

REPOSITIONING VICTORIAN SCIENCES

Shifting Centres in
Nineteenth-Century Scientific Thinking



Edited by

David Clifford
Elisabeth Wadge

Alex Warwick
Martin Willis

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Anthem Nineteenth Century Studies
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Anthem Press
London

Anthem Press
An imprint of Wimbledon Publishing Company
www.anthempress.com

This edition first published in UK and USA 2006
by ANTHEM PRESS
75-76 Blackfriars Road, London SE1 8HA, UK
or PO Box 9779, London SW19 7ZG, UK
and
244 Madison Ave. #116, New York, NY 10016, USA

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British Library Cataloguing in Publication Data
A catalogue record for this book is available from the British Library.

Library of Congress Cataloging in Publication Data
A catalog record for this book has been requested.

1 3 5 7 9 10 8 6 4 2

ISBN 1 84331 212 3 (Hbk)

Cover illustration: From the *Illustrated Police News*,
Saturday 20 May 1876; courtesy of the British Library.

Printed in Singapore

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1

MARGINS AND CENTRES

Alex Warwick

In 1859, Prince Albert addressed the British Association for the Advancement of Science (BAAS) as its President and attempted to assess the place of science in the modern State. In his description of science, he says:

The operation of Science then has been, systematically to divide human knowledge, and raise, as it were, the separate groups of subjects for scientific consideration, into different and distinct sciences. The tendency to create new sciences is particularly apparent in our present age, and is perhaps inseparable from so rapid a progress as we have seen in our days; for the acquaintance with and mastering of distinct branches in knowledge enables the eye, from the newly gained points of sight, to see the ramifications into which they divide themselves in strict consecutiveness and with logical necessity. But in thus gaining new centres of light, from which to direct our researches, and new and powerful means of adding to its ever-increasing treasures, Science approaches no nearer to the limits of its range, although travelling further and further from its original point of departure.¹

A little over a year later Albert was dead, but his vision of the material location for the operations of science was beginning to be accomplished. The sites in South Kensington that were purchased through the profits of the 1851 Great Exhibition and subsequent Parliamentary grant gradually

came to house something like the training schools and collections he had envisaged; and the Royal College of Art, Imperial College, the Victoria and Albert Museum and the museums of Natural History and Science now stand there. These are perhaps the physical manifestations of the ‘centres of light’ that Albert was determined to see established, but his confidence in the distinctiveness, the strict consecutiveness and logical necessity of their development is one that, although perhaps not surprisingly under question today, was by no means a certainty even as he spoke. Indeed, his entire address is threaded with anxieties about the future of science and the sense that a secure project is just slightly beyond arm’s reach. Despite, or perhaps because of, the pace and dynamism of scientific work in the three decades since the foundation of the BAAS, Albert’s speech contains exactly the same mixture of bold confidence and significant doubt as that of the first President, William Harcourt, at the Association’s founding in 1831. Harcourt was already concerned with what he called the dissolution of the ‘commonwealth of science’ as a consequence of its success and stated one of the purposes of the Association to be the review and direction of *all* scientific inquiry.² Albert echoes this; he sees the centres of light as new and as purposefully directing future work, and the qualities of centrality and direction as crucial to the progress of science:

Whilst the tendency to push systematic investigation in every direction enables the individual mind of man to bring all the power of which he is capable to bear on the specialities of his study, and enables a greater number of labourers to take part in the universal work, it may be feared that that consciousness of its unity which must pervade the whole of Science if it is not to lose its last and highest point of sight, may suffer. It has occasionally been given to rare intellects and the highest genius, to follow the various sciences in their divergent roads, and yet to preserve that point of sight from which alone their totality can be contemplated and directed. Yet how rare is the appearance of such gifted intellects! and if they be found at intervals, they remain still single individuals, with all the imperfections of human nature.³

The central questions of this volume revolve around many of the issues suggested by Albert’s speech: how the centres were created and defined, how they were contested, how the tracks of consecutiveness and logical necessity were imagined, constructed and maintained, and the tensions between ‘the universal work’, the individual disciplines and the ‘men of science’.

There is an obvious and immediate difficulty with terminology in using the word science, denoting as it does both a capitalized notion of ‘Science’

as a particular form of thinking or kind of knowledge and 'science' as an individual field of enquiry identified by object. The problems arise in the need to use both these words as they are not necessarily coterminous and not at all stable in their usage. A science should contain scientific thinking, but that is not the limit of its definition, and neither does scientific thinking belong only to the province of Science, or a single science, or arguably indeed any science at all. If, at the simplest level, a science denotes an area of study or knowledge that is unified by its object, and scientific thought a means of study or kind of knowledge that is characterized by a particular method, even those basic distinctions fail to hold more than momentarily. One of the reasons that they fail is located in the nineteenth-century period itself, in the great speed with which 'sciences' were named and began to carve identities for themselves, or rather these identities were carved for them, for to speak of science or sciences as though they have agency independent of the complex currents of class, gender, economics, politics or other elements of wider culture is to perpetrate a notion of Science as a neutral realm beyond the vagaries of human existence that still persists in common-sense thought today. The neutrality of Science is a conflicted notion even in Albert's words, where it is possessed of both the autonomy of consecutiveness and logical necessity, but also subject to creation and fragmentation of its unity by the actions of individuals upon it. He fears too the intrusion of 'human nature' into the realm that should exclude it and identifies collective work as the insurance against it. His observation of the contemporary tendency to create new sciences is accurate, though again it belies the complexity and instability of these processes of creation. If the creation of a new science is owed simply to the systematic identification of a new object of study, its coming into existence should be relatively straightforward, but what emerges alongside the 'new' sciences is a simultaneous uncertainty about what constitutes a delimited or even legitimate object of study. The rapid establishment of sciences and the equally rapid growth of sub-sciences or inter-disciplinary sciences attests to the actual instability of that which should be simply revealed in all its distinctiveness through the application of scientific method. Like those of many others in the field, Albert's and Harcourt's anxieties about the fragmentation of science are real, but at the same time as they are being voiced, there remains an equal drive in the directions that only serve to produce further sub-divisions of the universal work. What is also significant is that the apparently conflicting positions were often held by the same people, as Susan Cannon suggests:

Professionalisation, specialisation, elitism, national organisation and dreams of 'pure' science are separable variables which occur in different combinations in different persons.⁴

And, I would add, at different times. In the discussion of Science/science, there is a third term that hovers, pseudo-science. It is one that is of crucial importance in this collection of essays as it is frequently used interchangeably with the term marginal science.⁵ A definition which summarizes a general view of pseudo-science is that it is:

What appears as science, or represents itself as science. Or else it is what science proper – the scientific establishment, or the scientific inquisition – marks off as heretical.⁶

A great deal is suggested in this definition, but principally that there are two elements at work, one concerned with the theoretical criteria of 'true' science and one with social factors. In the former, pseudo-science appears as actively involved in making deliberately false claims, and in the latter, it appears as the passive victim of social and political interests. This definition also ignores that there is a relation between social and political interests and what it is possible to represent as science at any particular time. This collection rejects 'pseudo-science' as a useful term, but not the investigation of the historical contingency of designations of marginality. Roger Cooter asserts that the science/pseudo-science split is the active creation of intellectuals in a specific socio-economic context⁷ and with some reservations about the explicit Marxism of his analysis, the general aim of all the essays collected here is the investigation of that process of active creation of categories of centrality and marginality.

Of course, the urgency that lies within these arguments in the nineteenth century and today is not simply to do with terminology, but with the peculiar weight that is attached to science as truth, and to scientific knowledge as the truth about the universe we inhabit, and to that knowledge as constantly advancing. No historian, professional or amateur (to resort to the terms that Victorian scientific practice has bequeathed us), of literature, art or economics would think of or describe those fields as progressive, with knowledge and practice being built upon and advancing towards a goal, yet this conception is deeply embedded in both popular and academic views of science. This is clearly far too large a theoretical argument to pursue here, but part of the effect of this view has been to efface elements of the history of science as it is created, to conceive of it as still moving through central strands of true or right (if sometimes inadequate) ideas upon which reliable knowledge is built, and as having at best diversions or dead ends, or at worst parallel currents of lunacy. In this view, the nineteenth century appears as a period in which wise scientists like Darwin heroically discovered evolution and foolish men such as George Combe thought that they could assess people's personalities by the

bumps on their heads, yet in 1850, phrenology and physiognomy had a far wider and more socially respectable set of adherents than did transformism. Phrenology shifted from a central position where Combe's *Constitution of Man* could sell 40,000 copies when published in a cheap edition in 1835, and numbered J S Mill and G H Lewes among its believers, to a marginal place in the history of science as a misguided effort to understand the mental faculties. Despite its cultural centrality, however, phrenology was castigated as pseudo-scientific or pseudo-philosophical from its beginnings, indicating that centre and margin are problematic terms, suggesting as they do a more fixed order of authority and orthodoxy than was the case. As Alison Winter concisely states:

No science that we now regard as heterodox or marginal can be considered unambiguously to have held that status at a time when no orthodoxy existed to confer that upon them.⁸

What this collection of essays seeks to do is to examine the various fluctuations of the axes of orthodoxy and heterodoxy in the recognition of their locality and temporality by taking specific examples from across the nineteenth century and from a range of very different fields.

It is obviously not new to seek different or more nuanced pictures of culture through the investigation of apparent marginality, but one of the problems with attention to the cultural margins is the fluidity of what can be seen as centrality or marginality. The assumption of a consistency of marginality, and certainly of culture, is particularly difficult to sustain when examining the movements of science and scientific thinking in the nineteenth century. It is also not new to suggest that nineteenth-century science is deeply influenced by, as well as being an influence on, nineteenth-century culture. In 1978 Susan Cannon's book *Science in Culture* made clear in its title the premise of her work that the one is embedded in the other. Robert Young's statement in 1985 that 'at the heart of its science we find a culture's values'⁹ is a succinct assertion of a principle that has informed much subsequent work on the history of science, and Gillian Beer's 1983 book, *Darwin's Plots*, has proved to have opened a very fertile field of relations between science and culture, particularly literature.

Both of these influential texts, though, concentrate on biology and more specifically on the legacy of Darwin's theory of evolution through natural selection, and this has also been the case for the majority of subsequent work. This is not to criticize that focus: in many ways biology was the dominant science of the nineteenth century, and natural selection a theory with profound and far-reaching effects, but in 1840, William Whewell still had to qualify his

usage, saying that, 'the term Biology has of late become not uncommon among good writers',¹⁰ and the BAAS only included a Section of Biology for the first time in 1866. What is at stake in observing the establishment of such titles is not just the history of terminology, but the question of how the object and practice of a field like biology managed to displace natural history and natural philosophy and to become so strongly determined that it persists as one of the few scientific terminal examinations in the English education system. How do some sciences emerge to occupy central ground, and how do others become consigned to the margins?

The struggle at work here is around authority, in the various senses of the word. The first sense, familiar in histories of science, is that of being the author of a theory or work. This exemplifies the wish to identify one person as the originator or advancer of a particular field, and has the effect of centralizing that person in the heroic line of discovery and marginalizing other work. Richard Yeo points out a great irony here in the case of William Whewell:

That as the outstanding historian of science in his day he reinforced the perspective in which past science is viewed as a story of heroic individuals – usually great *men* – wresting secrets from Nature. In so far as his name was not firmly linked with such a discovery, Whewell was relegated to the sidelines of the story by his own historiography.¹¹

The history of individual authorities/authors deals very poorly with simultaneous work, as in the example of Darwin and Alfred Russel Wallace, where many millions more people have heard of Darwin than his contemporary. Wallace complicates the narrative by his independent proposal of a theory of evolution, albeit sketchier and different in some details, and the fact that the first public presentation of the theory of evolution through natural selection was a reading to the Linnean Society of Wallace's paper alongside Darwin's suppressed 1844 essay is overlooked. T H Huxley's clash with Bishop Wilberforce at the Oxford BAAS meeting in 1860 is preferred in cultural memory for its drama and for its supposed exemplification of the bitter opposition of old and new world views. Wallace's part has been largely forgotten and his later career seen as a confusion of misguided, and unscientific, enthusiasms.

To achieve authority through authorship also took on new importance as it increasingly became possible to make a career as a scientist, even though a career still did not necessarily mean a paid job, as in Thomas Huxley's famous complaint that 'science in England does everything – but *pay*. You may earn praise but not pudding.'¹² Pudding continued not to be widely available, and the picture of a steady drive towards a fully professional scientific class proves

to be far more complex. Morrell and Thackray's work on the BAAS, for example, argues very strongly that, despite what might be assumed to be the Association's interest in professionalization (given its aims of greater attention and fewer obstacles for science and scientists) it deliberately declined to pursue a number of crucial efforts connected with professionalism, such as government-paid posts, pupillage and pensions.¹³

A second sense of authority is not necessarily to do with authorship, but with being in recognized possession of a body of knowledge. There is of course a circular motion here whereby the constitution of particular bodies of knowledge produces both that knowledge and the authorities that recognize it, but it can also legitimate other forms of authority. Alison Winter discusses, for example, the conflicted field of mesmerism and the ways in which it was popular with groups as diverse as Radicals, Tory Evangelicals, Whig aristocrats and middle-class utilitarians because for each, it offered the possibility of solid proof of their political and social beliefs. If science is deployed to underpin authority in non-scientific arenas, the reverse is also true. Authority is borrowed for science from many other sources; Prince Albert's presidency of the British Association is perhaps only an example of a more active version of the appellation 'royal' that prefixed so many of the learned societies, or what Roger Cooter said of the BAAS, 'stuffing its committees with the titled and lettered'.¹⁴ Albert's presidency, though, nicely illustrates the bilateral associations of authority, where he was very conscious of his own ambiguous position, and sought to modernize the monarchy and secure its leadership of society by visible participation in the most contemporary of concerns, and the BAAS, having been founded partly as a challenge to the Royal Society, was able to find similar authority by other means. These two former types of authority are frequently combined in a third, an authority as a body of governance that licenses and controls practice in a field, and often too the physical and textual spaces in which knowledge is produced and disseminated.

One way of thinking through the fluctuations of authority is to consider a structure which underlies all the debates that are touched upon here: the knot of relations between place, practice and audience. It is perhaps here that we can trace in specific detail the changes in nineteenth-century science, and in these shifting relations a set of trajectories that do not suggest a teleology, and that radically undermine the notion of strict consecutiveness and logical necessity of development.

As Martin Willis and Don Opitz argue in their essays here, the location of science was a crucial question for nineteenth-century practitioners. The ideas of the public and the private that structured such major sections of nineteenth-century society had no less purchase in the area of science, but as with the inconsistent ways in which they functioned within society, so too

it was in science. As Willis observes of the Cavendish laboratory established in Cambridge University in 1871, it was possible for an apparently professional, public centrepiece of new scientific practice to be simultaneously still amateur and private in very significant ways. As Opitz points out, domestic (albeit aristocratic) spaces sustained an important strand of scientific and intellectual work that was by no means disconnected from the increasing number of professional institutions, even at the turn of the century.

As is also obvious from Prince Albert's address, private was widely assumed to mean individual, but again a synonymity between the terms private/individual and public/collective cannot be sustained. Despite an apparent move to 'public' science and the inclusiveness and accessibility that would suggest, this publicness has more in common with the description public school than it does with democratic participation. Public science is collapsed into 'popular' science with its pejorative associations of oversimplification and vulgar entertainment. In a wider sense too, a notion of the public relies upon the fiction of a singular populace, when it is clear that there is no such homogenous body that is not deeply fissured by gender and class as only the most obvious of the many differences.

Beyond the large categories of public and private, which prove not to be synonymous with centre and margin, the further sub-divisions of the public (or institutional) scientific space are equally hard to quantify. Not all of the institutions occupied the same status, and as the theoretical places of the disciplines shifted, so did the authority of the institutions. The reading of the Darwin and Wallace papers at the Linnean has been described as part of Joseph Hooker's attempt to revive that old institution in its new buildings in Piccadilly and to compete with the Geological and the Zoological Societies.¹⁵ Even within single institutions such as the Royal Society, there existed pockets of members who worked almost as though they were separate bodies. By far the most significant of these was the X club, the nine men who between them achieved, among other things, three presidencies of the Royal Society, five of the BAAS, thirteen scientific medals, eighteen honorary doctorates and two knighthoods.¹⁶

One thing that does unify the places of science mentioned in the last paragraph is that they were in London, as were very many others, and this fact represents another set of complications of the question of place, as there tended to be tensions between the metropolitan one-subject scientific societies and the more general literary and philosophical societies that flourished in the provincial towns that had been greatly expanded by the Industrial Revolution. Geographical location is significant, as Alastair Durie's essay here suggests, where he argues that hydropathy was able to establish a much more mainstream and respectable image in Scotland than it could achieve in

England by drawing on both the strong Scottish temperance movement and the more radical tradition of medicine as it was practised there.

Spaces of science were also not necessarily physical, and there have been a number of significant studies of the functions of the virtual spaces of journal publication and private correspondence¹⁷, and in this volume, James Mussell discusses the *Mechanic's Magazine* and identifies it as producing a polyvocal challenge from artisans all over Britain to the London-based and mainly middle-class Mechanics' Institution. Although private correspondence and, to a lesser extent, periodical publication did often provide an interstitial space that facilitated communication across lines of class, geography, professional status and even gender, it is important not to see them as ideal alternatives to the more fraught arenas of the London institutions. In the decade 1830–40, scientific articles in general periodicals only accounted for between 5 and 10 per cent of the total material¹⁸ and those periodicals, like the *Edinburgh*, *Westminster* and *Quarterly* reviews were frequently attached to very specific political interest and subject to market demands. Even when the dedicated science journals like *Nature* (1869) began to be published, they were also still associated with particular factions. For skilful manipulators too, letters and articles could be used as a means of establishing a career through the construction of temporary authority. Alison Winter explores the ways in which William Carpenter secured scientific respectability by creating an authoritative community through private correspondence with eminent figures, selected extracts from whose letters then appear as contexts in the publication of his own ideas, making it difficult for those figures then to dissent publicly from what they had assented to in private. Additionally, as Winter observes:

[T]he list of individuals marshalled to Carpenter's cause should not be understood as constituting a set of scientific leaders recognised by contemporaries as a bounded elite. Rather it was Carpenter's manoeuvre that asserted the existence and membership of a community of definitive experts – those individuals juxtaposed against one another as leaders in the field – and simultaneously, the status of orthodoxy for his own claims.¹⁹

This understanding of authority as local and temporal makes it possible to understand the movements of scientific authority without ascribing to them a teleology.

