Fryer's cultivator communities, not least due to the influence of Burbidge's Darwinian manual (Burbidge 1877).
[182] Description from his obituary (Hanbury 1928: 81).
[183] Anon 1890a: 391.

described the parsonage in her 1883 novel *The Curate in Charge*.[184] Linton took up botany, joining the Botanical Exchange Club in 1871 alongside his brother, also a vicar, and they cultivated British wild plants and studied hybrids together.[185]

The 'special knowledge' held by those local botanists working on hybrids translated into practices via the 'critical' study of plants. Linton marked one of his specimens of Graham's Willow as 'critically examined'. A 'critical authority' was able to identify willows, so perform a task that most botanists, whether philosophical or local, could not. 'Expert' or 'authority', historian Graeme Gooday reminds us, had distinct connotations in late Victorian Britain which have been lost today.[186] 'Expert' often held more derogatory associations as an untrustworthy hired expert witness in court cases. Similar care is needed when historicising the meanings of an individual described as a 'critical authority'. Today, 'critical' is used synonymously with 'difficult', for genera in which identification of species is complicated by certain reproductive mechanisms.[187] Yet, working 'critically', Buchanan White's obituarist explained, demanded 'unwearied patience' and 'mindedness' in the sense of not taking things at face value, formulating a view drawing on a range of evidence, and looking at many specimens (usually over many years).[188] Authority was context-dependent and performative, judged within and between botanical communities.[189]

In the 1880s, Linton began hybridising experiments to prove the parentage of naturally occurring hybrids and, specifically, to challenge Buchanan White's willow classification.[190] In 1894, Linton sent specimens to his close friend, fellow vicar-hybridist, Edward Marshall, also known for his 'special knowledge' of willowherb hybrids (*Epilobium* L.). One of the plants sent was a specimen of a re-made Graham's willow (Figure 3.8).[191] Marshall replied that he agreed with Linton: 'In a living state, I see a myrsenities parent in it far more than phylicifolia one, and this is still more plain from your dried examples.' He added with a nod to Graham's Willow in Hooker's

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[184] Olphiant 1883: 3.
[185] Hanbury 1928. Allen 1986: 78.
[186] Gooday 2008a.
[187] Stace 1989.
[188] Trail 1898: xxxvii.
[189] A point also made in Gooday 2008a, which supports the claim here in this thesis.
[190] Linton 1898 and Trail 1898: xxxvii-iii.
[191] See Hanbury 1928: 81 on their long friendship.

Student's Flora: 'Hooker's opinion on the point I regard as practically valueless.' Overall, Marshall felt that Linton was probably right. He could see 'no valid objection' to Linton's view 'perhaps White has not had it in cultivation?—The production of a different plant by artificial means could, of course, not be fatal to the accepted theory, since hybrids vary so much; still, the fact must have some weight.'[192] The 'theory' here was the accepted parentage of Graham's willow. A central problem with hybrids was their inherent variability, so inevitably, an experiment could only provide a positive match; if the experimental progeny did not resemble the putative hybrid plant in morphology, it was not possible to rule out hybridity. In this sense, hybrids were underdetermined even by experiment.[193] Linton published his re-made willow, the hybrid *myrsenities* × *herbacea,* and concluded 'I believe my observations furnish not only evidence, but all reasonable proof' of the hybrid origin of Graham's willow.[194]

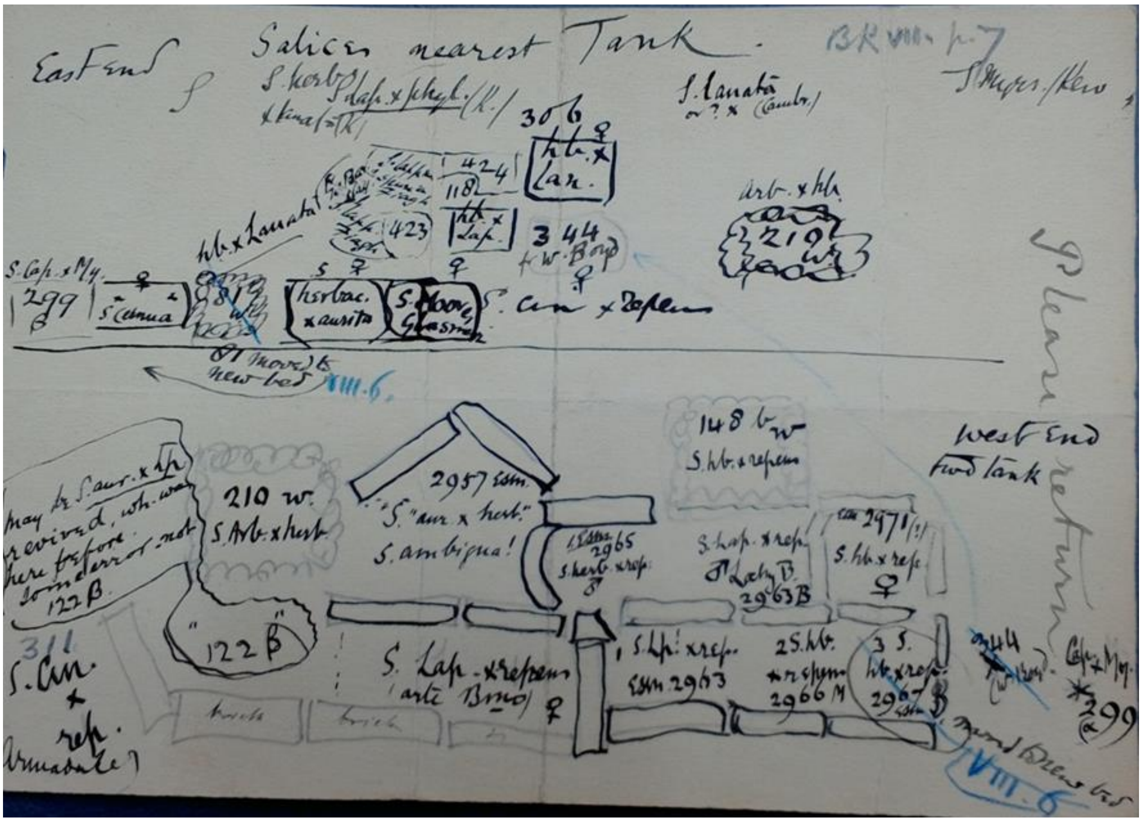

Figure 3.7: Sketch from Linton's archive showing diagram of his garden and the locations of his experimental willow plants including a hybrid *S. myrsenities × S. herbacea*, which Linton believed to be Graham's willow. From: EFL Archive, the collections of the NHM, London (Author's photograph, ©the author, on the advice of the IP Rights Officer at the NHM).

[192] Edward S. Marshall to E.F. Linton 7 June 1894 (note pasted to herbarium specimen of *Salix Grahamii* in Linton's herbarium in the British and Irish Collection at the Herbarium of the NHM).
[193] A point made by Fryer's biographer Chris Preston, pers. comm, 30 June 2017.
[194] Linton 1894: 40.

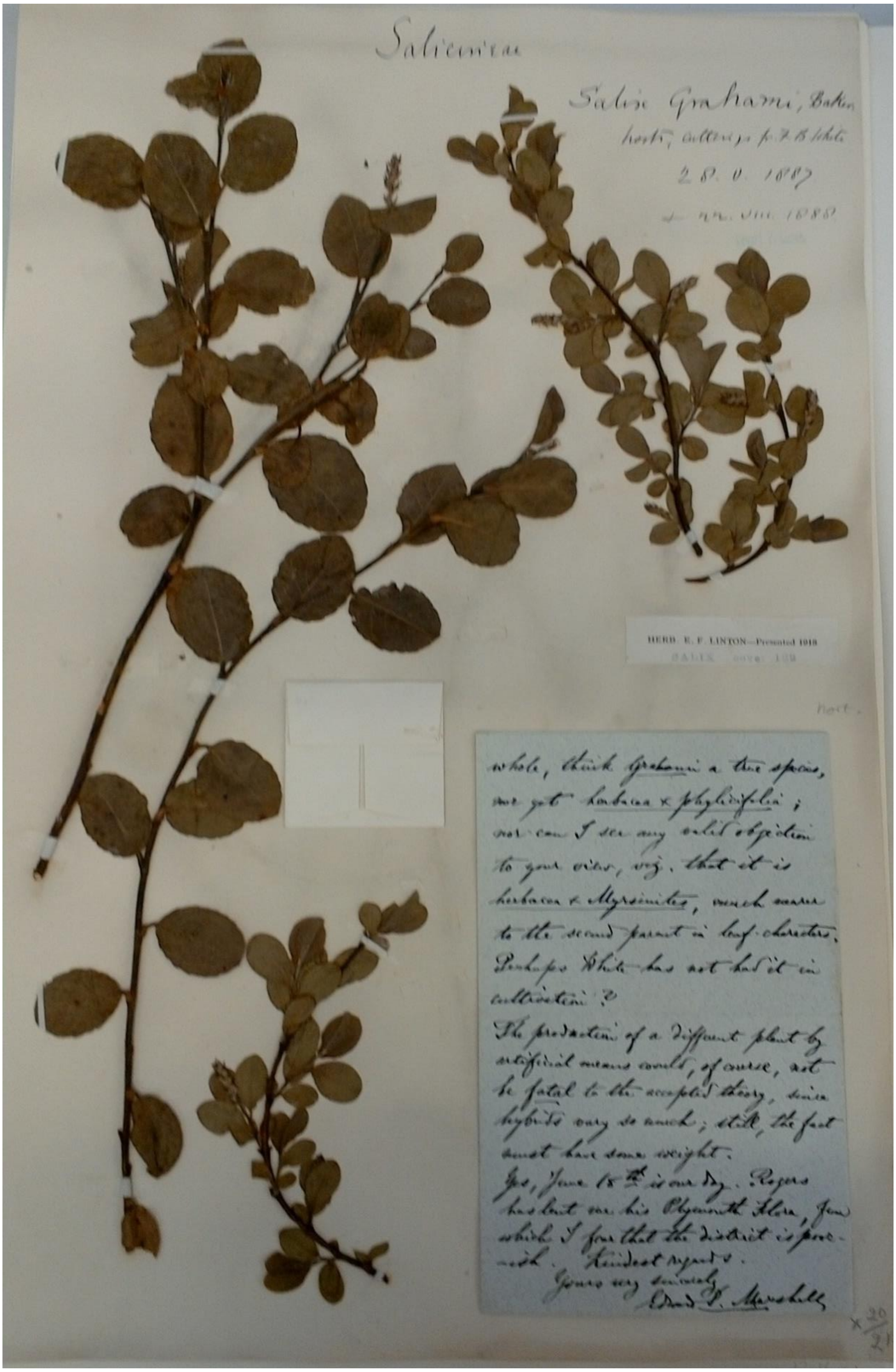

Figure 3.8: Graham's willow *Salix Grahami* herbarium sheet cultivated by E.F. Linton with pasted on letter, Edward S. Marshall to E.F. Linton, 7 June 1894. From: British and Irish Herbarium, the collections of the NHM, London. (Author's photograph, ©the author, on the advice of the IP Rights Officer at the NHM).

Late in life, in 1907, Linton surveyed the hybrids occurring wild in Britain, drawing on reports from his fellow 'critical men', Föcke's hybrid flora and specimens determined by him (herbarium specimens show that the German taxonomist visited Britain, going out botanising with Linton and Marshall), and, perhaps surprisingly, Darwin's *Origin of Species.* Linton repeatedly cited Darwin, for his collation of evidence about plant hybrids in certain genera, and for his own experimental confirmation of fertility or sterility of hybrids by hybridising.[195] In the early years of the twentieth century, at a time when Darwinism was overshadowed by several alternative evolutionary theories, local botanists interested in plant hybridity still looked to Darwin's botanical practices. A distinctly Darwinian attention to hybridising as a way of knowing about plants endured as part of taxonomy conducted by British local botanists.

Linton was able to claim what Boswell Syme had been unable to do, that Graham's willow was a hybrid, because as an experimental hybridist, his practical labour circumvented any anxieties over unphilosophical speculation. As an acknowledged 'critical man', in the 1890s he contemplated writing a textbook to replace Hooker's *Students' Flora*.[196] Linton was consulted by botanists from all over the country and abroad—as his archive correspondence testifies—including, for example, by Alfred Russel Wallace (1823-1913) on philosophical points of biogeography.[197] Experimental hybridising, as a taxonomic practice to re-make a putative hybrid, consolidated Linton as an authority, at a time when standard histories portray a widespread decline of non-academic practitioners in science.[198] Marshall complained of the 'crude and offhand' dismissal of hybrids and his, and Linton's, work as critical men by some Kew botanists.[199] However, as we shall see in the final chapter of this thesis, such hostile voices have been over-emphasised, given that other philosophical practitioners were convinced about willow and other hybrids, including as we saw in

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[195] Linton 1907 and 1907a.
[196] Allen 1986: 88.
[197] A.R.Wallace to E.F. Linton April 1896 (EFL Archive at NHM Miss. Corr. Box 1 of 2). H.J. Riddelsdell to E.F. Linton [undated c.1900] Apparently the first letter to Linton about Riddelsdell's plan for a *Flora of Glamorgan* (published 1907) asking for assistance from 'a critical man' with willows, brambles, roses etc. (EFL Archive at NHM Miss. Corr. Box 2 of 2).
[198] See the historiographical discussion in the introduction to this thesis.
[199] Marshall 1894: 290.

the introduction to this chapter, Hooker's son-in-law and successor as Director at Kew, William T. Thiselton Dyer.

As it turned out, Linton's diagnosis of Graham's Willow, based on his hybridising experiment, stood for eighty years.[200] There were, in fact, three species of willow involved in the parentage of Graham's willow, which remained unknown until the triple hybrid was re-synthesised in 2000. This hybridising experiment was not conducted by a molecular biologist in a laboratory; instead, by a retired teacher in his garden greenhouse.[201]

V. Conclusion

This chapter has made two novel claims contributing to the thesis' second core argument, a new account explaining attitudes to plant hybridity in Victorian science. Firstly, it has argued that the reluctance of some mid-century British botanists to accept hybridisation in nature is best explained as stemming from their commitment to the range of epistemological morals encompassed within being 'philosophical'. Secondly, it has shown that examining the story of willow hybrids reveals how a new Darwinian biology altered the practice of plant taxonomy in Britain.

Joseph Hooker was horrified when Darwin found a wild hybrid plant because widespread hybridity undermined the rationale for his lumping approach to taxonomy. For Charles Babington, plant hybrids were too readily used as a careless way to escape the labour required to make taxonomic decisions. Further, for John Boswell Syme, hybridisation in nature was a supposition, and therefore morally uncomfortable, despite his splitting inclination. Philosophical practitioners held differing views of hybridity that cannot be adequately explained by their religious belief, species concepts or lumping or splitting. Their opposition to hybridity related more generally to the epistemological morals expected of a philosophical practitioner, which deemed hybridisation too speculative to be acknowledged within science.

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[200] Meikle 1975.
[201] Tennant 2004: 77.

During the 1860s, as Darwin's reputation grew, so did the social respectability of hybridising as a form of experimentation. The theory of evolution by natural selection drew attention to hybridity as an *idea*; but it also encouraged the *practice* of physiological botanical studies, including hybridising. Several events coalesced during the 1860s raising the profile of the practice of plant hybridising within Darwinian biology. The connection made between plant hybrids in nature and experimental hybridising in Darwin's own botanical studies, in Wichura's willow book, in the Botanical Congress Rules of Nomenclature, and by the RHS Scientific Committee, socially elevated hybridising to a form of participatory science.

For the local botanist, the study of plant hybrids demanded field work and botanical experimentation involving growing fresh specimens, as well as hybridising, and fitted well within this emerging new Darwinian biology. During the 1870s, studying hybrids provided one way for local botanists who had been excluded from academic posts to develop a 'special knowledge'. Historicising 'special knowledge' and 'critical authority' in botanical science shows how those working on hybrids in groups like the willows became taxonomic authorities.

Finally, we must remember that, despite these instances of local practitioners presenting hybridising as experimentation and successfully contributing to taxonomy, Victorian social values nonetheless drew fault-lines between gardener and botanist; and between maker and thinker. As hybridising gained purchase as a form of Darwinian experimentation and participatory science, local practitioners were able to traverse these dichotomies, but not without challenge and criticism. The hybridising know-how of a cultivator and the field skills of a local botanist, combined as a way of making-as-knowing about plants, is a theme we pick up in the next chapter, in relation to plant breeders' interactions with academic botanists over fern hybrids.

Chapter 4
A 'New Truth': the Fernists and the Hybridising Experiment

I. Introduction

Elegant and easy to grow, combining 'grace, beauty and utility', ferns were the most sought-after household plant in the second half of the nineteenth century.[1] In 1881, one of their admirers, aristocratic devotee of science, and Fellow of the Royal Society, Edward Joseph Lowe (1825-1900), presented a paper to the Linnaean Society.[2] Lowe described a hybrid he had made between two fern species native to Britain, soft and hard shield fern (*Polystichum aculeatum* (L.) Roth. and *P. angulare* Presl.).[3]

Figure 4.1: Plates of the Shield Ferns, *Polystichum angulare* and *P. aculeatum.* From: Thomas Moore's the *Ferns of Great Britain and Ireland* (1855), plates VII and VIII, unpaginated (BHL Archive reproduced under the Creative Commons Attribution License).

[1] Birkenhead 1892: 7.

[2] Lowe was best known as an astronomer—his family owned a stately home and three observatories—and he was a founding member of the Meteorological Society (Cox 2004).

[3] Britten 1879-81: 141 and 144.

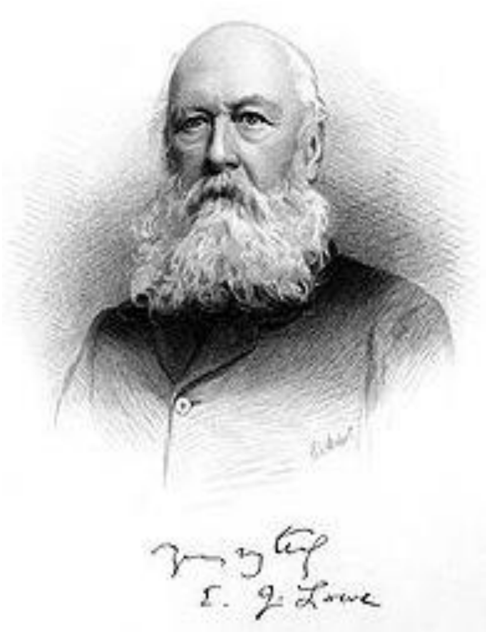

Figure 4.2: Portrait of Edward J. Lowe (1825-1900), nicknamed 'the big snowflake' on account of his billowing beard. From: Lowe 1895, front piece (BHL Archive reproduced under the Creative Commons Attribution License).

The hard and soft shield ferns are just that: one has soft papery fronds; the other has a more leathery texture, a feature not often apparent on dried herbarium material. Continental taxonomists united them as a single species.[4] British botanists believed their plants were different; indeed, at the outset of the Victorian period, the '*quaestio vexata* of British botany' was the identity of the intermediate forms of shield fern.[5] Lowe believed that he had resolved this long-standing question. The Linnaean Society, however, rejected Lowe's putative hybrid as 'not proved'. Lowe, outraged, complained of the 'distrust and disbelief' his hybridising work faced from botanists.[6]

Botanist-historians see Lowe as a 'pioneer' and claim that the 'greatest achievement' of fern breeders like Lowe was persuading botanists to accept fern hybridity.[7] The story continues that, despite breeders' claims, biologists still did not formally acknowledge Lowe's observations until 1947, when Professor Irene Manton's (1904-88) cytological work on the shield ferns confirmed, with chromosome counts, that the intermediate forms were hybrids.[8] Histories written by Manton's students proclaim that her research community at the University of Leeds was the first 'to unite the amateur grower and the professional researcher' in the study of fern hybridization.[9] In this chapter, we find that cultivators and researcher communities

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[4] Edgington 2013: 155-158.
[5] Grenville [1840] 1844: xx.
[6] Lowe 1895: 63.
[7] Allen 1969: 66, a view repeated by garden historians (notably Boyd 1992).
[8] Manton 1950: 154-157. On Irene Manton, see Williams 2015.
[9] Lovis 1967: 301-2. John Donald Lovis (1930-) was Manton's doctoral student and a member of the 'Leeds Group' of cytologists working on fern hybrids. See also a descriptive account in Gibby 1991.

were, in fact, corroborating and collaborating in the 1880s and 1890s over fern physiology.

In the previous chapter, we learnt how plant hybridising was conducted as a natural history practice within a new emerging Darwinian biology. Local cultivator-botanists' 'special knowledge' of hybridity allowed them to practice taxonomy and recognise hybrids in their own classifications. This taxonomic study of hybridity demanded physiological observations from the cultivation of fresh plants as well as examination of herbarium pressed material.

In this chapter, we see how, in the 1880s, a new botanical community, unique to Britain, the 'fernists' combined the practices of the local botanist as 'hunter' of ferns; the horticulturalist as 'cultivator'; and the commercial nurseryman as 'raiser' or breeder of novel ferns. The fernists conducted hybridising as a natural history practice, and to inform plant physiology. However, while the fernists presented their making-as-knowing as an experiment verifying the existence of fern hybrids in nature, this was opposed. The evidence suggests that fern hybridising did not satisfy the epistemic requirements for experimental method set by Kew's botanists. But the fernists persisted and communicated their studies using patronage and a mutual shared interest in ferns, which led to collaboration between fernists, Kew botanists and university cytologists. This story of fern hybridising supports the thesis' new account of why plant hybridity was contested in Victorian Britain: the diversity and debate around fern hybridity was a product of the interaction of distinct botanical communities claiming to make knowledge within nineteenth-century British science.

However, this chapter is not simply a narrative of how the fern hybrid gained credibility in science. For the fernists, claiming their hybridising practice as an experiment compensated for the uncertainty inherent in their method. Responses to fern hybridising contrasted to the fernists' observation of another aspect of fern reproduction, apospory, which was applauded almost immediately as a major contribution to science. Comparing these episodes brings out how interaction between botanical communities in the case of apospory produced a different outcome to the fernists' corroboration of their results within their own community.

This chapter also shows how tracking a practice like hybridising adds nuance to the historiography on the development of university science. Some academic biologists saw that plant hybridising might have utility for wider scientific purposes. Dr John Muirhead MacFarlane (1855-1943), a young plant morphologist at the University of Edinburgh, collaborated with breeders to study the microscopic anatomy of plant hybrids pursuant to his own version of Darwinism. For Professor John Bretland Farmer (1865-1944), a hybrid fern made by a nurseryman at Veitch & Son provided an example of how the study of not just ferns, but significantly, plant hybrids in general, could advance comparative physiology and cytology. Farmer's fern studies led to his innovative work on the physiology of cancer. Therefore, given this interest in plant hybridity from academics, historian Robert Olby's strong view that only horticulturalists were interested in plant hybridising during the 1890s stands revised. Further, in MacFarlane's and Farmer's programmatic studies, hybridisation was associated with more than investigations into heredity. The hybridising context of the 1890s might just as easily have produced a broader research programme into the role of hybridisation in evolution, as leading to the new science of genetics.

This chapter draws on previously unpublished archival sources, including herbarium specimens and nature prints, and is presented in three sections: The first section introduces the fernists, a botanical community unique to Britain, and their distinctive practices during the last quarter of the nineteenth century. The second section examines in detail the controversy over Lowe's re-made fern hybrid. To re-construct the opposition to Lowe's hybrid fern, we also examine the reception of the fernists' observation of apospory, and an analogous response to an experiment to produce an electric melon. The evidence suggests that fern hybridising did not satisfy the epistemic requirements for experimentation set by the philosophical botanists at Kew, in particular William T. Thiselton Dyer. The third section demonstrates how a fern hybrid, and the plant hybrid in general, was scientifically important in the 1890s. The chapter concludes that this story is a corrective to histories presenting a growing late century divide between the communities interested in the collecting, cultivating and other practices of natural history as distinct from academic biology, and to standard histories of the re-discovery moment of Mendelism.

II. ‘Hunters, Cultivators and Raisers’: the ‘Fernists’ in 1881

‘Many and Hot Discussions’ over Fern Hybrids

A botanical community unique to Britain, the ‘fernists’, emerged from among horticulturalists and local botanists during the last quarter of the nineteenth century.[10] The fernists’ practices included hybridising in combination with other natural history and commercial plant breeding activities. To place this botanical community into context, this section first considers the wider interest in fern hybridity around the time that Edward Lowe presented his hybrid fern at the Linnean Society.

In 1881, the British Museum opened its four and three-quarter acres of galleries to the public, including a dedicated space between its twin towers for the ‘cryptogams’, then comprising the ferns, algae, mosses and liverworts. The cryptogams had their own space in Britain’s newest celebration of science because they were still thought to have their own, rather mysterious, ways. [11] Cryptogam is from the Attic Greek for ‘hidden marriage’ because these plants have no obvious separate male and female organs.[12] Fern sexual generation could not be seen and therefore, Lowe complained, simply was not believed. Indeed, the fern plant we see is asexual, the sporophyte. We do not see the sexual generation, a tiny heart or kidney shaped thalloid structure one cell thick and under 10mm long, produced from the spores released by the sporophyte. [13] In 1849, the sexual life stage, the gametophyte, was observed microscopically and sex in ferns immediately raised the possibility of hybridity. However, most botanists thought that the antherzoids would be unable to reach the archegonia of another individual plant, never mind a plant of another species, so hybrids could not occur in nature.[14]

1881 was also the year that a German taxonomist, Wilhelm Olbers Föcke (1834-1922) produced the first ever hybrid flora. His *Die Pflanzen-Mischlinge* contained the

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[10] The term ‘fernist’ was applied to these practitioners by the BPS first President in Stansfield 1909: 45.
[11] The details here are from Owen 1881 and Owen 1879.
[12] Allen 1969: 72.
[13] The Swiss botanist and hybridist Carl Wilhelm von Nägeli (1817-91) observed spermatozoids in ferns in 1844 and the archegonia in 1848 (Scott 1891; Farley 1982).
[14] Lowe 1895: 11. On the history of the study of fern generation, see Farley 1982: 90-100 and Taiz and Taiz 2017: 477-88.

then definitive listing of wild plant hybrids reported across Europe. However, for ferns at least, Föcke was cautious. Compared to other plant groups, hybrid ferns were Föcke believed, very rare as identifiable entities. He listed the hybrid ferns proposed up until 1881, and rejected all of them, except for a spontaneous fern hybrid observed in a Botanic garden and accepted by German botanist Julius von Sach's (1832-97) *A Text-Book of Botany* (1875).[15] Föcke's view, then, was that ferns did hybridise spontaneously but that most of the reports of such hybrids were unreliable. British authorities were equally non-committal: William Thiselton-Dyer, head of Kew's new Jodrell Laboratory from 1878, and as the editor of the English translation of Sach's textbook, added a footnote to the text.[16] He cited two papers, with evidence both for, and against, hybrid ferns, which suggested that British opinion on fern hybridity remained undecided.[17]

In 1881, James Britten (1846-1924) had just completed his *European Ferns* (1879-81). Amusing and argumentative, Britten was an assistant at the botany department of the British Museum. He may have not had a university botany education, but he was a respected taxonomist, and relished challenging the botanists at Kew (his rivalry with Dyer became legendary). [18] Britten's fern monograph claimed that 'hybrids among the ferns, are not, perhaps, very uncommon'.[19] His opinion also reflected his background within local botanist communities, and his position as editor of their focal journal, *Journal of Botany, British and Foreign* (1863-1942). This journal was 'the most important medium for those interested in British botany' which gave voice to a community of local botanists, who, as we learnt in chapter three, were increasingly reporting their observations of naturally occurring plant hybrids across Britain.[20]

[15] Föcke 1881: 420-425.
[16] For biographical details on Dyer, see Thomason 2004 and 1987. His archive at Kew contains an unpublished draft for a life and letters style biography compiled by his wife focused around his educational achievements.
[17] Sachs 1875: 817 fn. 4.
[18] Anon. 1924. 'Thiselton-Dyer considered one [of Britten's witty attacks] so libellous that he threatened a lawsuit, and to avoid this Britten agreed to make a large donation to a charity. He gave it to Richmond Hospital whose grateful governors, ignorant of the reason for his donation, made him a privileged life governor, to his lasting amusement.' (Stearn 2004: unpaginated).
[19] Britten 1879: 91.
[20] Rendle 1924: 339. Britten was also prone to disagree with leading field botanists, see Allen 1986: 95.

