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Private Practices and Public Knowledge: Science, Professionalization and Gender in the Late Nineteenth Century

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<1>The term "scientist" was rarely used in the nineteenth century, its practitioners preferring phrases such as "man of science" that better expressed subordination to scientific practice. (Ross, 65-85; Barton, 80) Like the learned professions, men of science were entrusted with the production and possession of knowledge on behalf of the wider community and the label "man of science" signalled the sacrifice that justified the retention of this knowledge as private property. To be a man of science was to occupy the privileged position of speaking on behalf of the natural world, but the attribution of such a position depended upon the disinterestedness of those who held it. As David Vincent argues, guilds and trade societies were "stigmatized because their knowledge was uncodified, untestable, and protected for sectional gain rather than public benefit" whereas "the closures characteristic of the professions were held to be necessary for the maintenance of progressive standards of service from which everybody would ultimately gain." (23) The term "man of science" expressed the disinterestedness that validated scientific discoveries for the scientific community and affirmed their right to possess such knowledge on behalf of nineteenth-century society. Although inclusive in terms of social position – it expressed an identification with a discipline rather than a formal system of training and qualification – it was a term that explicitly offered masculinity as a prerequisite for the disinterested pursuit of knowledge.

<2>There were, of course, many women who engaged in scientific practice and whose achievements, with a degree of linguistic circumlocution, were fully recognized as contributions to science. However, although the "man" in man of science represented the abnegation of subjectivity in the pursuit of truth, it continued to signify masculinity. Ruth Barton writes:

> As labels, "men of science" and "scientific men" paralleled "men of letters" and "medical men", but the associated rhetoric seems richer in its implied values. Religious, military, imperial, and anti-aristocratic ideals were appealed to when men of science described themselves as patient workers, fearless explorers, conquerors of the unknown, disciplined soldiers, and single-minded searchers after truth. (90)

It is one part of this further set of associations that I explore below. As agents of revelation, men of science characterized themselves as standing on the brink of the unknown, revealing nature's secrets to one another on behalf of the broader nineteenth-century public. The masculine language in which such research was expressed was at odds with the practice of carrying it out:

whereas the search for truth was characterized as a noble struggle between the masculine ego and a mysterious feminine nature, the production of scientific knowledge required that the man of science temper his interventions to permit nature to act independently. This was particularly the case for textual representations of scientific practice as these accounts had to set out methodology in order to convince readers, both at the time of publication and for an indefinite period in the future, of the reality and autonomy of the natural phenomena they revealed. The moment of revelation, when the man of science was empowered to speak on behalf of nature, was crucial for the production of scientific knowledge, the cohesion of science as a profession, and the construction of the man of science as masculine hero. By examining the role of the gendered narrative subject in accounts of scientific revelation across different periodical genres, I explore how such moments operate to mark the possession of scientific knowledge even while participating in a wider print market predicated on its communication.

<3>I have divided this paper into three sections. The first examines the role of the press in enabling the interplay between publicity and privacy necessary for science to operate as a profession. Although this section identifies science as a profession due to the way knowledge functioned as property, I follow recent work in the history of science that questions the place of professionalization in structuring scientific culture in Britain. The remaining sections explore the way two very different periodicals represented the process of scientific revelation, in which hitherto unknown organisms and phenomena were revealed to an appropriate audience, through gendered narrative voices. The first considers how the *Journal of Physiology* provided the medium of communication for the new, laboratory-based discipline of physiology. By cultivating a network of assistant editors situated in laboratories around the world, the journal, published from Cambridge, standardized the representation of laboratory practice, creating a textual space that was independent of local context. The second uses *Great Thoughts*, a cheap, religious weekly, to situate popular science – often regarded as complementary but excluded from the process of professionalization – within the context of the revelation of the unknown. In this textual environment, gender plays a different role in the creation of trust: whereas men of science evacuated personality from their accounts of scientific discovery, popularizers foregrounded the personality of their narrators as a way of mediating the science for their readers. By contrasting the "genderless" (masculine) non-subjectivity of the learned *Journal of Physiology* with the very definite mediating subject of science popularisation in *Great Thoughts*, I consider the role of gender in marking the ownership of scientific knowledge.

Science, professionalization and periodicals in the nineteenth century

<4>The difficulty with the term "professionalization" is that it is both a useful description of a real historical transformation in the organization of science in the nineteenth century and an anachronistic term that overdetermines the differences between distinct scientific practices and those who carried them out. (Morrell, 981-2) Although it is easy to position nineteenth-century science as a moment of transition from an activity carried out by a range of self-funded, amateur individuals in gentlemanly networks to a laboratory-based practice, staffed by those who received remuneration for carrying out their duties, it is not possible to view this as a shift from an amateur science to one exclusively organized as a profession or even to posit a drive to professionalization as the cause of this change. The necessary learning and the importance of

disinterestedness to objectivity ensured that science was more than simply a hobby and, despite the lack of remuneration, was instead treated as a vocation much like the gentlemanly pursuit of the acknowledged learned professions. Different disciplines adopted aspects of professional science at different times and often for particular contingent reasons. Although such developments might appear to be the result of a single cause, they were actually realized as the result of competing impulses. Viewing the history of science in terms of professionalization risks privileging a single narrative at the expense of these multiple histories and so eliding the diverse people and practices that constituted nineteenth-century science.

<5>Although professionalization misrepresents the changes in the status and organization of science in the nineteenth century, the way science was situated with regards to the production and distribution of knowledge clearly aligns it with the professions. David Vincent argues that professions “straddled the boundary between the private and the public, the secret and the open”:

> They were independent of the state but dependent on its statutory protection. Their identity was defined collectively but their trade was practised individually. Their knowledge was established through open enquiry but its application was closed to the uncertificated. Whilst denying every connection with the occult societies of the past, they derived solidarity and status from their ownership of information denied to the population at large. (23)

Although it was rarely considered to be a practical way of earning a living, did not insist on any formal certification, and its practitioners did not see themselves exclusively as professionals, science, in the way that it embedded a disinterested search for truth within an exclusionary system of access, behaved like a profession. The communication of information was integral for both the careers of scientists and whatever they discovered. For new organisms and phenomena to be allocated a place in nature, their existence had to be validated by other scientists and, eventually, by the wider public. The various mechanisms for the allocation of scientific esteem encouraged disclosure among scientists yet its specialist practices, publications and language meant that its findings remained obscure to the uninitiated. Despite arguments for a common intellectual context in the nineteenth century, scientific knowledge participated in a mixed economy in which expertise and access were distributed unevenly. (Young, 126-163; Morus, 1-19; Dawson, “Literature and Science”, 310-314) Not all scientists were equal and not all readers had the expertise to interpret all scientific documents; in addition, the sheer amount of scientific information, published in a diverse range of print contexts, necessitated a partial reading that was often determined by contingent judgements as to merit or relevance. Scientists were aware that they needed to present their results for particular audiences, whether this meant publishing in the specialist journals of learned societies, in the high-brow periodicals that published science alongside other content, or as news in the newspaper press. There was also a range of writers, both men and women, who might not have considered themselves men of science but who specialized in writing scientific content for various audiences, including the uninitiated. The term popularization inadequately expresses all these different processes of science communication and the various motivations for carrying them out. (Cooter and Pumfrey, 237-67)