As I suggested earlier, the idea of progress in science is a particular problem, noticeably so in the nineteenth century because of the rise in historical consciousness that marked the period, but perhaps even more because

of the entanglement of the extremely powerful metaphor of evolution in questions of history. In spite the contradictions that exist with the theory of natural selection itself, the metaphor of evolution came to stand for ideas of movement towards perfection, with the increasing acceptance of the scientific theory lending credence to arguments of gradual improvement in every conceivable sphere. It is the metaphorical understanding of evolution that has informed the history of science, and it has been a challenge for the editors to arrange this book in a fashion that does not reproduce the kinds of assumptions about progress and unification that are being questioned within it.

The first section, *Shifted Centres*, puts together the discussion of areas and figures that have been sidelined in order to illuminate some elements of the matrix of influences that work to produce marginality.

The first two of these, by Caroline Trowbridge and Sherrie Lyons, present a number of aspects of this kind of shift, by examining, respectively, John Ruskin's involvement in geology and the sea serpent debate. Geology itself is certainly not a science that became discredited, during either the nineteenth or the twentieth century, but it did move from the central position that it had occupied up to the 1840s as a major theatre of scientific debate. Ruskin, in many other areas a figure of great influence and authority, was within his lifetime edged out of geological debates in which he had participated with some recognition and came to see himself as having a marginal voice at odds with a new orthodoxy. Lyons argues that sea serpents rose briefly from the status of myth as part of the struggle to differentiate palaeontology from geology, and served as an example of the contests over the nature of proper evidence and scientific process that marked the effort to professionalize the discipline.

Alastair Durie traces the trajectory of hydropathy, a course of treatment taken seriously enough for Tennyson, Carlyle and Darwin to follow it for their health problems. As well as providing an example of geographical differences between Scotland and England in views of the rigour and respectability of hydropathy, he shows how hydropathy established a position by shedding its associations with what were thought of as quackeries, such as homeopathy, and aligning itself with the very popular Scottish temperance movement. The subsequent fates of temperance and homeopathy are an interesting footnote to this essay; in the twenty-first century, homeopathy is one of the major practices in the still-argued arena of quackery and 'real' medicine, although at the present time, it appears to be gaining ground so rapidly that it can scarcely be called an alternative therapy any longer. Likewise, Alison Butler's essay includes discussion of the areas of antivivisection, vegetarianism and women's rights that are far more recognizable

in mainstream thought than they were in the nineteenth century, and she argues that Anna Kingsford used her marginalization by gender to move freely between the sphere of medicine in which she was trained and her belief in magic to construct an opposition to the practice of vivisection.

Phrenology occupied a position that was culturally central but scientifically marginal, and Lucy Hartley explores the tension between these positions and the way that phrenology served as a significant site of emerging definitions of scientific activity.

The second section, *Contested Knowledges*, picks up some of the debates in Hartley's essay to examine more closely examples of the various struggles to assert authority, method and disciplinary boundaries.

Susan Bernstein takes the figure of Lydia Becker, a botanist and member of the BAAS, to illuminate the complex position of female scientists and the effect of nineteenth-century gender ideology in designating, and marginalizing, sciences like botany as 'female'. Bernstein also shows how Becker's belief in women's rights informed her scientific studies and, in the other direction, how her observations on hermaphroditism in plants led her to an idea of the androgyny of the creative mind.

Elisabeth Wadge's essay uses the 1868 argument between John Tyndall and the medium Daniel Home in the national press to examine the grounds of their struggle, a struggle which represents the amateur performer's resistance to dismissal by the professional scientist and member of the Royal Society.

James Mussell's essay also concerns the nature of public debate conducted in the press as he examines the discourses of scientific counter-cultures as they were created through the *Mechanic's Magazine*. He argues that the publication was a textual site where science could be practised, shared and critiqued by a community of artisans in a way that encouraged debate and plurality rather than working to create or impose consensus.

Ted Underwood surveys responses to the principle of the conservation of energy to illustrate the coexistence of a general confusion about the theory with a general consensus about its centrality to science. He argues that the first law of thermodynamics was not initially valued as a 'conservation principle' because it was recognized instead as a statement about the origin and circulation of productive power because of the deep enmeshment of debates about natural force and productive power in political arguments about the special status, or otherwise, of agriculture.

John Lynch's essay charts the problematic transition which occurs when a scientific idea is forced to shift from acceptability to marginality, as tensions emerge between its corollaries and contemporary ideological trends. He argues that 'scriptural geology' became less and less useful as a means of reconciling observations about the earth with scriptural accounts of creation

as prominent geologists prioritized scientific proof over literal interpretation of scripture. In common with Bernstein's gender analysis, he notes the masculinizing of geology that took place as new practitioners sought to produce it as an active science and wrest control from amateurs.

The amateur/professional nexus is also the focus of Don Optiz's piece. As has been suggested earlier, the sites of science were no less contested than the ideas, and Optiz asserts here that historiographical insistence on the increasingly public and professional nature of nineteenth-century science has marginalized the vital contribution of the private research that went on into the twentieth century. He takes several groups who practised in domestic locations and shows the value of their work, as well as the close connections that they maintained with each other and with 'organized' science.

The third section, *Entering the Modern*, attempts to place the sciences and debates from the later Victorian period that have, in different ways, come to be seen as central to that period, and to the twentieth and twenty-first centuries. They also point to the tenuousness of that centrality.

Bernd Klähn's essay attempts to outline the process by which corpuscular models of light gave way to the diffraction models that would culminate in theories of wave dynamics. He locates this alongside new modes of political discourse and demonstrates that the development of modern sciences runs parallel to the motions of political and philosophical worldmaking.

Similarly, David Clifford argues that the popularity of Victorian lunar studies, coupled with the belief that the moon would forever remain out of reach, reveals one aspect of the ontological shift from traditional to modern understandings of creation. Clifford shows how the imaginative inscription of the moon by scientific and popular studies subtly reevaluated cosmological thought and placed planetary science within the prevailing secularism and those ideological positions it dominated.

By examining the emergence of psychoanalysis from neurology, philosophy, clinical practice and psychology, Peter Naccarato suggests that the early disciplinary tensions around Freud's work continued to mark its development and its acceptance and that the case study of psychoanalysis illustrates the competing methods and philosophies, and ultimately the inadequacies, of the hardening borders of late-Victorian science.

A similarly contested practice of the period was sexology, and Heike Bauer's essay traces an example of the susceptibility of scientific reputation to reassessment. Her historiographical treatment of sexology examines the relative critical fates of two early theorists, Karl Heinrich Ulrichs and Richard von Krafft-Ebing. The former worked on the margins and within a classical discourse, whereas Krafft-Ebing was a respected medico-legal authority, but Bauer contends that their positions have been reversed, with Ulrichs

accorded a central place in the history of homosexual rights campaigns and Krafft-Ebing displaced and vilified.

Martin Willis's paper follows the process of the founding of the British Institute of Preventive Medicine to show the vigorous conflict that took place between professional scientists and antivivisectionists over the kind of work that the Institute represented. He suggests that the methods employed by those late-Victorian scientists and campaigners in attempting to secure public discredit of their opponents served to polarize the debate about laboratory science in ways that have proved both enduring and damaging.

Willis's conclusions about the emergence of a view of science as immoral, extremely powerful, largely invisible and beyond the reach of public scrutiny work as a reminder of why the subjects of the essays in this collection are significant. The struggles over authority and the shifting grounds of marginality and centrality in the nineteenth century are what have formed both our practice and our opinions about science in the present. When read in this way, the apparent hegemony of modern science reappears as a discontinuous history of local constructions rooted deeply in social and political currents, a view that belies any sense of the ascending, logical and inevitable motion towards the completion of the universal work of knowledge that Prince Albert described a century and a half ago.

SECTION I

Shifted Centres

‘SPEAKERS CONCERNING THE EARTH’: RUSKIN’S GEOLOGY AFTER 1860

Caroline Trowbridge

To read the geological work of John Ruskin from the early 1860s until the mid-1880s is to look at a marginalized science through the lens of a marginal – and, ultimately, marginalized – practitioner. The inception of the dense, professional *Geological Magazine* in 1864 does show that able work was being done by an increasingly self-aware international community. But while geology was not being practised less, its significance in Victorian scientific culture was diminishing. Not only the question of evolution, but questions about the nervous system, solar power, sociology and the relation between mind and body began to edge geology out of its former place.¹ The science which had fed controversy about the age of the earth, the historical accuracy of the Bible and the origin and evolution of life began to shift to a science of filling informational gaps. Geology’s leading practitioners certainly resisted being relegated to the sidelines. As the Oxford geologist John Phillips lectured to the British Association in 1864:

The age of geological discovery is, by many persons, thought to have passed away with Hutton and Werner, Humboldt and Von Buch, Smith and Cuvier, Conybeare and Buckland, Forbes and De la Beche ... Yet in this very district, the most carefully examined perhaps of all the richly fossiliferous tracts of England, our friend Mr C Moore is finding a multitude of interesting forms of life of the later Triassic

age ... Nor is the practical application of our science less actively exercised.²

The optimism of this outlook is belied by its defensiveness. The intellectual heyday of the science had passed.

Ruskin was indisputably a central figure of the Victorian period and, from the age of 24 (when the first volume of *Modern Painters* was published to widespread acclaim), was accustomed to viewing himself in this light. But by the time he wrote *Deucalion* (1875–83), a collection of studies and former lectures on geology, he considered himself to be undervalued in a sphere where he felt he had exercised influence in the past and expected to remain an authority. His move to the margins and struggle there presents a less well-known side of Ruskin while at the same time indicating ways in which ideas about authority were changing. He developed strategies for constructing a different kind of scientific authority, the most effective of which was his tactic of bringing his established authority as an aesthetic and literary critic to bear on scientific matters. Tapping into widely held popular opinions about the natural world which were being stifled by the emergent orthodoxy, Ruskin's popular geology articulated problems and offered his own creative solutions, which the more mainstream professional voices of Victorian science could not satisfactorily answer.

Ruskin scholars have never accorded him a place as a respected, professional geologist.³ Throughout most of the twentieth century, Ruskin's scientific work was ignored, dismissed summarily or labelled 'rear-guard'.⁴ A revaluation of nineteenth-century geology did not begin until the 1970s, and the surge of interest in the interactions between literature and science sparked by Gillian Beer's *Darwin's Plots* (1983) prompted critical interest in the non-canonical Ruskin. In recent years, his science has received favourable attention in terms of his relationship to the work of other scientists, his growing sense of himself as a professional lecturer and his relation to gender politics.⁵ Even so, his science has only been recouped from 'rear-guard' to 'alternative' or 'dissenting'.⁶ Most of Ruskin's science *was* alternative when compared with the work of mainstream professional scientists of this period; but, as I will show, some facets of his geology were professionally current. Disregard of this aspect of his work can be found already during his life and immediately after his death in assessments which found the science relevant only in relation to his aesthetic criticism or literary style. An obituary of Ruskin in the *Journal of the Geological Society* maintains that 'we must not forget his services to our science, in directing the attention of artists and others to the effect of geological structure and of the characters of rocks on scenery'.⁷ In his obituary in the *Geological Magazine*, Ruskin is described as

'the great Art-writer and Critic of the century' who additionally possessed a little-known interest in geology.⁸ But this magazine, which had explicitly stated in 1864 that sub-standard work had no place in its forum, had published geological notices by Ruskin in 1865 and his series of seven papers 'On Banded and Brecciated Concretions' between 1867 and 1870. Ruskin's contributions would not have been published if they had not met the standard of the self-consciously professional magazine. Several of the papers even appeared in the magazine's most prominent position.

What is most striking about the *Geological Magazine's* obituary of Ruskin is that the writer mischaracterizes the methodology of Ruskin's papers within his own magazine. According to the obituary writer, these papers 'may still be read with intense pleasure for their wonderful word-painting'.⁹ In fact, the 1867–70 papers employed anything but 'word-painting'; they were intended to be highly technical. Here is a typical sample:

3. If a crystalline or agatescent mass is formed by addition of successive coats, I call the process 'accretion'.
4. But if the crystalline or agatescent mass separates itself out of another solid mass, as an imbedded crystal, or nodule, and then, within its substance, divides itself into coats, I call the process 'concretion'.¹⁰

Ruskin's project here is one of differentiation, of distinguishing phenomena and their properties into ever more discrete and specific parts. A combination of precision and profusion of information is intended to present a picture of an attentive, professional geologist. Given that Ruskin had delivered a lecture at the Royal Institution on geology two years earlier which did make use of figures of speech and elegant phraseology, I would argue that in the *Geological Magazine* articles, he deliberately stripped his science down to its empirical bones so as not to risk the very charge of 'word-painting' which might make the papers seem less professional. The irony is that even his self-consciously inelegant prose was remembered solely for its elegance. The obituary writer's mischaracterization suggests that he had not even read Ruskin's papers. It is true that by the time Ruskin died, he was a marginal scientist and knew himself to be so; but the obituary retrospectively marginalizes his geology of the late 1860s as well.

Ruskin's *Geological Magazine* papers also aligned themselves with emerging professional values in more ways than their style. A *topos* of incompleteness is both the origin of each of the papers and its sustaining force. About to discuss a question, Ruskin frequently inserts 'before we can touch on the question' and then alludes to, or even plunges into, an ancillary topic. Furthermore, at their conclusions, the papers always look ahead to unresolved

questions. This method encapsulates strategies at work in the contemporary literature of geological associations and periodicals for demonstrating the freshness and continued importance of the science. Ruskin's work in the 1860s and beyond also exhibited traits now considered essential to modern science. The question of glacier motion, the single debate to which he most persistently returned, was still very much open at this time. Far from being 'rear-guard', Ruskin was attacking a theory which has since been discredited. No one believes today that a glacier moves by cracking, filling the cracks with its melting run-off, and then freezing again, as Tyndall proposed in his well-received 'Regelation' theory. The idea that a glacier is viscous and therefore moves by flowing, originally stated by James Forbes and endorsed by Ruskin, is now held to be closer to its real mechanism of motion. The self-satisfaction with which each generation views its scientific orthodoxies always seems misplaced with hindsight, yet it was this self-satisfaction against which Ruskin was militating. I do not claim that in Ruskin an important contributor to geology has gone unrecognized, but some of his scientific values would be regarded today as healthy scepticism, and his geological work was considered professionally legitimate as late as about 1870. This aspect of his work went unacknowledged by geologists at his death and remains entirely unacknowledged today.

Despite Ruskin's evident inclination to produce professional geological work in the 1860s, why was he virtually ignored by the scientific community? One scholar of Victorian science has suggested that, given the unstable and contradictory climate of scientific research at this time, professionals were those who carefully assembled a body of prominent like-minded scientists around them.¹¹ Directing reception of work – in effect, marketing oneself as a scientist – became necessary for success. Ruskin desired to be part of a scientific community as well, and he picked up on this technique of broadcasting his connections. *Deucalion* contains a running correspondence highlighting the mutual professional respect between himself and James Clifton Ward, who was on the staff of the Geological Survey in Yorkshire. Ruskin includes entire letters from Ward in the text and repeatedly notes where Ward has pursued a question first posed by him. Ruskin also forefronts his personal professional relationships with others such as the physicist Oliver Lodge, Henry Woodward (Keeper of the geological department of the British Museum and editor of the *Geological Magazine* 1864–1900) and Henry Willett (an amateur geologist and benefactor of the Sheffield Museum).¹² But the community of like-minded scientists he formed around himself were not prominent scientific figures either during his lifetime (Lodge) or ever (Clifton Ward, Willett, Collingwood), belonged to the older generation (Forbes) or were dead (Saussure). Furthermore, Ruskin's authoritativeness

and wilfulness prevented him from having a truly equal or reciprocal relationship with any scientist. While subscribing to the idea of a scientific community, his claim in one of the *Geological Magazine* papers that he wrote it 'more with a view of obtaining some help in my own work than with any purpose of carrying forward the discussion of the subject myself' (26.41) indicates a somewhat different intention of having that community help *him* in his individual researches. His letters to Willett and Ward reveal how much time and energy he demanded from his correspondents. Ostensibly he and they were working together as equals; the reality is that for the most part Fletcher and Willett helped, and served, him.

In 1884, Ruskin wrote the introduction to a geological work, *The Limestone Alps of Savoy*, by his friend, secretary and former student W G Collingwood. Rather than addressing him as a colleague or a peer, as was the customary treatment of the younger generation of geologists by the older generation, Ruskin refers to Collingwood throughout as his 'pupil'. In the introduction, he recalls his surprise at Collingwood's geological aptitude and talents and also criticizes at length his shortcomings and oversights. Ruskin, in other words, extensively criticizes the very geological work which he was asked to introduce. What makes the introduction even more extraordinary is that Ruskin smugly announces a little experiment he performed on Collingwood. He had deliberately withheld from Collingwood important prior geological work on the same area because he was 'extremely desirous to see how far the conclusions of Professor Favre would be confirmed by an independent observer' (26.573). So, Ruskin writes:

I assured my friend . . . I would myself ransack the libraries of Geneva for whatever geological works could be of the smallest assistance to him. And so I did: but I only gave him those whose assistance to him was 'of the smallest!' – and locked Professor Favre carefully up in my own portmanteau. The result was absolutely satisfactory, and the corrections of his own views in points of detail which Mr Collingwood afterwards found necessary on comparing Professor Favre's sections with his own, were easily made and collected in the postscript to the third chapter. (26.573)

Ruskin deceives Collingwood and causes the younger man to spend his time redoing another geologist's work and later correcting his own work to bring it in line with the prior study. While outwardly valuing a spirit of co-operation, he here places creativity above building upon precedent and intentionally does not cooperate with Collingwood, treating him instead like a schoolboy. This approach hints at Ruskin's inability to reconcile his

assertive individuality – increasingly insistent and even shrill – with the conventions of scientific collaboration.