Figure 4.3: Portrait of James Britten (1846-1924) in 1889. From: albumen print, NPG Ax160656 (© National Portrait Gallery, London. Reproduced with permission for non-commercial scholarly use under Creative Commons License).

Hybridity had to be inferred from a combination of evidence: intermediate morphology, sterility, and field evidence of the putative hybrid physically growing with, one, or ideally both, parent species. Local botanists argued over whether fern specimens were hybrids or not, for example:

> *Asplenium trichomanes 'Confluens'* was found on a wall in Levens Park, Cumberland in 1870, 'a truly startling apparition among British ferns!', a putative hybrid between sea spleenwort *A. marinum* and maiden-hair spleenwort *A. adiantum-nigrum.* "Opinions" were generally in favour of it being the hybrid as the fronds were barren, but that was disputed by Mr Barnes as he twice searched for its putative parents nearby without success and "it would be a clever fern that could escape his eye." [21]

In another example, perhaps 'the greatest discovery of modern times' of the shield fern *Polystichum aculeatum* var. *pulcherrimum*, was 'a puzzler'; it 'may be a natural Hybrid' but that was debated in 'many and hot discussions'.[22]

Some local botanists certainly did not welcome the prospect of hybridity in ferns. For example, in 1877, the Reverend Gerard Edward Smith (1804-1881) felt that talk of potential hybridisation among the ferns was sufficiently important to research the latest scientific position while writing the preface to his local flora. [23] Smith

[21] Jones 1877: 106. (In appendices to Hayward 2015).
[22] Britten 1881: 368 citing Jones 1880: 225.
[23] Smith 1877: iii. For biographical details on Smith see Boulger and Goldbloom 2010a; Anon. 1882: 63.

believed that ferns remained as originally created and therefore were a link to God though the works of His Creation:

> [Ferns are] a work fresh from the hand of the Eternal Creator, for, I believe, no attempt to hybridize the species has succeeded; and we are carried back to the 'Beginning', and see it the same as it was then – the 'deshe' or 'sprouting plant' of Genesis 1 v11-12 – an unchanged and unchanging link of time between the present and all the past.[24]

Smith was voicing a religiously inspired aesthetic appreciation of the primordial character of ferns, which was, as we shall see in the final chapter of this thesis, a perspective shared by some horticulturalists who regarded man-made hybrid plants as unattractive and vulgar.

In these debates, local botanists were keen to preserve the validity of their observations. Yet at the same time, many like Smith were aware that, despite the many reports of fern hybrids, 'no proof of hybridization in these cases is given.'[25] Although supportive of the possibility of fern hybrids, Britten nonetheless also called for experimental evidence. Hybridity in ferns was, Britten acknowledged, still just a hypothesis: 'The subject of hybridity in ferns would certainly repay any attention bestowed upon it, and if a series of careful experiments were undertaken, the results would be interesting, and probably new.'[26] One botanical community challenged that 'unproven' verdict and claimed fern hybridising as its own contribution to science: the 'fernists'.

A Cult of Fernists Emerges

Hunting, collecting, cultivating and breeding of ferns was 'a branch of Botany exclusively British'.[27] Insurance businessman and fern breeder Chas Druery felt that there might be two reasons for the British study of pteridology. Firstly, the vast range of forms of ferns found growing in Britain were 'absolutely unique in the world.' Secondly, British nurserymen produced 'an infinity of forms', so the artificial cultivation of ferns might also have prompted this unique interest.[28] The 'British

[24] Smith 1877: iii-iv.
[25] Smith 1877: iii.
[26] Britten 1879: 91.
[27] Druery 1910: 5.
[28] Druery 1903: 1.

Fernists’ emerged from among a small group of nurserymen, horticulturalists and local botanists, initially in 1871 as the 'West of England Pteridological Society' who then published *The Occasional Paper of the British Pteridological Society No. 1* in April 1875.[29] A co-founder of the group and leading fernist was Thomas Moore (1821-87), curator of Chelsea Botanic Gardens, London (the physic garden of the Society of Apothecaries), editor of several horticultural periodicals, and who sat on various committees of the Royal Horticultural Society.[30] Moore’s reputation rested on his popular yet scholarly fern books, and more specifically, on combining his taxonomic practice with his knowledge of plant cultivation. Moore was undisputedly ‘still *the* leading fern man’ into the 1880s.[31] After 1876, the group apparently no longer met face to face, but was then formally reconstituted in 1891 as the British Pteridological Society.[32]

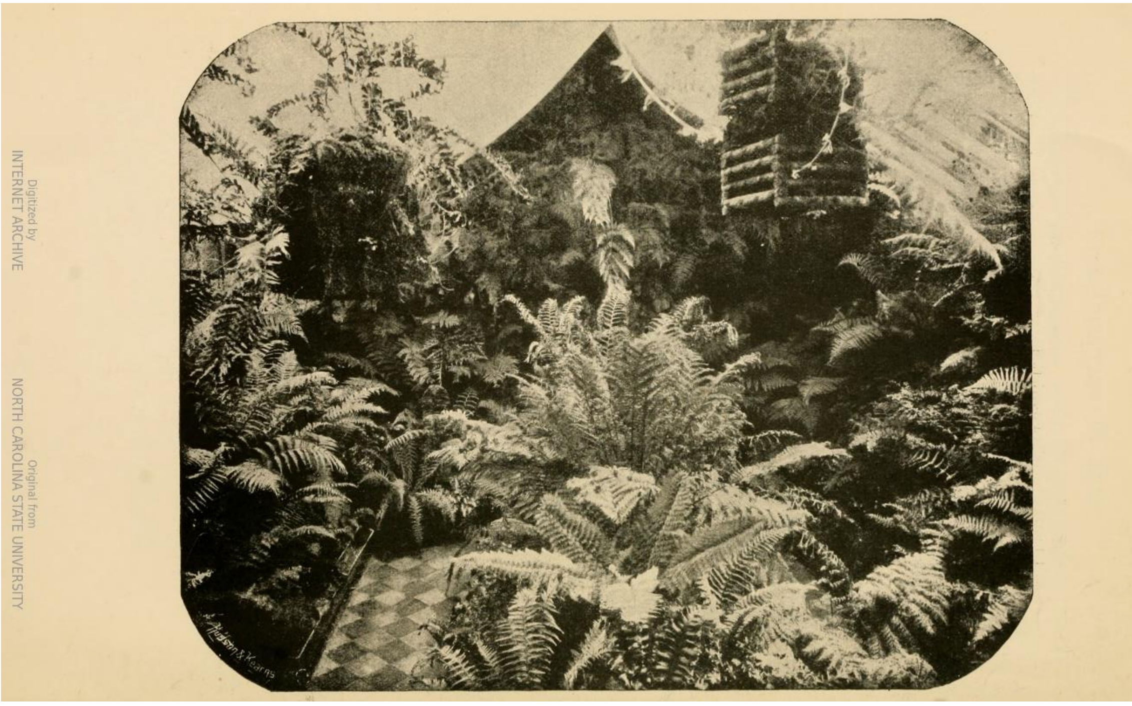

Figure 4.4: A late Victorian fernery. From: Druery 1910, front piece (Reproduced from the BHL Archive under the Creative Commons Attribution License).

[29] British Pteridological Society 1875. Known as ‘the first BPS’, see brief descriptive histories in Boyd 1992 and Hayward 2008.

[30] David Allen describes Moore as ‘opaque and grey’ (Allen 1969: 36-41) but he emerges as a dynamic and pragmatic character in a biography of the Physic Garden by Sue Minter (Minter 2000).

[31] The quote on Moore’s influence is from the perspective of a nurseryman fernist (Stansfield 1909: 46) and corroborated by Britten’s *European Ferns* largely following Moore’s classification and nomenclature.

[32] For a descriptive history of the society, see Dyce 1991.

These ‘fernists’ were, by 1881, about 30 to 40 mostly middle-class enthusiasts (even by 1905, membership was still only 50, ‘as large as at any former time’).[33] The personal reminiscences in 1909 of nurseryman Frederick Stansfield (1854-1937), the BPS’s first President, listed 33 men and four women as ‘early fern lovers’, including journeyman gardeners, commercial nurserymen, and at least two working class artisans who primarily earned a living by finding native ferns to sell. Stansfield’s grandfather had been a handloom weaver in Todmorden, between Leeds and Manchester, and founded a local botanists’ society as well as the family’s fern nursery.[34] Fernists were classified as ‘hunters’ or ‘cultivators’ (or both), or ‘raisers’. The hunters were local botanists who found unusual wild-growing fern varieties, the cultivators were gardeners who propagated these forms from sowing spores, and the raisers produced new types by cross-breeding or hybridising.[35] Edward Lowe regarded only the raisers as the true fernists: himself; his closest collaborator, Colonel Arthur Mowbray Jones (1820-89); Jones’ close friend, medical officer at Bridlington Lunatic Asylum, Edwin F. Fox (1814-91); and three nurserymen, Mr Stansfield, Mr Barnes, and Mr Clapham.[36] Fernists characteristically began as competent local botanists before then cultivating, and finally crossing, ferns. Abraham Stansfield of the family’s Todmorden Nursery, was ‘an excellent general botanist’ who ‘began very early to cultivate ferns and published a catalogue in 1852’ and was ‘one of the first’ to cross varieties in about 1865 and ‘did much to popularize and extend the cult.’[37]

However, ‘the cult’ was exactly what that term implied, a discrete coterie independent from the wider popular Victorian interest in ferns, referred to at the time as ‘pteridomania’ (from *Pteris*, the scientific name for brake, or bracken, the most useful British fern species producing livestock bedding, glass and soap).[38] Pteridomania began in the 1840s as fern collecting, both native British and exotic species, with

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[33] Phillips 1905: 6.
[34] Stansfield 1909. Biographical details are from Hall 1991 and on the social diversity of those involved in the study of ferns in the nineteenth and twentieth century, see Camus 1991.
[35] Stansfield 1909.
[36] Lowe 1890: 508. Jones reported that those who first cross-bred ferns as Lowe, J.M. Barnes, J.E. Mapplebeck, J. Moly and A. Stansfield (Jones 1888: 426 and 457-9). For a biography of Jones, see Hayward 2015:35-6.
[37] Stansfield 1909: 47. On Abraham Stansfield (1802-80), see Secord 2002.
[38] Kingsley 1856: 4-5. The classic social history of the Victorian fern craze remains Allen 1969, updated from a garden and architectural history perspective in Whittingham 2012.

interest expanding from the 1860s to other domestic 'plumy pets': fern designs unfurled on dresses, tableware, furniture, wallpaper, architectural details and, around 1890 (although the exact origin is unknown), on an English classic, the Custard Cream biscuit.[39] This cultural context is relevant because cross-breeding and hybridising ferns was unavoidably closely associated with commercial trade practices and the sheer abundance of consumer choice; and reminds us of Victorian close connections between plants and social status. With widespread popularity, came the sense of vulgarity that mass production invoked for many in the late Victorian period, and this too affected how the fernists defined their practice.

The fernists' interests contrasted with how botany was developing as a subject of academic study. In 1873, the editor of the *Gardeners' Chronicle* (Maxwell Masters) visited the new science school at South Kensington, London. He described the active observation of the class in 'work[ing] out for themselves with lens and microscope, dissecting needle and scalpel, reagent and pencil, the facts enunciated by the Professor' and drawing plant anatomy, using examples from the main groups across the whole vegetable kingdom over a few weeks.[40] This approach became known as the 'new botany' and formed a basis for science tuition at university, the model course at South Kensington (later to become Imperial College of the University of London and a centre for agricultural botany). [41] This teaching and research school was led by William Thiselton Dyer, and was connected, by an exchange of successive practitioners, to the new Jodrell Laboratory at Kew. Historian Bernard Thomason has shown how this approach to plant studies rapidly radiated out to Cambridge, then to Oxford, Glasgow and many provincial university colleges.[42] This form of botany was often called 'experimental', but, as historian John Pickstone points out, it was more akin to 'experiential' as there was little experimenting on plants, instead a focus on analysis of anatomical structure and inference of function and phylogeny.[43] In respect of the ferns, typically students would dissect and draw a single fern species as a model for the

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[39] Mabey 2015: 258-263 drawing on Allen 1969 and citing Shirley Hibberd's 'plumy pets'.
[40] Anon. [Masters, M.T.] 1873: 1013.
[41] It is difficult to define the 'new botany'. There were differences of institutional practice and of opinion about to what extent the new botany was a research agenda as well as a teaching curriculum and it was not a coherent programme during the 1870s or 1880s (Jackson 2015).
[42] Thomason 1987.
[43] Pickstone 2005: 54.

group.[44] It also contrasted, in the extreme, with the sort of botany that the fernists pursued, which was grounded in collecting, naming, observing and cultivating a great many varieties of each species, as well as making an herbarium, and focused on the whole living organism.

Nonetheless, the fernists believed that they were a scientific community. The first BPS's aims included 'all that has to do with the scientific study of the different parts of a fern, from its earliest germ to its final resting place in the herbarium of the enthusiast'.[45] This is important background for when we consider Edward Lowe's claim to have conducted an 'experiment' making the world's first hybrid fern. Lowe's practice was physiological and, as we will see, involved elements of experimentation such as microscope use and replication.

Before we consider the fernists' involvement with Kew botanists and academic physiology, the final part of this section considers what made the fernists a distinct botanical community. While the British Pteridological Society (BPS) has been studied to some extent by historians, none assess the fernists as a community in terms of their practices combining natural history and plant breeding.[46]

Collecting, Cultivating and Trade Practices Combine

During the consumer fern craze, and the 1870s growth of natural history societies, the fernists coalesced in perhaps an unexpected place, as a subscriber community around the circulation of an imaging technology. The practice of nature printing defined the early fernists.[47] This technique took a print from an intaglio plate created directly from an impressed plant specimen, to produce an image that depicted the frond fractals and detailed venation of a fern specimen especially well. In Britain, in 1853, Henry Bradbury, of the printers Bradbury and Evans, enlisted Moore to produce the text for a set of 17 folios of prints of British Ferns. Historian Naomi Hume contextualises Bradbury's work within the wider European development of the nature printing

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[44] Reynolds Green 1909: 26.
[45] Anon. 1875a: unpaginated.
[46] Probably because each historical account has been produced from the perspective of a local botanist (Allen 1969), or a horticulturalist (Boyd 1992) or an architectural historian (Whittingham 2012).
[47] On nature printing, the most detailed treatment is Hume 2011, but see also see Allen 1969: 50-2 and DiNoto and Winter 1999.

technique alongside photography, and concludes that, even if Bradbury had lived (he committed suicide at 29 by drinking soda water mixed with prussic acid) nature printing would never have become widely used in botany or for other scientific purposes. She situates nature printing within Lorraine Daston and Peter Galison's image standard of 'truth-to-nature'.[48] Botanists did not see any advantage in mechanical objective imaging; indeed, the botanical artist could selectively represent a plant, to focus the eye on the diagnostic characters of a specimen, which a direct copy would obscure. [49] However, for our purposes, Thomas Moore's conviction that nature printing might define a new botanical community of practice is important.[50]

Moore gushed over the stunning fern plates and considered their implications for his taxonomic practice. He could see that such a collection might portray the full range of variation within each species. These forms held 'botanical significance'; they were 'items in the mass of evidence' showing that a 'species generally have a wide range of form, even within narrow geographical limits' (such as the British Isles). Moore felt that the created species of Nature were 'something far more comprehensive than those of the botanist.'[51] Moore was celebrating what fernists like himself most sought out, the multitude of variation of form that the nature print so beautifully depicted:

> The rigid scientific botanist or pteridologist may perhaps experience a scientific shudder as he scans the long series of named forms which we have had occasion to record under some of the species; but he must recollect that if recognised and recorded at all, names are absolutely necessary to prevent general confusion; and recognise they most undoubtedly are by not a few who derive agreeable recreation, either in seeking them amidst, enchanting, rural scenery, where both mind and body derive benefit from the pursuit, or in tending and preserving them in their ferneries and rock gardens.[52]

The nature print image captured what 'rigid scientific' botanists were not interested in, the rare variations and nuances of form; what Chas Druery called 'a collector's "monstrosities" and therefore outside the scope of the serious botanist's

[48] Daston and Galison 2007: 105.
[49] Hume 2011: 58 citing Daston and Galison 2007: 108 and De Candolle 1880: 321, 363.
[50] Allen 2010: 245 on Moore's involvement with these nature prints.
[51] Moore 1859-60: xi.
[52] Moore 1859-60: x.

consideration' which Moore added, were nonetheless 'in general permanent and renewable from the spore.'[53]

The nature print technique directly led to the formation of a community of 48 subscribers to a series of nature printed folios of British ferns. Moore was a founder-member of the first BPS and involved in the production of these plates by Colonel Jones as *Nature-printed Impressions of the Varieties of the British Species of Ferns* (Jones 1876-80).[54] These prints were specifically to focus on the range of variations that standard texts omitted. It was to this audience—of cultivator-collectors—that Edward Lowe first presented his man-made hybrid fern, as a nature print.

The early fernists appear to have been most interested in collecting nature prints and cultivating fresh plants. Lowe's reference collection was in his garden: There were 'ferns lovely, ferns ugly, ferns like fairy dreams, and ferns like grim nightmares; ferns tall, crested, broad, narrow, long, short, curled, straight, twisted like mosses, or split up into fronds as delicate as the Maiden-hair, and as unlike their normal form as anything can be.' [55] Later, in a letter to Thiselton Dyer, Lowe enclosed another form of image, a cyanotype photograph (Figure 4.5, overleaf). Despite Anna Atkin's (1799-1871) use of cyanotypes in volumes on seaweeds and ferns, this imaging process was apparently little used by subsequent botanists.[56] Lowe's use of the cyanotype and the nature print provides important evidence of the fernists' distinctive practices compared to other botanical communities at this time. Most philosophical botanists, including those in museum and academic posts, dealt in pressed plants and herbarium specimens.[57] Along with these imaging technologies, borrowed via Moore's contacts with the printing industry, the fernists also adopted practices from the nursery trade. The use of listings in the fern nursery catalogue was another practice adopted from the commercial sector.

[53] Druery 1910: 6 Moore 1859-60: x.
[54] For a descriptive account of the production of Jones' nature prints, see Hayward 2015 and Boyd 1992.
[55] Anon. 1874 cited by Davies 1991: 192.
[56] Schaaf 1985 (updated 2018). The use of cyanotype photography by late nineteenth-century scientific communities remains unexplored (for a general descriptive history, see Ware 1999).
[57] On the primacy of the herbarium specimen, Daston and Galison 2007: 105-113.

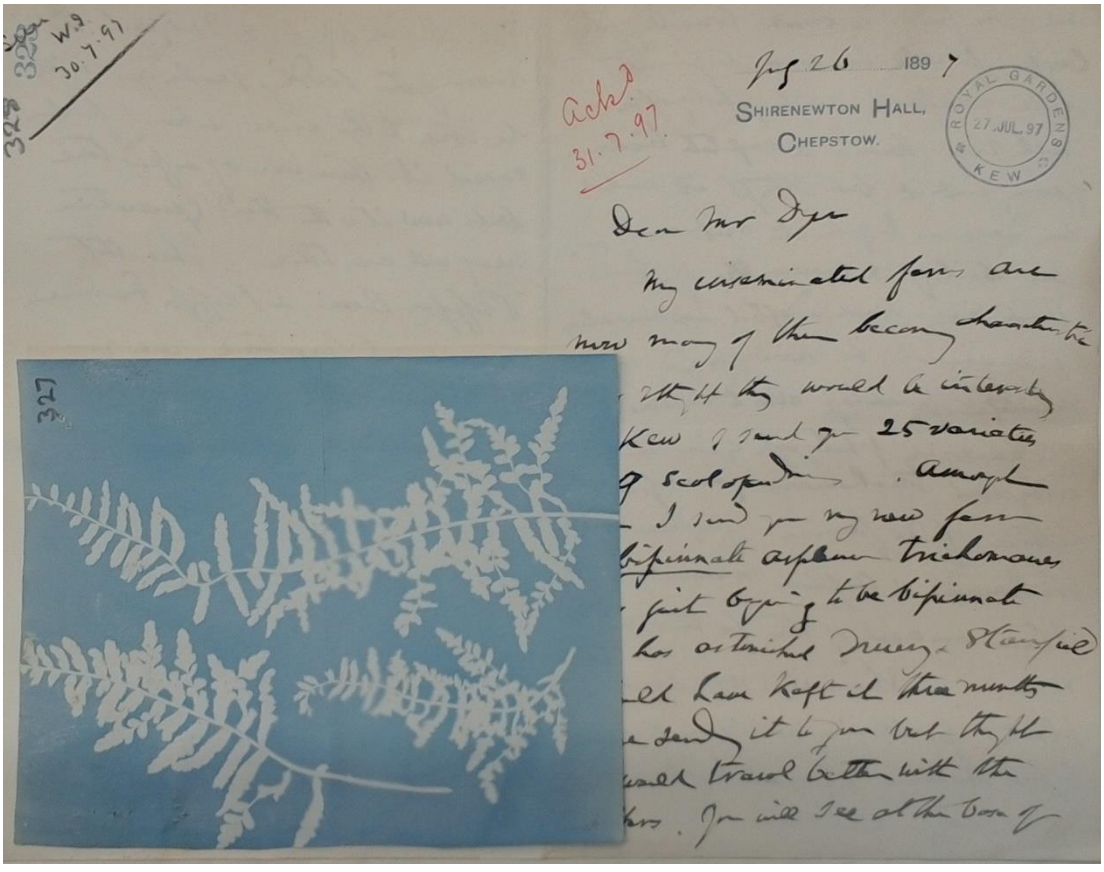

Figure 4.5: Cyanotype photograph of a fern in a letter from Edward J. Lowe to W. T. Thiselton Dyer, 26 July 1897. From: Director's Correspondence, William T. Thiselton Dyer, 1890-99, folio. 327-8. (© Kew Botanic Gardens, author's photograph reproduced with kind permission of the Board of Trustees of the Royal Botanic Gardens, Kew for non-commercial unpublished research use).

The use of a list of plant names as a research-enabling technology is socially more significant in Victorian science than has been acknowledged; as we saw in chapter two of this thesis, it powerfully defined which biological entities might be the subject of observation and study, and whoever controlled the list held a stake in how botany was practised. The *London Catalogue* remained the dominant list for local botanists seeking to make an herbarium collection to the end of the century, but for the fern collector, it held limited appeal: the list followed Joseph Hooker's classification of the ferns in his *Student's Flora* with 14 varieties under 46 species. By contrast, Moore's *Handbook* held 61 varieties under 44 species.[58] Yet in the catalogue of a specialist fern nursery, the number of forms available to collect far exceeded the philosophical botanists' lists. For example, fernist Frederick Stansfield's Nursery, at

[58] Moore 1857.

Sale, near Manchester was, for British Ferns, 'neither equalled nor approached by any other collection in the trade.' The catalogue held 502 varieties under 44 species, which the authors considered, were as distinct as 'the so-called species of the botanists, and, in many cases, more so.' (Figures 4.6 and 4.7).[59]

Figure 4.6: Cover of fern nursery catalogue: *Select List of British, Hardy Exotic, and Greenhouse Ferns Offered for Sale by F.W. & H. Stansfield,* Spring 1888, No. 6 (Stansfield 1888) (© The Lindley Library of the Royal Horticultural Society. Author's photograph, reproduced with permission for non-commercial research use).

[59] Stansfield 1888: front piece [unpaginated].

These catalogues from around 1880 used a nomenclatural system devised by fernist George B. Wollaston (1814-99), which the BPS then adopted from 1891.[60] At the RHS Fern Conference of 1890, the Kew botanist J.G. Baker complained about the 'complete chaos' of fern names. Baker asked that Edward Lowe draw up a standard list, as the only person considered knowledgeable enough of all the forms, wild and cultivated, to do so.[61]

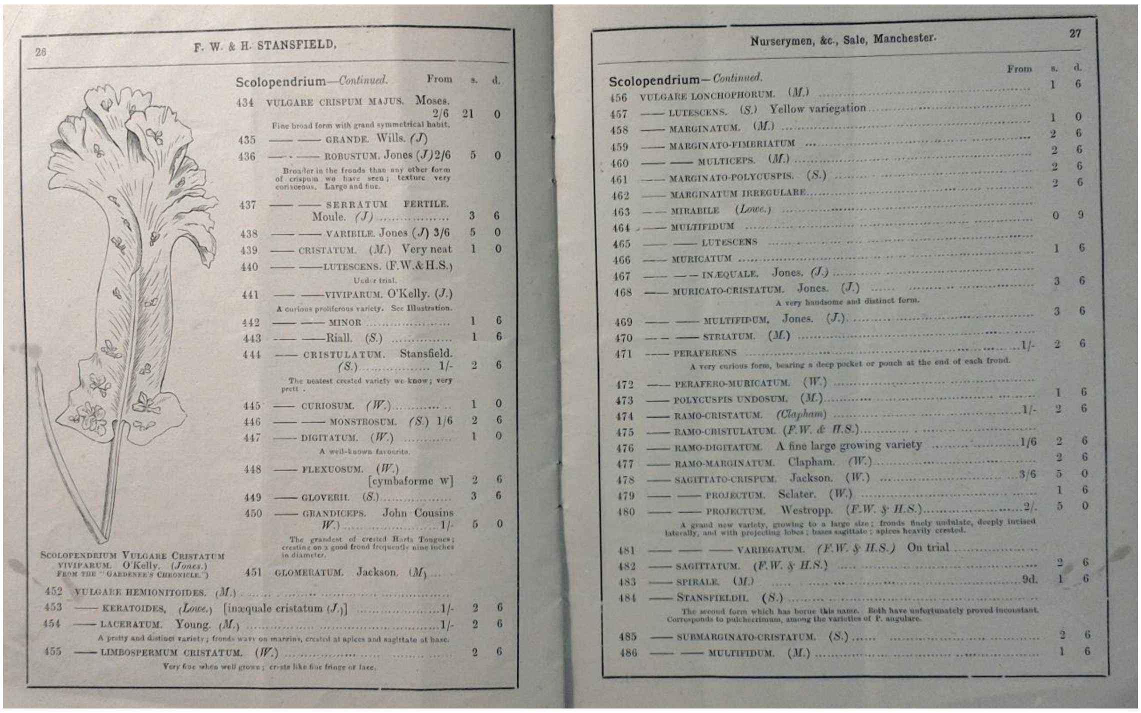

Figure 4.7: Some of the 94 varieties available for sale of the common British species, the hart's-tongue fern (*Scolopendrium vulgare*). From: Stansfield 1888: 26-27 (© The Lindley Library of the Royal Horticultural Society. Author's photograph, reproduced with permission for non-commercial research use).

We have seen how the fernists combined natural history collecting with cultivating and elements from commercial practices in printing and the plant trade. Their central practice was, however, fern breeding. The fernists' community included nurserymen who had pioneered the sowing of mixes of fern spores in the 1860s, and during this period, we know that increasing numbers of nurserymen were crossing ferns to produce novel forms for sale. Different sources produced different lists of nurseries involved, therefore it is safe to conclude that the evidence most likely underestimates the number of nurserymen involved in fern breeding. For example,

[60] Stansfield 1909: 48 and Anon. 1899: 5. See also Walters 1992 on the history of fern nomenclature.
[61] Anon. 1890b: 495-6.

one list from 1905 (with nurserymen annotated by the author of this thesis) shows five nurserymen participating in fern breeding:

Some of the Epochs of Fern Culture During the Last Fifty years

In 1851 mixed spores were first sown.