<6>Scientific practice produced knowledge that was seen as objective because those who produced it were disinterested; however, those disinterested practitioners were positioned within a hierarchy of esteem that was predicated on the select circulation of knowledge. The same print culture that circulated scientific knowledge in exchange for esteem also offered a market for the commodification of scientific research. Some individuals exploited the diversity of this culture to produce disinterested textual accounts of research in one textual environment while selling different versions of the same research for profit in another. The value of scientific knowledge depended upon the economy in which it functioned: the status of a "man" of science grew as he or she published in specialist scientific publications in exchange for esteem, but was unlikely to do so as a result of publishing in other environments for profit. These economies were not exclusive – writing by a recognized scientific authority conferred value, whether due to celebrity or authority, in non-scientific environments – but they were qualitatively distinct.

<7>By identifying themselves as "men of science", nineteenth-century scientists posited an inclusive identity that associated themselves with intellectual attainment independent of paid activity or day-to-day business but, in foregrounding the person rather than the practice, explicitly labelled the disinterested production of knowledge as masculine. (Barton, 90; Desmond, 39-40) Over-emphasizing the connection between professionalization and paid employment focuses attention on disciplinary and institutional exclusions at the expense of the role of gender in the production and dissemination of scientific knowledge. This is important as it is often professionalization that is offered as an explanation for the exclusion of women from scientific practice. Whereas the older, amateur science, it is argued, permitted both men and women to contribute to science at all levels (although, of course, subject to certain social barriers), the new, laboratory-based professional science routinely excluded women from becoming professional scientists by denying them access to institutions that were essential for valorizing scientific achievement and offering scientific employment. Equally, the professional scientist represented a certain form of masculinity: whereas the amateur scientist might have been a man or a woman, a working-class botanist or an aristocratic astronomer, the professional scientist was imagined as middle class and university-educated – prerequities that did not necessarily apply to the man of science.

<8>However, explanations based upon professionalization not only ignore the diversity of scientific practice throughout the period, but also the way in which scientific status was predicated on a gendered exploration of the unknown. The terms "professional" and "amateur" did not distinguish between insider and outsider – the scientific elite and the dilettante – nor did they describe a science that was modern compared with some old-fashioned, unorganized scientific past. Barton, in relating the ways in which scientists described one another, demonstrates that the language of professionalization was rarely used in an exclusionary or hierarchical sense. (Barton, 108) Rather, the terms "professional" and "amateur" were used to "distinguish the employed from the independent, the public pursuit rather than the private pursuit of science." (108) These terms were important as they distinguished science carried out as a responsibility to some sort of institution, whether a company, educational establishment, or government office, from that carried out independently of such ties. What mattered was the value of the science: an abstract quality that was evaluated and judged by the scientific community. This might be based upon the practical utility of the results, but was much more likely to reward contributions that advanced science as a body of knowledge. A scientific career

was thus characterized by intellectual attainment, usually recognized through membership of and awards from scientific societies, rather than employment by particular institutions. Salaried positions – even prestigious ones such as university professorships or government positions such as Astronomer Royal – entailed work that was perceived as qualitatively different to "true" scientific endeavour, even if important in its own right.

<9>Just as disinterestedness was used to justify claims for science as an authoritative epistemology, so this, in turn, was based upon the social respectability of the individual man of science. Science might have been a private activity, occurring in specific delimited locations and carried out in addition to the public responsibilities of the individual, but it was dependent on publicity for the verification and acceptance of results and the subsequent recognition of those who had achieved them. In fact, it was publicity that, perhaps paradoxically, permitted the necessary privacy for scientific research.

<10>The man of science represented a certain type of masculinity: someone who was fortunate (or ingenious enough) to equip himself with the necessary space, time, knowledge and equipment to carry out research and was then capable of accounting for that research in a way that allowed it to be scrutinized. As Gowan Dawson has demonstrated, the same qualities of self-renunciation and moral character that warranted objectivity also functioned to defend both science and men of science from accusations of immorality, atheism or political radicalism. (*Darwin*, 12-13) As these attributes were understood as social virtues, they also provided the justification for those who wished to establish science as an authoritative way of knowing and extend its application to topics previously considered beyond its province. (*Darwin*, 13) The power of science, its ability to uncover things concealed in the unknown, lay in its professed objectivity, a claim to represent natural processes with the minimum of human mediation. To guarantee this, scientists themselves had to act as mechanical instruments, registering the traces of phenomena, and be trusted to do so. Like all intellectual pursuits, however, science necessarily transgressed the boundaries of accepted knowledge in order to discover something new. Occurring behind closed doors, there was something inherently furtive about scientific investigations. The same private spaces that allowed experiments to take place, coupled with the esoteric processes and language of science, operated to shield the scientist from the oversight of the public. Necessary privacy thus teetered on the brink of unnecessary concealment. Just as the scientist granted publicity for his or her discoveries, so publicity was necessary to account for the process of discovery.

<11>The technology that enabled this double-edged publicity was print. Printed objects, specifically periodicals due to their timely appearance, were integral both to the construction of scientific knowledge and the status of science in nineteenth-century culture. Not only did they provide the context within which the process of discovery could be translated into a narrative, distributed over space, and preserved through time, but they also ensured science was regarded as private rather than secret, overseen by its elite participants rather than the broader public or its putative representative, the state. Periodicals permitted the distribution of esteem while also providing the publicity that new phenomena and organisms required before being acknowledged as in the world. The formulation "man of science" might have associated objectivity with masculinity, but it was structured through a print medium that was not only collaborative,

drawing upon the expertise of a range of men and women, but also represented a range of gendered subjectivities and discourse.