Another explanation for the retrospective dismissal of the professional aspects of Ruskin's geology is that during a time of a growing gap between professional and popular science, he wished to fit into both camps. In the 1860s, he produced radically different kinds of geological work: a popular geological lecture for the Royal Institution in 1863; *The Ethics of the Dust* in 1866, a highly unorthodox introduction to crystallography for young girls; and his series of papers in *The Geological Magazine* between 1867 and 1870. Huxley and Tyndall were themselves both professional researchers and popularizers of science, but Ruskin wished to be not only that but also a popular and professional practitioner of many disciplines. His perception of professional equality with Tyndall might seem justified, since he lectured at the Royal Institution three times in the 1860s, twice in the same month as Tyndall; but while Tyndall lectured exclusively on science, only one of Ruskin's three lectures was scientific (the others were on art and architecture). This was also the same period in which he was publishing the *Geological Magazine* papers. Ruskin did not aspire to intellectual possession of a field, believing that the 'interwoven temper' of his mind was responsible for the acuteness and reach of his ideas (35.56). But at a time of increasing specialization, interdisciplinarity was not looked upon favourably in professional circles. He considered himself an authority precisely because he worked in so many areas, whereas the conditions for being perceived an authority were changing. Many of Ruskin's reviewers from the 1860s onwards resisted the kind of epistemological interconnections which his popular audience found appealing, urging him to stick to his primary area of expertise, art.¹³

In the 1860s, then, Ruskin was a liminal figure, standing with one foot in professional geology and one foot in popular geology. A shift occurred, however, by the time he published *Deucalion*. An amalgam of popular and professional literary modes, lecture and chapter formats, science and other discourses, *Deucalion* defies categorization altogether. Ruskin constructs 'the alphabet' of minerals, gives an account of the three geological 'eras', and describes how weathering and erosion affect different kinds of rock. These parts of *Deucalion* read like a textbook, and indeed Ruskin intended it to be an educational tool. At times the work is also didactic, as he insists on the right ways of studying the natural world, and polemic, as he tears apart Tyndall's work on glacier motion. On the other hand, the technical letters from other geologists which he includes in liberal quantities, along with his own correspondingly technical notes on them, are clearly presented as professional research. By the time he wrote *Deucalion*, however, Ruskin was aware of his marginal status. He was aware that areas of apparent scientific

consensus had developed which he did not share. Much of *Deucalion* evinces a struggle against such marginalization, as Ruskin vacillates between a refusal to engage in debate and a scathing treatment of opposing theories, and between a triumphant tone and an irritated, even despondent one.

It has been widely held that the virulence of Ruskin's attacks on other scientists, particularly Tyndall, caused him to lose scientific credibility. Indeed, Ruskin's science is never coolly rational – in *Deucalion*, he elaborately satirizes Tyndall's accounts of his dangerous mountaineering in *The Glaciers of the Alps* (1860) and then wonders whether 'any stirring, or skimming, or other operation of a duly dynamic character, could have clarified from the scum of vanity the pease-porridge of his own wits' (26.283). But though Ruskin's words may have been more colourful, his tone was no more scathing or personal than that of any of the other players in the controversy. Still, Ruskin's argumentative fervour seems all the more excessive for its blind spots. His belief that Forbes had decided the question of glacier motion 'once and for all' obviates the kind of interrogation to which he himself was arguing that Tyndall's theory ought to be subjected. This sort of contradiction stemmed from a problematic distinction Ruskin made between fact and theory. While his distrust of theory was shared by many scientists of the period,¹⁴ he tended to regard theories with which he disagreed as 'vile speculations' needing to be discounted and theories with which he agreed as established 'facts'.

Ruskin tried to make the most of his marginal stance by positioning himself within *Deucalion* as an alternative who nevertheless possessed legitimate scientific authority. For those interested in learning about nature in the present day, these things 'are precisely what the geologists cannot tell you' (26.244). In one chapter, he sets up a hypothetical conversation between himself and 'the geologist' in which he figures as a little voice posing simple questions and uttering neglected but essential ideas, 'pursuing my poor little inquiry into these trifling matters' (26.244). He presents himself as an 'advanced student' (26.66) who is nevertheless *cleverer* than the modern scientists and makes *more* relevant observations. This role suits his ambivalent stance towards the science in which on the one hand he terms geologists 'Speakers concerning the Earth' (26.154–5) and on the other hand, finds that modern geology is misguided. Ruskin's position at the margins enabled him to participate without having to identify himself with other practitioners; but at the same time, it is evident in *Deucalion* that he still considered himself very much a professional – an outsider, but a professional nonetheless. He considered his own contributions to the science significant: he asserts that *Modern Painters IV* contains the most faithful drawings of the Alps and 'the first definitions of the manner in which their forms have been developed out of their crystalline rocks' (26.222), and that 'during the last

twenty years, none of my words have been set down untried; nor has any opponent succeeded in overthrowing a single sentence of them' (26.273). Ruskin's insistence on his scientific professionalism sat uneasily with his pose of 'advanced student' outsider. He wanted to be both a leader and a loner at a time when leading scientists depended on communal endorsement.

One of the ways in which Ruskin attempted to shore up his scientific authority was to draw on his other areas of professional expertise. In *Deucalion*, Ruskin's aesthetic expertise and literary virtuosity are at times turned into weapons against scientific orthodoxy. Aesthetics, for instance, help him to point out flaws in the diagrams of 'your modern geologists' because close examination reveals that these geologists drew the impossible scenario in which a folded rock bed at the top is double the length of the bed at the bottom. Diagrams of his own experiments on dyed layers of pie dough confirm these errors. Diagrams therefore allow Ruskin to evaluate the work of other geologists and then offer opposing evidence which readers can judge for themselves. Ruskin even suggests that accuracy and attractiveness go hand-in-hand, ruthlessly exposing Tyndall's failure on both counts and pointing out that his own diagrams meet both requirements.

Similarly, Ruskin's literariness becomes an important tool for constructing his authority in geological matters. Even though he complained that his work was too often valued purely for its literary merits rather than its scientific content (26.222), style is one of his most effective tools for convincing readers to understand geology as he does. So while he notes in *Deucalion*, 'I cannot finish my chapters with the ornamental fringes I used to twine for them' (26.123), he in fact 'twines' many an ornamental fringe throughout the work. His concern with etymology and correct word usage throughout *Deucalion* reveals his awareness that words are his province. He scoffs at bad classification, speculating that 'if this absurd practice of using foreign languages for the clarification of scientific principle still holds, what you, in compliment to Greece, call a "Dinothierium," Greece, in compliment to you, must call a "Nastybeastium"' (26.261). The thorough knowledge of the history and meaning of words is presented as an area of science in which he is an undisputed expert. Moreover, because Ruskin's scientific method is observational, it is necessarily descriptive. Being accurate and complex in his descriptions does justice to the scientific method and to the complexity he finds in geology itself, but it also grants him a measure of influence within the bounds he has circumscribed. After defining 'plastic', 'viscous' and 'ductile', he continues:

A plastic substance is not necessarily ductile, though gold is both; a viscous substance is not necessarily ductile, though treacle is both; and

the quality of elasticity, though practically inconsistent with the character either of a plastic body, or a viscous one, may enter both the one and the other as a gradually superadded or interferent condition, in certain states of congelation; as in Indian-rubber, glass, sealing-wax, asphalt, or basalt. (26.157)

There is unmistakably an element of virtuosity in this passage, a virtuosity which trumpets to his readers that he knows what he is talking about. The correct usage of words is more than a persuasive tool for Ruskin, but it is a persuasive tool. The knowledge of words, like the knowledge of images, is a source from which he expects to derive scientific authority.

At times in *Deucalion* an exultant tone predominates, when Ruskin points out the flaws and oversights of eminent geologists and asserts his aesthetic and literary powers. But even his precision is tinged with an excess that suggests a need to prove himself. A railroad whistle blew for two and a half minutes, he observes, and then adds a note at the bottom of the page: 'Counted by a watch, for I knew by its manner it would last, and measured it' (26.152). Hints of dissatisfaction with his scientific standing can be detected in the 1860s already, but the erratic, often strident tone of *Deucalion* reveals a sometimes mournful and sometimes angry realization that his work has not engaged youthful geologists. No student has taken up the unfinished work which he has again and again pointed out needs to be done (26.274), and he finds it 'somewhat hard upon me, now that I am sixty years old, and short of breath in going up hills, to have to compare, verify for myself, and reconcile as I may, these entirely adverse representations of the classical mountains of England' (26.287). Awareness begins to dawn of a gap between how he sees himself and how he must be perceived by others. While Ruskin wishes to see himself as the child who points out the emperor's nakedness, instead he finds himself in the position of a Cassandra.

Despite obvious frustrations, the sidelines were a fruitful place for Ruskin's geology. He accomplished there what he could not have accomplished had he continued to write in the stripped-down professional style of the 1867–70 *Geological Magazine* papers. For one thing, unlike other popularizers, he included polemic in his scientific work. Arabella Buckley and Charles Kingsley, both well-known writers of popular science, desired to stimulate readers by piquing their curiosity and encouraged them to use seeing as the key to understanding nature. Their goals and even their methods at times resemble Ruskin's closely. In *Madam How and Lady Why* (1870), Kingsley subscribes to the same ideas as Ruskin about the limits of proper knowledge and the balance among epistemologies, but he never moves from the general prioritization of humility to specific criticism. In *The Fairy-Land of Science*

(1879), Buckley avoids debate altogether, at one point noting that glaciers move very slowly, but ‘*how they move, we cannot stop to discuss now*’.¹⁵ How they moved was still a point of professional scientific debate at the time of the book’s publication. Ruskin, on the other hand, did not give readers an unproblematic picture of science. His popular science in *Ethics of the Dust* presents supposedly indisputable facts and straightforward means of finding them while at the same time noting that the facts *have* been disputed. As a result, he ends up giving his audience, both within the text and without, a somewhat embattled picture of science, politics and economics. In *Ethics*, when the Lecturer (who is a stand-in for Ruskin) asks the girls what it means to be alive, and they find the question ‘provoking’, the dialogue continues:

L: I do not see why it should be provoking to be asked what it is to be alive. Do you think you don’t know whether you are alive or not?

(Isabel *skips to the end of the room and back.*)

L: Yes, Isabel, that’s all very fine; and you and I may call that being alive: but a modern philosopher calls it being in a ‘mode of motion’. It requires a certain quantity of heat to take you to the sideboard; and exactly the same quantity to bring you back again. That’s all.

Isabel: No it isn’t. And besides, I’m not hot. (18.238)

Humour diffuses the possible threat posed by Tyndall’s theory in *Heat Considered as a Mode of Motion* (1863), but the science is still one of controversy. Ruskin’s devil’s advocate techniques encourage the children to see science as a field of debate and to bring their common sense and moral indignation to bear on this debate.

I would suggest that the polemical aspect of Ruskin’s popular science may explain the more favourable reception of the second edition of *Ethics* in 1877 (the first edition was a publishing disaster). Greg Myers has offered the explanation that ‘when its uselessness as crystallography was admitted, its usefulness to Victorian parents as a didactic moral fable could be rediscovered’.¹⁶ This is an interesting idea, but it is difficult to contend that readers would in fact have expected straightforward crystallography from the first edition either. A title such as the *ethics of the dust* would surely be a warning that the contents would not be purely scientific, and Ruskin’s preface makes it absolutely clear that the work will be whimsical. Moreover, the mid-Victorian reading public was hardly unaccustomed to or put off by the merging of science and morals.¹⁷ By the publication of the second edition

of *Ethics*, however, there was a more powerful sense that science could make statements immediately relevant to political economy, ethics and social interactions. After 1859, there certainly had been much discussion about whether Darwinian evolution degraded mankind; but the debates about materialism really took off in the 1870s when a synthesis of atomic theory, conservation of energy and evolution in the professional scientific community seemed to reduce life to mechanical forces.¹⁸ 'Scientific Naturalism' is the term taken up by Frank Turner for the movement in the second half of the century which achieved this synthesis and whose advocates extolled the unlimited power of a secular science. Advocates of scientific naturalism such as Tyndall, Huxley, W K Clifford and Herbert Spencer were prominently represented in leading scientific institutions and were effective publicists but their ideas did not escape resistance in the culture at large.¹⁹ Voices – both popular and professional – were increasingly raised to counteract the movement's limited view of existence. Rather than giving their children a mild storybook of scientific fables, parents might choose one which would encourage the right way to engage with the differing opinions they might confront. Instead of marking the 'end of a tradition' of the dialogue form as a method of popularizing science,²⁰ Ruskin's *Ethics* was a forerunner of the groundswell of objections to what was perceived as materialist science. The layered, eclectic and sometimes combative text appealed to a public feeling bludgeoned into a joyless conception of the natural world.

Ruskin's polemic popular science represents part of his contribution at the margins of geology, a contribution of criticism; but his opposition to materialist science finds its best expression as a positive assertion of the vitality of the natural world. In resisting the dogmatisms of scientific naturalism, Ruskin did not employ the sophisticated logical arguments that others used – for instance, that scientific men, whose very persistence in looking into things restricted to them or thought beyond them is what advanced science, now denounced inquiry into other unsettled questions (such as the existence of the soul) as unthinkable and unknowable – but his participation in the debate about materialism was both timely and relevant.²¹ A significant part of the dread of materialism was that it seemed to present a world not only devoid of the divine but robbed of life itself. Ruskin's marginal position enabled him to convey his particular vision of the vital workings of the natural world and its relationship with human life. Slippage from one kind of reading to another, a tendency to work around topics even while claiming to be direct, his continual focus on locating meaning: these are all strategies through which Ruskin communicated his alternative vision.

In *Ethics*, Ruskin remains elusive on the question 'What is it to be alive?' The 'materialist' view that life is a 'mode of motion' is dismissed early on, but

Ruskin takes his time in filling the gap left by modern science. He compares crystals with the girls in his audience more and more, moving from the analogy between the arrangement of atoms in a crystal and the appearance of his female audience when seated in an orderly fashion to an analogy between the moral characteristics of crystals and the moral life of the girls. At one point, he summarizes 'We have been hitherto talking, children, as if crystals might live, and play, and quarrel, and behave ill or well, according to their characters, without interruption from anything else' (18.325). Finally, the reader anticipates, he will explain that all of this has been an extended analogy. Instead he continues, 'But so far from this being so, nearly all crystals, whatever their characters, have to live a hard life of it, and meet with many misfortunes' (18.325). Rather than unmasking an analogy, Ruskin enters more deeply into it. Yet again, the girls ask him to be straight with them because 'we never understand how much you are in play, and how much in earnest' (18.340–1). Ruskin answers:

Neither do I understand, myself, my dear, how much I am in earnest. The stones puzzle me as much as I puzzle you. They look as if they were alive, and make me speak as if they were; and I do not in the least know how much truth there is in the appearance . . . All questions of this sort lead necessarily to the one main question, which we asked, before, in vain, 'What is it to be alive?' (18.341)

This assertion that the stones *make* him speak *as if they were alive* sounds very much like animism. Finally, he explains the crux of the position, which is that:

The presence of the spirit which culminates in your own life, shows itself in dawning, wherever the dust of the earth begins to assume any orderly and lovely state. You will find it impossible to separate this idea of gradated manifestation from that of the vital power. Things are not either wholly alive, or wholly dead. They are less or more alive. (18.346)

This recalls an earlier comment that people are always more or less asleep – that we have to prick ourselves awake if we are ever to see or do anything properly (18.331) – and also reminds us of his description of the purpose of the lectures: 'to awaken a "vital" interest in the subject of their study' (18.201). The subject of their study turns out to be their moral characters, and the interest turns out to be 'vital' in both senses of being an important interest and also a living, active interest. Ruskin never uses words carelessly, and

he makes explicit the etymological connection between virtue and vitality: 'The very word "virtue" means not "conduct", but "strength", vital energy in the heart' (18.288). This word 'vital', then, forms a bridge between the girls' physical lives and their developing moral lives. But it is the ambiguity, the sliding between science and ethics and etymology, which supports the bridge. If Ruskin had written in the style of the 1867–70 *Geological Magazine* papers, he would not have been able to move from one epistemology to another, nor would the girls have been forced to question these ideas for themselves. A marginal position, removed from the strictures of one professional discipline, therefore enabled Ruskin not only to cultivate a provocative ambiguity useful as a pedagogical tool but also to express the interconnections and layers essential to his view of the natural world.²²

Ruskin makes similar connections in *Deucalion*. In an 1880 lecture, he responds to one given by Huxley which had posited the development of snakes from lizards. By contrast, Ruskin supplies his own observations of the relations between species which are that 'all of these living forms, and the laws that rule them, are parables, when once you can read; but you can only read them through love, and the sense of beauty' (26.305). When he makes this lecture, entitled 'A Caution to Snakes', into a chapter of *Deucalion*, he changes the title to 'Living Waves' and adds a paragraph at the opening which links it to geology: 'I believe my readers will find their study of undulatory forces dealt through the shattered vertebrae of rocks, very materially enlivened, if not aided, by first observing the transitions of it through the adjusted vertebrae of the serpent' (26.296). Materials of the earth are included into the body of 'living forms' which are parables meant to be read with love, because the 'living waves' of snakes are meant to enlighten students about the 'living waves' of rocks. Ruskin's discernment of vitality in rocks enables inorganic particulars to be understood as living symbols. The exponents of scientific naturalism were offering explanations for how life was organized and operated, but they could not adequately answer what it did or should mean. Ruskin's marginal position enabled him to marshal the tools with which to address such a question. He said about an exegesis of Genesis that 'the marginal reading ... has definite value' (6.107); his own science at the margins is a science concerned not only with natural phenomena but with value itself.

This question he asks in *Ethics*, 'What is it to be alive?', is moreover, central to his scientific, artistic, social and literary projects. He asks this of the stones; he asks this of each individual human; he asks this of society. In *The Stones of Venice* (1851–3), he assesses the decline – or death – of societies on the basis of their moral health, as registered in artistic vitality. In *Unto This Last* (1860), Ruskin defines value as that which 'avails towards life' and

the proper science of political economy as that which 'teaches nations to desire and labour for the things that lead to life' (17.84–5). Much of his popular geology intends to show that earthly materials which seem to be dead are alive on literal and symbolic levels, and within a range of gradations (18.347). In an 1858 lecture he maintains, '[I]n a certain sense, and almost a literal one, we may say that iron rusted is Living; but when pure or polished Dead' (16.376–7), this because iron, similar to us, 'breathes' in air to become iron oxide. And the crimson colour of blood 'is connected, of course, with its vitality, and that vitality with the existence of iron as one of its substantial elements' (16.384). In making these connections, he slips from one discourse to another – from history to myth to empiricism to etymology and ethics and back again. In *Ethics*, Ruskin's ambiguous animism and his position that 'things are not either wholly alive, or wholly dead' leaves an important space in which meaning can be found. Vitality and virtue go hand in hand. It is in this blurred separation, or rather blurred connection, between the living and the non-living, where lessons about life can be found. Ruskin places an objection to his interwoven method in the voice of one of the girls who asks about his dream-vision of pyramid-building: 'But is not that only a personification?' He responds, 'If it be, what will you gain by unpersonifying it, or what right have you to do so? Cannot you accept the image given you, in its life ...? Take that rose crystal away with you, and think.' (18.232)

SWIMMING AT THE EDGES OF SCIENTIFIC RESPECTABILITY: SEA SERPENTS IN THE VICTORIAN ERA

Sherrie Lyons

When I left America in 1846, I believed in the sea serpent without having ever seen it.¹

Charles Lyell

For some time a number of ships had encountered 'an enormous thing,' a long object spindle-shaped, at times phosphorescent, and infinitely larger and more rapid in its movements than a whale.