In 1855 Mr. Lowe sowed his first mixed spores.

In 1860 Mr. Clapham sowed his first mixed spores. [**nurseryman**]

In 1862 Messrs. Ivery Do. [**nursery**]

In 1864 Mr. Craig Do. [**nurseryman**]

In 1865 Mr. Stansfield Do. [**nurseryman**]

In 1866 Mr. Mapplebeck and Dr Lyell sowed their first mixed spores.

In 1867 Mr. Forster and Mr Barnes [**nurseryman**]

In 1870 Mr. E. T. Fox, Col. Jones, Mr James sd. their first mixe. spores

In 1871 Mr. Hodgson Do.

In 1873 Mr. Elworthy Do.

In 1876 Mr. Forster and Mr. Moly Do.[62]

These practitioners were mixing spores from different varieties within the same species of fern, but the technique used was not widely known. In 1877, the Darwinian horticulturalist Frederick Burbidge (who we met in chapter three) described the male and female fern parts of a fern and detailed how 'a clever and careful manipulator might be able to produce hybrid ferns by removing the antherozoids by means of a drop of water on the hair-like point of a sable brush, and applying them to the archegonia or female ovary-like cells of another species.'[63] That was not how fern cross-breeding was conducted, but Burbidge apparently did not realise. It appears that there was some mystery surrounding the production of cross-bred ferns and the technique was not widely known among otherwise well-informed horticulturalists like Burbidge. Fern nursery catalogues do not illuminate the position any further, as there are no mentions of any fern cross-breeds or hybrids, even from catalogues produced in the 1880s and 1890s. Rather than interpreting this absence as hostility to hybrid productions, and given Burbidge's apparent lack of awareness of fern crossing, it

[62] Phillips 1905: 12-13.
[63] Burbidge 1877: 309.

seems more likely that breeders were carefully guarding their know-how from commercial competitors.

Historians suggest that fern nurseries moved from selling wild collected plants, to propagating from spores, to cross-breeding by mixing spores, in sequential phases; and hybridising was conducted solely by the fernists.[64] This division of history into phases imposes a linear, progressive sequence on the practices involved. This historiographical approach was characteristic of how late-nineteenth-century sources presented the history of fern breeding. It seems more likely that these practices, collecting from the wild, and fern breeding, co-existed more widely, at least during the closing decades of the century. At the same time, the fernists also stressed that their members were 'amateurs' or 'hobbyists'. These two claims were to distinguish their community, not from biologists, but from commercial fern breeders.

In 1888, Jones hinted at the wider uneasy relationship that the 'much-despised race of British fernists' held with other botanical communities.[65] Historians assume that botanists were concerned about over collecting for the fern trade, which exceeded even what was deemed acceptable within horticultural communities.[66] However, the worry was not that a fern might become extinct, or about damage to property (although those concerns were periodically voiced). Instead, the trade in wild collected ferns, and commercial breeding, was held responsible by Chas Druery for ruining the public reputation of British ferns. An over-popularity reduced the aesthetic quality of the ferns available for purchase and in cultivation. The situation was then saved only by the fernists:

> Imperfect types found a ready sale as curios, and encouraged by this, selective culture with the definite object in view of improving the types was left mainly to a few amateurs, and in time, as an inevitable result of a surfeit of monstrosities, the craze subsided and the lovely British ferns fell not merely into the background of public favour, but actually into such disrepute that the trade with a few exceptions, which can be counted on the fingers, literally knew them no more. Thanks, however, to a small body of amateur enthusiasts, aided by the exceptions in question, the cult of British ferns did not by any means die, and the choicest selected forms survived.[67]

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[64] Boyd 1992.
[65] Jones 1888: 341.
[66] Wilkinson 2002.
[67] Druery 1894: 5-6.

Druery spoke of the ‘choicest forms’ surviving, and he might have been thinking not of ferns, but instead of fernists, who first and foremost, were ‘amateur enthusiasts’ and not professional nurserymen. By 1900, Druery distanced members of the BPS from commerce altogether: ‘As all the members of our Society are practical hobby-riders, or they would hardly belong to it, I feel I may count upon some fellow feeling if I relate how I came to be a fern hobbyist...’ [68] Yet several members were nurserymen, although perhaps the most eminent among them, Frederick Stansfield, represented the shift that had taken place from 1870 to 1900: Stansfield ran his family fern nursery at Sale, Manchester alongside his practice as a successful physician. His professional career was in medicine rather than in trading ferns.

We have seen how the fernists combined the natural history practices conducted by local botanist communities, and native British plants cultivation practices among horticulturalists, with plant breeding and other research-enabling practices borrowed from commercial nurserymen and the printing trade. After the formation of the BPS in 1891, the fernists distanced themselves from the nursery business, stressing that the study of British ferns had been compromised by the popularity of inferior monstrosities produced by the over-inflated commercial trade.

The next section examines in detail the controversy that ran from 1881 to 1889 over Lowe’s re-made hybrid fern. The reason for that opposition is reconstructed, and as we will see, the evidence suggests that fern hybridising did not satisfy the epistemic requirements for experimentation set by the philosophical botanists behind the Council of the Linnean Society and the Royal Horticultural Society’s Scientific Committee. This demonstrates one of the wider aims of this thesis, to show that hybridising was a natural history practice, even though it might be considered experimentation in some contexts, but not in others.

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[68] Druery 1900: 5.

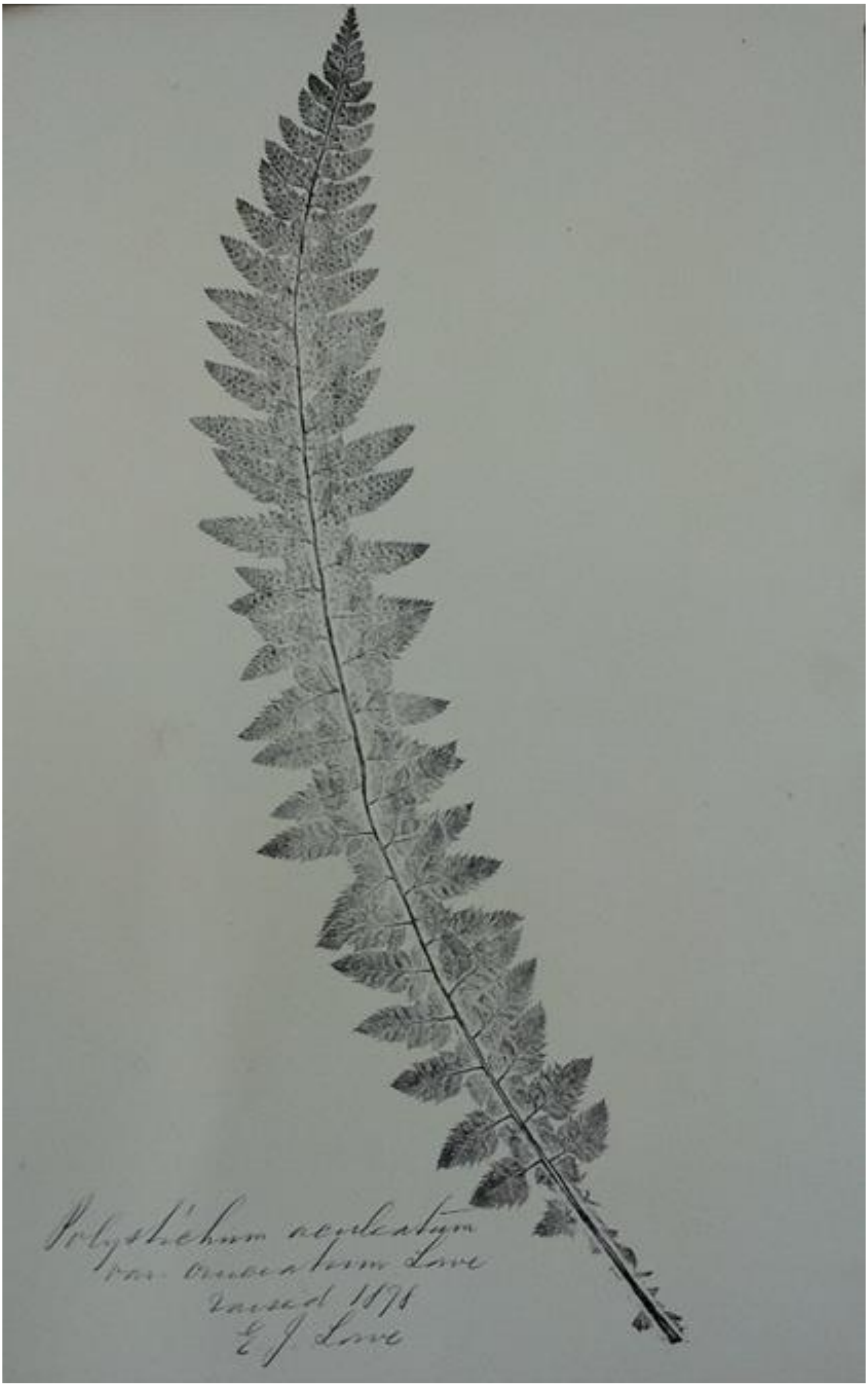

Figure 4.8: Edward J. Lowe's hybrid fern *Polystichum aculeatum 'Cruciatum'* from *Nature Prints* series 6. From: Jones 1880 (© British Pteridological Society, reproduced with permission for non-commercial research use).

III. A ‘clever and careful manipulator’: the Fernists’ Hybridising from 1881-1889

Edward Lowe’s Hybrid Fern

In 1875, 1876, or 1878, Edward Lowe raised a fern from combining the spores of two shield fern species.[69] He mixed the spores of *Polystichum aculeatum* var. *densum*, with those of another species *P. angulare* var. *Wakeleyanum*, in which ‘the pinnae were set in pairs at obtuse angles to each other, so that with the opposite pairs so characterized, a cross was formed, a rare feature and entirely unknown in *P. aculeatum*.’[70] The resulting progeny looked like *P. aculeatum* but had the cross-forming pinnae of the other parent. Colonel Jones’ *Nature Prints* series 6, published in December 1880, included Lowe’s shield fern labelled *Polystichum aculeatum* ‘Cruciatum’ (Figure 4.8). Jones’ accompanying text with the plate commented:

> If so, [i.e. that the mixing took place successfully] and one may be allowed to believe the evidence of one’s senses, the aculeatum has caught the cruciate character from angulare. In this batch the cruciate forms of angulare are very distinguishable from those of aculeatum — the two species do not appear to have merged into one another, but the cruciate character appears to have been, by some occult process, transferred from one species to another. These remarks are the result of personal observation.[71]

At the end of January 1881, Lowe exhibited his shield fern at a Linnean Society meeting. Lowe’s paper could have been rejected beforehand by the Council, but presumably his status as a Royal Society luminary ensured a hearing. The meeting report merely stated ‘the author’s experiments lead him to believe that *Polystichum aculeatum* and *P. angulare* are forms of one species’, and not that he had produced a man-made hybrid fern. [72] That was not how Lowe wished the episode to be remembered. According to Lowe’s autobiographical account of his life in botanical

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[69] The dates given by Jones on the nature print label (1878) differs from that stated by Lowe in his later account (1875) and from the date in Phillips 1905a: 13 (1876). Jones also stated that Lowe included *P. longitus* in the mixed spore sowing.
[70] Druery 1910.
[71] Jones 1880: 279.
[72] Anon. 1881: 331 and *Proceedings of the Linnean Society (Botany)* 1880-2 p.6. Lowe’s autobiography gives 1884, as the fern fronds had matured in 1882 (Lowe 1895). However, there is no published record of a contribution from Lowe for 1884 in the published proceedings of the Linnaean or in the Linnaean Society Council minute book. In 1896, Lowe stated he presented the fern in 1881 (Lowe 1896: 529).

science, *Fern Growing - Fifty Years' Experience in Crossing and Cultivation* (1895), the prime antagonist was Thomas Moore. According to Lowe, Moore commented: 'Botanists will not allow a possibility to cross ferns, though *somehow* I can see you have *the blood of each combined.*'[73] Lowe complained that Moore had decided that crossing and hybridising ferns was 'an impossible task' but did not elaborate further.[74] Moore did suspect that the shield ferns were conspecific, but maintained them as distinct species in his publications due to lack of a complete series of intermediate forms.[75] Therefore, if anything, Moore ought to have welcomed the hybrid fern as evidence in support of his own taxonomic view. Instead, it seems Moore was objecting to the physiological (not the taxonomic) issue, and doubted that ferns might reproduce sexually. In any event, Moore was not on the Council of the Linnean Society, who decided against publishing Lowe's fern paper in the Society's Proceedings.[76] Joseph Hooker was on the Council of the Linnean, and was succeeded in his position in 1884 by his son-in-law William Thistelton Dyer. [77] Historian Jim Endersby remarks on the reach of the botanists at Kew to control taxonomic decision-making in other institutions, for example, preventing a colonial botanist from publishing new fern names submitted to the Linnean Society.[78] Given that Kew effectively controlled much of the botanical business at the Linnean Society, it seems likely that Hooker or Thistelton Dyer was involved in the objections to Lowe's fern hybrid. Lowe blamed Moore in print in 1895, as the fern man had been conveniently dead for several years.

We have several versions of the fernists' side of the story, but, the views of those opposed to Lowe's hybrid fern need to be re-constructed.[79] To do so, we need to trace back to an earlier debate about fern hybridity in 1866. An editorial in the *Gardeners' Chronicle* discussed fern hybridising in response to a report of a hybrid fern in America. The author was probably either Moore, or Maxwell Masters (as they were

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[73] Lowe 1896: 529 (emphasis in the original).
[74] Lowe 1895: 11.
[75] Moore 1855.
[76] Council of the Linnean Society Minute Book, entries for papers submitted for reading before the Society, Lowe's paper 'On Some Hybrid British Ferns' for 20 January 1881 is marked '<u>not</u> to be published' (there is no entry for 1884 when Lowe later claimed he submitted his paper).
[77] Thistelton Dyer took over Hooker's seat on the Council of the Linnean Society on 1 May 1884 (Anon. 1886a: 14).
[78] Endersby 2008: 202.
[79] From Lowe 1890, 1895, 1896; Lowe and Jones 1889; Stansfield 1897.

co-editors of the paper in 1866). The editorial sets out the viewpoint of a philosophical practitioner who was knowledgeable about ferns and horticultural practices, and was most likely to have been Moore.[80]

The author felt that horticulturalists should try fern hybridising out, both to breed new forms, to investigate fern physiology, and prove the existence of hybridity in ferns:

> we do earnestly hope that, Fern-lovers will set systematically to work, to produce hybrid Ferns. How they may best succeed is told in the last Number of the "Journal of the Horticultural Society." And there is the more need for this, as not one of the so-called hybrid Ferns has as yet proved its claims to be so considered.[81]

But at the same time, the author was sceptical about reports of hybrid ferns, because 'the method of arriving at them is of the happy-go-lucky order, and is not likely to throw much light upon the phenomenon, nor does it hold out much promise of success.'[82] The 'happy-go-lucky' unreliable method was the mixing of spores. There is no reason to suspect that practitioners like this commentator (most likely Moore) substantially altered their opinion of fern crossing between 1866 and when Lowe presented his re-made hybrid fifteen or so years later. The objection from Moore in 1881, then, was to Lowe's method, and not to the possibility that fern hybrids existed, whether as manmade forms or in nature. Indeed, Moore was at this time heavily engaged with hybridising *Clemetis* L. species in collaboration with nurseryman George Jackson.[83] We have some evidence of what Kew botanists thought of hybrid ferns around this time.

A young botanical artist working in the Kew herbarium, Nicholas Edward Brown (1849-1934), had been engaged to complete the outstanding volume of the third edition of Boswell Syme's *English Botany*.[84] While Syme's main text stated that there

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[80] Moore edited the *Gardeners' Chronicle* from 1863 to 1866 (Elliott 2010s: 9). The next item was signed 'M.J.B.' which was probably Miles J. Berkeley (on Berkeley, see chapter one p.60 of this thesis) and could be taken to also refer to the preceding editorial. However, this editorial criticised Berkeley's piece in the RHS journal.
[81] Anon. [?Moore] 1866: 849.
[82] Anon. [?Moore] 1866: 849.
[83] Culminating in their joint publication *The Clematis as a Garden Flower* (1872) (see Minter 2000: 96-8).
[84] Allen 2010: 225.

was 'scarcely any doubt' about the hybrid nature of a featured fern, an addendum was most likely added by Brown, as follows:

> It is but an inference that ferns do produce hybrids, as it has never been actually proved by experiment, but every new intermediate form which exists in extremely small quantity and is found in circumstances where the supposed parents grow together adds to the probability of hybridization in ferns.[85]

Brown, writing in 1885 or 1886, carefully set out here the view of his superiors at Kew, citing the newly appointed Director of Kew, William T. Thiselton Dyer's verdict on fern hybrids in his translation of Sach's textbook. We have further evidence of Dyer's likely involvement from the Linnean Society's response to another fernist, Chas Druery, and his physiological observations in 1884.

Fernists Observe Apospory

The muted reception of Lowe's putative hybrid fern in 1881 contrasts the excitement provoked by Chas Druery's report in 1884 of fern apospory. This suggests that Dyer was involved in determining which observations might be acceptable to philosophical science, and we will see, might explain the rejection of Lowe's hybrid fern. Apospory is a form of asexual reproduction in which the spores develop directly into a prothallus bearing male and female organs. Druery presented a paper of his observations at the Linnean Society—just as Lowe had done—and we know that Thiselton Dyer was in the audience. Within a month Dyer announced the findings in *Nature* as 'one of the most interesting botanical observations which has been made for some time.'[86] Druery was swiftly elected a Full Fellow of the Linnean Society.

Thiselton Dyer arranged for Frederick Orpen Bower (1855-1948), an evolutionary morphologist who specialised in ferns at Kew's Jodrell Laboratory, to verify Druery's work by replicating and publishing it.[87] Bower visited Druery's fernery and took several specimens back to Kew. This corroboration was necessary, Dyer implied, as Druery was not competent to theorise about the significance of his observations: 'While every merit must be attributed to Mr. Druery for the first

[85] Boswell Syme 1886: 134.
[86] Thiselton Dyer 1884: 151.
[87] For biographical details on Bower, see Lang and Junker 2004.

observations of this important fact, he has with great liberality allowed Mr. Bower free liberty to discuss the histological and theoretical points involved.'[88]

Apospory was too important for evolutionary science to be left to Druery's interpretation. For Darwinians like Thiselton Dyer and Bower, it provided an important 'missing link': apospory was a suppression of the alternation of the two generations usual in ferns, and so it was an intermediate evolutionary state between the flowering plants and the fern allies and other lower plants. However, apospory was also 'of peculiar interest to fern hybridists'. There was no point in trying to hybridise two fern species if one of them reproduced asexually with apospory. [89] Druery's study had identified area of common interest between the fernists and biologists that we will return to in the final section of this chapter on collaborations between hybridists and academic biologists.

We have seen, therefore, that Kew botanists believed that any observation or experiment by the fernists must be corroborated by laboratory biologists. At the same time, in the 1880s, Lowe's fern hybridising was part of a long tradition of scientific experimentation in gardens on large estates of the British gentry and aristocracy.[90] So what might an acceptable garden hybridising experiment look like? Lowe adjusted his practice in response the rejection of his hybridising 'experiment'. We have Lowe's autobiographical account of how he responded to the rejection of his fern hybridising. Or at least, how he claimed he acted, because we only have Lowe's account with its inconsistencies. [91] However, we should not be dismayed by Lowe's misremembering. Indeed, Lowe's autobiography provides a historian with a far more interesting story than a chronicle of what happened, when. It reflects the criticisms he faced, for which we have scant other surviving source material. Further, as historian Jutta Schickore notes, before 1900, we rarely have detailed methodological accounts to underpin an experimenter's reasoning.[92]

[88] Thiselton Dyer 1884: 151.
[89] Druery 1895: 22
[90] See Gooday 2008 on domestic experimentation.
[91] Lovis 1967 remarked on the inconsistencies in dates and details throughout Lowe's autobiography and that John Lindley regarded Lowe's fern books as inaccurate.
[92] Schickore 2017.

Lowe did three things: Firstly, he instigated more crossing, to repeat the experiments himself; secondly, he recruited his fellow fernists to replicate his experiments; and thirdly, he wrote to the senior botanist of the day for support, Joseph Hooker. Lowe's initial response was to repeat his own hybridising. As Burbidge discussed, a common criticism faced by breeders was that a so-called hybrid was a one-off sport, a varietal cross or a selfed seedling, and not really a hybrid at all. Reproducing the same product quelled such doubters, at least within horticultural communities. Repeating an experiment was also a traditional approach taken by early modern experimenters. However, repetition was not widely seen as necessary by the 1880s; and by the twentieth century this requirement disappeared altogether, at least from explicit methodological discourse in encyclopaedias and scientific papers.[93] By contrast, replication, in the sense of repeating *another person's* experiment, was becoming an epistemic requirement in the 1880s where the context included, as with Lowe's work, controversial results. [94] This necessitated a scientific community of practitioners able and willing to conduct copies of the experiment. Lowe turned to his fellow fernists.

Lowe's work also illustrates the tension between what he could practically achieve and idealised mid-century philosophies of scientific method. Hybridising sat between the practice of noninterventionist observation, and experimentation as conceived of by nineteenth century philosophers John Stuart Mill (1806-73) and William Whewell (1794-1866). By the mid-Victorian period, philosophical practitioners believed that causes should be exposed through replication of variations to an experiment and some form of 'comparative' experimentation (the term 'control' was only just appearing in experimental discourse by the 1890s).[95] Philosophical botanists referred to the results of Druery's work as 'observations' yet Bower's work as 'experiments'. This was open social categorising, marking 'experiments' as the preserve of the biologists. Further, hybridising as experimentation faced the problem that there was no obvious 'comparison' available. Hybridising ferns also depended on indirect observation. For example, despite using a microscope during his hybridising to

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[93] Schickore 2017: 217-8.
[94] Schickore 2017: 44-5 and 217-18.
[95] Schickore 2017: 151-55 and 219 for an interesting history of technology argument about the use of the term 'control' in experimentation in science.

dissect the gametophyte, Lowe did not conduct the crossing microscopically to show, with direct observations, that the fertilised egg was formed from male and female gametes of two different species. Nor did he discuss how his spore mixing method prevented erroneous results from selfing or contamination. Burbidge's Manual drew attention to the problem encountered by 'many practical propagators' of ferns of 'contamination by unwanted spores'. He mentioned that one fern nurseryman advocated 'boiling or baking the soil' to kill unwanted spores, using it to pot up the fern immediately and covering it with 'an inverted bell-jar' to exclude 'free-growing intruders or "rogues".'[96]

Given these concerns about growing ferns and crossing plants were well-known (even if the crossing method for ferns, as we saw earlier, was not), it is unsurprising that Lowe's experiments were greeted with scepticism.[97] *The Athenaeum* newspaper, a leading weekly well-respected for its science commentary, in reviewing Lowe's book on fern crossing, explained the difference between fern hybridising and hybridising flowers: 'In the case of ordinary flowering plants the process of crossing can be readily effected directly by the manipulator, and all the earlier stages of the process can be easily seen, even by the unaided vision. But with ferns it is a different matter—direct evidence is not forthcoming.'[98] Therefore in respect of Lowe's more dubious results, 'Mr Lowe must wait until someone repeats his experiments with sufficient precautions against error.' The error might involve contamination by other spores or the composite forms being 'extreme instances of natural variation.'[99] This interpretation is corroborated by Chas Druery, in a review of fern hybridising, explaining its inherent 'risks, difficulties and uncertainties'.[100] The fernists did not see rogues, or other possible unobservable causes, as too much of a problem because of what they *could* see: the progeny resulting from the spore mix method were clearly intermediate in

[96] Burbidge 1877: 309. C.f. Radick and Charnley 2013 on 'rogues'.
[97] By 1900, biologists were still discussing the problem of how to hybridise ferns – the American Linnaean Fern Chapter discussed five different methods and all had limitations (Slosson 1900).
[98] Anon. 1896: 152.
[99] Anon. 1896: 152.
[100] Druery 1899: 288.

morphological form, which Druery took as 'conclusive evidences of the possibility of crossing.'[101]

Instead of addressing these methodological concerns, Lowe focused on attempting to establish that the progeny of his crosses were sterile (and therefore *bone fide* hybrids rather than cross-breeds). He sowed one hundred pans of spores over a 10-year period which did not germinate and persuaded his friends to do likewise, who he claimed got the same result.[102] Colonel Jones, however, stated to the gardening press that the hybrid plant given to him by Lowe was fertile 'for it has been proved by myself and others to be easy of reproduction from spores.'[103] It is unsurprising that these workers got conflicting results given that hybrid shield fern is usually sterile but sometimes fertile. What is interesting for this thesis is how the fernists dealt with that incongruity: by replication within their community, but not by collaboration beyond it. Lowe apparently controlled the published results: Colonel Jones' version of the story, with its fertile hybrid, only got as far as the Bristol Naturalists' Society.[104]

In January 1887, Moore died, thereby removing at least one objector to the hybrid fern, although the experimental work Lowe had instigated meant that he needed to wait until he had mature plants to exhibit. In 1888, Lowe and Jones together presented the hybrid shield fern to the BAAS meeting in Bath with a battery of new experimental results from hundreds of crossings conducted from 1885-8 by a group of fernists. There were 'now four clearly established cases in which the characters of distinct forms of *P. angulare* have been transferred to *P. aculeatum*.'[105] These results appeared in a paper in *Annals of Botany.* This journal had been founded the year before and was the most exciting botanical publication available (the *Annals* founding co-editor was Thiselton Dyer).[106] For the fernists, publication in this journal symbolised the acceptance of Lowe's experimentation by physiological science. Jones triumphantly announced: 'The crossing of ferns, like other new truths, has had to go

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101 Druery 1899: 292.
102 Lowe 1895.
103 Jones 1888: 458.
104 Jones 1888.
105 Lowe and Jones 1889: 30.
106 Jackson 2015.

through all the different stages of ridicule and incredulity, until the convictions of a few have at last forced conviction upon the majority, and the fact has received public recognition.'[107] There is a hint, however, that things were not so straight forward.

Lowe attributed the shift in opinion about fern hybrids to Joseph Hooker's intervention. According to Lowe, in 1885, he wrote to Hooker on another matter (about establishing a fernery of British 'abnormal' species at Kew) and received in reply 'a letter from Sir Joseph Hooker stating that the crossing of ferns was an acknowledged fact.'[108] In fact, Lowe had first written to Hooker about his hybrid fern in November 1880, presumably seeking support shortly before presenting his paper at the Linnean Society the following January 1881.[109] It is plausible that Lowe sought to bolster his standing as an experimenter by calling on traditional gentlemanly patronage. The language of Lowe and Jones' paper revealed how their 'manipulations' were conducted in the mind set of plant breeders, and their intentions were explicit: 'The number of forms to be obtained is past conception, and as the discovery of one truth is the stepping-stone to the discovery of even greater truths, so every new form that is raised enables the raiser or those following in his footsteps to produce countless other combinations.'[110] Lowe later claimed all his efforts were to convince botanists that ferns could be hybridised, but that looks rather like it was said with hindsight.[111] It seems more likely that Lowe appealed to Hooker, who advised him to do more experimental work, before then leaning on his son-in-law Dyer to allow Lowe and Jones to publish in the *Annals of Botany.* This view is supported by Dyer's response to cultivation experiments involving an 'electric melon'. The melon experiments provide a contemporary comparison to Lowe's hybridising, and support the contention that Lowe's work was rejected because it fell short of the epistemic standards Dyer expected for physiological experimentation.

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[107] Jones 1888: 341.
[108] Lowe and Jones 1889: 28. Lowe 1890: 508 gives the date of Hooker's letter as 1885. Hooker's letter has not been found.
[109] E.J. Lowe to Joseph Hooker, 27 November 1880, Director's Correspondence, J.D. Hooker, 1880-89, folio. 164.
[110] Lowe and Jones 1889: 30.
[111] Lowe 1895.