Genderless Subjects in Gendered Spaces: Periodicals, Laboratories, and the Production of Scientific Knowledge

<12>Physiology, the branch of biology devoted to the study of function and predicated upon laboratory practice, came to characterize a certain type of science in the late nineteenth century. Since the 1870s biology had become a discipline distinct from natural history and physiology. As a purely experimental sub-discipline that utilized laboratory techniques to study biological function in isolation, it played an important part in establishing the methodological differences between the two. However, the same assumptions, methods and practices that enabled physiology to mark the difference between an observational natural history and experimental biology also troubled the designation "man of science." The principles of abstraction that underpinned physiology, in which an individual specimen became representative of a biological function, was a product of its institutional organization. The necessity for laboratory space, expensive equipment, and a supply of specimens meant that physiology was institutionalized from its beginnings and that its practitioners tended to be paid employees attached to hospitals and universities rather than independent researchers. These positions were only available to the suitably qualified, whether through medicine or the biological and physiological training available in France and Germany. Once ensconced in the laboratory, however, the physiologist was subject to the same disciplinary codes as the specimen. It was the loss of humanity that this implied that perpetrated the popular image of the physiologist subordinating everything to the pursuit of cold, masculine and materialist science.(1)

<13>In 1870, Michael Foster, with the support of T.H. Huxley, was appointed Prælector in Physiology at Trinity College Cambridge. (Ruse, 144; Coleman, 3) Foster had trained under Huxley at the School of Mines and worked alongside him as a demonstrator until taking a post at University College London in 1867. Before his arrival at Cambridge there was no independent professorship in physiology, no journal exclusively devoted to it, and no recognition of it within the various sections of the British Association for the Advancement of Science. Whereas Huxley's institutional base at the School of Mines (later the Normal School in 1881 and then the Royal College of Science from 1890) was primarily a teaching post and not intended to produce graduate researchers, Foster used his position at Cambridge to create an internal research school that could, eventually, propagate a generation of physiological workers. (Desmond, 28-9) At the heart of the curriculum was Huxley and Henry Newell Martin's *A Course in Practical Instruction in Elementary Biology*, a textbook derived from Huxley's six week intensive teacher-training course, situating its experimental principles within the Cambridge Natural Science Tripos. (Geison, 142-3, 186; Gooday, 333-9) Foster benefited from Cambridge's lustre and the wealth it attracted. Not only did he have control of the George Henry Lewes Studentship of Physiology, created by George Eliot in 1879, but the Colleges of the University also offered scholarships and studentships that could be used to attract students. (Geison, 355-6) Foster's appointment at Cambridge coincided with an increase in funding for physiological science in higher education elsewhere. The Brackenbury Chair was created for pure physiological research within the medical school at Owens College, Manchester in 1873; the retirement of William

Sharpey at University College lead to the endowment of the Jodrell Chair in physiology 1874; Gerald Yeo took the first professorship in physiology at Kings College London in 1880; and John Burdon Sanderson, who had taken the Jodrell Chair, moved to Oxford in 1882 to occupy the new Waynflete Chair of Physiology. In 1883 Foster took the first University Chair in Physiology at Cambridge. These positions, in addition to those in biology and zoology also founded at this time, created a network of positions that could accommodate those trained in laboratory-based techniques and so sympathetic to physiological research. For instance, Foster's fellow demonstrator under Huxley, William Rutherford, took the chair at Edinburgh in 1874; his and Huxley's student, H. Newell Martin, took the biology chair at the newly established research university Johns Hopkins in 1876; and another student, John George Adami, pursued a distinguished career at Cambridge and then, from 1892, at McGill University, Montreal.

<14>Foster established physiology at Cambridge at the same time the controversy over vivisection shaped the wider perception of physiological research. Physiology was an emblematic nineteenth-century professional science, but it was the secrecy that it entailed rather than any unease over the loss of independence due to paid positions that troubled those opposed to vivisection. Resistance to the practice of vivisection was – and still is – expressed in terms of an opposition between sentiment and rationality. These gendered terms permitted resistance to vivisection to be dismissed as feminine: an unreasonable sympathy with animals driven by instinct, rather than with rational mankind. The contradictory constructions of gender made such arguments complex: femininity, for instance, was also associated with morality; however, the public role female anti-vivisectionists took to advocate their moral cause prompted accusations of unwomanliness. (Elston, 264) The way gender was used by both sides in the vivisection debates perpetrated the notion that physiological research, according to the antivivisectionists, was a perverted, inhuman pursuit carried out by male scientists who had become abstracted from the wider life of social relations. The walls of the laboratory operated both literally (as walls) and metaphorically (representing education, qualification, membership of networks etc) to prevent those outside from witnessing physiological practice and those within from tempering their investigations due to the influence of wider ethical principles. The privacy necessary to carry out scientific research thus became its pathological other – secrecy - as those outside suspected the walls of the laboratory to conceal activities that were not justified by their supposed ends. The Cruelty to Animals Act of 1876 breached the walls of the laboratory to provide legislative oversight over the space, an intrusion that was objected to by physiologists as an affront to their self-regulation that threatened their capacity for disinterested research. (Rupke, 188-208) For those privileged to be permitted to work within the laboratory, its privacy enabled the man of science to carry out investigations unfettered by social codes; however, for those outside its walls it was precisely this freedom that needed to be constrained.

<15>The privacy of the laboratory was made possible by the way in which it was open to publicity. The walls of the laboratory did not necessarily conceal but they did exclude through the selective way in which access was granted. For instance, most physiological laboratories were teaching institutions and so were necessarily open to their students. Access was conditional upon certain conditions – gender, education, contacts, physical dexterity, class – that ensured those permitted to enter were both capable of working within the laboratory and could recognize its aims.(2) In terms of research, similar restrictions applied. For work to be ratified and esteem allocated, it had to be presented to the appropriate audience in the appropriate manner, as I have

described. This meant publication in specialist journals in such a way that would convince readers as to the reality of what was represented. Such publications might not have been accessible to a wide range of readers but they permitted access to and memorialized distant laboratory events for those equipped to obtain and read them.

<16>The *Journal of Physiology* was designed to render the private, masculine practice of physiological research a textual space containing objective knowledge that could be reproduced beyond the walls of the laboratory. It was established in 1878 through funds advanced by A.G. Dew-Smith, Foster's scientific collaborator and the founder of the Cambridge Scientific Instrument Company and Cambridge Engraving Company. (Geison, 182, 187) These companies provided Foster's school with standardized equipment while also acting as booksellers, agents and publishers of the *Journal of Physiology* and advertising in its pages. Numbers of the *Journal* appeared whenever Foster had enough material. They cost between six and twelve shillings depending on their length and the number of illustrations, and were gathered into annual volumes. (Anonymous [Michael Foster], unpaginated note) Bulky, infrequent and expensive, the *Journal of Physiology* was not aimed at a commercial market but instead at Foster's network of physiological workers scattered across the globe. This was reflected in the discount offered to subscribers, who paid only one pound and one shilling for a year's numbers. Employing his contacts as assistant editors (Rutherford and Burdon-Sanderson in Britain and H.P. Bowditch, Martin, H.C. Wood, and R.H. Chittendon at various universities in the States), Foster ensured it was read and contributed to widely across the English-speaking world. Between 1878 and 1900, Geison records, the *Journal* published 700 papers from 337 individuals at 60 different institutions around the world. Contributions from Cambridge accounted for just under a quarter of these, with Foster's successor as both editor and Professor of Physiology, J.N. Langley, the most prolific author with 48 papers. (Geison, 367-9) To ensure it was considered in continental Europe, especially in the highly regarded German physiological laboratories, Foster produced an extensive free list. Although continental contributions were rare, the *Journal of Physiology* became the medium for European researchers to keep abreast of physiological research in Britain, the Empire, and the United States.