*Twenty Thousand Leagues Under the Sea*²

'The year 1866 was marked by a strange incident, an unexplained and inexplicable phenomenon ... which disturbed the maritime population and exciting the public mind in the interior of continents.' Thus, began Jules Verne's classic science fiction adventure *Twenty Thousand Leagues Under the Sea*. Published in 1870, the novel began with reports of an unknown monster sinking a number of ships. Verne's description of the sightings, the rumours and the debates surrounding the creature's existence accurately reflected the sea serpent periodical literature of the time.

Although sea serpents are most often associated with myth and legend, in the nineteenth century they achieved a margin of scientific legitimacy that they never had before or since, capturing the imagination of a public trying to make sense of the vast new developments in science. The discovery of fossils, particularly dinosaurs, provided tangible evidence for the existence of creatures which had been regarded as belonging to the realm of the fantastic. As paleontologists discovered plesiosaurs and ichthyosaurs, a dramatic increase in sightings of sea serpents also occurred. With the increasing number of exploratory voyages, a wealth of exotic organisms never seen before were brought back to be dissected and classified in an attempt to comprehend the natural history of the planet. Since the sea was still relatively unexplored, it seemed entirely plausible that strange creatures might inhabit its depths.

However, establishing sea serpents as a legitimate topic for research was problematic. In particular, if the sea serpent was to be taken seriously, then its existence would have to be compatible with the prevailing views of earth history. Yet, these views were in flux in large part due to the tremendous advances that were taking place in both the natural and physical sciences.

By the turn of the nineteenth century, most geologists viewed earth history developmentally and argued that general trends through time could be detected on a global scale. This directional view of earth history was based on the geophysical theory of a cooling earth. By the 1820s, the theory enjoyed considerable prestige. Louis Cordier had shown that a geothermal gradient was not only real but universal. The application of Fourier's physics had been used to explain both the idea of a cooling earth through time and the geothermal gradient. The many new fossil discoveries documented the successive appearance of increasingly complex organisms. Georges Cuvier's work clearly demonstrated that organisms had become extinct and that entirely new faunas had replaced them. Cuvier's work along with the evidence of a cooling earth resulted in organic and geological change being linked, each providing support for the other. A directionally changing environment explained the directionally changing nature of successive faunas.³

However, Charles Lyell challenged this commonly accepted view of earth history. In 1830, his *Principles of Geology* contrasted what became known as uniformitarianism with the prevailing catastrophism.⁴ Of the several aspects to uniformitarianism, one is especially relevant to the sea serpent controversy. Lyell revived James Hutton's view that the history of the earth exhibited a steady-state pattern of endlessly repeatable cycles.⁵ Such a position contradicted a directional view of earth history and led Lyell to become the sea serpent's most eminent advocate. Rather than the successive appearance of increasingly complex organisms, Lyell believed that representatives of all classes of organisms existed at all times. He claimed that the fossil record only

looked progressive because of its incompleteness. Such a position was not unreasonable in the 1830s. Paleontology as a rigorous discipline was barely 20 years old. Lyell maintained that 'reading' the fossil record was like reading a book that had most of the pages missing and few words left on the remaining pages. Since sampling of the record was quite limited and displayed many anomalies, the case for progression was far from conclusive. However, as the number of fossil finds continued to rise, this position became increasingly difficult to defend. Although other aspects of Lyell's uniformitarianism were relatively well accepted, the fossil record combined with the geophysical evidence of a cooling earth meant his steady-state cosmology made few converts. His minority viewpoint received a further challenge in 1844 with the publication of the *Vestiges of Creation*. The *Vestiges* argued not just for transmutation but clearly portrayed the fossil record as progressive, with more complex organisms appearing through time.

In trying to make his case for a steady-state world view, Lyell argued repeatedly that negative evidence should be discounted. Just because certain organisms *have not been* found in the older strata did not mean that we should conclude they *do not* exist. Yet, sediments from the Silurian had preserved nothing higher than molluscs and a few fish while birds and mammals did not show up until the Mesozoic [Figure 1]. The fossil record continued, then, to be the best evidence that the earth's history showed progressive changes. However, rather than trying to find fossil evidence of modern species, what if Lyell could find evidence of ancient organisms existing in the present? A sea serpent would be just such evidence.

Lyell began to collect documentation of sea serpent sightings. In North America, there had been a few scattered sightings between 1800 and 1810. They were dismissed as sharks, porpoises or other real creatures and essentially ignored. However, in 1817, such a rash of sightings occurred that the Linnaean Society appointed a committee to collect any evidence of the creature. Witnesses were asked to provide written testimony describing what they saw, which was then signed and sworn before a magistrate.⁶ For the next several years, there continued to be several sightings per year near the New England coast and these were frequently reported in the national press. *The Niagara Patriot* reported that the serpent had been seen quite clearly in the harbour of Gloucester. Mr W Sargent along with two young men were fishing and observed what they thought was a hake approaching.⁷ It came within a few feet of Mr Sargent's boat with its head about a foot above the water and passed under the boat with a gliding motion, 'apparently without any muscular exertion'. He estimated the creature was about 70–100 feet long. He attempted to get a barnacle from the serpent's back, but when he struck the water with an oar, 'the Serpent sunk beneath the surface without any struggle

TABLE OF STRATA AND ORDER OF APPEARANCE OF ANIMAL LIFE
UPON THE EARTH.

				MAN by Remains.			
TERTIARY or CENOZOIC	Turbary.	Eocene Miocene Pliocene Pleistocene	MAN by Remains.	by Weapons.			
	Shell-Marl.						
	Glacial Drift.						
	Brick Earth.						
	Norwich } Crag.						
	Red } Crag.						
	Coralline } Crag.						
	Faluns.						
	Molasse.						
	Gyps.						
SECONDARY or MEZOZOIC	London } Clays.	Eocene Miocene Pliocene Pleistocene	Eocene Miocene Pliocene Pleistocene	by Weapons.			
	Plastic } Clays.						
PRIMARY or PALEOZOIC	Maestricht.	Cretaceous	Cretaceous	by Weapons.			
	Upper Chalk.						
	Lower Chalk.						
	Upper Greensand.						
	Lower Greensand.						
PRIMARY or PALEOZOIC	Weald Clay.	Wealden	Wealden	by Weapons.			
	Hastings Sand.						
	Purbeck Beds.						
	Kimmeridgian.						
	Oxfordian.						
	Kellovian.						
	Forest Marble.						
	Bath-Stone.						
	Stonesfield Slate.						
	Great Oolite.						
PRIMARY or PALEOZOIC	Lias.	M. Oolite	M. Oolite	by Weapons.			
	Bone Bed.						
	U. New Red Sandstone.						
	Muschelkalk.						
	Bunter.						
PRIMARY or PALEOZOIC	Marl-Sand.	Permian	Permian	by Weapons.			
	Magnesian Limestone.						
	L. New Red Sandstone.						
PRIMARY or PALEOZOIC	Coal-Measures.	Carboniferous	Carboniferous	by Weapons.			
	Mountain Limestone.						
	Carboniferous Slate.						
PRIMARY or PALEOZOIC	U. Old Red Sandstone.	Devonian	Devonian	by Weapons.			
	Caithness Flags.						
	L. Old Red Sandstone.						
	Ludlow.						
	Wenlock.						
	Caradoc.						
	Llandeilo.						
	Lingula Flags.						
	Cambrian.						

Figure 1. Richard Owen, 'Table of Strata and Order of Appearance of Animal Life Upon the Earth', in *Paleontology* (Edinburgh, Black, 1861), Figure 1. From the collection of the author.

and disappeared'.⁸ A Captain Rich specifically went searching for the serpent but was unsuccessful. Yet his failure to capture the serpent 'cannot invalidate the mass of evidence which has been adduced by hundreds of witnesses in favour of the appearance of such an animal on our coast'.⁹ By September 1822, there were sightings virtually every day 'by different individuals of the highest respectability'.¹⁰

In the mid-1840s, another rash of sightings occurred both in North America and off the coast of Norway. Lyell received a letter from the geologist Dawson who was collecting evidence of a marine monster that had appeared at Mergomish in the Gulf of St Lawrence in August 1845. Dawson wrote that 'the serpent had been seen by several respectable intelligent men very close to the shore'. They described it as 'being about 200 feet long with an obtuse head like a seal It was seen to bend its body nearly into a circle, to move in vertical undulations and to raise its head above the water and maintain it in that position for 15 minutes'.¹¹ Lyell had also written to a variety of other people including the zoologist G R Waterhouse and a Colonel Perkins of Boston who had seen the sea serpent in 1817. Colonel Perkins had written up a detailed account of the serpent and Lyell had a long letter from the man that included his 1817 notes. Similar sightings had occurred throughout 1844–6. In addition, in July 1845 and August 1846, independent accounts of a marine animal appeared in Norway. In the same period, while visiting Boston in 1845, Lyell saw advertisements everywhere announcing that Mr Koch was going to exhibit the fossil skeleton of 'that colossal and terrible reptile the sea serpent which when alive measured 30 ft in circumference'.¹² It was 114 feet long, with enormous jaws, and its ribs formed an ovoid-shaped body with the vestiges of a pair of flippers. Dr Koch had mounted the skeleton in an undulating shape and had its head raised in a position that corresponded to the description from most of the eyewitness accounts of the sightings off the coast of New England. He named the monster *Hydragos sillimannii* in honour of Benjamin Silliman who had given his support in favour of the serpent's existence in 1827. Koch told the public that this fossil was the Leviathan of the Book of Job. The fossil had been removed from a bed in Alabama that Lyell later visited. He determined that the fossil was a zeuglodon which Richard Owen had first described as a huge extinct cetacean.¹³ Virtually every day, Lyell was being asked if this great fossil skeleton from Alabama was the same species as the sea serpent that had been seen on the coast near Boston in 1817. One of the people to visit Koch's display was the anatomist Jeffries Wyman who realized it was a hoax, consisting of bones from at least five fossil specimens of zeuglodon. Koch's fossil may have been a fake but it was put together from bones of real creatures from the past. As more and more fossil zeuglodons and other organisms were being found at this time, the existence of the sea serpent seemed entirely plausible.

Of all the sightings of the sea serpent in the nineteenth century, the one that received most attention was made by Captain Peter M'Quhae of the HMS *Daedalus* on 11 October 1848 [Figures 2, 3] who claimed 'that had it been a man of my acquaintance I should have easily recognized his features'.¹⁴ Accounts of the incident appeared in virtually all the London papers, most of which Lyell clipped and saved. However, Lyell was not the only scientist following the news of the *Daedalus*. Richard Owen, who from the 1830s through the 1870s kept a scrapbook of articles and newspaper clippings of sea serpent sightings, was also interested. But unlike Lyell, he was not kindly disposed to the sea serpent of Captain M'Quhae. The *Annals of Natural History* published the long letter he wrote to the *Times* giving his opinion of the nature of the monster. From all the evidence, Owen concluded that what was seen was most likely a large species of seal, probably the *Phoca proboscidea* or sea elephant. Owen not only discussed the *Daedalus* sighting but mentioned several other supposed accounts of serpents. One by one, he discredited each sighting as evidence of a relic from the past.

Owen became embroiled in the sea serpent controversy for a variety of reasons. He used the debates over the existence of sea serpents to argue that science rather than judicial procedures should be the voice of authority in matters of natural history.¹⁵

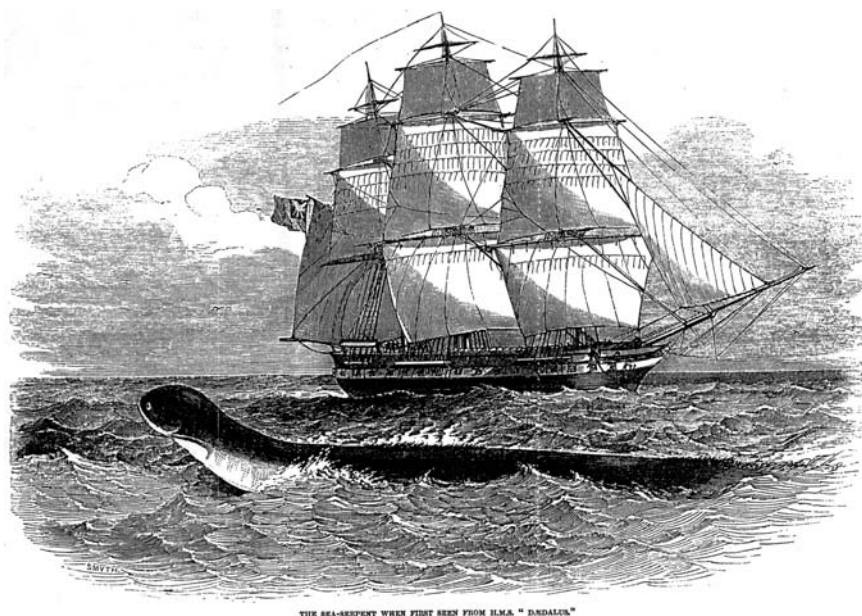


Figure 2. 'The Sea-Serpent When First Seen from HMS "Daedalus"', *The Illustrated London News*, (28 October 1848), p.264. By permission of the University of Liverpool Special Collections Department.

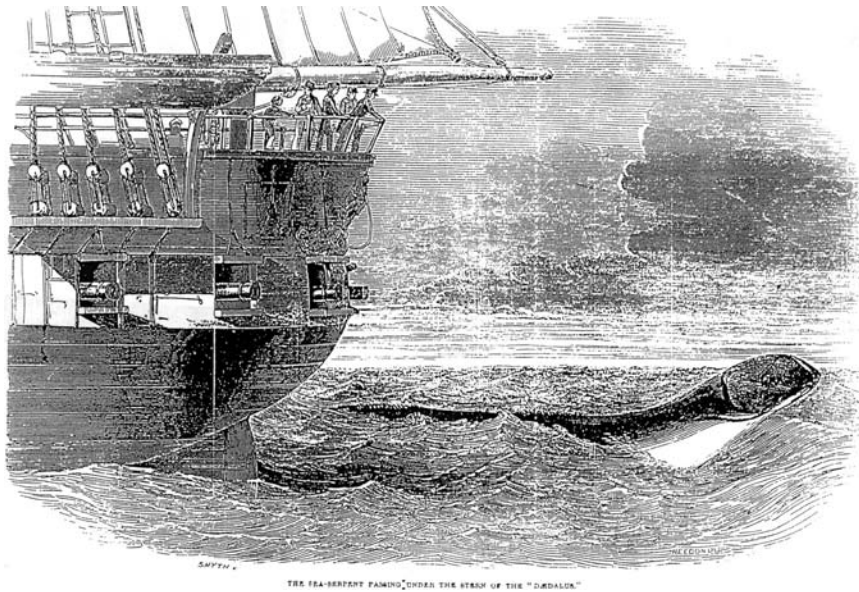


Figure 3. 'The Sea-Serpent Passing Under the Stern of the "Daedalus"', *The Illustrated London News*, (28 October 1848), p.264. By permission of the University of Liverpool Special Collections Department.

Scientific methodology and expertise were the proper means of establishing the truth. As the example of the Linnaean Society demonstrates, the language of the sea serpent literature was usually that of the courts. Eyewitness accounts, testimonies and written reports in the form of affidavits and depositions were all means of establishing the legitimacy of the serpent sightings. However, Owen rejected this evidence. Not only did he deliver a devastating critique of M'Quhae's description, but also he dismissed the entire collection of supposed sightings of sea serpents with the caustic comment that 'a larger body of evidence from eyewitnesses might be got together in proof of ghosts than of the sea serpent'.¹⁶

Owen maintained that all the eyewitness accounts were worthless if one did not have corroborating evidence in the form of actual specimens. He asked his readers: why have no remains ever been found in spite of extensive searching? If a sea serpent existed, the species must have been perpetuated through succeeding generations and many individuals must have lived and died. The coast of Norway had been thoroughly explored in search of Krakens¹⁷ and sea serpents, but not one skeleton, not even one bone of a nondescript or indeterminable monster had ever turned up. Although many Scandinavians believed in the reality of the Kraken, Owen pointed out that Norwegian museums were full of skeletons of whales, walruses, seals but no

monsters. Clearly having Lyell and the anti-progressionists in mind, Owen told his readers that the sea saurians of the secondary period were replaced in the tertiary by marine mammals. No remains of *cetacea* had been found in the Lias or Oolite and no remains of a plesiosaur or ichthyosaur or any other secondary reptile had been found in the Eocene or later tertiary deposits. No carcass of these ancient reptiles had ever been discovered in a recent or unfossilized state. In trying to discredit the eyewitness accounts, Owen wanted to demonstrate the implausibility of Lyell's view of earth history as well as take away the authority of the judiciary in deciding what constituted proper evidence.

Lyell was interested in the sea serpent because it would have provided evidence for his theory of climate, which in turn provided an explanation for his anti-progressionist view of the fossil record. Due to geological processes of erosion, and earthquakes, the distribution of the land and the sea changed, which in turn led to changes in the climate. His research indicated that geological and geographic changes did not seem directional at all. Rather, they appeared to be cyclical just as Hutton had proposed. Since species were adapted to their environmental conditions that were cyclic and recurrent, how could there be progression? As Lyell wrote to Gideon Mantell in 1830, his 'grand new theory would ... give you a receipt for growing tree ferns at the pole, or if it suits me pines at the equator; walruses under the line and crocodiles in the Arctic Circle'. Moreover, he continued, 'all these changes are to happen in the future again, and iguanodons and their congeners must as assuredly live again in the latitude of Cuckfield as they have done so'.¹⁸

Ironically, while Lyell's theory of climate made him interested in sea monsters as ancient relics from the past, the theory also led ultimately to his dismissing them as a real possibility. He recognized that environmental conditions were so complex, they would never repeat exactly. Furthermore, if the climate had truly changed, even if the change was cyclic, it would mean that organisms would not survive from a period of earth history when the climate was tropical. While Lyell may have believed that some day the plesiosaur might return, he also thought it was quite unlikely that a present day sea serpent would be a reptile. In the northern latitudes where the serpent was seen most often, reptiles were quite rare. In the immediate geological period, there was an absence of large reptiles, but in the colder latitude, like today, many huge sharks, narwhals and whales could be found. Lyell thought that a creature might exist that was some unknown species of one of these above-mentioned families and he saw 'no impropriety in its retaining the English name sea serpent'.¹⁹ Therefore, he claimed there was no reason to think a sea serpent could not exist, but he did not think it would be a reptile. Once again, Lyell used the same argument as when he was trying to make his case

against progression: not much weight should be given to negative evidence. However, if an organism was found, even if it was a different species but still representing a genus that was supposedly extinct, it would be a great boon to his anti-progressionist position, which he argued for throughout the 1850s.