Kew's Critique of Experiments on the Electric Melon

Charles William Siemens (1823-83) was a businessman and engineer, whose company illuminated the RHS Flower Show at South Kensington Gardens in 1879. Maxwell Masters wrote to him the following day, suggesting 'now that the apparatus is at Kensington, to contrive a few simple experiments' to see if electric light might extend flowering or reduce the time taken 'to force grapes &etc' which might be 'of no little commercial importance'.[112] From 1880-83, Siemens instigated a series of experiments to test the effects of electric light on plant growth. These experiments were not managed by Masters at the RHS gardens, but were instead conducted at Siemens' country estate, Sherwood Park, near Tunbridge Wells, Kent. Siemens demonstrated early successes of his 'electro-horticulture' to the Royal Society and invited Joseph Hooker to inspect the experiments himself.

National newspapers, as well as the *Gardeners' Chronicle*, ran sensationalist reports on Siemens 'entertaining' lecture at the Royal Society, including the *Daily News*: 'Dr Siemens exhibited to the audience a pot of tulips in bud, which the electric light brought into full bloom in some three-quarters of an hour.'[113] A more cautious report came from Thiselton Dyer for *Nature*, on 'vegetation under electric light'. Siemens' visually impressive work was 'on a far larger scale than is possible in a laboratory experiment, and has substituted for the sun a little sun of his own.'[114] The 'little sun' was an arc light used to illuminate vast areas of glasshouse, in itself newsworthy at this time, and manufactured by Siemen's company.[115] However, despite the apparent surface praise, Dyer challenged several aspects of Siemens' experiment, critiquing his botanical knowledge as well as his experimental method. [116]

[112] Cited in Pole 1888: 314-5.
[113] Anon., *Daily News,* 6 March 1880, cited in Thiselton Dyer 1880: 439. On the tradition of spectacle and public demonstration in the history of electricity, see Iwan Morus' work, in particular Morus 1998 and the *Isis* special edition 'Performing Science' (Morus 2010).
[114] On Dyer's authorship of this anonymous article, see Minnis 2015: 271 fn45.
[115] Arc lights, developed from the 1840s, produced a bright light controversially used to illuminate public places. For example, in 1881 there were a spate of newspaper reports of complaints, largely expressing the revulsion of nearby genteel householders at the intrusive, unflattering light (see Gooday 2008b: 155-62 and 161-2).
[116] Kate Minnis interprets Dyer's tone as 'the most considered response' but Dyer was also heavily critical of Siemen's methodology. Minnis does not situate Siemen's experiments within the historiography of science, other than to applaud Siemens as a pioneer of electro-horticulture (which had, in fact, a longer history, e.g. it was tried with potatoes in 1846 (see Lidwell-Durnin 2020) and Thiselton Dyer 1880 mentions experiments in 1836.

Dyer highlighted Siemens' lack of knowledge: he stated 'it is well known' that tulips were 'very sensible' to 'even small changes of temperature' and argued that the influence of temperature should be 'carefully eliminated' in Siemens' experiments. Dyer conceded that Siemen's results had some limited scientific interest before suggesting a series of improvements to the experimental method.[117] These experiments provide a contemporary comparison to Lowe's hybridising, as both were conducted in the early 1880s in gardens, and attributed to Fellows of the Royal Society of a similar social standing. Dyer's reaction to Siemens' experiments, by analogy, supports the contention that he rejected Lowe's work because it fell short of the epistemic standards expected by Kew botanists for physiological experimentation.

In 1883, Siemens' head gardener David Buchanan presented an electric light-enhanced melon to the RHS Scientific Committee. Practically, the fruit was small and tasteless; scientifically, the melon also failed to impress the committee. The experiments 'though highly interesting, have not yet been conducted with that precision and carefully comparative method which are imperative from the point of view of science, and without which, moreover, their value for practical purposes is seriously impaired.'[118] Dyer was chair of the Royal Horticultural Society's Scientific Committee and this was apparently his verdict. Maxwell Masters' moderating voice was heard stressing that the committee did not wish to discourage a gardener from experimental studies.[119]

Dyer's response to the electro-horticulture experiments suggests that he declined to engage with Lowe's work because of its methodological limitations. He must have been relatively uninterested, given that fern hybridity did not contribute to Darwinian theory in the way that Druery's apospory did. Dyer was known, however, as a stickler for proper procedure, to the point of some Kew staff regarding him as autocratic.[120] Once Dyer's wider perspective on experiment from the melon debacle is considered, it seems unlikely that he would have recognised the method used to

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[117] Thiselton Dyer 1880.
[118] Anon [Masters] 1883: 52.
[119] Anon. 1929.
[120] According to his obituarist in *Nature* (Anon. 1929: 214) and corroborated by a gardener's reminiscences in the materials collected by his wife for an unpublished 'life and letters' biography (Kew Archives Director's Correspondence for W.T.T.D.).

create Lowe's hybrid fern. Therefore, Lowe's and Jones' triumphant publication in the *Annals of Botany* looks more like the result of Hooker's tactful intervention to persuade his son-in-law to placate Lowe, who was, after all, a Fellow of the Royal Society.

In fact, in 1890 at the RHS Fern Conference, biologists again challenged Lowe's hybrid. In discussions after Lowe's paper, Professor Bower called for Lowe's claims to hybridise ferns to be 'put to a vigorous test'.[121] Lowe wrote to Dyer asking for further investigations to be conducted at Kew into fern hybridity but there is no record that Lowe pursued the matter further, even though he continued to correspond with Dyer about the fernery at Kew through the 1890s.[122]

Lowe did not help matters by using his fern hybridising to argue for a new theory of biological generation, which he coined the 'multiple parentage theory'. He cut up a fern prothallus to separate its male and female organs and then claimed he could impregnate the female thallus with male thalli from three or four varieties at the same time and 'produce plants showing all the characters of the four parents blended on one and the same frond.' He frequently exhibited specimens to support his theory.[123] In *Nature,* the anonymous reviewer of Lowe's autobiography *Fern Growing: Fifty Years' Experience of Fern Crossing and Hybridising* (1895) expected to find rather more on Lowe's cultivation and crossing techniques, and rather less on his pet theory of multiple parentage.[124] The consensus in biology was already firmly in favour of one egg and one sperm forming a gamete, but Lowe argued that there was no reason why the ferns might not be an exception to that rule. Unfortunately for Lowe, 1895 was also the year that cytological work showed that meiotic reduction took place. The chromosome theory of 1902 finally confirmed that Lowe's idea was untenable.[125] To Chas Druery and the fernists' community, Lowe had produced convincing evidence of

[121] Anon. 1890: 519.
[122] E.J. Lowe to W. Thiselton Dyer, 20 March 1885, Director's Correspondence, William T. Thiselton Dyer, 1880-89, folio 276. E.J. Lowe to W. Thiselton Dyer, 26 July 1897, Director's Correspondence, William T. Thiselton Dyer, 1890-99, folio. 328.
[123] Lowe 1895.
[124] Anon. 1895: 3-4.
[125] Lovis 1967.

fern hybridity.[126] However, at the same time, during the 1890s Druery distanced himself and the BPS from Lowe's multiple parentage theory.

This context shows how Lowe's autobiography was in effect a polemical last chance stand to promote his theory to biologists. In Michael Shortland and Richard Yeo's *Telling Lives in Science: Essays on Scientific Biography* (1996), Dorinda Outram portrays science autobiography as 'a life-saving reputational tool' in post-revolution France.[127] Similarly, Lowe attempted to use *Fern Growing* to save his scientific legacy. It was highly unusual for a living author to write a 'my life in science' style book. The respectable genre was the 'Life and Letters' produced posthumously by a relative or close friend, such as the biographies of John Stevens Henslow (1865), Herbert Spencer (1904), and Francis Galton (1914, 1924, 1930).[128] Charles Darwin's private autobiography for his family's consumption was only later published appended to a posthumous Life and Letters volume.[129] Thomas Henry Huxley was an exception, but his extreme discomfort at producing an autobiographic account of his science (1890) illustrates the point that, in the 1890s, science autobiography was stigmatised as ungentlemanly behaviour.[130] That Lowe resorted to doing so shows he was absolutely convinced in his multiple parentage theory to the point that he would risk his public reputation for it. Apparently Lowe's self-assessment of his reputation being in need of bolstering by an autobiography was accurate: When Lowe died in 1900, one of his obituarists hinted at fraud, or at least wishful thinking, commenting that Lowe's spore mixing 'produced some remarkable results, which, however, were not generally accepted as genuine.'[131]

Druery and Lowe were treated differently because each prioritised and developed interactions with different botanical communities. Druery collaborated with the Darwinians at Kew's Jodrell Laboratory. Conversely, Lowe chose to collaborate within his fernist community, but not beyond it. Lowe's hybrid fern and Sir William

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[126] Druery 1900a.
[127] Outram 1996.
[128] This section draws on Graham Gooday's unpublished presentation *Why Did Scientists Write Autobiographies?* (Gooday 2015).
[129] Darwin 1887.
[130] Gooday 2015.
[131] Anon. 1901: 186.

Siemens' electric melon were both investigations conducted in a domestic setting deemed curious and interesting but did not meet the epistemic values for botanical experiment, at this time set by the Council of the Linnean Society and the RHS Scientific Committee, both overseen by Thiselton Dyer at Kew.

In the final section, we find that the fernists and other horticultural or plant breeding communities were by no means the only botanical communities involved in hybridising during the 1880s. British university botanists were also interested in how the study of plant hybrids could advance evolutionary biology, comparative physiology and cytology. Later in the 1890s, we will see that fernists collaborated with Kew botanists who specialised in the physiology of ferns. First, we use a case study of the early academic career of John Muirhead MacFarlane (1855-1943) to consider the development of institutional botany from the mid-1880s, and collaboration between academics and horticultural plant breeders. [132]

IV. 'The resources of the garden': Fern Hybridising in 1890s University Science

Plant Hybrids in Laboratory Science

At the summer 1890 meeting of the BAAS at Leeds, John Muirhead MacFarlane gave a paper on the microscopic demonstration that he had performed at the March meeting of the Botanical Society of Edinburgh. His subject was the anatomical structure of a hybrid plant. MacFarlane was a lecturer at the 'botanical laboratory' at the Royal Botanic Gardens Edinburgh. Botany at Edinburgh maintained a breadth of biological interests by appointing home-grown lecturers like MacFarlane, unlike some of the other institutions which became dominated by plant physiologists in the 1890s.[133] The close connection between the Edinburgh botanic garden and the university also provided crucial facilities for MacFarlane's practical research into hybridity; there were greenhouses on site by 1886.[134] By comparison, botanists at the university of

[132] Professor of Botany at University of Pennsylvania 1893-1920. Bibliographical details from Waterston and Shearer 2006.
[133] Thomason 1987: 223-225.
[134] Oliver 1913.

Cambridge had to wait until 1906 for a greenhouse (which Cambridge botanist Francis Darwin eventually paid for out of his own pocket).[135] Edinburgh provided a distinct scientific community in which hybridising might be an integral part of science. Yet despite appearances the Edinburgh botanists were not an isolated community. They were highly engaged with botanists elsewhere, including at Kew.

MacFarlane's impetus for his hybrid studies seems to have involved contacts at Kew. In 1885, MacFarlane visited Maxwell Masters at Kew for several weeks during his work on orchids for his hybrid research. In letters exchanged with Kew's Director, Thiselton-Dyer, he mentioned an orchid sepal that Professor Oliver in the Jodrell Laboratory had passed to him, and that he had spent much of his time in the herbarium.[136] Here he met Robert Allen Rolfe (1855-1921), a former gardener and hybridist who was also emerging as the leading British orchid taxonomist.[137] As we will see in chapter five of this thesis, Rolfe was spending time liaising with nurserymen Veitch and Son to name their new orchid hybrids. MacFarlane also struck up a friendship with Maxwell Masters and connections were established between a trio of men, Masters, MacFarlane and Rolfe, who all believed that plant hybrids, hybridising and hybridisation were biologically important.

It was to Maxwell Masters that MacFarlane turned to publish his preliminary results in 1890 in the *Gardeners' Chronicle*, as he needed more fresh plant hybrid specimens for his research.[138] Then in May and June 1891, at a meeting of the Royal Society of Edinburgh, his work was noticed by Thiselton Dyer and applauded as 'the first modern study of plant hybrid comparative evolutionary morphology'.[139] MacFarlane's paper discussed the bearing of hybridity on the biological problems of heredity, the physiological causes of sterility, and also set a new scientific standard for the accurate identification of plant hybrids. [140]

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[135] Cock and Forsdyke 2008.
[136] John M. MacFarlane to W. Thiselton Dyer, 3 January 1885, Kew Archives Directors' Correspondence, W.T.T.D. 1880-1889, folio 30.
[137] On Rolfe, see a descriptive biography in Pridgeon 1993 and chapter five of this thesis.
[138] Macfarlane 1890.
[139] Steckbeck 1943: 488.
[140] Macfarlane 1895: 204. The paper was read at the Royal Society of Edinburgh on 4 May and 15 June 1891 and originally published in 1892.

The bulk of his paper aimed to resolve what had been the 'great difficulty in safely determining' the identification of a hybrid, 'the absence of sufficiently marked naked-eye characters in the parents and hybrid.' The solution was the latest in anatomical microscopic study of stomata, cell elements and other microscopic characters. These features distinguished species, as such 'minute peculiarities' were regarded as more truth-containing and therefore phylogenetically more reliable, than gross morphology, which often was influenced by 'trivial structural deviations.'[141] Due to improvements in the compound microscope, there were far more microscopic characters visible. MacFarlane felt his method would be more stable and reliable than the previous use of gross morphology. However, he felt caution was still required, as

> In such cases, nevertheless, it must be kept in mind that if their origin dates back over a long period such changes may subsequently have been affected in them by variation and selection that the comparison can only be approximate, unless indeed one were to produce the hybrid artificially, and find close microscopic resemblances between the natural and artificial types. In any case we consider it as undoubted that recognition of hybrids from careful microscopic study should now be possible in the great majority of cases.[142]

In most cases, microscope work was sufficient, but sometimes artificial hybrid re-making, by hybridising, would also be necessary. The most rigorous test was to re-make the putative hybrid plant artificially, and then compare that to the natural specimen under consideration.

MacFarlane ended his paper with a discussion of the possible origin of species from hybrids. Reviewing the literature and his own studies of over 60 hybrids, he argued that biologists increasingly believed that animal and plant hybrids could be 'tolerably, or even very fertile.' He contrasted this recent development of opinion with that held twenty years previously, which denied that hybrids could be sufficiently fertile and their progeny sufficiently strong and adaptable to survive and evolve into species. As we saw in chapter three, Wichura's willow experiments suggested that wild hybrids, by the fourth or fifth generation, would die out. Darwin (in private drafts for his unwritten book on variation in nature) thought that hybrids might evolve but only under certain conditions of cultivation, which may lead to their increased fertility and

[141] Macfarlane 1895: 204-205.
[142] Macfarlane 1895: 282.

adaptability.[143] Against that view, MacFarlane cited horticulturalists as 'ardent believers in the continued fertility of hybrids.'[144] However, more persuasive were his own field observations on the hybrid avens *Geum intermedium*, a widespread and abundant British woodland plant in the north of England and Scotland. We saw in chapter three how this plant was re-made by hybridising during the 1840s and 1850s (and, incidentally, by Gregor Mendel in the 1860s) and Joseph Hooker eventually accepted it as a probable hybrid. MacFarlane concluded:

> When one finds the undoubted hybrid between *Geum rivale* and *G. urbanum* frequently described by systematists as a species, and that in many places the hybrid is nearly or quite as abundant as either parent, that it freely produces good seeds, and further that it has, as we have already indicated, many points of superiority as a combined organism which neither parent possesses separately, we have good reason for the exercise of caution before pronouncing decisively against species production from hybrids.[145]

MacFarlane's interest in hybrids, as his knowledge of the Geums shows, was due to his field studies, which led him to believe that hybrids were likely to be frequent in nature. Second, his exposure to horticultural ideas about hybrids led him to connect the increasing reports of wild plant hybrids to his evolutionary studies. Writing in the third person in the preface to his 1918 theoretical text on evolution, MacFarlane explained:

> As a university student he was deeply impressed, during the epochal period from 1875 to 1882, by the varied—often conflicting—views on evolution presented by his teachers. So he early formed a resolve to investigate quietly, but as extensively as possible, the problems presented by organisms from the simplest types up to man himself.[146]

McFarlane believed hybridization might be important in evolutionary biology: 'I can scarcely doubt that some of our hybrids are artificial productions of what once flourished as the progenitors of our present day species.'[147] MacFarlane may have developed this evolutionary view in part from his dealings with his gardener-hybridist suppliers. From 1887-89 he had obtained different hybrids from botanic gardens and commercial nurseries and appealed to gardeners to send him more material. One such source of plant material in 1889 was Frederick Burbidge, who we have seen in the first

[143] O'Reilly 2014.
[144] MacFarlane 1895: 282.
[145] Macfarlane 1895: 282-283.
[146] Macfarlane 1918: preface p.iii.
[147] Macfarlane 1893 cited by Anon [Masters] 1893: 16.

section of this chapter, believed that Darwinian evolution should encompass a role for hybridisation.[148] Another supplier, the gardener-hybridist the Reverend Charles Wolley Dod (1826-1904), believed that hybrids might prove to be the missing links between ancestrally related groups and could be used phylogenetically to infer evolutionary relationships.[149]

Reviewing the evidence twenty years later, Professor of Plant Physiology and Biochemistry, Joseph Reynolds Green (1848-1914), remarked how the 'greater utilization of the resources of the garden' was one of the most marked features of the later decades of the nineteenth century.[150] This statement is interpreted by some historians to refer to the growth of university botanic gardens and greenhouse facilities around 1900.[151] However, we have seen how from around 1887, MacFarlane collaborated with, and relied on, resources provided by horticulturalists, nurserymen and gardeners. Indeed, he highlighted this debt at his BAAS and Edinburgh presentations:

> A wide field for observation and experimental research is open alike to the biologist and horticulturalist. For the biologist, many problems bearing on heredity, on cell-history, on the relations of species to each other, on the production of hybrids, and on the evolutionary origin of organisms, which have hitherto been greatly confined to the region of speculation, may thus be reduced to accurate scientific study. For the horticulturalist, who has already opened the way so much, further incentives to the prosecution of hybridisation experiments present themselves.[152]

In pursuing his studies of plant hybrids, MacFarlane also connected plant hybridity to evolutionary biology. In the next subsection, we see how Maxwell Masters picked up on this connection in an editorial in the *Gardeners' Chronicle*.

University Biologists Need Hybridists

Maxwell Masters enthusiastically announced MacFarlane's work to his *Gardeners' Chronicle* readers as 'the most important contribution to horticultural science' of the year and of 'the highest interest' to 'both physiological botany and to practical

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[148] Burbidge noted that he supplied plants to Macfarlane in the 1880s in an account of a garden hybrid raised at the College Botanic Gardens, Dublin (Burbidge 1895: 306).
[149] A master at Eaton and a well-known *Gardeners' Chronicle* correspondent and an esteemed horticultural lecturer (Anon 1890: 796; Desmond 1994: 752).
[150] Reynolds Green 1909: 26-7.
[151] Cittadino 2009.
[152] Macfarlane 1890: 544.

hybridists and grafters'. The reason given was two-fold: first, the paper demonstrated how microscopic anatomy could definitively distinguish hybrids from varieties.[153] Second, MacFarlane's work demonstrated a role for gardeners in biology. Masters argued that gardeners had contributed much to both systematic botany and to plant physiology.[154] While some aspects of physiology required a laboratory, 'there is ample field for the gardener in the careful observation of the more obvious phenomena of growth, and in the study of the influence of external conditions upon it, and he has abundant facilities for doing some kinds of experimental work.' That experimental work was plant hybridising: 'In particular, he can render great service alike to practical gardening and to botanical science by the practice of hybridising and crossing, particularly if he had a definite aim in view, and faithfully records alike the details as well as the results of his experiments.'[155]

Masters was writing in the wider context of the negotiations over the identity of 'botany' as a science and the expansion of government funding of science in the 1890s.[156] Masters knew that aspects of comparative morphology, evolutionary taxonomy, plant physiology, agricultural crop and biomedical developments all relied on hybridising in domestic and commercial settings, and the botanical communities formed around those places, to provide the resources lacking in the laboratory.

Indeed, as an example of such collaborations that Masters had envisaged, in 1896, the fernists consulted Professors Farmer and Bower about their new physiological observation.[157] Edward Lowe had observed what appeared to be another exception to the alternation of generations in ferns. This time round, Lowe's paper embodied the observations of not only two fellow fernists, but also those of the Professors to whom plant material had been sent, and whose reports were included in the resulting paper. Lowe's paper ended with a note that replications of his fern cultures were being undertaken by fernist Chas Druery and by biologists at the Jodrell

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[153] In particular, Macfarlane believed he had settled the debate over the famous graft hybrid *Cytisus Adami*, 'supporting the notion that it is a graft hybrid and that Adam's [i.e. the gardener-maker's] account of it is correct.'(Anon [Masters] 1893: 16).
[154] Anon [Masters] 1893: 16.
[155] Anon [Masters] 1893: 16.
[156] MacLeod 1971; Pickstone 2001 and 2005.
[157] Lowe 1896.

Laboratory, Kew. Fern reproductive physiology was an area of common interest producing collaborations between hybridists and academic biologists.[158] Leading fernists around this time also spoke about how their work contributed to academic science.[159] A measure of the fernists' success in penetrating academic communities was that, as a hybridist, Druery was asked to translate Gregor Mendel's paper (although a Cambridge biologist, William Bateson, mistakenly got the credit).[160]

However, despite Masters' enthusiasm, it turned out that MacFarlane's hybrid paper was a one-hit-wonder, his only internationally recognised contribution. Yet in the 1890s, things looked far more promising, for both MacFarlane and for academic studies of plant hybrids. The interest surrounding hybrids related to their potential role in evolution, as well as in understanding heredity. Indeed, at the time, it was the evolutionary dimension of MacFarlane's work that attracted the most attention. Dutch botanist Hugo De Vries acknowledged MacFarlane's 'excellent work' and developed it as part of his mutation theory. [161] Another British university botanist, Professor John Bretland Farmer (1865-1944), adopted the new methodology in MacFarlane's hybrid study, and applied it to ferns. Just as MacFarlane had been supplied with both plants and ideas by horticulturalists, in pursuing these studies, Farmer collaborated with, and relied on, resources provided by the garden.

Fern Hybrids and a Future for British Biology

In 1897, Britain celebrated Queen Victoria's diamond jubilee. The nation paused to reflect on what it meant to be Victorian. That spring Guglielmo Marconi transmitted the first long-distance radio message almost 4 miles across the Bristol Channel, inconceivable in 1837 when few would envisage the relentless innovations in communications, and ensuing changes to science and society, that the next sixty years would bring. Ironically, amidst what the Victorians saw as inevitable progress, as we saw in the introduction to this thesis, the method of plant hybridising had barely changed. It is somewhat surprising then that during the 1890s, the product of what

[158] Lowe 1896.
[159] E.g. Phillips 1901.
[160] Radick 2021: 252 fn. 40 citing Hall and Müller-Wille 2013.
[161] De Vries 1900: 72.

was a decidedly traditional technique should attract the attention of one of Britain's most innovative university botanists.

In 1892, John B. Farmer was appointed assistant professor of botany in the Royal College of Science at South Kensington, headquarters of the 'new botany'. He was also in charge of the experimental facilities at Chelsea Physic Garden.[162]

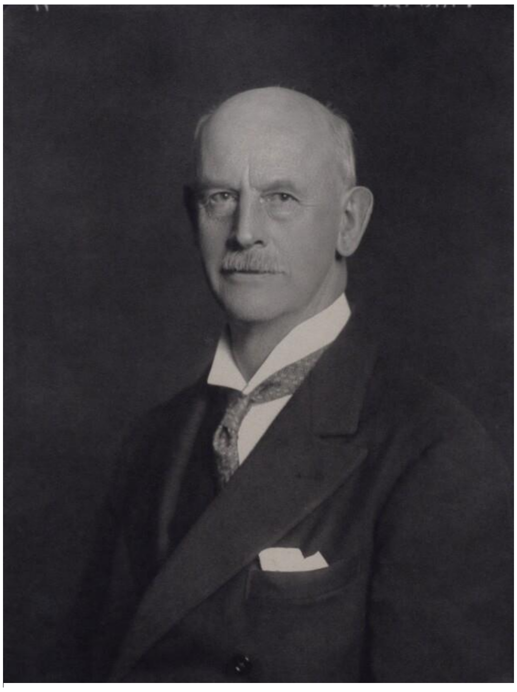

Figure 4.8: Portrait of Sir John B. Farmer (1865-1944). From: A bromide print, 23 November 1933, at the National Portrait Gallery, NPG x167488 (© National Portrait Gallery, London. Reproduced with permission for non-commercial scholarly use under Creative Commons License).

Farmer quickly established an international reputation in the new subject of cytology, later coining the term 'meiosis'.[163] And yet he retained a keen interest in gardening, horticulture and agriculture, and went on become editor of the *Gardeners' Chronicle* from 1904-07, as well as rapidly building an agricultural sciences department at London Imperial, to train botanists for work on the British Empire's plantations.[164]

At the 1897 BAAS meeting, Farmer gave a paper on 'the structure of a hybrid fern and its bearings on hybridity in general.'[165] Farmer had visited nurseryman George Schneider (1848-1917) of Veitch & Son, to see a man-made hybrid between a common polypody fern from Cornwall and a tropical South American fern.[166] Farmer was

[162] Radick 2021: 242.
[163] Later Professor of Botany at Imperial College of Science and Technology, London (Blackman 1945). Meiosis is the cell division process producing gametes and spores with half the chromosomal material of the parent cell.
[164] Thomason 1987: 191-2 and Blackman and Palladino 2008.
[165] Anon. 1897: 600.
[166] Schneider was a French gardener who moved from Paris to London in 1870 and worked for nurseries breeding ferns and orchids, publishing the well-received *The Book of Choice Ferns* (1892-4, 3 volumes). Veitch & Son's stock focused on imported ferns, so he was not one of the fernists (Desmond 1994: 612 taken from Anon. 1917: 11).

convinced the fern was a genuine hybrid.[167] He used MacFarlane's methodology to conduct an anatomical assessment of the plant and presented a paper to the Physiology Section at the BAAS, rather than to the recently reconvened Botany section. In 1895, a new BAAS section K was formed for botany, 'the first time botanists have had an independent organisation of their own' related a pleased William Thistelton-Dyer, who was an objector to 'the physiological versus the rest' split among botanists.[168] It seems likely that Farmer went to the physiologists to persuade these botanists of the significance of hybridity. Further, by 1897, the *Annals* editors were facing criticism that the journal was too descriptive and lacking quality papers on physiology.[169] Therefore, the publication of Farmer's hybrid piece suggests that Farmer was responding to editorial policy to enliven a comparative morphological account with proposed new directions in plant biology. It is also interesting that, of the range of topics that hybridity might investigate, Farmer chose to highlight the laws of heredity:

> Although it may be premature to speculate on the nature of the processes concerned in the production of hybrids, I am convinced that a careful study of these organisms [hybrids] will do much to throw light on the obscurities of heredity, and perhaps even on the essentials of ordinary fertilisation.[170]

Finally, Farmer's paper concluded: 'It behoves cytologists not to neglect such means of assistance as a careful study of the structure and origin of hybrids, in the garden as well as in the laboratory, is able to provide.'[171]

Farmer's article was a programmatic piece. He developed MacFarlane's approach and directly promoted the study of hybridisation to British plant physiologists. MacFarlane and Farmer's interest in hybrids embodied the intersection between horticulture (and specifically, commercial nurseries), natural history and university science. Overall, the study of fern hybridity suggests that the collaboration between naturalists and academics that Sam Alberti found in the northern industrial cities of Manchester and Leeds also occurred, in botanical science at least, around the long-standing botanical focal points of Kew and Edinburgh. More significantly, the

[167] The hybrid was a cross between *Polypodium vulgare* var. *elegantissimum* and *Polypodium aureum* (Farmer 1897: 534).
[168] Thistelton Dyer 1896: 836.
[169] Jackson 2015.
[170] Farmer 1897: 540.
[171] Farmer 1897: 543.

example of the fernists reveals the role of the commercial nursery trade in the development of university botanical practices.