<17>The *Journal of Physiology* ensured Cambridge became the centre for physiological research in Britain. No other journal in English was exclusively devoted to physiology and Foster ensured that few other journals were as widely distributed. However, its production costs, the expense incurred by postage, and the burdensome free list meant that it had little chance of ever gathering a profit. When Langley assumed the proprietorship of the *Journal* in 1894 and succeeded Foster as editor, the printers, C.J. Clay and Sons, became the publishers, distributing the *Journal* from the Cambridge University Press warehouse in London, and Langley courted some more advertisers, including Burroughs, Wellcome and Co., manufacturers of industrial and research chemicals. In 1892, citing the increased number of submissions, Foster had ended the practice of annual volumes, instead issuing them whenever the journal reached 500 pages. (Anonymous [Michael Foster], unpaginated insert) This ensured subscriptions came in more frequently, but Langley also regulated distribution by publishing numbers more or less monthly. By further extending the *Journal's* assistant editors from thirteen to twenty, ensuring each was either a professor or held a doctorate, Langley advertised the institutional qualifications of physiologists while connecting individual institutions through textual networks.

<18>Physiology offered standardized, mathematized descriptions of function that rendered the complexity of natural form as variation upon the surface of an underlying, comprehensible order. The reorientation of such abstraction as truth was accomplished through careful selection and preparation of specimens and the skilled operations of the researcher. Karen Knorr Cetina (116) suggests the "laboratory is an enhanced environment which improves upon the natural order in relation to the social order", a space that allows aspects of phenomena to become apparent and so meaningful according to the social codes of the scientists. Textual accounts of laboratory practice, in an attempt to reproduce the space of the laboratory, did something similar. Just as laboratory practice elided the interventions of the researcher in order to present the reality of natural function, so the textual accounts of physiological experiments elided the subjectivity of the narrator so that it appeared that the phenomena were narrating their own actions.

<19>The seeming deferral of narrative agency from narrator to phenomena being narrated was itself a narrative effect created by the author and enforced by the editor. Charles Scott Sherrington, who had studied under Foster and Langley at Cambridge from 1881-3, recorded Langley's editorial work in his entry for the *Dictionary of National Biography*:

> Langley saw to it that every paper issued in his *Journal* made not only a solid contribution to knowledge, but maintained the standard of form and style desired, saying what it had to say with succinctness, perfect lucidity, and a minimum of speculative discussion. He would, where he judged fit, almost entirely recast a paper, even of a distinguished contributor. (479)

According to Sherrington – a published poet who was very aware of the role of language in science (Smith, 297-304) – this set "a pattern in the presentation of scientific work which soon provided a boon to every reader wherever such material is used." (479) Universality, here, was achieved by reducing the contingent aspects of both the experiment and its telling so that both processes revealed something that was always waiting to be found. Laboratory conditions and practices ensured experiments were repeatable, testifying to the continued presence of the phenomena in the natural world; in their textual representations the agency of the narrative subject was elided so that the presence of the phenomena could appear to transcend the narrative that constituted their revelation.

<20>A paper by Sherrington published in the *Journal* shortly after Langley's assumption of the editorship demonstrates how textual accounts of the gendered space of the laboratory elided the narrator as subject in deference to the emergence of physiological functions from the complexity of the natural unknown. "On the Anatomical Constitution of Nerves of Skeletal Muscles; with Remarks on Recurrent Fibres in the Ventral Spinal Nerve-Root", published in 1894, was part of Sherrington's investigation into the nervous action that drives the "knee jerk": the automatic reflex by the leg when the knee is gently struck. It opens by outlining its two research questions – 1. in the quadricep and hamstring, which of the nerves are afferent (i.e. lead back to the central nervous system)? 2. is the nerve that is involved in the "knee jerk" constituted differently from those in the quadricep that are not? – before leading to a list of seventeen conclusions. In setting out his two questions in the initial portion of the paper but not revealing the answers until the end, Sherrington implies that this is knowledge concealed by nature while the reality was of course that he concealed it for his own narrative purposes. By keeping his conclusions until the

end, Sherrington creates the illusion of objectivity, of the nerves revealing themselves over the course of his paper. Just as aspects of the nerves emerge as objects of physiological discourse in the laboratory – and so paradoxically become part of nonhuman nature – so, in the textual account of this emergence, Sherrington hides his own agency as narrator to allow the nerves to seemingly narrate their own appearance in language.

<21>The bulk of the paper consisted of accounts and discussions of a series of experiments designed to ascertain the properties of various nerve fibres. These experiments – severing the nerve, recording rates of degeneration, measuring their length and diameter, counting the fibres in each nerve bundle, comparing different nerves in the same and different parts of the body – were all highly artificial scenarios conceived as a stage upon which the nerves could perform unaided by the experimenter. Even the selection of specimens was part of this. Sherrington drew his samples from three monkeys and nineteen cats but, even though there are quantifiable differences between the nervous systems of the two species, he disregarded them in favour of what they have in common. He also ignored the individuality of each animal: they are referred to in the singular as "Cat" or "Monkey" as if they were representative of their entire species. (Sherrington, 223) By ignoring the differences between individual specimens – and at one point he even compared them to a human foetus with *spina bifida* preserved in the Museum of St Thomas's Hospital – Sherrington posited the existence of an essential entity beneath superficial diversity. His experiments in the laboratory were intended to give this entity the opportunity to manifest itself; in the textual account, this ontological manifestation is represented through a provisional abdication of narrative authority. For instance, Sherrington at one point seeks to disprove the proposition, suggested by a rival, that certain nerve fibres "may be recurrent and derived from other peripheral nerves", by providing experimental accounts as evidence:

> Cat. Femoral (ant. crural) trunk cut at emergence from psoas muscle. Central stump 21 days later showed no degenerate fibres; in peripheral stump and branches to quadriceps I could find no myelinate fibres that were sound.
>
> Cat. Sciatic trunk cut above quadratus femoris. 21 days allowed for degeneration. No degenerate fibres detected proximal to trauma, no sound fibres detected distal to it. Similar experiment; union prevented by turning cut ends of trunk up and down. 88 days allowed for degeneration. No sound myelinate fibres in distal trunk and branches. (Sherrington 220)

The hand of the experimenter is here elided so that the nerves can take action: Sherrington, as experimenter, is indistinguishable from the other conditions that provide the context within which the nerves might act (or not). As narrative agent, Sherrington represented himself as a neutral witness, simply recording what occurred. This objectivity, however, was the result of the conferral of narrative authority from the nerves. As nonhuman agents the nerves, of course, could not narrate their actions and, just as the instruments were designed to record their traces in the laboratory, so Sherrington, as narrating subject, is reduced to another instrument, authorized to narrate on their behalf. (Latour, 132)