While this account makes Lyell's interest in the sea serpent reasonable, it is incomplete. Lyell seems to have marginalized his own interest in sea monsters. He had an entire folder devoted to the sea monster, but he made no reference to them in his extensive scientific correspondence or in any published material with the exception of the *Second Visit to the United States of North America*. Why was he so reluctant to link the possible existence of sea monsters to his theory of climate and the history of life? The answer lies in situating sea monsters within the developments occurring in the newly created disciplines of geology and paleontology.

In spite of the widespread interest in sea serpents, their investigation remained at the edges of scientific respectability for a variety of reasons. First, sea serpents were so closely tied to mythology that many people considered such investigations to be pseudoscientific. Even if, for example, mermaids had been dismissed from serious discussions about inhabitants of the sea, this did not prevent the public, both scientists and non-scientists from trying to fill the void with other mythological creatures.²⁰ There also had been numerous cases of mistaken identity, even if these did not represent deliberate hoaxes.

A particularly embarrassing case involved the American Linnaean Society. At the time Lyell was in Boston, a snake with a series of humps about three feet long washed ashore and was brought to the attention of the Linnaean Society. Many claimed it was an exact miniature of the sea serpent of which there had been numerous sightings in 1817, and which had recently been seen off the coast of Boston. The Linnaean Society prepared a detailed monograph on the specimen, declared it must be the young of the famous 1817 sea serpent and gave it the impressive name of *Scoliophys atlanticus*. The identification of the serpent helped legitimize the Society as a scientific organization.²¹ Unfortunately, much to the Society's dismay, Louis Agassiz later identified the specimen as a full-grown common New England species of land snake – the humps on its back, the result of a diseased spine [Figure 4].²²

The practice of science is full of misconceptions and mistakes. This alone is not enough to discredit a particular area of investigation. Furthermore, one has to make a distinction between those people who believed in the sea monster of ancient legend and those, like Lyell, who thought that once found it would be similar to known animals – it might be a fish, a reptile or mammal and it would look similar to a living or, as Lyell hoped, an extinct organism. The literature of the nineteenth century documents a lively and serious discussion about the sea serpent with a heated debate going on in respected

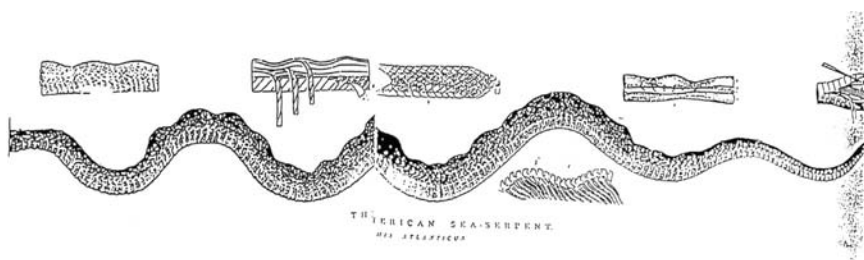


Figure 4. 'The American Sea-Serpent', in Eugene Batachelder, (ed.), *A Romance of the Sea Serpent or the Ichthyosaurus and a Collection of Ancient and Modern Authorities* (Cambridge, John Bartlett, 1849), p.117. By permission of the Boston Science Museum.

journals such as *Nature* and *Zoologist*. What kind of organisms were they and were they being mistaken for other known organisms? How did these descriptions compare with what the ancient saurians were believed to look like from fossil remains? How could these organisms survive for such a long period of time, especially if climatic conditions had changed from the past? Articles tied the search for the serpent to other types of investigations of the day.²³

Several prominent scientists clearly believed the sea serpent existed. Sir William Hooker claimed the serpent:

Can now no longer be considered in association with hydras and mermaids It [is] quite unconnected with the gigantic exploits of the God Thor or the fanciful absurdities of the Scandinavian mythology. We cannot suppose, that the most ultra-sceptical can now continue to doubt with regard to facts attested by such highly respectable witnesses.²⁴

Although Agassiz was responsible for discrediting the specimen of the Linnaean Society, from other evidence he claimed he could 'no longer doubt the existence of some large marine reptile allied to *Ichthyosaurus* and *Plesiosaurus*'.²⁵ Even that arch rationalist Thomas Huxley, who prided himself on his empiricism, wrote to the *Times* as late as 1893, 'there is no *a priori* reason that I know of why snake bodied reptiles from 50 feet long and upward should not disport themselves in our seas as did those of the Cretaceous epoch which geologically speaking is a mere yesterday'.²⁶

However, serpent investigations would remain marginalized because of boundary disputes surrounding the professionalization of geology. In the nineteenth century, a variety of distinct scientific disciplines emerged which often vied for power and authority. Indeed, the term scientist had only recently been introduced by William Whewell. While the gentlemen of science may not have earned their living as scientists, they had a definite

view of science they wanted to promote. They wanted distinct boundaries drawn between natural, religious and political knowledge, with natural knowledge being the most objective and deserving of superior status.²⁷

Whewell went even further in defining scientific practice. Although fellow member George Harcourt of the newly formed BAAS maintained that local observers could make valuable contributions to science by collecting and recording data, Whewell disagreed. Science was not just the collection of facts. Facts had to be connected to theory, 'even in subordinate contributions to science ... we might notice daily changes of the winds and skies ... which shall have no more value than a journal of dreams'.²⁸ If Whewell had no use for people accumulating meteorological data, certainly there was no place for amateur naturalists gathering information on sea serpents whose very existence was questionable.

With various kinds of inquiry jockeying for recognition as 'science', geologists in particular wanted to remove their investigations of natural history from the realm of myth and legend. Yet, it is not surprising that comparisons would be made between the fantastic fossil remains that were being discovered almost daily and the mythological creatures of ancient legends. Within anthropology, debates occurred about the meaning of myth and legend, some claiming that they should be taken as history and therefore represent some actual event in the past. Perhaps those mythological creatures did have some basis in reality. But investigations into fringe areas are always a risky career path to follow. For paleontologists who were struggling to achieve professional legitimacy, it was far safer to spend one's time constructing stratigraphical maps rather than seeking out illusive creatures that in the final analysis might still properly belong to the realm of imagination rather than the domain of the scientist.

Although the numerous eyewitness accounts provided compelling evidence of the serpent according to judicial standards, Owen tried to discredit these accounts in part by asking whom should we believe: a mariner or a professor of comparative anatomy? But that is precisely what people contested. Edward Newman, a respected amateur naturalist, claimed that 'the majority of our professors and curators would not know a whale from a porpoise, a shark from an *Ichthyosaurus* if they beheld these creatures in their native element'²⁹ and the professor's expertise was only relevant when creatures were stuffed with straw or reduced to skeletons or a few fragments were examined under a microscope. Newman would much rather trust the observations of the whaler, mariner or harpooner – the practical fisherman. It was an insult to people who made their living off the sea to claim that they would not know the difference between a school of seals or porpoises, a flock of birds and a sea serpent. But they worried that their claims would be disbelieved. Sailors often kept their sightings to themselves for fear of ridicule.³⁰

Newman recognized that a shaky consensus had emerged in the professional paleontological community and an unequivocal identification of a genuine sea serpent – that is a relic from the past – would ‘upset some cherished hypothesis, and lead to the necessity of numerous modifications of accepted scientific dogma’.³¹ Newman was referring to the progressive interpretation of the fossil record. Lyell’s steady-state views also challenged this interpretation and the serpent would have provided outstanding evidence for his anti-progressionist stance. But his minority position had already been the subject of ridicule [Figure 5]. He did not want his theory subjected to further criticism by having it associated with mythological creatures and a public all too quick to embrace the fantastic. Therefore, Lyell was unwilling to go public with his views and remained guarded in his private conversations with fellow geologists. Owen, however, was eager to use the sea monster controversy as a vehicle to promote his view of earth history, which in turn reflected the views of the scientific establishment.

Yet, as the anonymous author of an 1849 article entitled *An Essay on the Credibility of the Existence of the Kraken, Sea Serpents, and other Sea Monsters*



Figure 5. ‘Awful Changes’. Lithograph broadsheet by Henry De La Beche, 1830 in Frank Buckland, *Curiosities of Natural History* (London, Richard Bentley & Son, 1857). From the collection of the author.

stated, 'There are no monsters nowadays ... and incredible as the vague accounts of unclassified forms present themselves ... we may rest assured that if we could bring them within the reach of scientific examination, the supposed monsters would readily arrange themselves in the formulae of systematic arrangement'.³² He believed that the serpents would fill the gap between the cartilaginous fishes and amphibious reptiles – in particular the plesiosaurs.

The sea serpent brings to mind most often a creature of fantasy and legend. Yet, the serpent has a more complex history than such commonly held views convey. In particular, in the nineteenth century, the serpent resurfaced from the depths of sea lore as a topic worthy of serious scientific study. Cuvier wrote of the newly discovered fossil reptiles that their combinations of structure 'astonished naturalists [and] without the slightest doubt would seem incredible to anyone who had not been able to observe them himself The *Plesiosaurus* is perhaps the strangest of the inhabitants of the ancient world and the one which seems most to deserve the name of monster'.³³

The enormous increase in serpent sightings in the nineteenth century correlates to the discovery of fossil remains of ancient monsters from the past. However, Verne's *Twenty Thousand Leagues Under the Sea* provides additional insight into why investigations of the serpent peaked at this time. Verne drew on the Victorian desire to bring phenomena that were regarded as supernatural under the purview of scientific explanation. The novel is sprinkled with passages that could pass as an oceanographic text, but more importantly Verne's use of science provides the framework for the novel. Filled with encounters between the submarine *Nautilus* and various 'monsters' of the deep sea, the book captures both the fear and excitement of deep-sea exploration. Exquisitely outfitted, with every imaginable item of luxury, the *Nautilus* was a man-made monster, a work of genius, capable of amazing but also deadly feats. Its creator was 'a perfect archangel of hatred' who made the narrator and his two shipmates eternal prisoners as they explored the sea and discovered *real* monsters of the deep. Thus, sea monsters were portrayed as both fictional and real, but as embodied in the *Nautilus* itself, the sea monster represented both the utopian and sinister possibilities that advances in science and technology bring to society. The novel tapped into the Victorian anxiety over scientific findings that suggested we live in a materialistic world, devoid of spiritual meaning. Sea serpents, as a genuine, *natural* relic from the past, represented to Victorians a sense of continuity in a world that for many was changing far too rapidly. At the same time, the very elusiveness of the serpent left room for the imagination and gave hope to the idea that there would always be mysteries that would resist scientific explanation.

Although sea serpent investigations cannot be dismissed as mere pseudo-science, their marginalization is an example of how the increasing power and

prestige of science in Victorian England resulted in it gaining a virtual monopoly over the proper means of establishing the truth. Science has been so successful, in part, because in the words of Peter Medawar it has dealt with 'the art of the solvable'.³⁴ It can never be proven absolutely that something does not exist. However, the lack of specimens combined with what we know about the history of life from the fossil record eventually undermined the credibility of eyewitness accounts. This does not mean that every sighting has been accounted for, but it does mean that the elusive serpent did not fall into the category of solvable problems. While scientific developments were in large part responsible for the sea serpent's popularity and legitimacy, paradoxically those developments led ultimately to its demise as a topic of serious investigation.

‘THE DRUGS, THE BLISTER AND
THE LANCET ARE ALL LAID ASIDE’:
HYDROPATHY AND MEDICAL
ORTHODOXY IN SCOTLAND, 1840–1900

Alastair J Durie

The search for health is a constant in all societies, but the increasing wealth of Victorian society made it a realizable priority for an ever-widening clientele. And given the limitations of conventional medicine, with its reliance on techniques such as bleeding or drastic drug regimes, it was not surprising that a galaxy of alternative therapies offered themselves, which were then denounced as quackery by the profession. Some of the fringe movements were chimeras that flourished briefly and faded as quickly. Others showed more staying power, and among the weightier was hydropathy, a system centred on the use of a series of water treatments which originated in Austria in the 1820s at the Gräfenberg establishment of Vincent Priessnitz. Hydropathy was but one of several unorthodox medical therapies on offer in early Victorian Britain.¹ Where it differed from the other fringe movements – mesmerism, galvanism, botanism and even homeopathy – is that it attracted substantial long-term commercial investment. Whereas homeopathy, the most successful of its competitors on the medical margin, remained almost entirely a clinic and surgery bound enthusiasm, hydropathy established its place on both the therapeutic and the physical landscape. The first hydropathic, or water-cure, establishments in Britain appeared in the early 1840s in

and around London, subsequently spreading west and north. In England, it was, however, to become an increasingly provincial interest, surviving in its original curative form by the 1880s only at a few northern outposts such as Stockport. Hydropathy on the Continent had lost much of its momentum even before the death of Priessnitz in 1851. But in Scotland, hydropathy was a cause that showed staying power. After an early burst of enthusiasm in the 1840s, it stagnated in the following decade but regained impetus thereafter with a crescendo of promotions in the later 1870s:

The Scotch, who are pretty cautious in their undertakings, have plunged in a surprising manner into enterprises connected with Hydropathic Establishments. Within a very few years, above a dozen of these health resorts have sprung up in various parts of Scotland, north and south.²

The Scottish experience proved exceptional in the depth of commitment and its duration. Purpose-built cure establishments or 'hydros' were erected at Peebles, Pitlochry and Moffat, and in 1906, whereas there were some fifty hydros in England, many of which were quite small, Scotland had no fewer than fifteen, nearly all of which were sizeable establishments with 200 or 300 bedrooms available. In Scotland, the hydros had become favoured retreats and curative centres for the Scottish professional and middle classes. Nowhere in Europe had hydropathy taken such firm hold.

This study argues that part of the explanation for the success of hydropathy in Scotland lies in the close identification of hydropathy with that most respectable of all causes in Victorian Scotland, temperance. Hydropathy was less controversial than some of the other fringe therapies: the Scottish medical establishment took a much harder line in the early 1850s on homeopathy. Indeed, as hydropathy moderated its claims, so it became more acceptable to the profession: its value in the treatment of certain conditions, notably gout and rheumatism, became recognized. By the later nineteenth century, the medical curriculum was revised to allow some consideration of the positive benefits of this therapy, a change of which sons of the original pioneers took full advantage, a number qualifying with orthodox degrees.

News of hydropathy initially reached Britain in the early 1840s. Patients of Priessnitz who had benefited from his regime, such as Captain Claridge and John Gibbs from Wexford, became evangelists for hydropathy in letter and lecture. There were also British doctors, orthodox in their training, who themselves went to see Priessnitz in action, most of whom returned with positive views of this new therapeutic system. These included Dr James Wilson, who, with his friend Dr William Gully, was to

write extensively on the subject of hydropony. Priessnitz himself was no author, and it was left to others to explain the scientific rationale, describe the cases cured and systematize his treatments. An immense literature which was avidly read appeared in the 1840s and 1850s. There was, however, no equivalent in Britain to the Colleges of Hydro-therapeutic studies which sprang up in the Eastern United States, such as Dr Robert Trall's Hydropathic and Physiological School in New York, where hydropathic studies were taught in a scheduled curriculum.³

The initial reception given to hydropony by the medical profession was mostly hostile. It was lumped together with a range of other unorthodox therapies. 'Whenever any one of that triad of cheateries, mesmerism, homeopathy and hydropony is sanctioned, there, we say, is latent or open quackery' thundered *The Lancet*.⁴ There was, however, a long and respectable tradition of water treatments, notably at spas, and even cold-water cures for fever had their supporters, which may have partially restrained the criticism of hydropony (though not necessarily of the hydroponists). Unlike most quack systems, no promise was held out of quick miracle cures. Hydropathic treatment involved weeks of attention and a regulated regime. But there was professional hostility to hydropony, and it was reinforced by the lack of medical qualifications held by some of its early practitioner-advocates, and perhaps also by jealousy of their ability to make profits. Added to this was a dislike of its Continental origins, 'latterly issued from that hot-bed of absurdities, Austria'.⁵ There was a deep distrust of its founder, Vincent Priessnitz, an uneducated peasant, about whom many observers were sceptical. Much of this criticism of Priessnitz was *ad hominem* as much as scientific; he was foreign, uneducated and shifty. 'The quack cheat, PRIESSNITZ' is what *The Lancet* dubbed him in 14 November 1846. Even those who saw value in his system of hydropony were not necessarily convinced by him. In 1857, William Meikle was finishing his medical training at the University of Edinburgh before starting hydropathic practice in Aberdeen. His thesis discussed wet-sheet packing, which for Meikle was a valuable therapeutic tool. He owned that it was 'the idea of the Silesian peasant who first employed it' but allowed that 'we could perhaps have wished for it a more dignified origin'.⁶

Priessnitz's hydropathic methods seemed to work, which is what mattered to those seeking relief from their ill health, for whom orthodox medicine seemed to do little or nothing. Visitors to Gräfenberg, whether lay or professional, reshaped and selected his treatments into a package which worked for them, retaining the core but discarding elements which made no sense to them. Observers agreed that at its heart was a belief that 'curative treatment was effected through the instrumentality of the nervous system', that the water treatments could be used either to sedate or stimulate the patient.