Farmer's hybrid fern study had some surprising ramifications. Instead of producing further studies on heredity, it led to his work on the cytology of ferns – and cancerous cells. In *Nature* an anonymous polemical piece complaining about the lack of botanical research facilities in Britain presented the anatomical study of fern hybrids as a prime example of the value of taxonomic science for its own sake:

> Of course, no reader of this journal is likely to undervalue abstract science, and most of them are well aware of the enormous value of the practical results that can and do result from it. But even such persons must have been startled to find how the observations of Bower and others on the minute anatomy of the prothallus and spore-producing tissues of ferns, observations which might have been thought to be too abstruse and recondite to be of any practical value whatever, have directly led up to the extremely important researches of Farmer and associates into the essential nature of cancer![172]

V. Conclusion

This chapter illustrates the two core themes of this thesis: Firstly, a botanical community unique to Britain, the fernists, conducted hybridising for diverse reasons, including to inform plant physiology. The fernists combined natural history practices conducted by local botanist communities, of collecting and cultivating native British fern plants, with plant breeding and other research-enabling practices borrowed from commercial nurserymen. Fern hybridising, as making-as-knowing, was a core element of their natural history practice.

Secondly, the diversity and debate around fern hybridity was a product of the varied cultural contexts of knowledge-making in Victorian science, which we see persisting to the closing decade of the century. The fernists presented fern hybridising as an experiment to address the uncertainties involved in their method. Their knowledge claim to have made a fern hybrid was contested, as their methods did not meet the epistemic requirements for physiological experimentation set by Thiselton Dyer at Kew. However, the fernists persisted, and communicated their studies using

[172] Anon 1904: 539.

patronage and a mutual shared interest in ferns, which led to collaboration between fernists, Kew botanists and university cytologists.

A further point of relevance to this thesis' aims is that this chapter shows how consideration of the *intersection* of scientific communities has explanatory power for writing the history of science. Edward Lowe and Charles Druery studied the same subject—fern physiology—yet were treated differently because they engaged with different communities. Druery collaborated beyond his own community, whereas Lowe, in the 1880s, did not, and relied on patronage to get his work acknowledged. After 1890, Druery's careful collaborations enabled the fernists created a scientific identity for themselves within Victorian science.

This chapter also revises historian Robert Olby's strong view that only horticulturalists were interested in plant hybrids and hybridising in the 1890s. Academic researchers investigated plant hybridity and collaborated with the fernists, a corrective to standard histories presenting a growing divide between the communities interested in natural history and in professional biology. These corroborations and collaborations occurred because biologists needed access to fresh plant material and new subjects to study microscopically; and reciprocally, fernists and other plant hybridists wanted their observations verified by academic science. Professor John Farmer and Dr John MacFarlane believed that the plant hybrid was scientifically important as an ideal organism to be investigated microscopically in evolutionary comparative anatomy and by the emerging new field of cytology.

Moreover, in MacFarlane's and Farmer's programmatic studies, hybridisation was associated with more than investigations into heredity. The plant hybridising context of the 1890s might just as easily have produced a broader research programme into the role of hybridisation in evolution, as leading to the practice of Mendelian crossing and the new science of genetics. We pick up this point in the final chapter of this thesis, focused on 1899.

Having seen the critical responses to the garden experimentation involved in Edward Lowe's fern hybrid and William Siemen's electric melon, in the final chapter of this thesis we consider how Maxwell Masters' commitment to hybridising coloured the history of plant hybridity in Britain produced at the first RHS Hybridization Conference in 1899. This thesis reads that contribution not as a history, but as an emblem of the contemporary culture that gave rise to it: a rhetorical campaign to elevate the status of horticulture as a part of botanical science.

Chapter 5
Re-telling the Story: Masters, Rolfe and the Hagiography of Hybridity

I. Introduction

Between 1837 and 1899, the time period of this thesis, a clichéd caricature of the Victorian age has everything changing: isolated rural communities transform into urban, inter-connected populations; the railways, steam printing press, telegraph and penny post revolutionise communications; a practitioner of science develops from an amateur gentleman to a salaried scientist; and in the science of life, Darwinism changes the view of the species from a fixed creation to something many were not quite sure about, not even by the turn of the century. Yet at the same time, amidst such uncertainties and what must have seemed to many like inevitable technological progress, in some ways little changed in our story. We have uncovered a more complex history about Victorian science and society in this thesis, which reveals continuity as well as change.

One continuity was the method of hybridising itself. By 1837, crossing plants using hand pollination was a well-established practice and when the first man-made orchid hybrid flowered in 1856, this seemed only to confirm that practitioners had attained a Baconian mastery of the manipulation of nature. While cultivators developed their methods of propagation (especially for the tricky-to-grow orchids), the technique of hybridising (and of cross-breeding) remained essentially the same.[1] Among horticulturalists, throughout the Victorian period, crossing was consistently conducted in combination with selection. Even by 1899, John Hardie Wilson (1858-1920) lecturer in agriculture at the University of St. Andrews, Scotland, was using the hybridising method described by Anderson-Henry (which originated from a gardener in around 1837).[2] Anderson-Henry was, by 1873, 'the standard authority' on hybridising

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[1] For a descriptive history of orchid propagation, see Arditti 1984. On the late-century commercial trade in orchids, see Olby 2000.

[2] C.f. The description of hybridising in the introduction to this thesis, p. 34-5.

(this was a fair claim, despite the biographer-author's aim to praise).[3] Wilson's equipment list remained the same as Anderson-Henry's, except, instead of a muslin bag to exclude insects and protect the crossed plant, Wilson designed his own 'wooden box with removable glass panels in front and back and on the top' (shown in Figure 5.1). Further, the key challenge for a hybridist, Wilson emphasised, remained the same as fifty years earlier: to know 'which flowers should be used and when the operation should be done.'[4] Wilson's solution was to 'imitate nature' drawing on 'a good grip of systematic botany and floral biology.' [5] Another continuity, revealed in this chapter, was that botanical communities remained as divided over plant hybrids as they had been in the 1840s debate over the oxlips; and hybridising was still more readily associated with gardening and commerce than with science. Therefore, poised at the cusp of a new century, Maxwell Masters looked back and sought to explain why.

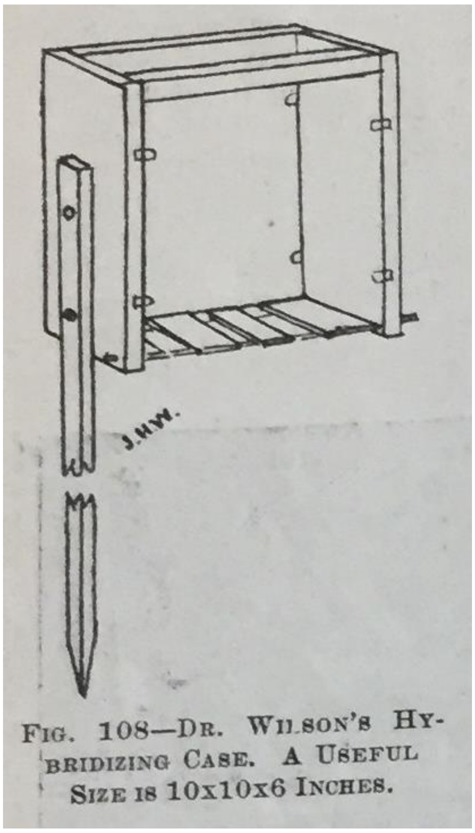

Figure 5.1: Illustration of John Hardie Wilson's (1858-1920) Hybridizing Case. From: Wilson 1899: 416. (Reproduced from the BHL Archive under the Creative Commons Attribution License).

In 1900, Maxwell Masters' editorial in the *Gardeners' Chronicle* described how 'the art of the hybridiser' had 'proved beyond doubt' that a wild orchid, *Cattleya* × *whitei,* really *was* a hybrid (Figure 5.2). Masters explained that, since 1886, nineteen wild orchid hybrids had been 'proved scientifically' to be hybrids. The article then linked this interest in hybrids to 'the universal spread of the theory of evolution' and the acceptance, at last, of the hybrid in nature as a natural entity: 'It will be felt that the time has come for the recognition

[3] Anon. 1873: 399.
[4] Wilson 1899: 415.
[5] Wilson 1899: 416.

of hybrids. We do not think that the most conservative botanist could reject or ignore natural hybrids, when proof of their nature is forthcoming'.[6]

Figure 5.2: Cattleya × whitei, White's Hybrid Orchid, a natural hybrid *between Cattleya schilleriana* and *C. warneri.* From: the *Botanical Magazine*, August 1900 [unpaginated]. (Reproduced from the BHL Archive under the Creative Commons Attribution License).

[6] Masters 1900a: 170.

Masters' comment on the re-made orchid hybrid was part of a series of remarks over twenty-five years, emphasising how hybridising might be 'scientific'. These comments had culminated the year before, in his opening address at the first International Conference on Hybridization in July 1899. At the conference, together with his colleague at the Kew Herbarium, Robert Allen Rolfe (1855-1921), Masters established an inaugural history of plant hybridity in Britain. The term 'hybridity' here encompasses both the study of plant hybrids in the wild or the garden; and the practice of hybridising as a tool within the sciences of plant taxonomy and physiology. The Masters-Rolfe history spoke about all of these elements, not just on artificial hybridising or the classification of wild plant hybrids. Above all, this history held that plant hybrids, and the horticultural hybridists who made or re-made them, had been unjustly ignored or rejected.

This chapter sets out the first historical analysis of the Masters-Rolfe history and draws on unpublished sources from archives at Kew, and the RHS' Lindley Library. A central question for this Chapter is why was *this* history written? The Chapter argues that this history was an expression of the culture that produced it, the interaction between botanical communities embodied by Maxwell Masters' and Allen Rolfe's own lived experience, and the wider late-century science and religion discourse. Masters' historiography was part of his on-going rhetorical campaign to present horticultural hybridising as a contribution to scientific knowledge-making, to elevate what he saw as an unjustly neglected horticulture, especially after a debacle between the RHS and the Royal Society in 1887. He situated hostility to plant hybrids and hybridising within the late-century Huxleyan narrative of an inevitable conflict between science and religion, a message which contemporary newspaper reporters, and later historians, then amplified. Rolfe had deep-seated personal reasons for focusing on hostility to hybrids in his historiography, and back shadowed his contemporary conflicts across the entire nineteenth century. Yet the details of Rolfe's own historiography support a core aim of this thesis, to show that there were diverse views about plant hybridity throughout the Victorian period.

The argument in this chapter is presented in three steps: First, it begins with an analysis of the history of plant hybridity presented in Maxwell Masters' introductory address at the 1899 hybridization conference and in Robert Allen Rolfe's follow up talk the next day. Then the Masters-Rolfe history is set in its wider context, of immediate responses to it from some public audiences, and further unpacking of the religion versus science theme by considering contemporary Christian-inspired horticultural aesthetics. The second section shows how this history was part of a series of efforts by Maxwell Masters to present horticultural hybridising as scientific knowledge-making. The section analyses the origin of his interpretation that 'prejudice' against plant hybrids and hybridising stemmed from Christian beliefs. Then the third section considers how Rolfe's lived experience at Kew explains the content of his version of the history. The Chapter concludes that Masters and Rolfe produced an inaugural history for an imagined new collaborative evolutionary science between horticulturalists and biologists, utilising hybridising as an experimental tool. This history then shaped subsequent portrayals of Victorian attitudes to plant hybridity throughout the twentieth century.

II. Flower Shows to Science: An Inaugural History of 'Hybridization' in Britain

The Masters-Rolfe History

At the end of May 1899, Inner Temple gardens in central London hosted one of the RHS' classic flower shows, the Great Spring Show. Attendance was 'simply enormous', comprising HRH the Prince of Wales, and 'lords, ladies and gentlemen' who were 'addicted to the pursuit and most gracious of all costly of tastes', namely gardening. While bands played, and high tea was served, 'fashionable London and not a few real lovers of flowers', visited tents full of nurserymen's displays of hybrid roses, orchids and begonias.[7] A star attraction was Messrs. Sutton's new hybrid toadflax (*Nemesia* Vent.), and almost everything was for sale.[8]

Six weeks later, the Royal Horticultural Society's international conference on 'Hybridization (the Cross-Breeding of Species) and on the Cross-Breeding of Varieties'

[7] Anon. 1899a: 838 and Anon. 1899b: 12.

[8] Many gardening commentators complained that the shows were more about staging a grand entertainment than encouraging gardening (Elliott 2004:119). On the history of RHS flower shows, see Elliott 2001 and Elliott 2004 chapter 7.

similarly involved a display of plants in marquees, an eight-course banquet with a centre piece of enormous hybrid waterlilies, and a Garden Party attended by over 250 guests via a specially laid on train service from London Victoria. [9] But it also marked a recent departure for the Society. The conference objective was to discuss the scientific aspects of cross-breeding and hybridisation. The 'scientific' theme continued at the Conference dinner in a speech by the guest of honour, the Master of the Rolls (John Lindley's son) who praised the Society's recent policy of encouraging plant breeding in place of collecting from the wild: 'You have turned to scientific men'.[10] Those 'scientific men' were the hybridists.

Maxwell Masters, in his introductory address setting the tone for the two-day event, stressed that the object of the conference was to 'secure progress' for horticulture; and in keeping with widespread late-Victorian ideals, he saw science as the way forward: 'I believe that the future of horticulture depends very greatly on well-directed experiment.'[11] Masters also aimed to establish an inaugural history. In fact, the leading 'historian of hybridisation', Dr Wilhelm Olbers Föcke (1834-1922), had been invited to preside over the meeting, and in whose absence, Masters took the chair.[12] It was, after all, 1899. The crossroads of a new century prompted reflection on the past 100 years. At the BAAS meeting that summer, the BAAS President and renowned *Iris* hybridist Sir Michael Foster (1836-1907) spoke of scientific progress since 1799. Foster was also a leading Huxleyan educationalist, who had established physiology as a university discipline in England. Foster did so by writing a history, rooting his studies in the physiological project of the great anatomists from the sixteenth century onwards.[13]

'Hybridisation' (as Masters referred to artificial hybridising as this throughout his speech) was 'the most important subject in modern progressive experimental horticulture.' [14] And this progress was attributed to British horticulturalists, with London nurseryman Thomas Fairchild (1667-1729) as the first person in any country

[9] Wilks 1900: 40-53.
[10] Lindley 1900: 47 cited by Olby 2000: 1046.
[11] Masters 1900: 55.
[12] Masters 1900: 55.
[13] Olby 1997; Foster 1901.
[14] Masters 1900: 57.

who 'formed an artificial hybrid purposely' in around 1719, although Masters' claim was problematic as no one knew if Fairchild's mule was deliberately made, or accidently found.[15] However, it did not matter. Any history needed a founding father, and Masters had found two, an English first with Fairchild, and far more socially respectable, the aristocratic horticulturalist and BAAS gentleman we met in chapter one, William Herbert.

Masters continued by explaining that horticultural progress had been held back by 'objections and prejudices' from two sources: religious beliefs and botanists' views about hybridising. First, 'many worthy people' he explained, believed that hybridising was 'an impious interference with the Laws of Nature.' However, he then explained that William Herbert had countered that view with the 'best answer to this prejudice', by showing that plant hybrids occurred in nature, 'for if such forms exist in nature, then there can be no impropriety in producing them by the art of the gardener'. However, while concerns about impiety had passed, botanists still objected to the making of artificial plant hybrids because these inconveniently introduced confusion into their classifications.[16] Masters then stated that these 'prejudices' were over thanks to Darwin, as species were no longer 'sacrosanct' and classifications were now recognised as just man-made systems: 'Darwin has taught us to welcome hybridisation as one means of ascertaining the true relationships of plants… For scientific reasons, no less than practical purposes, the study of cross-breeding is most important.'[17] As we saw in chapter three, Masters praised Darwin's 'wonderful book', the *Origin of Species,* and had corresponded regularly with Darwin for twenty years.[18] He believed that, thanks to Darwin, the practice of hybridising was directly relevant to a science of evolutionary taxonomy. Significantly, however, Masters expanded his earlier view to argue now that, in addition to the use of hybridising as a Darwinian practice, Darwinian

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[15] *The Times* reporting on Masters' address (Anon. 1899c: 13). Zirkle 1935 introduces this doubt over Fairchild's activities. For an unsung hero-style biography of Fairchild, see Leapman 2000. In fact, the practice of crossing plants is probably as old as crop cultivation itself, given the Babylonian Code of Hammurabi c.1750 BCE includes hand pollination of the dioecious date palm (Taiz and Taiz 2017).
[16] Masters 1900:57-8.
[17] Masters 1900: 58.
[18] Masters 1862: 216.

theory also supported the existence of the fertile hybrid (in animals as well as plants) because the species was no longer fixed.

The next day, Masters' colleague at the Kew Herbarium, Robert Allen Rolfe, developed Masters' address in a lengthy paper about the recognition of wild plant hybrids and the role of hybridising in systematic botany (as taxonomic botany was now known).

Figure 5.3: Portrait of Robert Allen Rolfe (1865-1921). From: Stapf 1921: 5. (Reproduced from the BHL Archive under the Creative Commons Attribution License).

His talk opened with William Herbert's statement from 1837, which we discussed in the first chapter of this thesis, that botanists 'did not thank him for his mules'.[19] Rolfe compared Herbert's report of opposition to his hybridising to a recent controversy over willowherb (*Epilobium* L.) hybrids. We will consider this dispute later in this chapter, but significantly, Rolfe concluded that this disagreement showed 'the diverse views which still prevail.'[20] However, Rolfe then, for the rest of his paper, emphasised one side of this debate, the 'scepticism, and even prejudice' from (unnamed) botanists producing classifications who 'regarded unsympathetically or ignored' plant hybrids.[21] Rolfe then set out an extensive historical survey of examples of hybridising experiments that 'proved' plant hybrids existed in nature. This demonstrated the plurality of views he had mentioned earlier, in regard to willows, mulleins, Primulas, Ericas, ferns, wheat and finally his specialism, orchids, showing that botanists did not universally oppose hybridity. Despite this evidence of diversity, Rolfe then concluded his long talk with the example of the red and white campions (*Lychnis* L. species),

[19] Rolfe relayed in detail what Herbert reported (Herbert 1837: 335-7).
[20] Allen Rolfe 1900: 182.
[21] Allen Rolfe 1900: 182-3.

which he felt represented all other cases, and which showed that 'natural hybrids have been largely ignored or got rid of'.[22] He repeated Masters' claim that nurserymen had hidden the identity of *Erica* hybrids because it was 'almost an impious thing to raise hybrid plants' but did not elaborate any further on this statement.

Rolfe also spoke about what was becoming known as 'hybrid evolution'. Fertile hybrids which persisted might be termed 'hybrid races' which could not be distinguished from 'species' other than in the fact that a hybrid has a known origin, whereas a species had an unknown origin.[23] He concluded that, under a 'combination of circumstances' including environmental conditions favouring the hybrid over either parent species, a hybrid might become dominant to produce a hybrid race that persists – or even a new species. Then hybridisation becomes of

> special significance to the systematists, and a knowledge of the behaviour of artificial hybrids under cultivation, and the relation they bear to their parents, should help him greatly in the identification of those which occur spontaneously in a wild state. And a correct idea of the existence, behaviour, and distribution of natural hybrids where their life is untrammelled should throw further light on the very origin of species…[24]

This was more of a bold move than is obvious today. Rolfe was, of course, merely repeating a long-held and widespread belief among horticulturalists, that hybridisation might produce new species. However, as we will see in the final section of this chapter, he was indebted for his position at Kew to Maxwell Masters and accountable to Thistelton-Dyer. Masters and Dyer were orthodox Darwinians, which meant evolution must take place gradually, not by sudden jumps—or saltations—produced in a few generations by hybridisation.

Overall, Rolfe emphasised that horticultural hybridists had been slow to engage with taxonomic botanists, and ended his talk by concluding that together these two communities might collaborate over hybridising experiments which ironically, may prove to be 'the only trustworthy means of saving systematic botany from its own confusion.'[25] We have scant evidence of the reaction to Rolfe's paper, but it was

[22] Allen Rolfe 1900: 198.
[23] Rolfe 1900: 200.
[24] Rolfe 1900: 202.
[25] Allen Rolfe 1900: 202 citing from a paper in *Nature* by fellow orchid breeder, Charles Chamberlain Hurst (1870-1947) (Hurst 1898).

lengthy, and perhaps it is therefore unsurprising that the audience took away what was probably most familiar to them, the religion versus science story.

A Narrative of Opposition to Hybridising

Several newspaper reporters heard that plant hybridising was important for science, progress, evolution and Darwinism, but also connected with religious prejudice. The plant hybrid, and specifically crossed species, represented important technological progress: The *Morning Post* decided that the plant species hybrid was an 'invention'.[26] The *Liverpool Mercury* reported that horticulturalists and their practice of hybridising might be the next big thing in biology:

> Its deliberations and the experiments and experience of its members will go a long way to strengthen the Darwinian theory relating to species, and it is evident from its proceedings that an enormous impulse is on the eve of being imparted to hybridisation.[27]

However, more newspaper journalists picked up on the religion and science story. *The Times* reporter (who actually attended the conference) heard that 'in Herbert's day' there was a 'great prejudice against hybridization among certain religious people' and 'the prejudice against hybridization was carried so far that nurserymen were afraid to exhibit hybrid plants in the Royal Horticultural Society's gardens, because they might injure the feelings of some over-sensitive religious persons; and they therefore exhibited them as wild species from abroad.' That prejudice was now over, but 'they now had to meet a prejudice of another kind of which he felt ashamed. He meant the prejudice which existed in the minds of some botanists against hybridization.'[28] *The Times* report mentioned 'prejudice' four times. *Country Life Illustrated* felt 'we have been hearing many interesting things about the hybridisation of plants' but picked out the following as the most remarkable fact among the other 'floral information': that 'a large number of people object to hybridising plants on religious grounds.'[29] The *Whitby Gazette* selected that 'the chairman announced there is no such thing as species' and that 'now that the secret is

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[26] Anon. 1899f: 7.
[27] Anon. 1899g: 6. The text's content and context shows that the author was talking about artificial hybridising, not natural hybridisation.
[28] Anon. 1899c: 13.
[29] Anon 1899j: 68.

out, it is as well to have it argued plainly that hybridisation is neither unnatural nor immoral.'[30] Similarly the *Middlesex Independent* stated 'There was a prejudice against the new method, and the Chairman was not sure that it had yet died out. It was said that the hybridists were contravening the laws of Providence.'[31]

It is striking that local and national newspapers, representing the Tory, liberal and independently-inclined political spectrum, independently wrote about the first day of the conference (there was apparently no repurposing of each other's copy) and in these different reports, each journalist picked up that hybridising had been held back by religious beliefs. Two reports presented plant hybridising as a new method or invention. Despite Masters' talk mentioning a long history of plant hybridising, it seems likely that his audience heard that hybridising was novel *because* it was closely connected in his rhetoric to progress and science. We can see the start of a subtle shifting of Masters' and Rolfe's story (at least, the version of their history reported officially in the conference proceedings). Instead of a brief mention of religious opposition limited to the Georgian decades of the century, with Victorian progress launched decisively with William Herbert's hybridising in 1837, hostility to plant hybrids, and hybridising, was projected across the entire nineteenth century. The shift appears to have been the result of Rolfe's account of contemporaneous hostility to hybrid willowherbs, which his audience assumed was motivated by religious sentiment (Rolfe himself gave no explanation).

Before we examine how Masters' earlier writings were the source of the religion versus science story, and what Rolfe witnessed in the willowherb debacle, we consider how the Master-Rolfe history drew on, yet then obscured, a different form of hostility to plant hybrids: this opposition was not from taxonomic botanists or from wider pious publics, but from horticulturalists themselves.

Aesthetic Objections to Hybridising

Historians usually cite evidence from the writings of two horticulturalists, James Bateman (1811-97), and John Forbes Watson (1827-72)(Figure 5.4), in support of the view that Christian belief resulted in a hostility to the plant hybrid and to the practice

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[30] Anon. 1899h: 7.
[31] Anon. 1899i: 3.

of hybridising. Instead, this chapter interprets these texts as evidence of the varied cultural contexts of knowledge-making in nineteenth-century Britain, which included a close relationship between the expression of faith and the practice of science.[32] However, that expression of religious sentiment did not result in a view of hybridising as impious or contrary to scripture. Instead, the plant hybrid was vulgar and ugly; simply too bourgeoisie to be tasteful. Forbes Watson's deep Christian belief produced a positive preference for wild flowers over artificial plant hybrids; whereas Bateman's criticisms concerned orchid hybrids only, and were most likely wholly class-based, rather than related to his convictions about the New Creation.

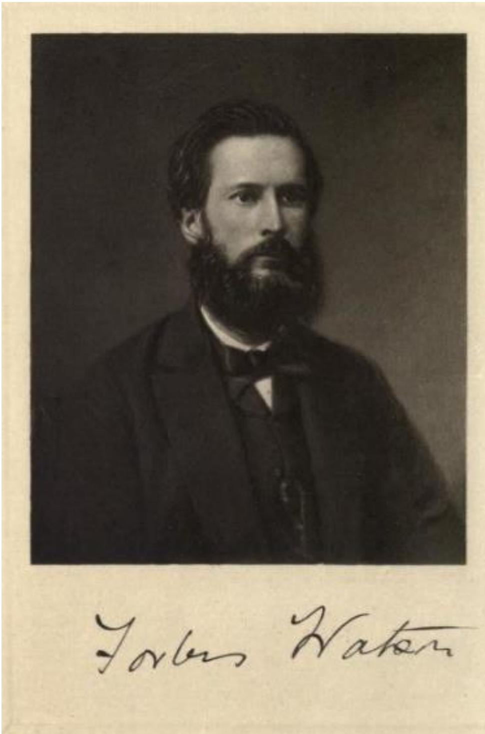

Figure 5.4: Portrait of John Forbes Watson (d.1872). From: Forbes Watson 1872, front piece. (Reproduced from the BHL Archive under the Creative Commons Attribution License).

John Forbes Watson coupled his love of plants with a Christian aesthetic philosophy in his book *Flowers and Gardens: Notes on Plant Beauty* (1872).[33] Forbes Watson objected to the 'mischief done to taste by a too exclusive attention to highly cultivated plants.'[34] Double-flowers were 'an imperfect creation' and instead a gardener should look to 'the pure works of God' for the 'best criterion for judging the works of man.' Pouring 'scorn' on wild plants as unfashionable was 'snobbish'.[35] Instead, the middle-class gardener should consider growing species that are accustomed to the climate and conditions in a garden. A wild dog rose was preferable to over-bred fancy hybrid roses. Variegated foliage was particularly vulgar which 'can only produce evil without

[32] Brooke and Cantor 1998.
[33] A surgeon who died in 1872 (as a friend who published his book records in its preface) and therefore is not the same person as the Forbes Watson who, from 1858-1880, was Director of the India Museum in London where his work included collecting Indian textiles and plants of economic value.
[34] Forbes Watson 1872: 172.
[35] Forbes Watson 1872: 203.

end.'[36] He was not advocating growing wild flowers as these species would decline under cultivation; instead he promoted letting the garden 'be to the wild *idem in altero*, that is to say, let it be mainly stocked with plants of close affinity to our own, so as to be adapted to our climate….yet more splendid species.'[37]

The relation between mid-Victorian Christian sentiment and the natural world was most highly developed by the aesthetic movement founded by John Ruskin (1819-1900) in his volumes *Modern Painters* (although Ruskin himself had an unsettled relationship with Christianity).[38] Ruskin's way of seeing was unlikely to have been acknowledged by local botanists obsessing over accuracy and striving to find their place in an increasingly prescriptive way of doing science (Ruskin was largely ignored by the *Journal of Botany*, for example).[39] Forbes Watson's aesthetic, he explained in his book's preface, was drawn in part from John Ruskin's way of seeing plants, and directly derived from his belief that God created wild forms and so these were aesthetically preferable to anything bred by man. Watson had 'a botanical training as a medical man' and described what it meant to him to be a Ruskinian-inspired local botanist: 'Some are nothing more than hard-headed collectors of names … seeking after rare or novel species' but 'for others' (implicitly himself included) 'the beauty is of more importance than the science.'[40] Watson is an interesting character because he combined his botanical and horticultural interests. And because his rhetoric evidences the wider sentiment among horticulturalists of a 'back to nature' aesthetic (more usually attributed to William Robinson's challenge the fashionable bedding system in the best-selling *Wild Garden* (1870)).[41]

However, garden historians claim that Forbes Watson 'went even further, to attack the florist's interest in hybridization; a wild rose was the work of God and to prefer a cultivated variety was an impiety.'[42] This is not how Forbes Watson wrote about plants. He did not say that the cultivated rose was impious; variegated foliage was 'evil' because he found it offensive to good taste. His aesthetic was derived from a

[36] Forbes Watson 1872: 205.
[37] Forbes Watson 1872: 166.
[38] For biographical details, see Hewison 2016.
[39] Blunt 1950: 231-235.
[40] Forbes Watson 1872: 172 and 205.
[41] Elliott 1986.
[42] Elliott 1986: 151-2.

positive preference for the Works of the Creation but was also shot through with the deep-seated Victorian sense of class-based moralising: that manufactured plants were vulgar, associated with trade and the bourgeoisie, who should not be snobbish about the wild plants enjoyed by the lower classes unable to afford fancy garden blooms.