<22>Like the photomicrographs or the columns of numerical data that were published as part of his experimental account, these passages appear to bear the direct impressions of phenomena

rather than offer mediated accounts of them. Unlike the usual rhetorical description of scientific practice, in which a male scientist reveals the secrets of a feminine nature, here a much more powerful textual situation is enacted. By removing himself from the text as narrator, Sherrington granted nature the power to reveal its – not her – own secrets. However, nature did not speak for itself and the practices that permitted this to happen – both textual and experimental – were complex and required vast resources. Sherrington had to obtain a laboratory, staff (he credited his laboratory assistant, his colleagues, the museum curator at St Thomas"s, and the technician who produced his images), and specimens in order to undertake his research. He had to carefully collate his results, create a narrative, and set forth clear experimental detail. This textual account had then to be submitted to a journal, be refereed, edited and published. The masculinity that underpinned this discourse was not simply the result of men talking to men within scientific culture. Although this permitted a substantial elision of difference, in which masculinity became the default gendered subject position and, to some extent, facilitated trust, it was not enough to produce the necessary objectivity that would allow natural phenomena to speak for themselves. This objectivity, whether achieved in the laboratory or represented in texts, was predicated upon the absence of a gendered body achieved through the use of an extensive repertoire of cultural resources. For a scientific paper to be successful it had to convince the reader of the autonomous reality of what it described. This, in turn, was dependent upon the reader's recognition of resources that made it possible.

Popularizing the Unknown: Structuring Revelation in *Great Thoughts*

<23>The *Journal of Physiology* was a commercial venture whose authors did not expect to receive remuneration for their contributions. Their reward was to be acknowledged as men of science, revealing order in the natural world. But there was a market for such revelations elsewhere in nineteenth-century print culture. Science popularization – here referring to science writing that did not necessarily expect its readers to practice science – necessarily interposed the popularizer between the reader and scientific research. The popularizer and the man of science might be the same individual, but the two practices were different: one was intended for a select audience, already constituted as insiders by their shared expertise; and the other intended for a wider public, unknown to the popularizer. Unlike the science intended to be read by other scientists, popular accounts could not stage the emergence of phenomena in the laboratory through language as the reader could not be expected to possess or acknowledge the cultural resources that permitted objectivity, whether in the laboratory or in written reports. Instead, popularizers had to narrate science in a way that would translate it for the audience, placing it within the context of everyday life. The narrator, whether a fully-formed character within the text or simply implied through the presence of a narrative, represented the mediating role of the popularizer in textual form, accounting for the difference between the world of science and that of the reader through the structured revelation of information. The way popular narratives were told might have celebrated the man of science, but they also demonstrated the skill of the writer, producing compelling accounts that had value in the wider periodical market.

<24>Part of the mediating role played by popularizers was to humanize science. Like most specialist science periodicals, papers published in the *Journal of Physiology* were always signed in order that priority and esteem be duly acknowledged. In popular accounts, signature depended

on the policy of the publication as a whole, but authors could create textual identities to better mediate between the reader and the science. As Bernard Lightman has argued, the majority of popular science writing was produced by clergymen and women, two constituencies disenfranchized by the way masculinity was deployed by men of science like T.H. Huxley as a prerequisite to speak authoritatively on behalf of nature. (Lightman, *Victorian Popularizers*, 95-165) Although figures such as Huxley and John Tyndall were well-regarded for their popular work, there was a vast market for those who could situate scientific progress and knowledge within moral and religious frameworks. As the authority of such writing did not necessarily depend on the scientific standing of the author, but was usually conferred by the way in which they narrated the science, writers were free to use a range of genres and textual personae depending on the particular title for which they wrote. As dedicated science columns were fairly common across a range of periodicals, there were plenty of scientific books to review, and science itself was newsworthy, a degree of scientific knowledge provided men or women with an opportunity to obtain an income through writing.

<25>Edith Gray Wheelwright, a journalist, novelist and writer of popular science who contributed widely across the late nineteenth-century press, began her career publishing popular science in the evangelical penny weekly *Great Thoughts*. Wheelwright gained her first experience of the press entering essay competitions in the *Girl's Own Paper* between 1881-1886. In 1890, at the age of twenty two, Wheelwright won the essay competition of the "*Great Thoughts* Literary Circle" with an essay on "The Intellect of Woman" that took Mary Somerville, widely regarded as the most eminent female popularizer of science, as its subject. Prize-winning essays were awarded a guinea, suggesting that Wheelwright was already writing for money. They were also an easy way for the editor of *Great Thoughts*, the Wesleyan Methodist minister Robert Percival Downes, to obtain copy and it was probably this essay, published on 15 February 1890, that brought Wheelwright to his attention.

<26>*Great Thoughts* was a cheap illustrated weekly that attempted to distribute improving reading as widely as possible (it claimed a circulation between 50,000-100,000 a week (Anonymous [Robert P. Downes], unpaginated)). Although part of a publishing tradition stretching back through the Religious Tract Society and Society for Promoting Christian Knowledge, Downes's innovation was to incorporate elements of the new journalism into his new publication. Founded in 1884, three years after the appearance of *Tit-Bits*, *Great Thoughts* was clearly conceived along similar lines. Like *Tit-Bits*, *Great Thoughts* cost a penny, consisted of short miscellaneous articles, proclaimed that it carried an insurance policy on its cover, made use of interviews, cross heads and other typographical features, and contained a range of competitions and notes to correspondents. Where it differed was in Downes's conception of its role. Whereas *Tit-Bits* was cheap and disposable, intended to occupy minds that might otherwise find more harmful reading while still being entertaining enough to earn a substantial profit, *Great Thoughts* was intended to counter what Downes saw as the triviality of the popular press. (Mussell, 100-104) Downes claimed *Great Thoughts* was an attempt "to give to the country, in a cheap and attractive form, some hints and fragments of a nobler literature." ("The Editor" [Robert P. Downes], unpaginated) These "hints and fragments" he conceived as "treasures […] gems, whose price is above rubies" that lie obscured in the works of the past and "still fall and gleam around us from the pulpit, platform and the press." ("The Editor" [Robert P. Downes], unpaginated). Although *Great Thoughts* was based upon the explicitly commercial

form of new journalism, it was intended, particularly when issues were bound up with their elaborate indices, to provide a compilation of valuable reading that would entertain and instruct the busy reader into the future.