Priessnitz saw the use of shower and bath water, externally, penetrating the porous membrane of the skin as augmenting the nervous system and flushing morbidity to the surface at which it would emerge as boils, the so-called crisis which was a visible sign that the process of cure was under way; perspiration induced by being wrapped in a sheet aided this process. It was a natural system of cure which introduced no alien or noxious substances, which, or so the purists argued, only irritated the body. Dr Gully summed up the principles of the water cure thus:

Pure water, pure air, proper diet and regulated exercise are the great agents in effecting the cure of disease by aiding the natural efforts of the body, through the instrumentality of the nervous system ... the due apportionment of these agents, according to the powers of the constitution and the phases of disease being ascertained by minute medical examination.⁷

There was, in fact, a considerable overlap between spa treatments and hydropathy. Both insisted on the value of good food and a controlled diet, fresh air and exercise; but whereas the skill of the spa physician lay in matching the particular mineral qualities of a spring to the needs of his patient, the hydropathic doctor used his judgement to prepare the right combination of water treatments for his client. What added credence to the scientific rationale were the results: people seemed to get better, and patients were prepared to go public in their support of their treatment.⁸ Although there were failures and even deaths while under hydropathic supervision, hydropathy did have a good enough record of cure and sufficient medical, therapeutic and scientific rationale to persuade some orthodox physicians of its value in some conditions. There was a sufficient coherence of scientific explanation, as well as empirical evidence in the form of cures, to convince Charles Darwin among others of its virtue. It was not just that it offered a less unpleasant alternative to the demands of conventional medicine, focused on the 'drugs, the blister and the lancet' but that it appeared to work. Early hydropathy had given the impression that it could be a universal panacea, but quickly came to focus on indigestion, gout, consumption, insanity, alcoholism and women's complaints, areas where orthodox medicine was achieving little. It also increasingly catered for those suffering from nervous conditions, whether the overworked cleric or the highly strung lady.

Hydropathy was not a static therapy. The core treatments of the wet sheet (in which patients were wrapped), the bath and the shower or the cold douche were adapted and new elements added including, for example, Gully's spirit lamp. From the 1860s, every significant hydro incorporated

a set of Turkish baths, which was an innovation pioneered in Ireland by Dr Richard Barter. Nor was the practice of hydrotherapy a closed system. Some practitioners remained absolutely loyal to the Priessnitzian doctrines but others were more eclectic, prepared to include elements drawn from allopathy and even galvanism or mesmerism. The Scots-trained Dr William Macleod, in charge of the renowned Ben Rhydding Hydro in Yorkshire, was a case in point: 'He employs other means (sanctioned by the wisdom of the orthodox portion of the medical profession) with considerable judgement in the treatment of various disorders at his establishment.'⁹ Drugs, even alcohol, and homeopathic remedies were deployed as he judged appropriate, grafting these onto a hydrotherapeutic regime. Qualified hydrotherapeutic doctors tended to take a more relaxed approach to other therapies and could draw on drugs and other elements of 'orthodox medicine' or indeed 'unorthodox' regimes such as homeopathy. P S Brown has argued that 'the gulf between the lay hydrotherapists and the regular practitioners was clear enough. The unqualified could find no virtue in orthodox medical doctrine.'¹⁰ But even lay practitioners such as Archibald Hunter did not endorse a blanket prohibition on the use of drugs, and generally, as time went on, the advocates of hydrotherapy recognized its limitations just as mainstream medicine brought itself to accept that some of the treatments, and the regime, had their part in dealing with specific conditions. A sympathetic publication said, 'hydrotherapy is a recognized phase of medical opinion, but its disciples must not bring it into contempt by riding their speciality to death'.¹¹ Hydrotherapy's later renaming as hydrotherapy and association to balneology and climatology brought it into the mainstream of orthodox medicine.

When, in the early 1840s, hydrotherapy first made its appearance in Scotland, the attitude of the profession was suspicious, and attempts in 1848 to establish a hydrotherapeutic ward in a Glasgow fever hospital were resisted. Thereafter, attitudes became more tolerant. For many doctors, the therapeutic regimes of the hydrotherapeutic establishments and the spas were not very different; they appeared to do some people good, and there could be little to criticize in a regime that laid such emphasis on 'clean living'. The prevailing attitude is caught in a 84-page pamphlet by a Glasgow physician, Dr Davidson, *The Sophistry of Empiricism*, which appeared in 1853, and was hailed by the Scottish medical press as an 'admirable exposure of the most concentrated kinds of quackery in vogue at the present time'. It dealt with mesmerism, homeopathy, herbalism, bonesetters and of course hydrotherapy, and it is noticeable that the last escaped the most lightly. Davidson would have no truck with the notion that it was a panacea: he worried about its direction by unqualified persons and doubted its theoretical rationale. But he did allow that the regime would help some and that water treatments

could be of service for gout and rheumatism. He concluded that 'Hydrotherapy, on the whole, is the most harmless of the false systems before us. Nay, we will go a step farther and admit that in many cases of chronic ailments it may be and has been of great service.'¹²

The harshest condemnation was reserved for homeopathy, which was brought under heavy fire in Scotland¹³ and was effectively outlawed. A doctor who practised homeopathy was dismissed from Aberdeen Royal Infirmary in 1872. Though hydropathic treatments were lampooned, there was no popular exposé of hydropathy to match the many directed against homeopathy.¹⁴ Other factors may have played their part in this easier ride. The first was simply that hydropathy in Scotland in the 1840s and 1850s had but a very minor presence. There was only one active hydropathic society, at Glasgow, but not much activity elsewhere. In England, the dozens of hydropathic practitioners – lay and qualified – and their many establishments may well have been drawing off business from the profession, but that was not the case in Scotland, where there were only two or three small operations at Rothesay, Dunoon and Aberdeen. None of these was achieving anything like the success at Ben Rhydding of Dr Macleod – thought to have amassed a fortune of over £100,000 by the time of his death in 1873. A second factor was the absence in Scotland of any scandals with which hydropathy in England was beset. There were deaths at Scottish hydros, including in 1862 the editor of the *Aberdeen Herald* and even an accidental shooting. But there was nothing comparable in Scotland to the controversy over the death in January 1845 of Sir Francis Burdett, allegedly as the result of hydropathic treatment for his gout or the trial two years later of a leading lay practitioner, 'Dr' James Ellis of Sudbrooke Park,¹⁵ accused of manslaughter through maltreatment. Of course, the fact that so much was made of these cases in part reflects how seriously hydropathy was taken in England and the desire of the orthodox establishment to discredit it; but in Scotland, there was no such pressure.

The first hydropaths in Scotland in the 1840s, few though they were, covered the entire spectrum from fully trained mainstream medical practitioners, who switched from allopathy to hydropathy, through some who were not medically ignorant if not formally qualified, to those with no formal background at all. Dr Rowland East was firmly in the first camp, a qualified surgeon, who had had himself visited Gräfenberg and observed Priessnitz in action. He returned absolutely convinced of the value of hydropathy as a 'powerful remedial agent guaranteed by fact and based on philosophy'.¹⁶ His therapeutic manifesto was hard-line: his was a hydropathic regime with no concessions: 'The drugs, the blister and the lancet are all laid aside'.¹⁷ Nevertheless, there were limitations: 'we are not of those who rashly assert that water cures all disease or that its effects are miraculous'. What was

distinctive about his establishment at Dunoon was that his curative programme stressed the virtues of sea-air ('as a collateral agent') and salt-water bathing, splicing in elements from a therapy that had become popular, that of salt-water treatment. Patients were encouraged to complete their treatment by bathing in the open sea; 'a dip in the briny billow may be given as a finishing off to the perfectly hydropathized frame'.¹⁸

His was a successful venture. It received a ringing endorsement from the egregious Professor Blackie, who first went to Dunoon in April 1849 under pressure from his wife.¹⁹ East dedicated his next (and last) book on hydropathy to Blackie. But within two years, East had gone from Scotland and from the hydropathic scene. He gave up his practice at Dunoon in 1851, moved south and shortly thereafter abandoned all medicine entirely for the Anglican ministry. The early 1850s seem to have been a hard time for the hydropathic cause in Scotland. In 1851, Dr John Paisley, who for several years had successfully run the Grasmere Hydro in the Lake District, had opened a Hydropathic Institute in Edinburgh. Paisley was a convinced hydropathic doctor but quite prepared to recognize the virtues of other systems, orthodox and new. 'The reformed practice of Medicine I understand to be a combination of Hydropathy, Homeopathy and Allopathy, and whatever exists now, or may in future times arise capable of giving relief to or curing the almost innumerable ills that sinful flesh is heir to'.²⁰ His venture lasted but a short time, and that left the Scottish scene rather bare. In the west, there was only Dr William Paterson, another Scottish doctor who had visited Priessnitz in person, with a small residential hydro in Rothesay. The main centre of hydropathy in Scotland shifted to Aberdeen, where the Rev Alexander Munro, with Blackie's support, had opened a small establishment.

Munro was an entirely different type of practitioner from either Paterson or East. He was no professional, but an outsider both in religious terms in Scotland as he was not Presbyterian, and unqualified as a medical man, though he did attend some classes at the University of Aberdeen in order to equip himself. A very influential figure in the Scottish hydropathic movement, he lacked both the means and the personality to be a Scottish version of John Smedley of Matlock, who pursued hydropathy and evangelical Protestantism with an equal passion. Munro, as a pastor in a small fringe Protestant denomination distinctly outside the mainstream of Presbyterian orthodoxy, fits the model of the radical outsider. He belonged to the Evangelical Union, which supplied a number of other key figures – James Kirk at Seamill, Alexander Stewart on Deeside, William Crombie at Melrose and John Becket at Dunblane – to the hydropathic movement in Scotland. His interests in medicine grew out of his belief that he and his colleagues needed to be able to minister to physical as well as spiritual needs among his

congregation. 'We are not recommending that a minister of the gospel should become a general medical practitioner, but a safe adviser in every ordinary case of disease that would come his way in the discharge of ordinary ministerial duties.'²¹ Not for him a narrow focus on matters of doctrine; his was an emphasis on the wider issues of the health of society. Moreover, his ministry in the Evangelical Union was not a well-paid calling, lacking as it did any manse or glebe to bolster the stipend, and the fees gained by treating patients or lecturing on subjects such as temperance and hydropathy were welcome additions. Money was always a problem for Munro, even though his hydropathic pamphlets sold well, and his hydropathic career was underpinned by the support of wealthy patrons, themselves either members of the Evangelical Union or other minority denominations such as the United Presbyterians.

The common denominator between Munro, his backers and the clientele at his hydros was the temperance cause. A strong supporter of Munro was John Davie, a retired Dunfermline draper and man of many causes; Chartism, Peace, vegetarianism, antivivisection, women's suffrage and of course temperance. Davie had been instrumental in the formation at Dunfermline in 1830 of the first Total Abstinence Society in Scotland. Temperance and hydropathy were in the words of James Haughton, an important Irish supporter of both causes, almost 'twins by birth'.²² The connection had been there from the first arrival of hydropathy in Britain when temperance found in hydropathy a therapy which it could endorse without any qualm. Father Mathew, whose work in Ireland and in Britain earned him the title of 'the Apostle of Temperance', sent Priessnitz one of his temperance medals, praising him as 'one who had much promoted the cause of temperance'. The connection was to be found everywhere; most of the early hydropaths were temperance advocates by conviction and the MD which Dr Ralph Grindrod, a hydropathic doctor at Great Malvern held was a Lambeth degree awarded in part for his services to temperance. Munro was, therefore, one of a like-minded group who were as likely to communicate with each other over temperance as hydropathic matters, and who possessed a wider vision for their cause in society. Dr John Balbirnie, who had a career as a hydropathic practitioner both in England and in Scotland, insisted that 'the cause of abstinence from intoxicating drinks and the Water Cure contain such seeds of moral principle and social regeneration as render them not only the hand maids of science and humanity but the allies of religion'.²³

Yet, with time the relationship faded; in the later nineteenth century, even some Scottish hydropaths relaxed the prohibition and a few hydros even began to make wine available. It happened earlier and more generally south of the border. The old austerity persisted at places such as Stockport

and Llandudno, where Kenworthy and Roberts would allow no departure from the old ideal. But in Scotland, the ban on alcohol for patients and visitors alike remained more firmly in place and for longer. Alexander Stewart's experiments with an extract from red Peruvian bark to treat alcoholism at Heathcot Hydro near Aberdeen were widely reported and strongly endorsed by the religious press.²⁴ What in the Scottish context made hydropathy into a movement of substance and persuaded hard-headed Lowland accountants, lawyers and financiers to invest in it was the identification with the temperance cause. And Munro was the key figure in this key association. It was not the science of hydropathy that made it respectable in Scotland: it was the regime and culture.

A pastor of some ten years' standing, Alexander Munro had begun to make his mark in hydropathic circles in 1848, when he started to publish *The Journal of Health*, in which, according to his introductory address, hydropathy and its regime were to be the chief topic.²⁵ It ran for two years and was reasonably successful, the circulation rising from 800 to over 2,000. But as he lacked medical training, the medical queries that readers sent in were referred to a practitioner in England, probably Dr William Macleod. That this was a deficiency, he recognized, and in January 1850, he informed his readers that 'the Editor is at present in attendance on a course of medical study, with a view to being able to exert a greater influence in the promotion of health, and this with the cordial consent and cooperation of his congregation'.²⁶ The records of Aberdeen University show him to have attended courses over the next few years in Chemistry and Anatomy (1849-50), Surgery, Practice of Medicine and Midwifery (1851-2 and 1853-53) but he did not take the full degree. Significantly, as part of the desire for respectability, his son Alexander Munro was to qualify as a doctor at Aberdeen in 1870 and to assist his father as physician in residence at both Waverley and Forbes.

This pattern of sons pursuing medicine in the steps of their unqualified fathers was widespread. Among other pioneers whose sons followed this road were Archibald Hunter, of Glasgow and then of Bridge of Allan, Andrew Philp, owner of the hydros at Rothesay and Dunblane and William Crombie, Evangelical Union minister and in charge of the St Helen's annex to the Waverley hydro in Melrose. The sons thus avoided the hostility that their fathers experienced over their lack of professional credentials, and it was yet another stage on the road to respectability for hydropathy. Munro, for instance, had come under heavy fire in June 1861 for his rashness in styling himself 'Dr' on the title pages of his publications on the basis on an award of the degree of MD from the New York Hygeio-Therapeutic College, a radical institution chartered by the legislature of New York.²⁷ The controversy had been sparked off by a reviewer in the Scottish press of Munro's *Guide to*

Hydropathy stating that he was not an educated medical man, to which Munro had objected. The critic had then asked in *The Scotsman*:

Will AM [Alexander Munro] be pleased to inform the public where, and to what extent his medical dedication was pursued; if he was, or now is, a registered medical practitioner, and if the MD he now attaches to his name was obtained after due examination in a British University or lately conferred on him by an American hydropathic or pseudo medical college or university.²⁸

The attack, it should be noted, was more on his presumption than his practice.

Another to call himself 'Dr' on the basis of a doubtful American degree was Archibald Hunter. Embracing hydropathy after a successful business career in Glasgow, although a staunch United Presbyterian elder, he had no known interests in other causes. Hunter lacked any medical training, although in later life, he was to insist that he had supplemented private study with attendance 'of an interrupted kind'²⁹ at the physiological and anatomical classes of Glasgow University. He was himself an enthusiast for colonic irrigation, flushing out the impurities of the system and for the role of diet in cure. Two of his sons qualified as doctors at Glasgow University: Francis, who died of tuberculosis in Australia aged only 27 and William Bell, who became Medical Superintendent at Smedley's Matlock Hydro. William was a well-known authority who contributed the article on Hydropathy in the ninth edition of the *Encyclopaedia Britannica* (1881); his other writings included an essay on the cold-water regime in fever. If Archibald Hunter was little known outside hydropathic circles, his second wife, Annie Steedman, was distinctly radical; an ardent food reformer and vegetarian,³⁰ she outlived her husband by 20 years, dying in April 1914.

With Hunter's death in 1894, the end of the lay hydropath in Scotland was very much in sight, with only Alexander Stewart of Deeside left of the old school. By the 1890s, virtually all the other Scottish hydros had – and had had for many years – a qualified doctor in charge of the medical department. This change in personnel – towards the professionalized physician – was paradoxically paralleled by a change in function, a reorientation from cure to comfort, a trend which was well established by the mid-1880s. It was not a change of which Hunter approved, and he saw the spate of commercial failures in 1882 and 1883 among the newer Scottish hydros as a deserved reckoning for their falling away from the old ideals:

The fact that so many of our Scotch Establishments, (luxurious retreats of fashion rather than retreats from care and business or

household labours) have succumbed to the pressure of hard times, ought not to mislead society as to the true cause of their failure. [They are] hydropathic only in name ... It has been proved by the lamentable failures of late years that not making the curative element a primary feature, the commercial basis is otherwise too narrow a foundation for success.³¹

The change in the focus of the Scottish hydros was a process that had begun in the 1860s as they had become the business and cultural retreats of the better-off among the temperance cause in Scotland. By means of special funds and subsidy, their personal appreciation of the benefits of a period at a hydro was shared among the deserving of that ilk: church workers, ministers and missionaries, people who needed a break rather a cure. Hydropathy *per se* was no longer the rationale for the hydros. There were still invalids but they were now in a minority. There had always been a need, even in the early days, given that hydrotherapeutic cures took time, for entertainment and amusement for the patients either indoors or outside: this side of matters had come to be central rather than supplementary for the growing number of guests who were not undergoing sustained treatment. As William Chambers observed in 1878:

On water alone, applied externally or internally, does not rest the main attraction of these institutions. Whatever they may have been at the outset, they are now simply resorts for a pleasant and healthful means of recreation, along with simplicity of diet, and the beneficial effects of pure air and unimpeded sunshine.³²

A guidebook writer some 15 years later agreed with him about the changed focus of the Scottish hydros:

At most of these establishments – in summer at least – the baths fall quite into the background. The best part of the visitors are young people who as yet know nothing of rheumatics or sluggish livers, so wet packs and cold douches will be naturally in less demand than tennis balls and dance music.³³

Whereas many of the new hydros which had opened in Scotland during the mania of the period 1877–82 had quickly become insolvent at heavy loss to their shareholders, the older hydros consistently stayed in profit, less because they remained orientated to hydropathy (although that helped) than because they were not handicapped by ambitious building and heavy borrowing.

Among these steady performers were the hydros at Seamill, Rothesay, Crieff, Forres and Deeside where the last of the line of Evangelical Union ministers to turn to the hydropathic business was in charge. Alexander Stewart had had, like Munro, a small church in Aberdeen, and also was prominent in the temperance movement. He shared, therefore, core values and interests with Munro, whom he knew well. Ordained in 1864, he then embarked on a programme of medical studies spread out over the next ten years, which approximated to what would have been required for a full degree. Stewart was a prolific writer and debater on matters theological, scientific and hydropathic: a paper on Evolution, for example, which he had read to the University Students' Medical Society, was issued as a pamphlet in March 1875.³⁴ In the following month, Stewart became Medical Superintendent of Heathcot Hydro some six miles outside Aberdeen. He subsequently obtained three degrees from American institutions, the first of which was an MD in 1877, an LLD the following year, and finally a DD from an Anthropological University in St Louis, Missouri.