Forbes Watson's position was part of a wider cultural reaction against the commodification of the plant, by the nursery trade, which we also heard voiced by the fernist Chas Druery in chapter four of this thesis. In an earlier example, the Manchester shoemaker Richard Buxton (1786-1865) announced in 1849 that the inhabitants of large manufacturing towns sought out wild flowers in preference to

> flowers as seen in the gardens near towns, in a somewhat unnatural state. The gaudy beauties of the flower-garden may strike by their splendid colour, and charm by their novelty — but, for true beauty and grace, they are not to be compared with our wild flowers any more than the natural is to the artificial. Indeed, strictly speaking, many of the tenants of our gardens may be termed partly artificial flowers. By means of innoculation, forcing, and the application of stimulating manures, some of our native flowers can scarcely be recognized with the old stock from which they sprung.[43]

This aesthetic polarisation, between manmade imitation versus God-given beauty of the original stock, natural versus artificial, also affected Victorian attitudes to the breeding of orchids. Horticultural heavyweight James Bateman was well-known for disliking orchid hybrids.

In 1864, writing under the pseudonym *Serapias*, Bateman objected to the 'misplaced ingenuity' of Veitch & Son's hybridists and exclaimed 'hybridise everything else if you will, but spare, oh spare the orchids!'[44] Some historians assume that a deeply religious man like Bateman would regard hybridising as tampering with God's creation. [45] However, Bateman appreciated other plant hybrids; it was only *orchid* hybrids that he objected to:

> Mr Bateman said that he had hoped that Orchids constituted a Royal race into whose preserves the hybridist would not dare to enter, and as much as he appreciated his [i.e. the hybridist's] labours in other parts of Flora's dominion he nonetheless felt a kind of inward satisfaction whenever failure attended his attempts to raise cross-bred Orchids. In the case before him [*Calanthe* x *veitchii*] however, he was forced to admit, though it nearly choked him to do so, that a magnificent result had been obtained

[43] Buxton 1849: xii. On Buxton, see Secord 2004 and 2002.
[44] Anon. 1864: 341. See Elliott 2010 on Bateman's pseudonym.
[45] Shephard 2003: 158 and Elliott 2010a: 17.

> inasmuch as the hybrid in question was certainly one of the finest Winter-blooming Orchids in cultivation.[46]

Two elements of this quotation are important in explaining Bateman's motivation. First, he refers to orchids as a 'Royal race'; second, the production of a beautiful and useful winter-flowering plant made a hybrid orchid (just about) acceptable. As Jim Endersby has shown, Bateman saw orchids as desirable because the plants were exclusive and expensive.[47] By the 1870s, mass-produced orchid hybrids began to appear alongside a flurry of 'how to grow' articles and books aimed at the popular gardening market.[48] Nurseryman Benjamin Samuel Williams (1822-90) advertised

Figure 5.5: 'Cheap Orchids' nursery advertisement. From: Williams 1894, endpiece (Reproduced from the BHL Archive under the Creative Commons Attribution License).

[46] Anon. 1865: 1109.
[47] As Jim Endersby stresses (although without mentioning orchid hybridising) (Endersby 2016).
[48] Elliot 2010a:32-33.

'cheap orchids' for the first time in the back of the seventh edition (1894) of his best-selling *Orchid-Grower's Manual* (Figure 5.5). Williams had forecast this development as early as the Manual's second edition in 1862, when he encouraged readers to start hybridising orchids.[49] Jim Endersby does not mention orchid hybrids, but points out that Bateman disliked this democratisation of orchid collecting.[50] Finally, there is an oft-cited passage quoting Bateman at the 1885 RHS Orchid Conference:

> I have been brought up with the very strongest abhorrence of hybridizers. (Laughter.) I fell into evil hands early in life. My first Orchid-growing friend was Mr. Huntly. When I paid Mr. Huntly a visit at his snug rectory in Huntingdonshire, he pointed out to me his cacti and his Orchids, and said, " I like those plants, in fact they are the only plants I grow, because those fiends (meaning the hybridizers) cannot touch them." (Laughter.) You must make a little allowance for a botanist, for hybridizers do give botanists a lot of trouble (laughter) but, however strong my prejudices were, I must confess that when I saw such plants as the *Cattleya* downstairs, if I was not converted, I was, at all events, what comes to the same thing, shut up. (Laughter).[51]

We have seen how Bateman's remark is interpreted by some historians as evidence of religious opposition to orchid hybrids, presumably because Bateman's early influence in this story was a vicar. Yet in this passage Bateman is talking about hybridists giving botanists trouble; indeed, much of the conference time was spent on debating what to do about orchid nomenclature given the huge numbers of new artificial hybrids.

Taken together this evidence suggests that Bateman disliked orchid hybrids because he knew that they would become, like other hybrids, an affordable commodity; and once this had happened, by 1885, he joked about the matter. Bateman's comments at the conference do not sound like those of a man who feared that hybridising was tampering with the Creation. Bateman devoted much of his life and resources to collecting what he referred to as a 'Royal race'. An analogy to the human races was unabashed and arguably revealing. As we explored in chapter one of the thesis, an on-going cultural undercurrent, that ran throughout the Victorian period, saw plant hybrids as inferior aesthetically and biologically, and by analogy, akin to the child of mixed-race parents.[52]

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[49] Williams 1862: 30-3.
[50] Endersby 2016: 78-9.
[51] Mason 1886: 49.
[52] While the history of how plant hybridity fed into Victorian debates over miscegenation is beyond the scope of this thesis, this connection is also significant for early twentieth-century plant breeders in their wider context of eugenics (see Curry 2021).

A careful re-reading of the key sources usually cited in relation to Forbes Watson and Bateman shows that at stake for both men were their aesthetics (refracted through the lens of a Christian culture) rather than any naive conception that, as devout Christians, they saw hybridising as interfering with God's works or contrary to scripture. This latter explanation is arguably too easily seized upon because, after Darwin, we expect to find religion versus science debates in the second half of the nineteenth century.[53] And as historian Ian Hesketh reminded us in the introduction to this thesis, such convenient histories often obscure more nuanced, inconvenient ones – in this case, about snobbish collectors; in other stories yet to be told, about human hybrids and the history of scientific racism. In the next section, we trace the origin of Maxwell Masters' history at the 1899 conference, to demonstrate how this was the culmination of his long campaign to promote hybridising and hybridists as contributing to knowledge-making in science.

III. Maxwell Masters: Origins of a Religion Versus Science Narrative

Creating a Science of Hybridising

By 1899, Maxwell Masters was a household name and a public face of horticulture.[54] We saw in the last section how Masters set out his own version of a history of the practice of hybridising in Britain and connected it to plant taxonomy and physiology. That history emphasised opposition to plant hybridity. In this section, we see how Masters' address at the Hybridization Conference drew directly on his series of editorials in the *Gardeners' Chronicle* from the preceding twenty-five years.

In January 1874, when the *Chronicle* split from the *Agricultural Gazette*, the editorial noted that

> Hybridisation—selection—progressive evolution—is it not by these that practical horticulture has been so much enriched of late years? The theories based on these facts may or may not be correct: no one supposes they are absolutely so; but that

[53] Bowler 2007.

[54] Maxwell Masters was included in *Who's Who* as someone whose 'ability has brought him before the public' (Anon. 1897a: 580); and his name was 'almost a household word' (Anon. 1907: 157).

> there is—must be—a very large infusion of truth in them as is evidenced by the great advantage which follows from their application to practice. [55]

Masters in this editorial saw a direct link from horticultural hybridising to his version of a progressive Darwinism and a connection between practical breeding and evolutionary science.[56] As we noted in the introduction to this thesis, ornamental plant breeders had been combining crossing with selection from early on in the nineteenth century; whereas as we discussed in chapter three, cereal crop breeders in Britain and Europe generally used selection alone until the 1880s. These different approaches reflected what practitioners believed about the distinct flower anatomy and reproductive behaviour of each plant family.[57] Therefore, for Masters, it was hybridising which marked horticulture out from agriculture.

Masters first wrote about a history of hybridising in 1880 on the retirement of a celebrated Scottish head gardener and hybridist, Andrew Turnbull (1804-86).[58] Turnbull talked about his experience hybridising *Ericas* (Cape Heaths) in the 1820s. He remarked that some of the hybrids produced ‘around eighty to one hundred years from the present date’ (i.e. from around 1780 to 1800) were listed as ‘good species’ in ‘Loudon’s Catalogue’, the first edition appearing in 1826.[59] In the 1820s, it was common practice to name garden hybrids with a species epithet in Britain as well as in France.[60] However, this observation was then elaborated on in the *Gardeners’ Chronicle*:

> It seems that in the times of which Mr Turnbull was speaking, it was considered by a certain section of no doubt worthy people to be almost an impious thing to raise hybrid plants. It was deemed a sacrilegious interference with the laws of the CREATOR, and so strong was this prejudice in certain quarters that some of the nurserymen at that day were fain to conceal the hybrid parentage of the plants they offered, and to catalogue them as if they were imported species from the Cape! [61]

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[55] Anon. [Masters] 1874: 21.
[56] This thesis follows the convention among historians to attribute *Gardeners’ Chronicle* editorials after 1867 to Maxwell Masters’ sole authorship, whereas various other authors contributed book reviews and other articles.
[57] Wilson 1899.
[58] On Turnbull, see Desmond 1994: 695; Anon. 1874 and Musgrave 2007 (a social history of head gardeners’ contributions to Victorian horticulture).
[59] Loudon 1826. Anon. [Masters] 1880a: 179.
[60] Oghina-Pavie 2015.
[61] Anon. [Masters] 1880: 177-8.

Masters continued that this hostility to plant hybrids may seem ‘preposterous enough now-a-days’ but reminded the reader that ‘certain current scientific notions’ which are opposed for theological reasons now, will, in future, be as accepted, as hybrid *Ericas* are now. It seems likely that Masters was referring to religious opposition to Darwinian theory. Therefore, as he developed his history of hybridising the following year in response to the publication of Föcke’s path-defining hybrid flora, he was perhaps motivated by a wider contemporary message regarding science and religion.

RHS historian Brent Elliott notes that the *Gardeners’ Chronicle* in 1875-80 carried book reviews making the point that Darwinian ideas supported those of natural theology. [62] As Elliott points out, during the decades after the publication of Darwin’s *Origin of Species,* many of the *Chronicle’s* readers saw no conflict between their understanding of Darwinism and their religious beliefs. By contrast, Masters chose to see religious belief opposing hybridising. Masters’ approach to plant hybrids may have been affected by his reading of John William Draper’s (1811-82) book on *History of the Conflict between Religion and Science* (1874). Draper was an Anglo-American physician, Professor of Chemistry at New York University and a Darwinian, who attended the debate over the *Origin of Species* at the Oxford meeting of the BAAS in 1860.[63] Masters certainly read this book, as his annotations are on the copy in the Lindley library, and although we cannot date these marginalia, it seems possible that he read Draper’s conflict thesis into his version of Turnbull’s testimony. That was a misreading. Had Masters tracked down the origin of the quip at Turnbull’s retirement party, he would have found that in 1843 William Herbert elaborated on the supposed widespread opposition to Cape Heath hybrids:

> It is now known that the late Mr Rollisson on Tooting raised many Ericae by cross-breeding, as for instance jasminiflora between ampullacea and Aytoni, as well as many others which were figured by Andrews as new species from the Cape, but which will not be found amongst any specimens of African plants. The cultivator thought his

[62] Elliott 2010a: 67, citing: “No more persuasive apostle of natural theology, no more powerful advocate of the argument furnished by design and adaptation, ever lived than CHARLES DARWIN” (Anon. 1875b: 308) and the anonymous review of Darwin’s *Power of the Movement in Plants* (Anon. 1880). This reviewer was most likely the Reverend Miles Berkeley, given the similar content of his review of Darwin’s orchid book (c.f. chapter three of this thesis).
[63] For biographical details, see Ungureanu 2015.

> plants would have been undervalued, if their true origin had been declared, and he would have lost the monopoly.[64]

This quotation from Herbert shows that what was at stake for nurserymen between about 1790 and 1830 had nothing to do with religious sentiment: it was about maintaining the commercial value of plants as rare imports, rather than admitting their manmade origin. Therefore, before the Victorian period, the manmade plant hybrid was already a commodity at the bottom of a pyramidal social hierarchy of plants: at the apex, a rare exotic natural orchid species; and near the base, mass-produced artificial hybrids of common, easy-to-grow genera popular with florists, like pelargoniums. The Cape Heath species started out somewhere near the top, until they then went out of fashion around 1840, largely as hybridising made their forms too easily available. However, Masters it seems was determined to interpret his sources to fit the view that horticultural hybridising was opposed on grounds of impiety, and repeated this message in the form of a history. [65]

Constructing a History

Seven years later, the publication of in 1881 of Wilhelm Olbers Föcke's hybrid flora prompted Masters to reflect on, and reiterate, the parallels between hybridising by gardeners and hybridisation in nature. Föcke's book contained a chapter titled 'History of Plant Hybrids' and this prompted Masters to open his review with some history of his own:

> Hybridising was formerly regarded as a sacrilegious subversion of Nature, and those who practised the art were stigmatised as mischievous intermeddlers with the works of the Creator. Most botanists entertained a dislike for cultivated exotic plants as there was always a danger of their not being "true species"; and some writers would have consigned all hybrids to the rubbish-heap as being of impure descent. But in spite of the opposition and contempt of some botanists, gardeners did not stay their hands in the work of rearing novelties, heedless of the "confusion" they were causing. In time, too, some botanists began to appreciate the fact of the existence of hybrids in a natural state, and now the general conception of a species, even by those who do not accept the doctrine of evolution in its entirety, is so different that the interest attaching to a plant is in proportion to what is known of its origin.[66]

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[64] Herbert 1843: folio 4-5 [unpaginated].
[65] The *Gardeners' Chronicle* ran leaders on hybridisation on 8 January 1881, 15 October 1881, 7 July 1883, 15 Oct 1891, 7 January 1893 and 4 February 1899.
[66] Anon. [Masters] 1881: 48.

In this passage, Masters made two points about the history of breeder-botanist relations over hybrids and hybridising: first, that man-made hybrids and the practice of hybridising were opposed as sacrilegious (he did not specify by whom); second, that botanists rejected hybrids as these entities confused their classifications. Then 'in time' some botanists recognised 'the existence of hybrids in a natural state'. Masters stated that he drew on Föcke's history, which mentioned that 'conservative' botanists opposed accepting 'so many' hybrids 'because they were more inclined to believe in variations and intermediate forms.'[67] As we saw earlier in this thesis, that was the position taken in chapter one by John Henslow, and in chapter three by Joseph Hooker. As Masters outlines in this quote, after Darwin, there was an increased interest in plant hybrids. However, Föcke's account was not specific to a country and was far more measured than Masters' version quoted above; further, while Föcke said that some opposition came from those supporting the 'constancy of species', in other words, species fixism, those people were not necessarily botanists. While Masters certainly drew on Föcke's account, it seems that he supplemented his portrayal of a long history in Britain of hostility to hybrids and hybridists from other sources.

One source Masters drew on was William Herbert's comment in 1837, that taxonomists 'did not thank him for his mules'. As we discussed in chapter one, the opposition to Herbert's hybrids related either to Knight's opposition, around 1818, or to a more recent memorable squabble with a rival taxonomist working on daffodils. Later in October 1881, Masters repeated his history, but this time emphasising opposition from botanists over confusion to classifications, the charge that Herbert mentioned in 1837, and Föcke had highlighted. He stressed that hybridising might lead to progress, and therefore aligned hybridising with science. He wrote:

> It is not so very long ago that hybridisation was looked on askance by botanists as tending to confuse what were termed species, and as introducing inextricable confusion in neatly devised plans of classification and so forth. [The work of hybridists] has proved of the utmost value from a scientific point of view, as teaching us how Nature herself operates, and as giving us an insight into her methods, and thus of greatly improving our knowledge of natural forms, the manner in which they vary, and the limitations and extent of their variation. So far from hybridisation introducing confusion, properly looked at it extends our knowledge, opens up new avenues for future progress, gives us more worlds to conquer – and all this in addition to the more

[67] Föcke 1913: 408-9.

> direct increase of practically useful facts. All honour then to Mr Dominy and his associates. Would they were more numerous and acted on well-devised lines of action. We cannot doubt, from the results already obtained, that progress in this direction will be secured by this and other means, and progress is what we must all aim at if we are even to hold our own. [68]

Masters comment in the previous quotation, that horticulturalists must embrace progress to ‘hold our own’, is revealing. It supports the interpretation that Masters rhetorically developed hybridising as an experimental practice (and therefore as contributing to science) in order to bolster the social position of horticulture, especially given its close association with trade.

Indeed, Maxwell Masters felt that horticulture was grossly under-appreciated. A few years later, in 1887, the RHS experienced a public embarrassment when it got into a financial dispute over unpaid rent due to the Royal Commissioners, for the Society’s occupation of Kensington Gardens. For Masters, the incident represented a lack of recognition of horticulture among the scientific establishment and by the government. In a letter to *Nature*, Masters argued that while readers might assume that horticulture was only of interest as a ‘pleasant pastime’ or as a ‘commercial enterprise’ the society both made use of and generated scientific knowledge. Masters cited Darwin’s *Origin of Species* and the *Variation of Plants and Animals under Domestication*, arguing that because gardeners conduct hybridising experiments ‘from utilitarian rather than scientific points of view’ this did not preclude these from being valuable for science, as Darwin’s work showed; further, ‘without the aid of a Society much of the experience gained would be lost to science.’ The RHS should have received a ‘more respectful and sympathetic treatment’ and support from the Royal Society.[69]

This political statement was surrounded by a scattering of comments in the *Gardeners’ Chronicle* making related points. Hybridising, rather than merely cross-breeding, was not only progressive science, but also ‘revolutionary’ because it was worthy of government attention (and implicitly, state funding):

> We know nothing of Bulgarian atrocities or other news of the week that will for a moment compare with the interest attaching to Messrs. Veitch’s hybrid between Sophronitis and Cattleya, described by Professor Reichenbach at p.263. We are

[68] Anon. [Masters] 1881a: 501.
[69] Masters 1887: 177.

> confident that Mr Seden has accomplished a revolution in botanical science which in its possibilities and far-reaching significance puts Home Rule or any ephemeral accident of that kind quite in the shade. If politicians cannot see it in the same light, that is not our fault.[70]

Hybridising might also generate important novel knowledge for plant physiology. For example, the *Gardeners' Chronicle* reported on a nurseryman, Charles Noble (1817-c.1890) who laboured for 25 years to produce a white form of *Clematis* by hybridising and finally got a marketable product in 1883, selling 'a good many thousands' by 1888. However, some plants reverted, and he 'had some very pointed questions put to me respecting these abnormals, one or two abusive letters, and one case of a Dutchman who refused to pay.'[71] The *Gardeners' Chronicle* responded by encouraging Noble to 'stand firm against prejudice against hybrids' and by praising his contribution to botanical science. Mr Noble had demonstrated by his hybridising the 'dissociation of hybrid characters, a very obscure point in vegetable physiology' elucidated by his 'authentic history' of crossing.[72] This may have been Masters' former co-editor Thomas Moore writing, as the leading authority on *Clematis* L., although Moore was unlikely to have made such a pointed comment on physiology. The 'dissociation of hybrid characters' was indeed a little-known point, perhaps taken from Darwin's *Variation,* and Moore, unlike Masters, was no Darwinian.[73] Further, the praise here for Mr Noble's scientific record keeping and contribution to physiology, was becoming a favourite theme in Masters' editorials.

In 1893, as we saw in chapter four, Maxwell Masters' returned to the topic of hybridising as a contribution to science in response to the publication of John MacFarlane's study of plant hybrids. Masters' editorial emphasised how many practical gardeners had contributed to systematic botany, and that gardeners might also conduct physiological experiments by hybridising, as long as they kept accurate records.[74] Masters' editorial also reflected the wider late-century context among plant breeders in Europe and America, highlighted by historians Berris Charnley and Philip Turtle, of a reputational community of practice based around accurate record keeping.

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[70] Anon. [Masters] 1886: 275.
[71] Noble 1888: 152. For biographical details of Noble, see Desmond 1994: 520.
[72] Anon. [Masters] 1888: 159.
[73] Darwin 1868 v.II: 270 discussed in O'Reilly 2014: 57-8.
[74] Anon. [Masters, M.T.]. 1893.

Those documentary practices dated back to the eighteenth century, and, as we have seen in this thesis, other botanists, horticulturalists and gardeners referred to hybridising as experimentation. Like those practitioners before him, Masters also appealed to hybridising as experimentation to elevate the social status of horticulture.

Introducing the 1899 Conference

In February 1899, Masters introduced the forthcoming 'Hybridization Conference' to his Chronicle audience by rehearsing his history again, remarking: 'in the old days when "species" were looked on as something sacrosanct' some people deemed hybridising 'impious'. This 'fictitious religious difficulty' ended, Masters argued, once William Herbert 'proved' that plant hybrids occurred in nature 'by obtaining the same hybrid by artificial means'.[75] He then offered a Darwinian solution: a science of evolution as Darwin had practised it, combining horticulture with biology. Yet in doing so, Masters also encouraged the idea of widespread opposition to plant hybrids and hybridising, and he aligned hybridising with Darwinian biology in a wider conflict narrative between religion and science.

What does this mean for the standard early history of genetics? Rather than portraying Masters as enthusiastically supporting William Bateson's experimental agenda, instead it can be argued that Masters was *continuing* to promote his own conception of a scientific experimental horticulture based around hybridising. Bateson was not the first to suggest the keeping of accurate experimental data in his paper at the 1899 Hybridization Conference.[76] Masters had been calling for this for three decades, no doubt acutely aware that his own father had died without keeping records of his many hybridising experiments that had proved so fruitful for Darwin's theorising. It seems more likely that Bateson was delivering the message that Masters had crafted for him. This re-interpretation is supported by historian Gregory Radick's recent extensive re-appraisal of the history of genetics around 1899. Radick likewise sees Bateson as not particularly interested in the RHS' agenda, which did not feature heredity at this time, and certainly not sharing Masters' vision of a unified plant

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[75] Anon. [Masters, M.T.] 1899j: 72.
[76] Bateson 1900: 59.

science combining horticulture and biology. [77] This view is also hinted at by historian Robert Olby, as we mentioned in the introduction to this chapter, yet Olby contradicts his own point about the 1899 conference elsewhere, as he also argues that horticulture was 'the baptismal font of genetics'.[78] Hybridising as a practice was important for the development of Mendelism and the science of genetics, but the RHS itself did not, in 1899, focus on how horticulturalists might contribute to studies of heredity. Instead, their central interest was in how hybridising was an experimental tool for constructing evolutionary classifications, and how hybridisation might be important within a revised version of Darwinism. These twin claims, in fact, had been the focus of several botanists since 1837, both before and after Darwin, irrespective of whether the species might be fixed (as Henslow saw it) or fluid (as the Darwinian Masters believed). This persuasively shows how the argument that plant hybridity became important to science thanks to the species concept shifting from fixed to fluid does not hold: the connection between hybridising as an experimental tool to investigate classifications continued from when it was used by Henslow and Herbert in the 1830s to Masters' advocacy of the practice in 1899. Irrespective of the different views these practitioners held of 'the species', hybridising was consistently used or portrayed as an experimental practice within the conduct of the science of taxonomy. Therefore, hybridising was both a source of conflict and debate, and, concurrently, a practice that connected different botanical communities over time throughout the Victorian period.

However, plant hybridising was unavoidably practised within a social and cultural context, and the realities of a gardener-hybridist attempting to contribute to science were more problematic than Masters' idealised vision suggests. These social challenges are explored in the treatment of the botanist who co-authored the 1899 conference history, Robert Allen Rolfe, and we will see how his lived experience shaped his historiography.

[77] Radick 2021 (forthcoming): 213-4.
[78] Olby 2000a.