<27>The majority of each issue was made up of short excerpts from other writers, past and present, usually gathered by Downes under a single heading to illustrate a particular topic or address a set of his readers. Original content was limited to some serial articles, usually by Downes himself, on religious, moral or literary subjects, occasional interviews by Raymond Blathwayt, science and, from October 1890, serial fiction. As most of *Great Thoughts* was derived from the published work of others, it involved demanding editorial work but was fairly cheap to produce. Downes, copying *Tit-Bits*, targeted his improving reading at the busy commuter but also included content aimed at women and children and intended it to be consumed in the home. (Mussell, 103) The interviews and serial literature were fairly typical of the genre but Downes's introduction of them eight years into the run suggests he was attempting to compete directly with other weeklies aimed explicitly at women. The inclusion of science amongst this original content was part of Downes's wider ideological motivation for the publication. In his book, *Pillars of Our Faith: A Study in Christian Evidence* (1893), he set out to demonstrate that science need not lead to agnosticism. For Downes, science was an authoritative way of understanding the material universe, but was limited to the visible realm. He claimed that he rejoiced "in her triumphs" and fully recognized "the service she has rendered in giving men a larger control over the forces of Nature, together with a grander conception of the universe." (15) However, the "power which subdues the earth and spans the heavens has no view and no surmise of the invisible and eternal" and so, Downes argued, we must "guard against the conclusions of science when, stepping beyond her legitimate province, she attempts to dogmatize about those things which lie beyond her ken." (15-6) Here science acted as a feminine muse, inspiring the man of science to investigate the natural world. What concerned Downes was the specialism that this implied: using Darwin, the archetypal man of science, as an example, Downes warned that, "as a result of his ardent devotion to the study of physical phenomena", he abandoned religion and could no longer appreciate poetry and so, "except in his own sphere of inquiry, would be a most unreliable guide." (15) Darwin's devotion was admirable: it identified him as a man of science but, at the same time, it restricted the jurisdiction of his expertise to the observable material (and so fallen and feminine) world at the expense of the invisible and eternal, which Downes considered the "underlying reality." (20) Although Darwin's humanity offered a contrast to the perceived cruelty of the physiologist, even his scientific practice could presage a hyper-masculine scientism caused by (yet dependent upon) abstraction from other cultural discourse.

<28>Downes began publishing regular articles on science in *Great Thoughts* concurrently with the serialization of his *Pillars of Our Faith* in 1891. The inclusion of science alongside the poetry and religion already published within the periodical allowed him to illustrate the thesis of *Pillars*, engaging with science while safely accommodating it alongside other discourses to avoid specialization. The first contributions were from Grant Allen – another writer earning a living through popular science (Morton, 185-227) – published on 17 January 1891 to coincide with an interview with him in the same number, but it was not until 14 February 1891 that the commencement of H.B.M Buchanan's "Popular Science Notes" (later just "Popular Science") established science as a regular department in the publication. These articles, however, did not

restrict themselves to the visible and transient material world, but made visible hitherto unknown or unsuspected phenomena. Rather than restrict science to the material world, this approach situated science on the threshold of the unobservable. The diverse phenomena that were described in each instalment of "Popular Science" might have provided salutary moral lessons, but it was in demonstrating that the secrets of nature were part of a comprehensible ordered system that they constituted the evidence for design.

<29> Edith Gray Wheelwright's first scientific contributions to *Great Thoughts* were on the typically feminine science of botany (Wheelwright, "Climbing Plants"; Wheelwright, "Self Defence") and were published separately from Buchanan's "Popular Science." However, when Buchanan's series came to an end in 1892, Wheelwright contributed a series of articles to "Popular Science" prior to the serialization of her first novel, *The Vengeance of Medea*, in *Great Thoughts* in October 1893. The didactic role "Popular Science" played in *Great Thoughts* made it amenable to writers willing to present science in the moral or religious frameworks common to much popular science writing. All of Wheelwright's contributions to "Popular Science" were signed with her full name, foregrounding her gender as a writer of popular science journalism. Although she was the only woman to contribute to "Popular Science" in *Great Thoughts*, Wheelwright was aligning herself with other successful woman science writers such as Margaret Gatty and Arabella Buckley who positioned themselves as inheritors of a recognizable feminine tradition producing science writing for popular audiences. (Lightman, "Voices of Nature", 192-199). Wheelwright's articles, like most popular science writing, were predicated on a double disclosure: firstly, she used science to disclose hitherto unsuspected complexity within known natural forms; then, secondly, she used science to account for this complexity in order to represent it as ordered and, most importantly, already understood by men of science. For instance, the first of her articles, "The Romance of a Limestone Rock" tells of a lonely rock that is suddenly transformed into a thriving ecosystem by the arrival of some spores of moss:

> Life develops quickly in her humbler children, and soon a tuft of brightest green had made its home on the rocky bed – a delicate cobweb of moss! It grew on unmolested, the exquisite complexity of its life history not less beautiful because hidden from the gaze of man. Tiny sexual organs were formed; and from the male cells issued swarms of the motile bodies known as Antherozoids, which, in their turn, crowded round the stationery female cell and fertilized it. (110)

The science here provides detail at the microscopic level, both making visible and aestheticizing phenomena that would otherwise have occurred out of sight. It is this revelation of complexity that allows her to draw her moral. Wheelwright concludes "[h]e to whose reverent gaze nature has revealed her mysteries – who can trace the workings of that marvellous, orderly life which breathes in each insect, tree, and flower, can never in the world be truly solitary." (110) By revealing hidden order in the natural world, form is attributed to otherwise unpredictable phenomena; by tracing these forms, which are there but hidden, Wheelwright suggests, one is inevitably led to recognize their beauty and so their Design.

<30>Wheelwright posits an active Nature, revealing her mysteries to those who revere her. But, in reality, nature does not reveal or conceal anything. Rather, it is Wheelwright herself who

conceals both the hidden phenomena and their explanation from her readers in order that she can reveal them at the appropriate time. The rock is fictional, existing only in Wheelwright's text; the science, however, exists beyond it and it is this that she wants to communicate. By moving from the fictional rock, which only she can know, to the hidden complexity, which is already understood by scientists, Wheelwright makes herself (and her narrative) indispensable in helping the reader to trace order in nature. She does something similar in her next article, "Nature's Degenerate Children", which was split over two issues in December 1892. This article sets out to disprove what Wheelwright calls the Sunday School version of parasitism that condemns parasites as "the curse of the soil and the enemy of God and man." (175) This article is less literary, instead giving the natural history of various botanical and mycological parasites in order to represent them as part of the wider system of nature and not some aberrant exception to it. (175) At the end of the first part of the article, Wheelwright finishes with a caution:

> Bacteriology is too wide a subject to be adequately traversed here, but I can only hope that a brief epitome in a future article of recent researches in another direction may not be entirely scorned. The field of human inquiry runs parallel with time, and perhaps the study of natural science gains an additional charm from the fact that though "knowledge grows from more to more" it never is exhausted. (175)

<31>Wheelwright connects the unfinished nature of her article, with another part to come sometime in the future, with science through the quotation by Tennyson. Readers would have to read her next instalment to learn more about this sub-visible realm and then return to some other piece of writing at some unspecified time in the future to find out what else science had discovered. In neither case was nature posited as ultimately incomprehensible, only incomprehensible to the present. In the second part, Wheelwright repeats her warning about the incompleteness of scientific knowledge:

> A complete interpretation of these phenomena is not yet ours, but every year adds something to the field of our slowly-advancing knowledge, and I venture to think that those whose interest leads them to watch the progress of discovery will not wait in vain. (223)

The incompleteness of science makes it newsworthy, with discoveries possible at any moment. Although disenfranchised from carrying out scientific research, Wheelwright here established a role for herself in its communication. Her articles offered a way of looking at the world that would, in turn, permit her readers to comprehend the order concealed beneath superficial surface phenomena. Science was presented as the authoritative way to uncover this order, but it was Wheelwright's narratives – and the gendered narrative voice that tells them – that situated it within the broader ideological frameworks that offered value to the reader.