By the later years of Stewart's career, it was evident even in this bastion of the old traditions that the cultural tone of the hydro was far more important to the clientele than the curative. The Lord Provost of Aberdeen in 1900 spoke warmly of his hydro as a place under Stewart's management which was conducted on religious principles, always a 'safeguard and a certificate that people could send their families there'.³⁵ The great value of this hydro lay – in his view – in the way that through 'a few days' rest and recreation there, great benefit could be derived by those who were run down, particularly the gentler sex ... worn out with the continual worry of housekeeping'. The prospectus, while mentioning the baths and their experienced attendants, stressed the luxury of the rooms, the house facilities, the golf course and fishing. Hydropathy had largely been forgotten, and the true patient was a rare sight. And while after his death in 1909, one of Stewart's daughters briefly acted as Managing Director, the son who in 1907 had qualified in medicine, Alexander Graham, never followed in his father's footsteps and moved away to a practice in Margate. The dynastic line was broken. This change in function had become a general feature of the Scottish hydros: the cure department was no longer central. As one journalist observed, 'hydropathy in its present day application has really lost its dictionary meaning. It is really a social cult and a kind of social curative agency rather than a treatment for the ailments of the flesh.'³⁶

Essentially, hydropathy had become more of a respectable culture than a radical therapy; the hydro a place for right-living folk rather than the really ill; for conversation rather than cure, billiards as much as the baths. It was a trade-off: as hydropathy became more about regime, diet, exercise and a range

of water treatments, so it gained acceptance among the medical profession, who were inclined to view it as a system which could do some good rather as the spas did. The two systems came together in the professional and the public mind, and indeed there were similarities. Both required the provision of entertainment and facilities for the recovering patient and their family or friends. Hydros found their profit in catering for the visitor who needed rest and recreation more than for the patient who needed cure. The hydropathic establishment had become a hydro-hotel, a high-class temperance institution, respectable and relaxing. The old hydropathic system, at the fringe of medical therapies, had mutated into a bath-based range of treatments to which orthodox opinion took no exception.

ANNA KINGSFORD: SCIENTIST AND SORCERESS

Alison Butler

Victorian England witnessed the curious intellectual union of scientific naturalism and the belief in magic as these two seemingly opposed systems of thought rose to prominence in the second half of the nineteenth century. The doctrine of the former was formulated in part by the demands of scientists such as Thomas Huxley and John Tyndall. Naturalist thinkers wished to establish a scientifically directed culture and to eradicate religious belief and other such 'superstitious' thought entirely. Scientific naturalism maintained that belief in any non-physical agencies was superstitious and indicative of a culturally dysfunctional society. The doctrine of the latter was expressed in esoteric societies such as the Theosophical Society and the Hermetic Society and sought to establish, within the natural world and governed by natural laws, the mystery and spirituality traditionally associated with religious thinking and belief in magic. The people who belonged to these esoteric and magical societies that were springing up at the end of the century understood magic as the interaction of the human will and imagination with visible and invisible correspondences within the natural world in order to effect change. The expression of this belief is provided below in an address given by William Wynn Westcott, a London coroner and co-founder of the late nineteenth-century magical society, the Hermetic Order of the Golden Dawn, to members of the Order's Isis-Urania Temple in London:

If you find a teacher who can call down Rain from a cloudless sky, you do not see a Miracle, you see only that your master is possessed of more

knowledge of nature's laws, and has more powerful faculties than you yet possess. It is a folly to think that any finality of science has been reached by modern Thought; it is equally a folly to think that no knowledge once common to the learned has ever lapsed.¹

Despite the differing goals expressed in these two doctrines, it is apparent from the belief expressed above that there were similarities in their methodology and in their origins. Scientific naturalism claimed that the natural world revealed by the senses was the only true reality. Adherents to mysticism and magic claimed the same; however, they were not convinced that the senses and the abilities of the senses as recognized by science were, as yet, definitive. Both were attempting to reformulate an understanding of how the world operated according to natural principles and correspondences, and both stemmed from disillusionment with the Christian faith and in response to giant strides in scientific methodology.

On the one hand, this disillusionment led to the outright rejection of religion in the form of scientific naturalism, popularized by leading scientific figures such as Huxley and Tyndall; on the other hand, disillusionment led to an opposing result demonstrated in an attempt to restore the Christian faith through a reversion to a perceived pure state of the religion in the midst of a set of intellectual and popular movements. This set included Hermeticism, theosophy, spiritualism and occultism, all of which were enjoying increasing popularity at the same time that scientific naturalism was digging its claws in against the perceived folly of religion. Proponents of these somewhat marginalized movements and systems of beliefs found themselves engaged in the dispute between science and religion. Not wishing to discard all of the accompanying attributes of the Christian faith, these men and women sought a reconciliation of science with religion and turned to the basic tenets and literature of Christianity, esotericism and western magic to provide the terminology. Esotericism was understood as a form of hermeneutics characterized by exclusive access to the comprehension of symbol, myth and reality through an individual progression of illumination of a higher level of knowledge.² Western magic was understood as the synthesis of various systems of thought including Neoplatonism, Gnosticism, Hermeticism, Christian cabala, astrology and ritual magic, which was first fully developed in the Renaissance by scholars such as Marsilio Ficino, Pico della Mirandola and Cornelius Agrippa.

One of the most prominent supporters of this type of reconciliation was Anna Bonus Kingsford (1846–88), an extraordinary Christian mystic and seer with a medical degree and a passion for social reform. Her odd assortment of interests included magic, medicine, antivivisectionism, women's

rights and vegetarianism. Kingsford represented an unusual blend of scientific and magical ideas, and, despite her opposition to certain scientific methods and ethics, her own work was very much influenced by the scientific mentality of the era. The dogmatism and intolerance that characterized the scientific naturalists also lay behind Kingsford's doctrines and beliefs, as did the understanding that the world was governed by natural principles and correspondences. Victorian scientific naturalism and Kingsford's Christian esotericism emerged in the same intellectual environment in an attempt to reconcile the same intellectual hurdles and, therefore, they shared common traits.

Kingsford joined proponents of scientific naturalism in condemning the popular form of the Christian faith in the late nineteenth century. At the same time, she lashed out against these same materialists, using their own methods, language and theories, for their neglect of the spiritual. Ahead of her time, Kingsford championed esoteric Christianity through lectures on science and sorcery, and through her own scientific and magical activities. Her actions contributed to a major reaction against scientific naturalism at the end of the nineteenth century that held that science was not the enemy of religion and indeed supported the values expressed in the Christian faith.³ This reaction came from proponents of both science and religion alike. Kingsford represents the same desire for reconciliation of science with religion as was expressed in this later reaction; however, she came neither from a strictly scientific nor religious background, but rather from an esoteric, scientific, spiritual and magical background. An examination of her life and her scientific and magical activities will demonstrate the participation of the marginalized population that she represented, in an intellectual battle that characterized late-Victorian England. Through such an exploration, the unexpected yet logical compatibility of Victorian science and magic will also be uncovered. It will be shown how, culturally and socially, the assimilation of these apparent opposing systems of thought was made possible in nineteenth-century England and why it was an entirely appropriate and sensible marriage of ideas.

Before embarking on this examination, two pitfalls must be identified and avoided. In order to study this apparent paradoxical emergence of two seemingly disparate intellectual movements, it is important to understand that systems of thought mingle. There is no clear delineation between the separate strands of the intellectual history of nineteenth-century England. It is only the objectivity and the remoteness of modern scholars that allow for such an indulgent view of history. That one of these systems of thought is associated with magic presents another potential stumbling block for the scholar. For in the academic study of magic there is a tendency to indulge in what has been

fittingly called 'present-centredness'. T G Ashplant and Adrian Wilson coined the term in order to describe the propensity to study the past while simultaneously maintaining perceptual and conceptual categories of the present.⁴ 'Present-centredness' involves the transferral to the past of an assumption that beliefs of the present do not require explanation. This means that beliefs similar to present-day beliefs are also left unexplained, therefore giving them a privileged status. For example, Ashplant and Wilson argue that the academic study of magic in the western world frequently fails to explain the emergence of rationalism and scepticism in a world dominated by magical thought, yet attempts to explain why this magical belief exists as if it is the anomaly.⁵ The study of magic is particularly susceptible to this type of present-centredness because of the traditional understanding of magic as belonging to the primitive and the irrational. This approach to studying the past will be of no use here and will only serve to further marginalize certain 'sidelined' sciences. In order to understand how a scientifically oriented figure such as Kingsford dabbled in black magic in support of her scientific convictions, we must attempt to understand science and magic as she did rather than as exclusive and disparate notions.

Kingsford's scientific career began in 1874 when she set out for Paris to commence studies towards a medical degree. She completed her degree in 1880 with a thesis on vegetarianism, *The Perfect Way in Diet*.⁶ For some time, Kingsford practised as a physician in London but her interest in religion and esotericism soon overshadowed her medical work and she dedicated much of her time to writing books and giving lectures to spiritualist associations, vegetarian societies, the Theosophical Society and her own creation, the Hermetic Society. Her debut esoteric work *The Perfect Way, or the Finding of Christ* was published in 1882 and was written in conjunction with Edward Maitland (1824–97), her partner in spiritual work. Maitland, a mystical writer and former commissioner of crown lands in Australia, joined forces with Kingsford in her literary and spiritual pursuits, as well as in her crusade against materialism and vivisection, which will be addressed in more detail below. *The Perfect Way* is the most comprehensive record of the theological system Kingsford was attempting to establish as a literary peace treaty to the intellectual battle between science and religion. This system was supposedly founded at the beginning of existence and was inclusive of all religions. Its key elements were duality, universalism, pantheism, Gnosticism, Hermeticism and evolutionism, reflecting the intellectual atmosphere of the late nineteenth century. At the time of its publication, *The Perfect Way* was unmatched for its comprehensiveness, logic and appeal. Kingsford and Maitland were seeking a reconciliation of science and religion by offering a different interpretation of the opposing systems, one that placed them within a harmonious and

necessary balance. In this new interpretation, religion was criticized for having strayed from its original purpose as the 'culture of the soul' while the current state of science 'falsely so-called' was condemned for having surrendered credibility in its embracing of materialism:

science, – 'falsely so-called,' – in its ignorance of the nature of Substance, credits Matter with a power of evolution while denying to it the properties through which alone evolution can occur, namely, inhering life and consciousness ... Mysticism, on the contrary, existence – or, more properly, Being – and consciousness are terms synonymous and interchangeable; and all Substance, under whatever mode manifested, continues still to be, in some mode, consciousness. And inasmuch as Substance itself is incapable of development, in the sense of becoming more or better than it originally is, development is not of the qualities of substance, but of the manifestation of those qualities in individuated portions of it, a process which – consisting in the unfoldment of qualities already subsisting, but latent – may fairly be designated *evolution*.⁷

Kingsford uses the language and theory of science in a re-presentation of Neoplatonic and Gnostic doctrines. The spiritual and the material have become unbalanced in a Fall of the Church that historically represents the biblical parable of the Fall of Man. Recognition of the true nature of Man as a composite of both reason and intuition will lead the soul back to its proper path towards the divine and leave behind the temptation of matter and its accompanying limitations. This process is seen by Kingsford and Maitland as an evolutionary one: 'For the doctrine of creation by evolution is, as already stated, a true doctrine; and it is true as regards both man's physical and man's spiritual history.'⁸ Thus the authors agree with the founding principle of the scientific naturalists and concur that the current state of the Christian faith is irrational, confounding and erroneous. Unlike their scientific contemporaries, Kingsford and Maitland do not subsequently reject religion; instead they seek its cure through the re-establishment of a supposed original state of religion and a reinterpretation of it through science's most recently discovered doctrines.

In another work, Kingsford states the purpose of *The Perfect Way* and the system described therein as one intended to 'meet the prevailing urgent need of the age, for a perfect system of thought and rule of life, by re-constructing Religion on a scientific, and science on a religious, basis'.⁹ This balance was the goal of all of Kingsford's work, and her ability to identify scientific tenets with religious doctrine was a remarkable feat in an age in which religion and

science appeared to many as bitter opponents in an intellectual and perhaps spiritual conflict.

The balance which Kingsford sought between the spiritual and material in her esoteric work also extended to her understanding of the science of medicine. She emphasized the religious affiliation of medicine in the early days of its historical development under the patronage of various deities and its association with a priestly office. Kingsford embraced this perceived ancient understanding of medicine as a science dependent upon the health, not only of the body, but also of the soul. For her, the titles 'priest' and 'healer' were interchangeable:

The relation between soundness of soul and soundness of body was then held to be of the closest, and the health-giving man, the therapist, was one who cured the body by means of knowledge, Divine alike in its source and in its method.¹⁰

Not only did Kingsford perceive medicine to fall within the domain of religion, she also understood the proper application of medical science to be akin to what she called 'true magic'.¹¹ An evil twin, the unholy art of black magic grew alongside this true magic and developed into what Kingsford perceived to be modern medicine and what many of her scientific colleagues were practising. The difference between the two, for Kingsford, was that true magic worked in harmony and reverence with Nature and the divine, while black magic wrested Nature's secrets from Her through force. The latter was Kingsford's interpretation of the medical research being carried out in her day, an erroneous and evil form of true magic and true medicine.

Ultimately Kingsford's system was based on religion and a recovery of its untainted first form. How the knowledge of this supposed original and pure state of religion was revealed to Kingsford is quite interesting. She experienced visions in her sleep, claiming that no drugs or dream-producing agents produced them. Kingsford believed her visions were divinely sent to enable her to recover and present to the world this ancient theological system. She had other dreams and visions in this manner, which she put to commercial and practical use, drawing upon them to compile magazine stories – ghost stories and fairy tales – for publication and the collection of short stories *Dreams and Dream-Stories*.¹²

According to Maitland's biography, Kingsford also experienced spiritual possession while awake. A victim of chronic pulmonary illness, an affliction to which she eventually succumbed in 1888, Kingsford became a regular user of chloroform. Under the influence of this substance, Kingsford engaged Maitland in conversations in which Maitland believed that other

beings, spiritual beings, spoke through Kingsford.¹³ Spiritualism and its accompanying forms of mediumship enjoyed great popularity in late-Victorian England and such events were not uncommon in certain circles. The association of the passive female – more passive still if prone to illness – with strong powers of mediumship was well established. Spiritualists perceived illness as a potential condition for establishing the power associated with being a medium.¹⁴ Once more, as with her embracing of evolution, Kingsford was in fashion with her visions and voices. She believed her ‘skill’ in this area to be superior to that of others of her acquaintance. ‘So far I have not met any one in whom the dreaming faculty appears to be either so strongly or strangely developed as in myself.’¹⁵

One of the most interesting aspects of Kingsford’s belief system is her identification of science with sorcery. This identification is apparent in the many written works and lectures she gave on yet another of her passions, vivisection. Kingsford was radically opposed to this practice of scientific research having gained familiarity with its methods while in Paris.¹⁶ In her lectures, she vilified the vivisector as a black magician and a monster and claimed that the realm of sorcery had invaded the realm of science. Kingsford’s imaginative comparison juxtaposed the methods of vivisection with the blood sacrifices associated with the rituals of black magic. Part of this association of sorcery with science was no doubt in defence of similar charges being made against spiritualism. In an address entitled *Violationism: or sorcery in science*, presented to members of the British National Association of Spiritualists, Kingsford proposed to ‘shew that sorcery has indeed been revived in modern times to a considerable extent ... not in the domain of Spiritualism, but in that of “Science” itself’.¹⁷

In this address, Kingsford shows the close and obvious relationship vivisection has with science, the domain in which it originated and in which it thrived. She associates its practice with the tenets of science, namely materialism and atheism. At the same time, she categorizes vivisection as a ‘resuscitation of the old and hideous cultus of the Black Art’ and claims a family resemblance between the laboratory of the vivisector and the records of the medieval sorcerer.¹⁸ This simultaneous association of vivisection with both science and sorcery demonstrates that to Kingsford, and likely to her contemporaries, the scientific and the magical domains were not distinct and exclusive of each other. This merging of the two domains is in itself a reconciliation. Kingsford’s depiction of science as playing a magical role through sorcery shows that it was possible to perceive the scientific as magical and vice versa, a perception, indeed, possible throughout the history of western science and magic. This close relationship between a magical interpretation of the world and a scientific one is apparent in Kingsford’s work.

The practices of one scientist in particular outraged Kingsford to such an extent that she attempted to murder him through the occult projection of her will as a destructive force. This scientist was Louis Pasteur. In an address to the North London Anti-Vivisection Society in 1886, Kingsford called Pasteur 'Beelzebub', the 'agent of the Pit' and the 'demon of Vivisection'.¹⁹ Her loathing of vivisection was such that, with some success, she also attempted to murder two other vivisectioners in Paris while she was studying there, and employed the same means to finish off Pasteur but was here unsuccessful. These means were magical, positioning Kingsford as the good sorceress battling against the black magicians of science. She believed that, through the projection of her will, she could send an invisible destructive force to harm Pasteur and the other vivisectioners. Before recalling the murderous incidents that followed from this belief, how Kingsford commenced her magical career must be established. Her initial involvement in occultism began during her medical studies in Paris, where occult studies were enjoying growing popularity due to the development of French *occultisme*, an intellectual movement based on the belief and practice of magic, characterized by the addition of the tarot to a previously established mélange of esoteric and magical systems of thought. This movement was spearheaded by the great French occultist, Eliphas Lévi (Alphonse Louis Constant, 1810–75). The rise in popularity of *occultisme* in France predated and influenced the late-Victorian revival of ritual magic, and Kingsford was one of a number aiding its transmission to England. During her studies in Paris, Kingsford met a fellow student whom Maitland describes as a proficient occultist of black magic or sorcery.²⁰ This person, referred to by Maitland as Professor O, took Kingsford on as a student in an undescribed course of study that presumably concerned occult matters. Shortly after she commenced these studies, Kingsford began to have disturbing visions and dreams in which male demons attempted to seduce her. She believed that the central demon in these dreams was her occult instructor who was causing the dreams in an effort to gain power over her. Kingsford believed that she had fallen victim to an unwilling fascination for him. When she eventually broke free of this spell, Professor O was stricken with what Maitland calls 'brain-fever'. Both Kingsford and Maitland believed that breaking the hold of fascination in some way caused this illness.²¹

This was Kingsford's first encounter with occultism and her first attempt at magic, using her own will to break the hold of another's. Her success no doubt encouraged her to focus this same procedure to battle those agents of the Pit, the scientists involved in vivisection. Kingsford's first victim was Claude Bernard, a scientist experimenting on animal heat. In a passionate argument with one of her professors over Bernard's methods, Maitland claims

that she 'invoked the wrath of God upon him [Bernard], at the same moment hurling her whole spiritual being at him with all her might, as if with intent then and there to smite him with destruction'.²² Six weeks later, Bernard was dead. The official post mortem named Bright's disease as the cause of death, a disease Bernard was investigating by inducing it in animals. According to a mutual friend of Bernard and Kingsford, at the same moment which Kingsford hurled her spiritual wrath at the scientist, Bernard, who also held an interest in occultism, felt as if he had been struck with some poisonous substance. He never recovered from this attack.²³ This account is interesting in that it provides yet another example of a scientist holding occult beliefs, demonstrating the overlap of scientific thought with magical thought in the late nineteenth century.