IV. Robert Rolfe: Contemporary Conflicts Colour the Past

In-fighting in the Herbarium

Robert Allen Rolfe was born on the Duke of Portland's Welbeck Estate, in Sherwood Forest, Nottinghamshire and grew up within what his seniors at Kew saw as humble origins.[79] He came to Kew in 1879 as a gardener aged 24, and the following year sat an open competitive botany examination to win a post as a Second Assistant in the Kew Herbarium. The third-placed candidate, Henry Groves (the same age as Rolfe but a university-educated gentleman) contested the results. Groves protested that the test was unfair, as it contained too few British plants for identification, and the format was unacceptable as he had been led to expect an open book exam by none other than Sir Joseph Hooker. Maxwell Masters, who had set the exam, emphatically refuted these protests, insisting that Rolfe was the better botanist (no doubt due to his experience of plants from around the world as a gardener on a large estate). Masters and Hooker were close enough that Groves' formal outburst was dismissed, and Rolfe secured the post.[80]

Masters regularly visited the Herbarium at Kew and Rolfe explained 'Dr M and I are very good friends.'[81] Maxwell Masters was a well-respected taxonomist, and a Fellow of the Royal Society (still an elite club for philosophical practitioners) but most important of all, he could rely on Hooker's patronage ('Hooker made me' he later commented to a friend and thanked Hooker for 'liberating me from a life of slavery and poverty').[82] Masters was also an astute diplomat. In 1872, he was probably instrumental in allying with Hooker and raising a petition against the government's attempt to split the Director of Kew's control over both botany and horticulture, thereby securing his own position as well as that of Hooker. [83] Masters' advice would

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[79] Biographical details on Rolfe are from his obituaries in Stapf 1921; Anon. 1921 and Anon. 1921a. See also the hagiography in Pridgeon 1993. These sources are balanced in this account by Rolfe's letters and the wider context at Kew and among botanist communities.
[80] Maxwell Masters to W. Thiselton Dyer, 6 June 1880, and associated letters marked 'private' over the Groves dispute, Kew Archive Misc. Correspondence of Herbarium Staff (1879-93) folios 48-51.
[81] R. A. Rolfe to Mr Hurst, 19 February 1899, CCH Archive (ADD.7955/1/55 folio 2).
[82] Maxwell T. Masters to Joseph Hooker, 19 July 1899, MTM Archive folio 47.
[83] Elliott 2004: 343.

have been important for Rolfe as he built his own reputation as a taxonomist specialising in orchids. Therefore, it is unsurprising that, in the mid-1880s, Rolfe put into practice what Masters argued for in the *Gardeners' Chronicle:* Rolfe began hybridising species in the orchid genus *Phalaenopsis,* to verify taxonomic claims.[84]

Rolfe had a troubled relationship with his colleagues at the Kew Herbarium from the outset. A year after starting in the role, he accused the Senior Second Assistant Nicholas Edward Brown (1849-1934) of destroying or copying his naming labels, alleging that Brown had passed off the specimens as his own work.[85] Rolfe explained on a note on an herbarium ticket (a name label):

> This I gave to Brown with the above particulars on a neat ticket and thought as it was almost the first specimen I had put in Herb. of my own accord I had done something. When it got back to me next laying in my ticket was minus, and the information copied by Brown. As several others were also destroyed (to my knowledge) I read Brown a lecture, to which he replied he never kept tickets of anybody's. This very thing caused several quarrels between us and I know I have been angry more than once & told him I would not allow my tickets to be destroyed…[86]

In 1887, Rolfe complained officially to the Keeper of the Herbarium and Jodrell Laboratory: 'The fact is Brown & I cannot agree about the value of certain tickets of mine & takes upon himself to destroy them…the only motive I can see for it is to make it look as if he did most of the work in the place.' Rolfe then proceeded to point out that he had been overlooked for promotion: 'I must be candid and say that as I began at the laying in stage it was not altogether unnatural that I should hope to get promoted to something else someday and now I feel that the more responsible work of determining collections is given to a junior officer.' [87] Rolfe's post involved much 'laying in' or filing herbarium sheets away, a tedious and menial task compared to the scientific work of producing taxonomic monographs. His complaint was rejected, and

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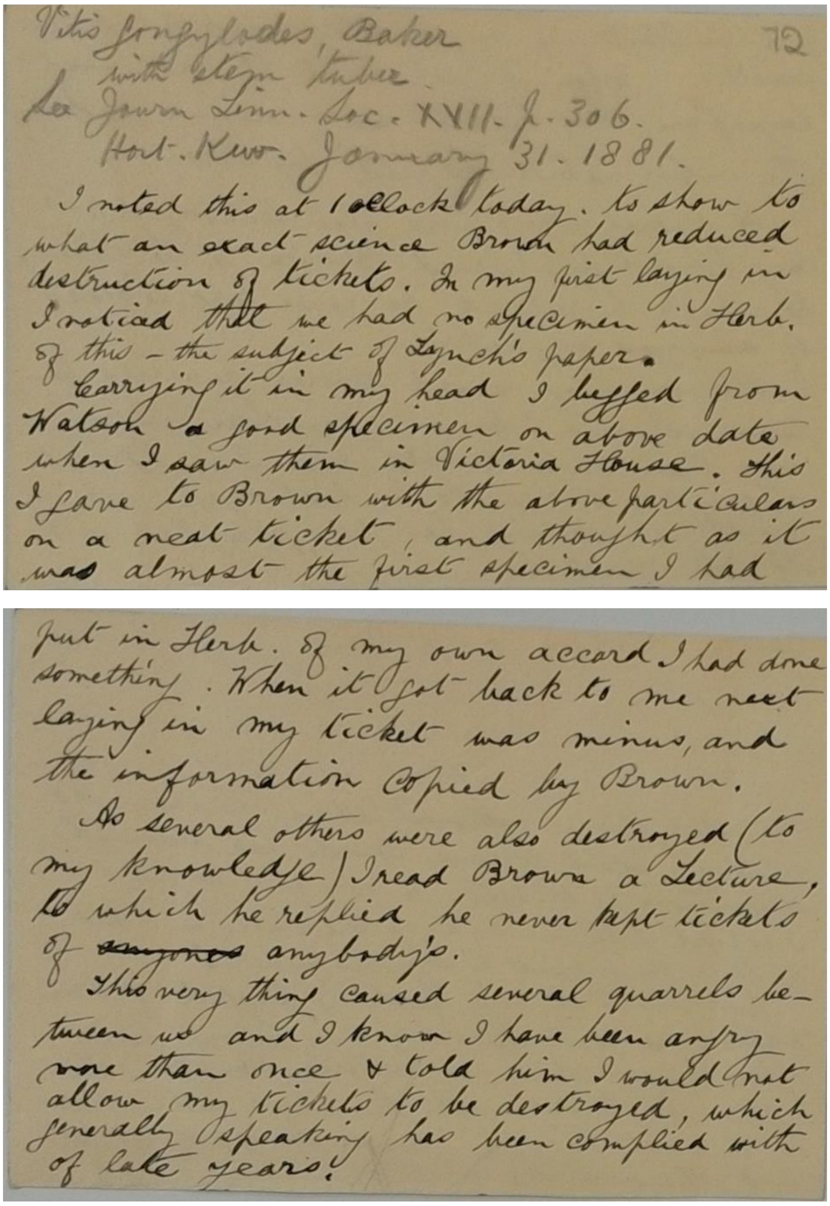
[84] See Elliott 2013: 17 and the primary sources in Anon. 1886: 201-2 and Rolfe 1886: 169.

[85] Desmond 1994: 107-8. In a biographical extract, Brown is portrayed as a quiet and unassuming man, whose skills as a botanical artist secured him the Herbarium post (Bynum and Bynum 2017: 160-1). See also chapter four on Brown and hybrid ferns.

[86] R.A.R. note on an herbarium ticket dated 31 January 1881. Kew Archives Misc. Correspondence of Herbarium Staff (1879-93 folio 72).

[87] A. R. Rolfe to Prof. Oliver, 8 January 1887 [letter folded and repaired with Sellotape therefore clearly retained as evidence of his complaint] Kew Archives Misc. Correspondence of Herbarium Staff (1879-93 folio 72).

he responded with another upset letter asserting that Brown had acted ‘deliberately & intentionally’.[88]


Vitis gongylodes, Baker
with stem tuber.
See Journ. Linn. Soc. XVII. p. 306.
Hort. Kew. January 31. 1881.
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I noted this at 1 o'clock today, to show to what an exact science Brown had reduced destruction of tickets. In my first laying in I noticed that we had no specimen in Herb. of this – the subject of Lynch's paper.
Carrying it in my head I begged from Watson a good specimen on above date when I saw them in Victoria House. This I gave to Brown with the above particulars on a neat ticket, and thought as it was almost the first specimen I had put in Herb. of my own accord I had done something. When it got back to me next laying in my ticket was minus, and the information copied by Brown.
As several others were also destroyed (to my knowledge) I read Brown a Lecture, to which he replied he never kept tickets of ~~anyones~~ anybody's.
This very thing caused several quarrels between us and I know I have been angry more than once & told him I would not allow my tickets to be destroyed, which generally speaking has been complied with of late years!

Figure 5.6: Robert Allen Rolfe’s (R.A.R.) note on an herbarium ticket dated 31 January 1881. Kew Archives Misc. Correspondence of Herbarium Staff (1879-93 folio 72) (© Kew Botanic Gardens, author’s photograph reproduced with kind permission of the Board of Trustees of the Royal Botanic Gardens, Kew for non-commercial unpublished research use).

Rolfe’s drawn-out dispute with his colleague and sense of being wronged is significant because in his 1899 history, Rolfe recounted an episode involving Brown, and used this as evidence of hostility to hybrids from botanists. In 1892 Brown, along with a recently retired colonial civil servant, who had worked under Hooker on the

[88] A. R. Rolfe to Prof. Oliver, 11 January 1887, Kew Archives Misc. Correspondence of Herbarium Staff (1879-93 folio 73).

Indian flora, Charles Baron Clarke (1832-1906), confronted a local botanist over willowherb (*Epilobium* L.) hybrids.[89] A vicar we met in chapter three, Edward S. Marshall, had obtained the German Professor Haussknecht's monograph on *Epilobium* (1884), which contained over sixty hybrids. He then spent four years collecting British willowherbs and sending them to the Professor for determination. Marshall announced he had found 27 natural hybrids, of which two were new to science. He noted how 'the local botanists' could do much to increase knowledge of British hybrids.[90] However, Baron Clarke disputed the German Professor's hybrids as 'altogether beyond me.'[91] There was 'no evidence whatever adduced to show that the plants in question are hybrids – far less what their parents were.' He complained of 'hybrid-mongers' who were naming as hybrids accidental forms within the variation of a single species: 'their hybrids are not hybrids between any two plants that ever lived, either species, crosses, or individuals, but hybrids between two of the hybrid-monger's own diagnoses.'[92] Brown agreed: 'Possibly some of them may be hybrids, but those that I have seen, named by Prof. Haussknecht, Rev. E.S. Marshall, and others, appear to me at the utmost but trifling variations of one or other of their supposed parents …. I see no use in inserting in our floras descriptions of such plants.' [93] Marshall retorted that hybrids were in some genera 'rare or non-existent' but in others they were frequent. He pointed out Clarke had 'admitted that they occur between the willows' therefore why, Marshall asked, 'is he [Brown] so positive that there is no such thing, among the willowherbs?'[94]

There were at least two responses to Marshall's question. Clarke's words openly pointed to his view that, like his mentor Hooker, he saw any over-naming of entities that he disagreed with as inherently unphilosophical behaviour. As a self-styled 'hobbyist' yet Kew-trained, he held to the Hookerian virtues even more determinedly than younger salaried taxonomists at Kew. 'Monger' was a trader—in cheese or fish or plant specimens—and by the 1800s was commonly used to mean a disreputable and

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[89] For biographical details of Baron Clarke, see Desmond 2004.
[90] Marshall 1890: 3.
[91] Clarke 1891: 228.
[92] Clarke 1892: 80.
[93] Boswell Syme 1892: 175. Brown was charged with completing the accounts for the third edition of *English Botany* on behalf of an aging and ill Syme (Allen 2010).
[94] Marshall 1892: 107.

dishonest dealer.[95] Marshall was a person whose science was suspect *because* of his association with a commodity, the plant hybrid.[96] It was a considerable and public insult, in print in the leading journal subscribed to by most of Marshalls' fellow local botanist community. Marshall was upset but stoically continued publishing accounts of plant hybrids. He decided that Clarke and Brown were following the official line on hybrids at Kew, ultimately emanating from Thiselton Dyer.

Confronting Thiselton Dyer

The clearest picture of Dyer's view of plant hybridity was revealed in what became known as the *Cineraria* controversy. In 1895, this public row sprouted between Cambridge zoologist and Darwinian doubter William Bateson, and Dyer, in the pages of *Nature,* over a genus of gaudy daises.[97] Dyer believed that the multitude of varieties of this garden plant had a single origin in common. They were all derived from the wild species by gradual human selection, 'the accumulation of small differences'; in other words, this evidence from the garden bolstered the scope of what natural selection might do.[98] Bateson countered that the cultivated form of *Cineraria* had originated by hybridisation of several species and that there was also evidence of some sports giving rise to stable varieties, researching from the 'ordinary manuals' of plant breeders.[99] Dyer retaliated, objecting to 'the danger of accepting horticultural evidence as to hybridity': one hybrid turned out to be mistaken for a growth stage; another for a very fine form of a putative parent species; and lastly, in the case of a valuable orchid, the hybrid was simply a fake.[100] Although experiments had proved that plant hybrids were much more widespread than previously thought (we saw how he acknowledged willow hybrids in chapter three) he also believed that hybrids were over-recognised, and that 'palpable objective proof of the fact' of hybridity was essential.[101] As we saw in chapter

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[95] O.E.D. notes this nineteenth-century usage with examples (Anon. 2020 [unpaginated]).
[96] Allen 1976: 189-90 and on 'species-mongers', see Ritvo 1997a: 344 and Endersby 2008: 270-72 and 325. See Olby 2000 on the plant hybrid as a commodity in the 1890s.
[97] The dispute also involved the biometrician Walter Frank Raphael Weldon (1860-1906). For the most recent extended analysis and re-interpretation of this debate, see Radick 2021: 169-172.
[98] Bateson 1895: 605 citing Thiselton Dyer.
[99] Bateson 1895: 606 (citing Burbidge's Manual).
[100] Thiselton Dyer 1895: 128.
[101] Thiselton Dyer 1895: 129.

four, he also had certain expectations for what that experimentation might look like. However, there was also a more powerful underlying issue for Dyer. He preferred to see the garden cineraria originate from a single species as an example of gradual Darwinian evolution, rather than what many gardeners believed, that the cineraria had been produced by spontaneous hybridisation or artificial hybridising, or a mix of both. Horticulturalist Frederick Burbidge reminded that botanists like Dyer should remember Darwin's reliance on evidence from plant and animal breeders.[102] Ironically for everyone, Darwin himself had opted for a hybrid origin: Cinerarias were 'probably derived from several fructicose or herbaceous species, much intercrossed'.[103]

In taking a view more Darwinian than Darwin, Dyer may well have been persuaded by a *Gardeners' Chronicle* article from 1888 on these well-known showy plants. Masters had amended the piece slightly to convey a pro-Darwinian message that the array of garden forms were all produced by selection from a single wild type. Masters commissioned an accompanying woodcut of the garden forms to 'illustrate the survival of the fittest according to the ideas of the florist'.[104] Once the Cineraria controversy erupted in 1895, Masters backtracked, printing a fudged account excusing the earlier article as having been written 'without reference to hybridity'.[105] When asked privately by William Bateson, he stated that he thought that a hybrid origin was 'more likely to be correct than Dyer's notion that it is of pure descent.'[106] The edits to this 1888 article were also another example of Masters' rhetorical flourish deployed to emphasise the Darwinian nature of horticultural plant breeding without, as he later admitted, 'examining the evidence in support of it or otherwise.'[107] In a final twist for our story, the author of the 1888 article was Rolfe, who felt his own view had been mis-represented by Master's edits, and conveyed a private message to Bateson

[102] F. W. Burbidge to William Bateson, 10 June 1895, WB Archive (CUL Special Collections B.10).
[103] Darwin 1876: 335.
[104] Rolfe 1888: 657.
[105] Anon. [Masters, M.T.] 1895: 90.
[106] Maxwell T. Masters to William Bateson, 16 May 1895, WB Archive (CUL Special Collections B.10).
[107] Anon. [Masters, M.T.] 1895: 90.

explaining 'that he himself would be the last man to contradict the plausible hybrid origin, as he is always on the lookout for hybrid orchids.'[108]

Therefore, given Dyer's view of the prevalence (as he saw it) for the unjustified recognition of hybrids, unsurprisingly Rolfe faced opposition from Dyer to his work on describing orchids. These were, by the 1890s, mostly 'artificial' hybrids (as Rolfe called them in 1893) produced by nurserymen. Recording their origin and parentage, Rolfe believed, was important for its own sake, and because 'of the light it throws on the occurrence of hybrids in a wild state.'[109] Rolfe's activities were, in effect, partly regulatory: fake orchid hybrids were a significant concern in the 1890s. For example, aniline dyes were used to paint markings on to petals (a technique still used in commercial floristry today).[110] Rolfe appears to have got into trouble directly with Dyer more than once about spending too much time on hybrid orchids and allegedly neglecting his official duties. Dyer sent a stern memo to the Keeper of the Herbarium about 'naming orchids' especially 'garden hybrids' and Maxwell Masters intervened, directly defending Rolfe.[111] Dyer was apparently upset: Masters had to apologise for being 'rough upon you'; but then Dyer apparently gave in.[112] Masters and Rolfe heard (as we only have their side of the story) that describing these manufactured plants was inappropriate use of staff time because it was horticultural work, so unacceptable for a Kew Herbarium botanist. Rolfe had to apologise by admitting that 'I extremely regret that I have described <u>new species of Orchids</u> in the Orchid Review, which I should not have done…' [113] There was an implicit undercurrent of disapproval from Dyer stemming from Rolfe's association with commercial trade; and a sense that Rolfe was indignant, emphasising that he was describing new species, not hybrids.

Rolfe also clashed with Dyer over the scientific journal he founded, the *Orchid Review*. Dyer initially banned the journal in accordance with regulations impeding civil

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[108] Rolfe was concerned enough that his article may have misled Dyer to ask J.G. Baker for advice and to relay a message to Bateson (in a letter from Isaac Henry Burkill (1870-1965) to William Bateson, 17 December 1895, WB Archive (CUL Special Collections B.10).
[109] Rolfe 1893: 1.
[110] Anon. 1897b.
[111] Memorandum signed W.T.T.D., 28 March 1891, MTM Archive, folio 90. Maxwell T. Masters to W. Thiselton Dyer, 13 April 1891, MTM Archive, folio 91.
[112] Maxwell T. Masters to W. Thiselton Dyer, 4 August 1891, MTM Archive, folio 93.
[113] R.A. Rolfe to W. Thiselton Dyer, 5 October 1893, Kew Archive Director's Correspondence DC/100/f.224.

servants from publishing without prior permission; 'public material, specimens or information' must not be used for private work.[114] The prohibition came in irate red ink on an internal memo: 'Please tell Mr Rolfe I absolutely prohibit his engaging in any enterprise of the sort in any shape or form.'[115] Rolfe was quick to argue back that his periodical was conducted to counteract the control of the nursery trade on orchid taxonomy and nomenclature (and the influence of German hybrid mongers, given that Professor Reichenbach named new hybrids for Veitch and Co 'judging from appearances only'). [116] Rolfe explained:

> Journal is funded by "an independent gentleman" on condition it is "kept absolutely independent of trade domination" … The proposal has really arisen from a growing feeling that information with regards to orchids is too scattered, and the influence of the trade too dominant.[117]

After several suitably apologetic memos from Rolfe, Dyer allowed the *Review* to continue under anonymous editorship, on an understanding that Rolfe conducted the work outside of official hours.[118]

Later, when Brown was promoted to assistant keeper in 1909, Rolfe protested in a long letter to the new Director of Kew that he had been 'passed over' and set out his twenty-nine year career history.[119] He commented later to a friend, that since the early 1880s he had been 'burning the midnight oil' at 'my special work', and in producing monographs on orchid genera, mostly in his own time. 'I have almost created a new department by strenuous work on a very large, difficult and specially neglected group.'[120] He felt his life's work was unacknowledged. The Director of Kew's handwritten note at the foot of Rolfe's 1909 letter recorded that the situation could not be helped; a more senior, better paid role involved interaction with visitors to the Herbarium; Rolfe was simply unsuited to this aspect of the work as he was profoundly

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[114] Kew Internal Memorandum, 19 November 1891, Kew Archive Misc. Correspondence of Herbarium Staff (1879-93) folio 60.
[115] Memorandum W.T.T.D. to R.A. Rolfe, 17 December 1892, Kew Archive Director's Correspondence DC100/f.202.
[116] Masters 1900: 57.
[117] R.A. Rolfe to W. Thiselton Dyer, 18 December 1892, Kew Archive Misc. Correspondence of Herbarium Staff (1879-93) folio 62.
[118] R.A. Rolfe to W. Thiselton Dyer, 21 December 1892, Kew Archive Misc. Correspondence of Herbarium Staff (1879-93) folio 64.
[119] R.A. Rolfe to Col. D. Prain, 6 November 1909, Kew Archives Misc. Correspondence of Herbarium Staff (1893-1921) folio 66-7.
[120] R.A. Rolfe to Mr Hurst, 19 March 1901, CUL Special Collections CCH Archive (Add.7955/2/59).

deaf: 'I have seen Mr Rolfe today and explained to him that what he looks upon as "passing over" him is a circumstance that implies no reflection on his ability or industry but one that follows from the physical defect due to his deafness.' [121] In a final unfortunate twist, Rolfe died a few months before he was due to retire and travel for the first time overseas, to Central America, to see his beloved orchids in the wild.[122]

Writing a History

We have a little direct evidence of why Rolfe wrote the history he did. We do know that Maxwell Masters invited Rolfe to deliver at paper at the Hybridisation conference in around December 1898 or January 1899. Rolfe was also thinking about Masters' views of how hybridists might prove natural hybrids by experimental crossing, and in a January 1899 letter to a commercial orchid breeder, Charles Chamberlain Hurst (1870-1947) echoed Masters' phrase that plant hybrids were often relegated to the 'rubbish-heap'.[123] The closest account about the conference paper is in two letters from February 1899, from Rolfe to Hurst, and to Linton, the willow authority we met in chapter four.[124] To Hurst, Rolfe explained:

> I am preparing for the Hybridisation Conference a paper – 'Hybridisation viewed from the standpoint of systematic botany.' *Proved* natural hybrids, and views of sceptics, will, of course, come in, and perhaps I can point a moral and adorn a tale about some of those genera, as Salix, Epilobium etc., which are the despair of systematic botanists.[125]

Rolfe also explained to Linton that he was aiming:

> To collect a few of the conflicting opinions in respect of them [natural plant hybrids]. And in order to show that the question has passed beyond the stage of mere "pious opinion" I am anxious to collect evidence of any case where a supposed natural hybrid has also been <u>raised artificially</u>. I have several most interesting cases among orchids (which you know form rather a hobby of mine), but I think the willows will furnish several other beautiful examples.[126]

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[121] Memo signed D.P. 8 November 1909 on the foot of R.A. Rolfe to Col. D. Prain, 6 November 1909, Kew Archives Misc. Correspondence of Herbarium Staff (1893-1921) folio 67.
[122] Anon. 1921a.
[123] R.A. Rolfe to Mr Hurst, 17 January 1900, CUL Special Collections CCH Archive (Add.7955/2/30).
[124] For biographical details, see his wife's hagiography (Hurst 1949) and Cock and Forsdyke 2008, chapter ten.
[125] R.A. Rolfe to Mr Hurst, 31 January 1899, CUL Special Collections CCH Archive (Add.7955/1/53) cited in Hurst 1949:381.
[126] R.A. Rolfe to Mr Linton, 2 February 1899 (EFL Archive at NHM Miss. Corr. Box 2 of 2).

Clearly the 1891 spat over willowherb hybrids was also on his mind, along with Brown's scepticism and his dismissal of Rolfe's early classification attempts (the two men still did not get along after nearly twenty years) as he 'adorned a tale' about the relations between botanists and hybridists. Emulating Masters, he believed that horticultural hybridising might be conducted scientifically, by accurate observations and record keeping, but also might generate scientific knowledge. Taxonomy, in respect of the identification of plant hybrids, should no longer rely on the opinions of the taxonomist, and he wanted examples of how hybridising had resolved conflicting opinions about the identity of a specimen. Later in his actual paper at the Conference, Rolfe explained the barriers, as perceived then, to this project. He stressed that botanists had not believed 'hybridists', and the breeders did not take much notice of the botanists; there was a 'want of sympathy' between these communities.[127] He was experiencing these challenges directly within his orchid work, as a botanist trying to work with plant breeders.

As Rolfe began drafting his history of hybridising in February 1899, he was also thinking about a recent dispute with a prominent orchid breeder over hybrid orchids among the species of *Odontoglossum* Kunth.[128] Rolfe felt that 'It was the sneering way he spoke of experts &c & doing without them that I did not like, and do not, for it pointed to me so very explicitly & I have scarcely earned that.'[129] He explained to Hurst that he launched the *Orchid Review* to create communication between breeders and botanists, specifically to facilitate and centralise orchid hybrid classification, and to generate experimental facts.[130] His journal, Rolfe argued, established an orchidology community, so that there was no need for any new Society: 'It would be possible to ask if an Orchid Society would do more than I have done'. [131] Therefore, while Rolfe believed that science, in the form of his journal, might unify practitioners, he was frustrated by hostility from some orchid breeders.

[127] Rolfe 1900:182.
[128] R.A. Rolfe to Mr Hurst, 11 February 1899, CUL Special Collections CCH Archive (Add.7955/1/54).
[129] R.A. Rolfe to Mr Hurst, 19 February 1899, CUL Special Collections CCH Archive (Add.7955/1/55).
[130] R.A. Rolfe to Mr Hurst, 19 February 1899, CUL Special Collections CCH Archive (Add.7955/1/55).
[131] R.A. Rolfe to Mr Hurst, 19 February 1899, CUL Special Collections CCH Archive (Add.7955/1/55).

Rolfe also conducted his own hybridising experiments to 'prove' natural hybrids in an orchid genus.[132] His forthcoming *Orchid Stud Book* (1909), co-authored with Hurst, would have 'natural hybrids to be omitted unless proved'.[133] His overall thinking around the time that he composed his conference talk in early 1899 is reflected by his later summation of his experimental hybridising work. In 1908, at the anniversary celebration of the famous reading of the Darwin and Wallace papers on the origin of species, at the Linnean Society (of which he was still only an Associate Fellow), Rolfe displayed his re-made hybrid Ontoglossums. He stressed that he was only able to display a complete history of this genus thanks to the popularity of the group as garden plants and concluded about hybridisation:

> The subject is worthy of increased attention, for crossing increases variability, and variation is the material on which natural selection works. Many hybrids are completely fertile, and spontaneous hybrids often possess such distinctive features as to have been described as new species or as varieties of one or the other parent. Their permanence is a matter for further study and experiment. Thus hybridisation is a question of great biological importance, and one to be taken into consideration in discussing the very origin of species, indeed it is probably of more importance than has been yet realised.[134]

While Rolfe's history supported Masters' version of events, and cited Masters' account, he was not the 'unassuming' victim of events that his only biographer portrays.[135] This style of unsung hero biography denies Rolfe's agency and is not explanatory. Rolfe spoke out when he felt strongly about an issue, and determinedly sought to create a community of orchidologists with himself embedded as *the* authority on orchid taxonomy. His historiography reflected Masters' views but also his own frustrations at Kew. While it is anachronistic and therefore misleading to portray Rolfe as experiencing disability discrimination as we understand this today, it seems likely that Rolfe's frustrations caused by how others responded to his deafness, the opposition he felt he faced in studying orchid hybrids, and his lack of promotion, all affected his history writing.

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[132] R.A. Rolfe to Mr Hurst, 10 July 1902, CUL Special Collections CCH Archive (Add.7955/2/66).
[133] R.A. Rolfe to Mr Hurst, 14 March 1901, CUL Special Collections CCH Archive (Add.7955/2/58).
[134] Rolfe 1908: 72.
[135] Pridgeon 1993: 46. Rolfe 1900: 197 quotes Anon. [Masters, M.T.] 1880 and 1880a on impiety and Cape heath hybrids.

V. Conclusion

Maxwell Masters, the son of a nurseryman hybridist, saw hybridising as a technological improvement that also functioned as a tool to produce knowledge of importance for science; and the scientific content of this maker's knowledge was no less important than its utility in improving flowers, fruit and other crops. Masters believed that hybridising had been held back by ill-informed prejudice, whether religious, or from a broader need among botanists to comply with established systems. Further, underlying these sentiments, connecting horticulture with science might neutralise the persistent, socially uncomfortable connection between hybridising and trade. Rolfe followed the views of his senior colleague and elaborated on Masters' version of opposition to hybridity. Both men produced histories which reflected their contemporary frustrations.

Therefore, we can understand why Masters and Rolfe chose to emphasise opposition to plant hybrids and hybridising. Yet as Rolfe himself mentioned (but then glossed over), there were diverse views about plant hybridity. This evidence from Rolfe supports a core aim of this thesis, to show there was no generalised opposition to plant hybrids or hybridising. This thesis has revealed a more nuanced story than we might expect if we took the Masters-Rolfe historiography at face value. In addition to allowing for the personal biases and hagiographic elements of their historiography, Masters and Rolfe's account was also an emblem of the wider culture in which it was produced.

The immediate responses to the conference from newspaper reporters show that the story of prejudice against hybrids and hybridising was attuned with wider narratives over an inevitable conflict between religion and science. Masters' view originated with his interpretation of a single report from a nurseryman. It turns out that specific story was about maintaining the commercial value for plant stock in the eyes of customers, who saw manmade hybrids as inferior to rare, imported species. Another, less obvious and underlying reason, may have been that Masters was reacting against a trend in contemporary horticultural aesthetics, which praised the natural, divinely-created form over the artificial hybrid. This is not to claim that no one objected to hybridising on religious grounds, or to deny that botanists sometimes did 'reject or ignore' hybrids; instead, the claim is that both Masters and Rolfe over-emphasised a conflict narrative due to their personal perspectives and the culture in which they were writing.