<32>Her third contribution, "The Autobiography of a Fossil", was written in a well-established genre of popular science writing, the fictional autobiography. If "The Romance of a Limestone Rock" and "Nature's Degenerate Children" recognized the authority of science as beyond the text, but offered their narratives instead as a means of situating science within the imagined cultural world of their readers, then "The Autobiography of a Fossil" completes the circuit by having the fossil itself provide its narrative. The fossil's voice is not given gender, but

Wheelwright's ventriloquism is marked by both genre and signature as feminine. By giving the fossil voice, Wheelwright challenges the authority of science to speak on its behalf, entirely displacing its narratives with her interpretation of them.

<33>Science complemented the other components of *Great Thoughts*, but it had to be presented as part of a narrative. Partly, this was because its technicalities could be better explained when mediated by a narrator. But it was also because the articles published as "Popular Science" took the natural world, in the present, as an object of study, as opposed to the literature of the past. By carefully structuring revelations from the natural world that demonstrated its unity, not only was the natural world made available to readers but science was presented as a way to mediate the unknown. The authority of the popularizer was not based solely upon the identity of the author but was conferred by the way in which they narrated science. Although popularizers certainly did situate themselves with regards to scientific research – in Wheelwright's case through an evident knowledge of technical terms rather than reputation – the science they narrated was already finished and the act of discovery was being retold for the benefit of the reader. The specialist languages of science were presented as opaque to readers and so the function of popular science was to communicate its discoveries in language that could be understood. The unknown therefore played a part in popular science, but it was restricted to a function of narrative. It was the popularizer, represented as a gendered individual, who kept secrets from the reader, not nature, and he or she did so in order to place the inaccessible results of scientific practice within a wider system of order. The natural unknown became recontextualized in this context into something provisional, a product of human ignorance rather than genuine chaos. In situating science within broader narratives, popularizers might have reasserted the right of men of science to investigate the secrets of nature, but they reserved for themselves the right to explain the broader significance of such secrets to their readers.

Conclusion

<34>What is at stake in moments of revelation are narrative conclusions: the revealing of an ending and the revelation of form. The authority of the nineteenth-century man of science, as today, was predicated upon creating a scenario in which nature would reveal hitherto unsuspected complexity and order. This process, whether carried out in a specific scientific site or recreated through a textual account, positioned the man of science as responsible witness who could provide the context for the emergence of the sensible edges of phenomena, but not be considered responsible for them. This entailed a loss of agency, a stepping back from the experimental scenario that removed the subjectivity of the witness so that he or she could provide an objective perspective on what occurred. This was even more important in narrative accounts of scientific practice as these had to convince others as to the reality of the events that they described. Yet, because these events were narrated, such accounts were even more contrived: authors had not only to represent themselves as passive and disinterested within the laboratory, but also appear to abdicate their responsibility as narrators for the narrative. The neutral, ostensibly genderless narrative voice of specialist scientific accounts was the result of a certain style of writing designed to present scientific practice to particular readers who could judge what it represented and confer esteem. Men of science did not own the phenomena or organisms that they described since their descriptions were predicated on positioning nature as an

agent of revelation. But, as their signatures suggest, they did own their accounts of scientific practice, and it was the circulation of these that structured science as a profession. Although the term "man of science" was offered as inclusive, and the individual man of science was celebrated for his personal attributes rather than his social background, the production and reception of textual accounts of scientific practice depended on access to substantial cultural resources, whether equipment, privacy, or education, and these were unequally distributed throughout nineteenth-century culture. The circulation of these accounts in print culture cohered men of science as a particular cultural group, with certain shared interests and responsibilities, but the resources that made such accounts possible determined who had access to it.

<35>Women, in particular, found it difficult to acquire the necessary resources to present themselves as credible witnesses in scientific practice. As men of science formalized the criteria for objectivity, further masculinizing scientific research, they simultaneously widened the niche, already populated by successful women writers, for those able to communicate scientific research to non-specialist audiences. Popular science writing celebrated the genius of the man of science and reasserted his right to reveal natural forms. But these accounts, written to address an audience that, by definition, were less knowledgeable about science than the author, conferred a different sort of authority through their telling. Although they often reiterated the idea that nature concealed secrets that the scientist must find out, popular science narratives demonstrated that this knowledge was actually held by the popularizer and deployed in order to create a compelling narrative. It was not nature who concealed or was forced to reveal anything, but rather the science writer him or herself. By using science to reveal unsuspected complexity hidden in natural forms, and then using science once more to account for it, popularizers demonstrated their importance as mediators while also marking their ownership of that particular act of mediation. In demonstrating their narrative agency, the popular science writer displayed their scientific knowledge as well as his or her skill as a storyteller, both valuable commodities in the market for print.

<36>Both sets of narratives, those intended for other men of science and those for wider audiences, revealed and then accounted for unsuspected complexity hidden within natural forms. However, the way in which revelation was used in each case determined the ownership of knowledge. For those writing in specialist scientific journals such as the *Journal of Physiology*, narratives asserted ownership over an act of discovery: a revelation of form disclosed during scientific practice. It was this knowledge, circulated in a form that permitted others suitably qualified to verify it, which constituted science as a profession. Popular science writing, such as that in *Great Thoughts*, was explicitly mediated, with an author concealing and revealing the knowledge necessary to explain natural phenomena. Whereas the papers in scientific journals elided the cultural identity of the man of science in order to assert his authority, popular science articles asserted the identity of the narrative agent in order to account for the science. By foregrounding the mediating role of the narrative subject, this textual strategy could not assert ownership over natural phenomena but could over its particular account of them. Unlike the narratives by men of science that attempted to offer definitive accounts of whatever they described, popular science narratives could and did produce multiple representations of a single phenomenon. Like other aspects of nineteenth-century journalism, the subjectivity of the popular science writer was the result of a textual performance intended to both create text as commodity and assert ownership over it. Men of science and popular science writers used the

narrative subject to produce knowledge in different ways; as the professions in general were based upon the circulation of knowledge, it is only through examining how knowledge was exchanged in different textual economies that we can understand the conditions of inclusion and exclusion that defined them.

Endnotes

(1)The physiologist is quite a common figure in late nineteenth-century fiction and consistently stands for a science that subordinates everything to the search for scientific fact. See, for instance, Wilkie Collins's *Heart and Science*, H.G. Wells's *The Island of Dr Moreau* and Arthur Conan Doyle's stories *The Parasite* and "The Physiologist's Wife". For an account of H.G. Wells, vivisection and physiology see Willis (201-234).(^)

(2)Despite the gendering of physiology as masculine, women were not necessarily excluded: in Cambridge, for instance, Foster's physiological lectures and demonstrations were open to women students and, in 1884, the Balfour Biological Laboratory for Women opened to provide a space for women to prepare for the Natural Science Tripos even though they were still not recognized as members of the university (Richmond, 422–55).(^)

Works Cited

Anonymous [Michael Foster]. Untitled note on back cover. *Journal of Physiology* 11 (1890): unpaginated.

Anonymous [Michael Foster]. Untitled insert. *Journal of Physiology* 13 (1892): unpaginated.

Anonymous [Robert P. Downes]. "A Greeting to Our Readers." *Great Thoughts* 13 (1890): unpaginated.

Barton, Ruth. " 'Men of Science'": Language, Identity, and Professionalization in the Mid-Victorian Scientific Community." *History of Science* 41 (2003): 73–119.

Cetina, Karen Knorr. "The Couch, the Cathedral, and the Laboratory: On the Relationship between Experiment and Laboratory in Science." In *Science as Practice and Culture*. Edited by Andrew Pickering. Chicago, Ill: University of Chicago Press, 1992. 113–138.

Coleman, William. *Biology in the Nineteenth Century: Problems of Form, Function, and Transformation*. Cambridge: Cambridge University Press, 1997.

Collins, Wilkie. *Heart and Science*. Ed. Steve Farmer. 1883; Toronto: Broadview Press, 1996.

Cooter, R. and S. Pumfrey. "Separate Spheres and Public Places: Reflections on the History of Science Popularization and Science in Popular Culture." *History of Science* 32 (1994) 237-267

Dawson, Gowan. "Literature and Science under the Microscope." *Journal of Victorian Culture* 11 (2006): 301-315.

---. *Darwin, Literature and Victorian Respectability* (Cambridge: Cambridge University Press, 2007).

Desmond, Adrian. "Redefining the X Axis: 'Professionals', 'Amateurs' and the Making of Mid-Victorian Biology – A Progress Report." *Journal of the History of Biology* 34 (2001): 3–50.

Downes, Robert P. *Pillars of Our Faith: A Study in Christian Evidence*. London: A.W. Hall, 1893.

Doyle, Arthur Conan. *The Parasite*. London: Constable, 1894.

Doyle, Arthur Conan. "The Physiologist's Wife". *Blackwood's Edinburgh Magazine* 148 (1890): 339–351.

"The Editor" [Robert P. Downes]. "Preface". *Great Thoughts* 1 (1884): unpaginated.

Elston, Mary Ann. "Women and Anti-vivisection in Victorian England, 1870-1900." In *Vivisection in Historical Perspective*. Ed. Niclaas A. Rupke. London: Croom Helm, 1987. pp. 259-294.

Geison, Gerald. *Michael Foster and the Cambridge School of Physiology : The Scientific Enterprise in late Victorian Society*. Princeton, NJ: Princeton University Press, 1978.

Gooday, Graeme. " 'Nature' in the Laboratory: Domestication and Discipline with the Microscope in Victorian Life Science." *British Journal for the History of Science* 24 (1991): 307–341.

Latour, Bruno. *Pandora's Hope: Essays on the Reality of Science Studies*. Cambridge, MA: Harvard University Press, 1999.

Lightman, Bernard. " 'The Voices of Nature': Popularizing Victorian Science." In *Victorian Science in Context*. Ed. Bernard Lightman. Chicago: University of Chicago Press, 1997. 187–211.

Lightman, Bernard. *Victorian Popularizers of Science*. Chicago: University of Chicago Press, 2007.

Morrell, J.B. “Professionalisation.” *Companion to the History of Modern Science*. Ed. R.C. Olby, G.N. Cantor, J.R.R. Christie and M.J.S. Hodge. London: Routledge, 1996. 980–989.

Morton, Peter. “Grant Allen’s Publications: A Checklist.” In *Grant Allen: Literature and Cultural Politics*. Ed. William Greenslade and Terence Rodgers. Aldershot: Ashgate, 2005. 185–227.

Morus, Iwan. “Replacing Victoria’s Scientific Culture.” *Nineteenth: Interdisciplinary Studies in the Long Nineteenth Century* 2 (2006): 1–19.

Mussell, James. *Science, Time and Space in the Nineteenth-Century Periodical Press*. Aldershot: Ashgate, 2007.

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

Ross, Sidney. “Scientist: The Story of a Word.” *Annals of Science* 18 (1962): 65–85.

Rupke, Nicolaas. “Pro-Vivisection in England in the Early 1880s: Arguments and Motives.” *Vivisection in Historical Perspective*. Edited by Niclaas A. Rupke. London: Croom Helm, 1987. pp. 188–208.

Ruse, Michael. “Thomas Henry Huxley and the Status of Evolution as Science.” In *Thomas Henry Huxley’s Place in Science and Letters*. Ed. Alan P. Barr. Athens and London: University of Georgia Press, 1997. pp.140-158.

Sherrington, C.S. “On the Anatomical Constitution of Nerves of Skeletal Muscle; with Remarks on Recurrent Fibres in the Ventral Spinal Nerve-root.” *Journal of Physiology* 17 (1894): 210-258.

Sherrington, C.S. “Langley, John Newport (1852-1925).” *Dictionary of National Biography 1922-1930*. Ed. J.R.H. Weaver. London: Oxford University Press, 1937: 478-81.

Smith, Roger. “The Embodiment of Value: C.S. Sherrington and the Cultivation of Science.” *British Journal for the History of Science* 33 (2000): 283–311.

Vincent, David. *The Culture of Secrecy: Britain, 1832-1998*. Oxford: Oxford University Press, 1998.

Wells, H.G. *The Island of Doctor Moreau*. London: Heinemann, 1896.

Wheelwright, Edith Gray. “Climbing Plants.” *Great Thoughts* 14 (1890): 243.

Wheelwright, Edith Gray. “Self-Defence in the Vegetable World.” *Great Thoughts* 17 (1892): 219–220.

Wheelwright, Edith Gray. “The Romance of a Limestone Rock.” *Great Thoughts* 18 (1892): 109-10.

Wheelwright, Edith Gray. “Nature’s Degenerate Children.” *Great Thoughts* 18 (1892): 175.

Wheelwright, Edith Gray. “Nature’s Degenerate Children. Part II.” *Great Thoughts* 18 (1892): 223.

Wheelwright, Edith Gray. “The Autobiography of a Fossil.” *Great Thoughts* 18 (1893): 321.

Willis, Martin. *Mesmerists, Monsters and Machines: Science Fiction and the Cultures of Science in the Nineteenth Century*. Kent State, OH: Kent State University Press, 2006.

Young, Robert M. *Darwin’s Metaphor: Nature’s Place in Victorian Culture*. Cambridge: Cambridge University Press, 1985.