The Masters-Rolfe history, its challenges for the historian notwithstanding, is important for three disparate reasons:

Firstly, this history spoke of contemporary aspirations for horticulture, and for botanical communities more generally. Masters and Rolfe together produced an inaugural history for an imagined new collaborative evolutionary science between cultivators, local botanists and biologists, utilising hybridising as experimentation. The publication in 1900 of White's Hybrid Orchid in Kew's premier taxonomic journal launched this ambition (the article was written by Rolfe, undeterred by the announcement a few months earlier of Mendel's paper, which he found unconvincing).[136] This thesis demonstrates that hybridising as experimentation, as a tool within botanical science, was not a new idea in 1899, and as we saw in chapters three and four, it remained contested, including by some of the Kew botanists whom Masters and Rolfe worked alongside who did not share their horticultural perspective. Creating a unified history was one way in which the different botanical communities involved in hybridity might come together.

Secondly, Masters' and Rolfe's version of events was eminently successful at establishing a new history. Within horticultural communities, the *Garden* reflected on the 1899 conference as 'a notable event, not merely of the past year, but of the entire century' as it demonstrated 'hybridisation' (meaning artificial hybridising) as 'the most splendid chapter' in the history of nineteenth century horticulture.[137] That history was in part successful because it aligned with a wider discourse about religion versus science, so contemporary audiences seized on Masters' message, that horticulture had been unjustly held back by religious belief and deserved recognition for its scientific work. And perhaps most significantly, this history, as we saw in the introduction to this

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[136] Rolfe planned to 'test a few Mendelian ideas' by hybridising but was highly sceptical (R.A. Rolfe to Mr Hurst, 10 July 1902, CUL Special Collections CCH Archive (Add.7955/2/66).
[137] Anon. 1899: 21 and 94.

thesis, then repeatedly shaped subsequent portrayals throughout the twentieth century of Victorian attitudes to plant hybridity.

Finally, the future biology that Masters and Rolfe envisaged was evolutionary in nature, but focused around species formation and experimental taxonomy, as much as heredity. This supports what we found in chapter four: The plant hybridising context of the 1890s might just as easily have produced a broader research programme into the role of hybridisation in evolution, as leading to the practice of Mendelian crossing and the new science of genetics. Therefore, this chapter produces a follow-on need to revise our interpretation of the 1899 Hybridization Conference. The 1899 conference is best understood as part of the story that this thesis tells, rather than interpreted as the prelude for a series of conferences on what became known as genetics. This conclusion, it turns out, had been drawn by historian Robert Olby, but was then subsumed in his later, broader thesis about horticultural hybridising as pivotal in the emergence of experimental and statistical practices within Mendelism. [138]

We will consider other major revisions that this thesis has prompted for our histories of plant breeding, natural history, science and religion, and scientific communities in the conclusion to this thesis.

[138] Olby 1985: 124-6 which he then moderates in Olby 2000: 1047.

Conclusion
Plant Hybridising Within Victorian Science

> Side by side with a love of gardens in the English tradition is a love of 'wild nature' outside the enclosed garden. How far are these two passions linked and how far are they opposed? [1]

Max Walters (1920-2005),
Director of Cambridge Botanic Garden

I. Introduction

By way of preview of these final reflections, consider the view of Max Walters in the quotation above. Walters asked this question at the end of the twentieth century, at a time when many cultivated plants were seen as rogue invaders of the countryside, 'aliens' out-competing—or even hybridising out—native species. A high-profile victim of this perceived hybridisation threat was the iconic British wild flower, the bluebell (*Hyacinthoides non-scripta* (L.) Chouard ex Rothm.) apparently facing extinction by hybridisation with an invasive non-native or 'alien' garden plant, the Spanish bluebell *H. hispanica* (Mill.) Rothm.[2] Walters wished to re-orientate this polarised debate and stress the common ground between wild and cultivated plants.

This thesis has shown that, in line with Walter's position, gardening and farming on the one hand, and the study of wild plants on the other, are historically more linked than we might have imagined. This thesis has found that hybridising was a shared, yet frequently contested, practice at the intersections of gardening, farming, plant breeding and natural history, and which contributed to knowledge-making in the sciences of taxonomy and physiology. A theme throughout this thesis is that Victorian plant knowledge communities were intimately entwined. Practitioners hybridising made and re-made plant hybrids to understand their taxonomic relationships and physiological functioning, as well as for the more obvious ends of producing novel forms for profit or pleasure. Therefore, hybridising as artisanal making-as-knowing is

[1] Walters 1993: 11.
[2] Kohn *et al*. 2019; Ruhsam 2020.

revealed as more important for understanding Victorian science than has been previously acknowledged. A provocative question remains, then, which we will consider at the end of this concluding chapter: should hybridising be admitted as a key part of 'natural history', today?

This thesis set out to provide an answer to two fundamentally linked questions about Victorian botany: To whom did the study of plant hybridity and the practice of plant hybridising matter? And what motives underlay those committed to plant hybridity and what motivated their opponents? Sections II and III firstly answer these questions in light of the episodes presented in the previous chapters, while considering what changed between 1837 and 1899, some of the limitations of this thesis, and areas for further research. Section IV explores what wider lessons the thesis has for historians of science and the benefits of examining history through the lens of scientific communities, including those communities focused on particular plant groups. The thesis ends with reflections on what this new history of Victorian plant knowledge communities means for contemporary debates over the place of cultivation practices in today's biological sciences.

II. To Whom Did the Study of Plant Hybridity and the Practice of Hybridising Matter?

This thesis claims that diverse interconnected botanical communities used the practice of hybridising to inform the sciences of taxonomy and physiology. Plant hybridising was far more than a plant breeding practice. In making this claim, the thesis provides an important corrective to three strands within the current historiography of the practices of Victorian botany.

First, it shows that the cultivator's practice of hybridising was also a natural history practice. Despite the views of historians to the contrary, in chapter one we saw how in the 1830s philosophical botanists, local botanists and cultivators all practised hybridising. These three botanical communities held overlapping yet different views of hybridity. Philosophical practitioners used hybridising as a tool to experimentally reduce the number of named species. Local botanists' observations of intermediate plant forms might be interpreted as hybrids, or as evidence of the transition of one species into another. Cultivators were absolutely convinced that hybridising created new permanent forms, even new species. Due to the specific taxonomic and physiological questions that the crossing of putative species raised, cross-breeding was not relevant to these investigations, and it was hybridising that specifically contributed knowledge-making to science. While the existence of natural hybridisation was contested, no botanical community held the generalised opposition to plant hybrids or hybridising commonly supposed by contemporary historians.

Secondly, hybridity mattered to philosophical and local botanists and cultivators as part of the practices of a new Darwinian biology emerging during the 1860s. As Darwin's reputation grew, so did the social respectability of hybridising as a form of experimentation. The theory of evolution by natural selection drew attention to hybridity as an *idea*; but it also encouraged the *practice* of physiological botanical studies, including hybridising. Several events coalesced during the 1860s raising the profile of the practice of plant hybridising within an emerging new Darwinian science. That hybridising mattered in the practices of a new Darwinian biology explains shifts in Victorian understanding of plants better than any focus on Darwinism or species concepts. The practices of the new Darwinian biology and its focus on hybrids enabled local botanists to become taxonomic authorities, developing their 'special knowledge' of observing the plant hybrid in nature, using hybridising as a tool to study plant classification, and producing taxonomic monographs.

Thirdly, the study presents a continuity thesis of the importance of plant hybridising in science between 1837 and 1899. While plant propagation techniques developed in some plant groups, most notably in orchids, hybridising as a practice distinct from other elements of plant breeding remained little changed. Taken collectively, the thesis chapters demonstrate that, during the 1830s, plant hybridising emerged as a tool used to investigate plant taxonomy and physiology, and by the 1880s the plant hybrid became an object of study of academic botanists working in taxonomy and, later, the new field of cytology. Contrary to a view held by some historians of plant breeding, hybridising was not especially reinvigorated or newly incorporated into science during the 1890s or around the re-discovery moment of Mendelism in 1900.

In answer to the first thesis question, then, during the Victorian period in Britain, multiple intersecting botanical communities interested in plant hybridity interacted around botanical and horticultural societies, periodicals and floras. The practice of plant hybridising mattered to all of them in different, yet sometimes overlapping, ways. These plant knowledge-making community interactions were a mix of conflicts, corroboration by philosophical or academic practitioners of cultivators' and local botanists' findings, and some collaboration between communities. These collaborations took place more readily between horticulturalists and academic communities, than between local botanists and academic practitioners, where in fact there remained considerable conflict over plant hybridity (reflected in chapter five by Robert Alan Rolfe's lived experiences at Kew herbarium). This suggests that horticulture did provide an unusual space for collaborative knowledge-making, as Helen Curry proposes for the 1940s, and this thesis suggests for the last quarter of the nineteenth century, but not because, as Robert Olby claims, horticulturalists were the only practitioners hybridising.

III. What Motives Underlay those Committed to Plant Hybridity and What Motivated their Opponents?

We have seen how Victorian plant knowledge communities were intimately entwined and hybridising was a tool used by practitioners to understand plant taxonomic relationships and physiological functioning, as well as for the more obvious ends of producing novel forms for profit or pleasure. We have also seen how this diversity produced debate, as different botanical communities held divergent views of whether their hybridising practice confirmed the existence of the plant hybrid in nature.

The thesis offers a new account of why plant hybridity was contested in Victorian Britain. The thesis argues that the diversity and debate around hybridity was more a product of the varied cultural contexts of knowledge-making in nineteenth-century British science than any conceptual barriers derived from religious beliefs. These concluding reflections are set out in four parts: First, we discuss the motives of those committed to plant hybridity; secondly, how the thesis problematises religious belief as *the* explanation for opposition to the plant hybrid in nature and to the practice of hybridising; thirdly, we explore how the thesis reveals other cultural contexts accounting for diverse attitudes to plant hybridity; and finally, we discuss the origin of the view that religious opposition best explains Victorian attitudes to plant hybridity.

Motives of Practitioners Committed to Plant Hybridity

The thesis shows that those committed to plant hybridity were overwhelmingly motivated by their experiential knowledge derived from their own observations: of putative hybrid plants in wild nature, or from their own hybridising practice, or both of these. Chapter one shows this was the case in respect of philosophical practitioners such as John Henslow, John Lindley and horticulturalist William Herbert, as well as local botanists, gardeners and farmers. Botanical communities interpreted an intermediate form differently according to what was at stake in calling it a hybrid or not. In chapter two, hybrids were associated with unphilosophical ‘rustic’ beliefs and a philosophical practitioner might therefore remain cautious about the hypothesis of hybridisation. Philosophical botanists attempted to discipline the reports of plant hybrids by calling for hybridising to be conducted as an experimental test of the knowledge-claims made. Even after hybridising was formally acknowledged as knowledge-making at the International Botanical Conference in 1867, by the 1880s, hybridists’ knowledge claims were still subjected to scrutiny by philosophical practitioners, who regulated the circumstances under which hybridising might be acknowledged as experimentation. Overall, the thesis demonstrates that the on-going social tension, between gardening, making-as-knowing, and the science of botany, lay at the centre of the complexity of attitudes toward plant hybridity.

Motives of Opponents to the Plant Hybrid and Hybridising

Historians frequently claim that some Victorian botanists rejected plant hybrids, and that there was wider opposition to plant hybridising in society, in both instances attributing this hostility to religious beliefs. The thesis reveals this view as naïve and over-simplistic. In chapter two we saw how far from opposing hybrids, some Christian practitioners embraced hybridisation, preferring this explanation of botanists' observations to the more radical alternative, the transmutation of species. However, this is not to say that religious motives were irrelevant to the study of plant hybridity, but that their relevance should be explored in more nuanced ways. For example, chapter one suggests that an underlying reason for practitioners to reject hybrids was a religiously based commitment to ending slavery, especially characteristic of members of the Unitarian Church. Historians of science hold that abolitionist politics lay behind debates over domestic animal breeding in 1830-50s Britain and America, and the thesis affirms this link too in respect of plant breeding. Further research is needed to explore what underpinned practitioners' taxonomic treatment of plant hybrids. One possible motive that the thesis has revealed is a Christian-inspired horticultural aesthetics, which elevated the natural, divinely created form over the artificial hybrid. The plant hybrid, as a readily available commodity, was also less socially respectable than a rare wild collected species, a point which emerges in the thesis in chapter five.

The religion versus science account of plant hybridity in Victorian Britain is a convenient history, and such histories often obscure more nuanced, inconvenient ones – in this case, about snobbish collectors; and, potentially, in other stories yet to be told, about human hybrids and the history of scientific racism. Whether hybridity was seen as aesthetically and biologically positive (producing hybrid vigour) or detrimental (producing ugly, diseased or degenerate forms) may also have impacted attitudes to miscegenation, which historians have recently begun exploring. For example, Warwick Anderson, in a postcolonial history of science study, shows that his actor's anthropological investigation of the human 'hybrid' population on Pitcairn Island led him to oppose scientific racism during the 1930s.[3] This human race and science context is of interest for further research, especially for historians of late Victorian horticulture and social Darwinism, and early twentieth-century agriculture and eugenics.[4]

[3] Anderson 2012.
[4] Recently explored in Curry 2021.

Other Cultural Contexts of Plant Hybridity

This thesis has exposed an ideological factor which also played a role in the diverse attitudes towards plant hybridity, the epistemological morals expected of a philosophical practitioner. We saw in chapter three how philosophical practitioners held differing views of hybridity that cannot be adequately explained by their religious belief, species concepts or their taxonomic approach to lumping or splitting. Their opposition related more generally to the epistemological morals expected of a philosophical practitioner. Plant hybridisation in nature was too speculative to be acknowledged within philosophical science. A deep-seated concern about the politically dangerous connotations of speculation drove philosophical botanists to hesitate over hybrids, especially when combined with hybridising results from 'unreliable observers', like lower class gardeners, or middle-class industrialists seen as dabbling in botany to enhance their civic status. The distrust of the lower class or hobby hybridist became less significant after Darwin's botanical experimental practice established a new way of practising botany. Therefore, one of the effects of the new Darwinian biology was to elevate the social respectability of hybridising practice.

In sum, the thesis therefore exposes several wider cultural contexts of Victorian science explaining attitudes to plant hybridity, including abolitionist politics, horticultural aesthetics, and the epistemological morals expected of a philosophical practitioner. Further studies might uncover more such cultural contexts, and enable some prioritisation between these factors.

Origin of the Conflict Historiography

Finally, the claim in the thesis that religious motives fall short of explaining the range of Victorian attitudes to plant hybridity raises the question, whence did this persuasive idea originate? Chapter five argues that the often-repeated conflict thesis, of botanists' opposition to plant hybridity, arose from the hagiography of hybridity launched at the 1899 Royal Horticultural Society's International Conference on Hybridization. The two papers delivered at that conference on the history of hybridising drew on several primary sources, of which one, from 1880, suggested that a nurseryman has passed off his Cape Heath hybrids as wild collected species, for fear that the hybrid productions would be rejected a 'sacrilegious interference with the laws of the Creator'. In fact, that story turns out to have been of a nurseryman maintaining the commercial value of his hybrid plants, by concealing their true origin as manmade hybrids. These biased interpretations in the Masters-Rolfe history have been uncritically followed in some high-profile twenty-first century histories of horticulture and natural history. The thesis shows that we have been too quick to see religion as *the* explanandum accounting for attitudes to plant hybridity, when the diversity of Victorian views on hybrids necessitates a pluralistic explanatory history. This reflection leads us to consider in the next section the question: what take-away lessons does the thesis offer historians of science?

IV. Insights for Historians of Science

This thesis has answered questions about the history of attitudes to the practice of plant hybridising and to the plant hybrid in nature. We have also seen how the received view in the historiography, that religious belief explains Victorian attitudes to plant hybridity, is problematised, and the thesis reveals a range of wider cultural factors which played a role in the diverse attitudes towards plant hybridity. The episodes examined in this thesis offer historians of science three new insights regarding historiographical approaches to scientific communities, making-as-knowing, and plant breeding and Mendelism.

Intersectional Spaces Between Scientific Communities

The first insight for historians is to look to the *intersections* of scientific communities as a fruitful approach to the historiography of science. One example from chapter four shows how the intersection of scientific communities has explanatory power for writing the history of science. Edward Lowe and Charles Druery studied the same subject—fern physiology—yet were treated differently because they engaged with different communities. Druery collaborated beyond his own community, whereas Lowe, in the 1880s, did not, and relied on Royal Society patronage to get his work acknowledged. After 1890, Druery's careful collaborations enabled the fernists to create a scientific identity for themselves within Victorian science.

A challenge, then, is how a historian might identify such intersectional spaces. The thesis seeks out spaces where philosophical, local and cultivator practitioners took part in knowledge production via the study of particular plant groups, such as oxlips, willows, or ferns. An object-focused approach to the historiography of science is well-developed, but in the history of biology, it is less appreciated how the object of study *within* natural history categories such as 'botany' might produce communities of diverse practitioners who otherwise might not come into contact. One example is provided in chapter two, where the subscriber communities of two quite different periodicals intersected around a debate over oxlips. A focus on the object-as-subject also produced some novel insights: a focus on the mock oxlip, instead of on Charles Darwin, produces a hidden history of the relation between hybridisation and transmutation.

While there are these advantages, the plant-focused chapters in this thesis captured some aspects of the history of attitudes to plant hybridity, but also sacrificed a more complete history. The thesis has no grand narrative to offer a complete explanation of what Victorian botanists believed about plant hybrids; instead, it presents a series of episodes illustrating the debate and complexity that the topic provoked. Recently, historians of science have addressed criticism of such complexity theses as abandoning too far any sense of coherent narrative direction.[5] In particular, the promotion of a complexity principle in science and religion historiography has led some historians to suggest that 'mere complexity' is to some extent a trivial truism, and that we should therefore still seek meta-narratives and, further, focus on the critique of simplistic histories.[6]

In response, therefore, despite having no master narrative, this thesis not only debunks the simplistic story about plant hybridity and religion, but also draws some meta-level conclusions. In section II above, we have seen a continuity thesis emerge about plant hybridising as a scientific practice in Victorian Britain; further, from

[5] Guldi and Armitage 2014. Historians of science respond in a 2016 volume of *Isis.*

[6] Lightman 2019, discussing Harrison 2019 (defending master-narratives), and other papers in the same volume reviewing the complexity principle in science-religion historiography.

comparing the thesis' chronological breadth of episodes from the 1840s, 1860s and 1880s respectively, we have highlighted the shifting status of garden experimentation within science. The transfer of hybridising between botanical communities eroded earlier attempts to demark socially the science of 'botany' from horticulture and plant breeding. In addition, a complexity thesis allows us to do counterfactual history to imagine other futures for biology at 1899, explored in chapters four and five. These fruitful debates revealed in the thesis suggest that there is opportunity to expand the thesis' questions into new contexts in time and place, especially to compare the British with the American experience, and to consider the interactions between plant knowledge communities elsewhere, such as in Middle Eastern and Indo-East Asian contexts with a long history of plant breeding activities.

Making-as-knowing and Experimentation

The second insight relates to some historiographical reconsiderations of natural history practices, given that we are claiming that these practices in the Victorian period included hybridising. Hybridising has been claimed, at certain times and in some places, as experimentation; in particular, that this 'experiment' was a form of artisanal maker's knowledge integral to scientific knowledge-making.

The thesis does not claim that plant hybridising as an instrument is evidence of natural history practice being experimental, or that hybridising became experimentation at a certain time, as these two observations fluctuate with context: hybridising was experimentation for Henslow in the 1830s (if conducted by a philosophical practitioner or *en masse* by his *cohors botanicorum*), but not for Thiselton Dyer in 1880s. This thesis therefore does not make any claims about experimentation, other than to support historians' understanding that the use of the term 'experiment' from the 1820s onwards was often a rhetorical appeal to be 'scientific'. In chapter four, the fernists dealt with uncertainty in their methodology by attempting to present their hybridising practice as an experiment. After heavy criticism, and considerable effort at replication, their fern hybrid was acknowledged by publication in the *Annals of Botany*. Yet in that paper, the fernists openly paraded their actual motive, to breed collectable hybrids, rather than to investigate fern physiology.

To use Helen Curry's term, these 'tinkering technologists' might not appear relevant to academic science, and their claims were contested, yet their observations ensured that they and their plants had a place in late-century physiology and cytology, which led to some unexpected outcomes, including a development in the study of cancer.

This story of hybridising in Victorian Britain is certainly not one of a biological-technological innovation producing new developments; instead, it is of a practice used and re-imagined in different knowledge-making cultural contexts. We also saw how hybridising provoked conflict over the oxlip and over the fern hybrid, rather than resolved it. Hybridising therefore provides an example for historians pointing to another way in which plant breeding might be, as Jonathan Harwood puts it, 'a locus of conflict'.[7] Overall, these Victorian plant knowledge communities were driven as much as by communication and debate, as by consensus, a conclusion which this thesis shows applied as much in 1837 as in 1899.

Another insight is that the thesis shows how a recent innovation in the historiography of early modern science is relevant to Victorian botany. This is the idea that craft knowledge-makers possess a discrete yet transferable mode of scientific knowledge-making, sometimes referred to as 'artisanal epistemology'. An artisanal practice did not simply support botanical studies; hybridising was an integral part of decision-making about plant classification within taxonomy throughout the period of this thesis. In chapters three and four the thesis sharpens the role of little-known practitioners, who, by the late century were participating as scientific authorities via their experiential knowledge-making, in plant taxonomy (chapter three) and physiology (chapter four). A significant gap in some chapters remains of working class or women's voices, despite the use as source material of periodicals read by these actors. For now, we can agree that this thesis is not a comprehensive account of plant hybridity in Victorian Britain, although it does point us towards a far more complex and sophisticated history than we might otherwise possess.

[7] Harwood 2015: 328.

Through a Mendelian Glass, Darkly

Our third, and final, insight addresses historians of biology, plant breeding and Mendelism. We saw in chapter three how local botanists connected the study of plant hybridity to their own version of Darwinism. In chapter four, academic botanists investigated plant hybridity within evolutionary biology, whereas connections with the study of heredity, while acknowledged, were side-lined. The plant hybridising context of the 1880s and 1890s included a widespread interest in the role of hybridisation in evolution, among local botanists, plant breeders, horticulturalists, and university biologists. Therefore, the plant hybridising context of the 1890s might just as easily have produced a broader research programme into the role of hybridisation in evolution, as leading to the practice of Mendelian crossing and the new science of genetics. This counterfactual suggests that the 1890s 'era of the hybrid' (to use historian Robert Olby's phrase) was important, but not for the reasons we assume, which connect the practice of hybridising in a straight line to genetics. Instead, this thesis is supported by historian Gregory Radick's forthcoming *Disputed Inheritance* (2021), in which he argues that the emergence of the science of genetics in the form that, eventually, took place, was by no means inevitable. In a way, Mendelism merely refocused everyone—as a breath of fresh air—and instead of obsessing over an intractable problem about hybrids and species formation, biologists were reorientated towards a new area of research. Unfortunately, looking back from later vantage points, historians using a Mendelian-tinted lens have obscured the diverse nature of interests in plant hybridity. Hybrid evolution was another path that biology might have taken after 1899.

Finally, we reflect on how this thesis might speak to botanical practitioners and wider society today. Looking back to the Victorian practice of hybridising and cultivating within plant taxonomy and physiology prompts us to reconsider how we define natural history in the twenty-first century.

V. Insights for Scientists and Society Today

We have seen how the thesis reveals plant hybridising as a natural history practice throughout the Victorian period, contributing a craft practice of making-as-knowing to the sciences of taxonomy and physiology. Paying close attention to the diverse groups interested in plant hybridity brings their differing responses into sharper focus, notwithstanding their shared practice of hybridising. In doing so, we have seen how labelling hybridising as 'experiment' was a knowledge-making claim, made by, for example, gardeners in the 1840s and by fernists in the 1880s, but nonetheless hybridising amounted to experimentation in some contexts but not in others. This leads us to reflect on other ways in which science demarcation impacts on how hybridising is seen today: whether a gardening practice can be a part of science?

In 2018, prompted by the outcry over the Oxford Junior Dictionary excluding words like 'fern', 'willow' and 'primrose' from its revised edition, the Green Party began campaigning for a General Certificate in Secondary Education (GCSE) in Natural History.[8] The campaign revealed that many people are unsure as to what exactly 'natural history' is, suggesting that the syllabus should include global climate change, environmental politics, ecosystems, and autecology; few respondents to the consultation included plant and animal taxonomy. The proposed definition from the exam board OCR is:

> Natural History focuses on understanding the rich and diverse natural world. Through observational study (generating systematic records of direct and indirect observations, often made over long periods of time) and investigation, natural history seeks to understand the diversity, complexities, and interconnectedness, of life on earth in contrasting habitats. Natural history explores how our natural world has been shaped, and how it continues to change, both by natural processes and through human intervention.[9]

As some respondents commented, this definition presents natural history as the observational elements of the science of ecology.[10] A deep issue seems to be a reluctance to accept that taxonomy is the fundamental science of natural history on its

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[8] Anon. 2018a. The GCSE is the general school-leaving examination sat by young people at 16 years of age, who may then either start employment, or further or higher education. At present, students can study either biology, physics, or chemistry or general sciences. Plant and animal taxonomy is not taught, nor is ecology or environmental science, other than as minor topics within biology or geography.
[9] OCR 2021.
[10] As does Wilkinson 2021.

own terms, without needing any connection to wider environmental topics. Further, there was no suggestion in the consultation process that cultivating, or curating in a botanic garden or seed bank, might be an element of natural history practice. ‘Natural history’ is apparently more ‘educational’ as a form of environmental science, ecology or politics. Some botanists suggest this reflects educationalists’ unease with ‘observing’ and ‘identifying’, and breeding practices like hybridising as ‘making’, which are seen as low-grade cognitive skills.[11] By contrast, desirable skills are ‘analysing’ or ‘evaluating’, which involve recognising over-arching patterns, relations or systems, as in the central questions of ecology. There remains an unspoken worry that natural history is not really knowledge-making, and certainly not ‘science’.

Plant breeding, like natural history, faces a similar science demarcation issue. Historians continue to debate the claim that the ‘art’ of breeding at some point—either at 1900 with the advent of Mendelism, or during the twentieth century—evolved into a ‘science’.[12] This shift is widely attributed to genetics providing a new degree of precision in plant breeding or as the practitioner or site of practice moving away from the ‘amateur’s’ farm or garden and into the ‘expert’ molecular biology laboratory, research institution or industrial complex.[13] An assumption is that plant breeding must be technoscience, and gardening is not, even if we remain unsure about when or how this distinction evolved. Yet Victorian plant hybridising co-existed as both technological ‘art’ and contributed to taxonomic and physiological ‘science’, which suggests that such parallel projects might function simultaneously today. Therefore, we can answer our question at the outset of this concluding reflection ‘should cultivating be a positive part of natural history?’ in the affirmative: growing plants is a fundamental element of knowing about plants. An increased sensitivity to the range of practices and knowledge embodied within ‘botany’ and ‘natural history’ may result in a redefinition and expansion of our contemporary category of ‘science’.

A final message from this thesis relates to its title. This deliberately echoes the 1928 book *Plant Hybridization Before Mendel* which remains a widely used reference for botanists writing history. This thesis’s ambition was to revise that account with a

[11] Warren, Ashton and Townsend 2015.
[12] For a concise review, see Holmes 2017.
[13] Fitzgerald 1993: 342 cited by Holmes 2017: 217.

reappraisal of Victorian botany as knowledge-making about plants. Both scientists and historians of science alike have assumed that, before 1900, hybridising was of interest only to horticulturalists and agriculturalists, who were then instrumental in bringing this practice to the attention of academic science during the closing decades of the nineteenth century. However, in seeking to explain the advent of Mendelism, we have obscured the fact that the practice of plant hybridising informed much more than plant breeder know-how about inheritance or the wider cultures of heredity. The plant hybrid and the practice of hybridising were of interest to diverse overlapping plant knowledge communities contributing to the sciences of taxonomy and physiology. The well-worn one-dimensional conflict thesis over plant hybridity in Victorian Britain is replaced with a picture of diversity and debate.

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