Kingsford's second victim was a casualty in her magical war against her primary foe, that demon of vivisection, Pasteur. After the first success, Kingsford began to tailor her technique so as to use this perceived power, not in a moment of passion and rage, but as a directed and intentional tool against vivisectionists. In the summer of 1886, she accepted professional advice on this matter from 'a notable expert ... well versed in Hermetic and Kabalistic science' who had 'attained his proficiency in the best schools'.²⁴ It has been suggested that this was Samuel Liddell MacGregor Mathers, another founder of England's most influential magical society, the Hermetic Order of the Golden Dawn.²⁵ MacGregor Mathers would certainly have been qualified to give Kingsford the magical advice she required to work effectively against Pasteur. This advice was that she should acquire power over elemental forces on behalf of the animals being experimented upon, in order to direct these forces with her will against the scientists and to inflict physical suffering upon them.²⁶

Kingsford acted upon this advice, and as has been stated, managed to claim a victory, if only an incidental casualty in her attack upon Pasteur, in the form of Professor Paul Bert. She told Maitland that she had coupled Bert with Pasteur in her occult projections. 'I killed Paul Bert, as I killed Claude Bernard; as I will kill Louis Pasteur; and after him the whole tribe of vivisectionists, if I live long enough.'²⁷

Her sustained attacks against Pasteur did not result in his death, although Kingsford and Maitland believed they had achieved partial success when Pasteur set aside his research momentarily to recover from an illness on the Riviera.²⁸ Such success was not without its penalty, however, and Kingsford paid the price:

Whether I brought it upon myself occultly by means of my projections against Pasteur, which, not being sufficiently strongly impelled or

skillfully directed, recoiled upon myself – a supposition which I have some grounds for thinking probably correct – or whether the whole weight of my karma has fallen on me *en bloc* as a result of my entry upon a certain occult period of my career, matters not very much.²⁹

After her attack on Pasteur, Kingsford fell ill and never fully recovered.

In her use and understanding of magic, Kingsford provides important historical links in two different capacities. First, her interpretation of the use of the individual will in magic bridges the gap between a Neoplatonic comprehension and the late nineteenth-century one, popularized by the Order of the Golden Dawn. In Neoplatonic magic, the human will is joined with the divine will, yet the human will holds the more passive role. In later nineteenth-century magic, the human will gains much more control and independence in this union with the divine. Kingsford believed that the human will, when united with the divine will, can effect physical change. Thus she justified her murderous intentions and actions by the belief that they were divinely guided. This understanding of the individual will as instrumental in working magic is a development of the Neoplatonic understanding of magic and a marginalized form of an ancient magical technique called theurgy that obtains its power from a merging of the human and divine wills. In the fourth century in *De mysteriis Aegyptiorum*, Iamblichus described theurgy as a process of divine inspiration or manifestation in which the individual's imagination is manipulated by the will of the divinity.³⁰ Kingsford's development is evident in the positioning of the individual will as a more active force in the process. This development led to the very form of magic that members of the Golden Dawn – an order established in 1888, the year of Kingsford's death – went on to perfect.

Kingsford also represents an interesting link in the role of women in marginalized intellectual and social movements. As Alex Owen has shown in her study on gender and power in Victorian spiritualism, nineteenth-century women had access to power through their complete passivity as mediums.³¹ Kingsford, with her dreams and visions, is an example of this on the one hand, while on the other she represents a role for women that developed shortly after her death. Victorian ritual magic, as practised in the Golden Dawn, offered women the opportunity to become practising magicians and to learn techniques to use their will as a creative and powerful force. This active role was enthusiastically taken up by dozens of women before the end of the century and resulted in some of the most talented female magicians including tea-heiress Annie Horniman, the actress Florence Farr and the artist Mina Mathers (Bergson), sister of philosopher Henri Bergson. Kingsford was well acquainted with two of the Order's founders mentioned above, MacGregor Mathers and Westcott. Both had lectured at her Hermetic

Society and MacGregor Mathers dedicated his translation of a cabalistic text to both her and Maitland.³² If MacGregor Mathers was indeed the notable expert who advised Kingsford on her magical tactics in fighting the vivisectionists, then a definite example exists of an exchange of ideas concerning magical technique. Kingsford's influence upon the Golden Dawn was substantial by contributing to the development of the Order's theory and structure. In her magical beliefs and activities, she created a unique historical link to both the evolution of magical practice in the nineteenth century, and the role of women in marginalized intellectual pursuits.

Most importantly for the central question, however, Kingsford represented a curious blend of the characteristics of two seemingly opposed popular systems of thought. She may have been one of many who were seeking a reconciliation of science with religion in the face of a wide-scale lapse in the Christian faith and the untiring onslaught of scientific naturalism and materialism. Kingsford, however, was unique in her approach. The major reaction against scientific naturalism occurred in the 1890s, two years after Kingsford's death, leading to the development of an evolutionary natural theology that opposed the aggressive materialism and rationalism of scientific naturalism.³³ Kingsford's system of reconciliation came a decade earlier and was unique in its method and content. It demonstrates a grey area, apart from the apparent black and white of science and religion, and represents the intellectual and spiritual concerns of a sidelined segment of Victorian thought, that of the occultists. Her attempt to reconcile science and religion by recovering, through spiritualistic means, an original theological system characterized by pantheism, Gnosticism, Hermeticism, duality and evolutionism is indicative that the battle between science and religion was a far-reaching intellectual crisis affecting even the marginalized movements of spiritualism, esotericism and occultism. Her depiction of scientists as sorcerers, a depiction that deflected apparent criticism of the morality of spiritualism, questions any clear delineation between science and magic. The irony of Kingsford's subsequent magical attacks against these scientists, easily interpreted as intended acts of sorcery, further confuses any distinction.

The ability to merge major doctrines and tenets from both Victorian science and magic, as demonstrated throughout Kingsford's work, demonstrates their compatibility. Her work and influence were strengthened through their comprehensiveness of contemporary intellectual movements. Both Victorian science and magic emerged in their respective forms as attempted answers to the intellectual crisis brought on by scientific advances and religious disillusionment. Kingsford's attempt came in the form of a self-conscious expression of a multi-faceted reflection of Victorian intellectual life, thus breaking down distinctions between central and marginal quests for intellectual truth.

A SCIENCE FOR ONE OR A SCIENCE FOR ALL? PHYSIOGNOMY, SELF-HELP AND THE PRACTICAL BENEFITS OF SCIENCE

Lucy Hartley

Perhaps more than any other historical period, the Victorian age emphasized the link between the management of morals and the social health of the nation. If an individual could exercise control over his or her own behaviour, regulating undesirable propensities and developing positive faculties, then society would function according to the organic principles of steady growth, gradual development and eventual transformation.¹ The tradition of self-help books promoted this idea of governance in that they offered practical guides to the working and middle classes about almost any aspect of human life, from conduct to cooking. Samuel Smiles's *Self-Help* (1859) was a hugely influential early example of a genre of writing that has again become popular for its advocacy of the benefits of individual action as a means towards self-knowledge and personal development. Then, as now, this idea of self-help was underpinned by hard work and perseverance, both virtues that had currency in all classes of society.

It might seem curious to group practical guides to the mind with cookery books, but a defining characteristic of the Victorian period was its preoccupation with seeing the hidden patterns that lay beneath the surface of things. Of special interest were explorations of the workings of the human mind

because they provided insights into individuality and autonomy, and in particular the significance of will. The practices of physiognomy and phrenology fed this interest in mind and body, providing readings of external (physical) appearance that illuminated internal (mental) form.² Of course, attempts to describe the core of our nature, or what it is that defines us as human beings, have a long history that extends to the present day, most notably in the form of the Human Genome Project. However, the emergence of physiognomy in Britain in the early years of the nineteenth century, followed a decade or so later by phrenology, represents an important juncture in the history of the relationship between individual and society, when a specifically cultural notion of science provided a mediating nexus for the personal and the social. What makes physiognomy and phrenology worth studying is that they proffered a notion of science, loosely conceived, which relied on the idea of an intermediary – an enlightened observer – who interpreted their teachings for the general public and then offered instruction about how to practise physiognomy or phrenology oneself.³ Together they raise a number of important questions: were physiognomy and phrenology the exclusive preserve of the amateur or the professional? Did their teachings provide practical guides to human nature or an esoteric system of classification? To what extent were the discourses of religion or science used to underline these teachings?

Physiognomy and phrenology can be grouped together because they shared a common goal in seeking to delve into the recesses of the mind by interpreting the contours of the head, front or back; consequently, the strength of a particular attribute was always linked to its size. There were important differences in their practices, however. Physiognomy inferred the workings of the mind from the movements of facial expressions, whereas phrenology posited an understanding of the brain (as the organ of the mind) by using the bumps on the head to map different character attributes; so, the former was idealist in style and content while the latter was distinctively materialist. Importantly, the reception and influence of these practices were linked to the question of whether the logic of science and the beliefs of religion were compatible: the teachings of physiognomy were usually discussed in a religious context, so a knowledge of the moral self was linked with the state of the soul, but those of phrenology were understood in a developmental one, hence an understanding of the intellectual self was connected to the powers of the mind.⁴

Often called ‘pseudo-sciences’⁵ and sometimes ‘alternative’ or even ‘folk’ sciences, physiognomy and phrenology did not fit easily into the various moulds of science that were starting to take shape at this time. The boundaries between different forms of intellectual enquiry had not yet hardened into the disciplinary divisions that exist today; for, as Alison Winter has argued,

'early Victorian science was volatile and underdetermined, people could not agree about what one could safely claim about natural law, nor was it obvious when, where, and to whom such claims could be made'.⁶ Nonetheless, new advances in medicine and the human sciences did mean that 'doing science' required an increasing degree of technical training and expertise.⁷ By comparison, physiognomy and phrenology required little in the form of specialist knowledge or understanding; they were constructed as forms of science by their practitioners but the laboratory in which their experiments were conducted was ordinary everyday life itself. Hence their popularity rested on the assumption that science could be understood and practised by amateurs, outside the confines of the university or the asylum, the laboratory or the operating theatre. The consequence was that physiognomy and phrenology were received as something like 'do-it-yourself' sciences, which were important (and influential) because they galvanized interest in the nature of selfhood, and advanced the possibility that an evolving understanding of the self, and the ways in which it could be regulated, might contribute to collective social governance.

Taking the case of physiognomy in particular, I want explore the correlation between self, science and society that its teachings posit, terms that can be coupled together with ideas about the control and management of the individual. This is a matter of broadly historical as well as historically specific relevance. From the classical tradition of Aristotle⁸ to Giovanni Battista della Porta and Charles Le Brun in the sixteenth and seventeenth centuries, and also Johann Caspar Lavater to Charles Bell and Charles Darwin in the eighteenth and nineteenth centuries, physiognomy has been praised as a self-styled and popular science of man and also pilloried as a mystical and highly deterministic form of prophecy.⁹ But in recent years studies of physiognomy have emphasized its stabilizing influence on society; as Mary Cowling suggests, it was 'a means of bringing order into an ever-increasing, even bewildering variety of human types and social classes: a localized variation of what was being performed on a global scale by anthropologists'.¹⁰ Like Cowling's, critical works have tended to use artistic and literary examples in order to demonstrate the influence of physiognomy on descriptions of character and expression; thus, a picture has been painted of the effects of physiognomy as an interpretive tool without necessarily considering the ideas and assumptions that were the impetus for its teachings. By contrast, I want to offer a different perspective on physiognomy and its teachings, one that explores its scientific aspirations as well as its social uses, and so revises the critical orthodoxy on the subject. For it is my contention that physiognomy has a currency in the sphere of science that is of equal importance to its impact on the artistic and literary spheres. Therefore, I would like to examine three main issues: firstly, the claim that physiognomy was a

popular science of character, which was predicated on a common-sense intuition of an individual's appearance and behaviour; secondly, the tension that resulted between an understanding of physiognomy as a practical science for the masses compared to an esoteric activity reserved only for the specially initiated; and, finally, the extent to which these two issues allow us to tie the practice of physiognomy (and phrenology) to the growth of interest in self-help manuals in Victorian – and also current – times. These issues underpinned the appeal of physiognomy in two distinct phases of nineteenth-century British culture – the first wave from 1800 to 1830 and the second from 1870 to 1880 – and help also to explain its subsequent demise in the final decade of the century.

The publication of Johann Caspar Lavater's *Physiognomical Fragments, for the Promotion of the Knowledge and Love of Mankind* – initially in German between 1775 and 1778, then French from 1781, and eventually English from 1789¹¹ – was in large part responsible for the revival of interest in physiognomy in the metropolitan cultures of nineteenth-century Europe. The first publication of Lavater's work in English was an expensive five-volume edition, *Essays on Physiognomy: Designed to Promote the Knowledge and Love of Mankind* (1789), translated by Henry Hunter. A subsequent and cheaper edition was produced by Thomas Holcroft, *Essays on Physiognomy; for the Promotion of the Knowledge and Love of Mankind; written in the German Language by J. C. Lavater*, and translated into English by Thomas Holcroft (1789–93); this edition was reissued more than 18 times over the next 80 years and remained the one of the most popular scholarly editions of Lavater's work.¹² A collection of observations and aphorisms, with an eclectic array of illustrations and silhouettes – including some well-known names like Moses Mendelssohn and Laurence Sterne – Lavater's physiognomic teachings transformed that which was taken to be peculiar and distinctive to each individual into that which was common and shared by all individuals.

All that was personal about an individual's self was contained in the face, Lavater maintained. Hence, the 'thirteen faces of both men and women' (Figure 1) represent a variety of different facial expressions, and presumably a range of emotions too, that can be read and understood by smoothing out the details of each individual countenance and then imagining what each profile says about his or her character. A contemporary definition from *The Encyclopaedia Britannica* (1810) affirms this. Physiognomy, it says, is derived from Greek terms for nature and knowledge, and it denotes

the knowledge of the internal properties of any corporeal existence from the external appearances. [Physiognomists] among physicians, denote such signs as, being taken from the countenance, serve to indicate the



Figure 1. Johann Caspar Lavater, 'Thirteen faces of both men and women', in *Essays on Physiognomy: designed to promote the knowledge and love of mankind*, translated by Thomas Holcroft (London, William Tegg & Co., 1848), Pl. XII. From the author's personal collection.

state, disposition, &c. both of the body and mind: and hence the art of reducing these signs to practice is termed physiognomy.¹³

This suggests that the practice of physiognomy is akin to the practice of medicine; and, importantly, the ability to see facial expressions as signs of character is described as a skill, albeit an artistic one. But there is a contradiction here, between the physiognomist as scientific practitioner and as artistic inventor, which neatly captures the strengths and also the weaknesses of treating physiognomy as a mainstream, if popular, science.

Fundamental to Lavater's conception of mankind was an essentialist explanation of character, which was based on what was apparent (rather than imperceptible or occult) in human nature. 'Of all earthly creatures, man is the most perfect', he explained:

the most imbued with the principles of life. Each particle of matter is an immensity, each leaf a world, each insect an inexplicable compendium. Who, then, shall enumerate the gradations between insect and man? In him the powers of nature are united. He is the essence of creation. The son of earth, he is the earth's lord; the summary and central point of all existence, of all powers, of all life, on that earth which he inhabits.¹⁴

There was no way, Lavater confessed, that he could teach mankind all the Divine alphabet that would be needed to translate the language of nature; nonetheless, he could make some of its characters transparent to the enlightened observer as physiognomy was designed to reveal things in nature that eluded the immediate comprehension of the senses. Mankind might be flawed, but Lavater offered the possibility that an individual could improve his or her self-knowledge, as well as knowledge of other individuals and perhaps the Creator, by reference to external appearances. He thus explained:

Physiognomy, whether understood in its most extensive or confined signification, is the origin of all human decisions, efforts, actions, expectations, fears and hopes; from the cradle to the grave . . . from the worm we tread on to the most sublime of philosophers . . . physiognomy is the origin of all that we do and suffer.

Physiognomical teachings offered a compelling way of defining and explaining the scope of our instinctive responses to others, and Lavater stressed the fact that we make such judgements ordinarily in respect of an individual's character. Central to this claim was the discriminatory practice of the physiognomist,

who worked inductively in using particular observations to stand for general types and so discovering the universal truths that inhere in specific experiences. This was a way of seeing that could be practised by all people, provided they had the help of a little physiognomic training and also a belief in the benefits of a moral existence. So how did the physiognomist – the enlightened as opposed to the ordinary observer – make sense of these expressions?

The main task for the physiognomist was to render each appearance characterful; that is, to take the external form of an individual as exemplary of his or her internal character. However, it was not sufficient simply to look at a fellow human being in order to comprehend the ‘character peculiar to its nature and essence’. At the heart of the physiognomical system was a description of the natural kinds of existence, the main types contained in the natural world and manifest in the lives of mankind:

To know – to desire – to act – Or accurately to observe and meditate –
To perceive and to wish – To possess the power of motion and of resistance – These combined, constitute man an animal, intellectual, and moral being.¹⁶

These three kinds of self – animal, intellectual and moral – provided a means of categorizing human nature since each type had a distinctive physical character. The first was animal life, localized in the belly and including the organs of reproduction; the second was moral life, focused on the breast with the heart as its focal point; and the third was intellectual life, located in the head with the eye as its locus. Lavater claimed that the face was exemplary of these three classes of life in that it crystallized the strengths and weaknesses of an individual’s character; hence the mouth and the chin constituted animal life; the nose and cheeks were moral life; and the forehead to the eyebrows was intellectual life. This is conveyed in the illustration series ‘Four heads; comparison of man with monkey and man with bull’, which captures wonderfully the distinction between a man with animalistic features, who is like a bull, and one with intellectual features, who is like a monkey (Figure 2).

In sum, Lavater constructed a scale of personality traits in order to make the variety of human facial expressions comprehensible in relation to a fixed series of character types: on the one side were ‘normal’ attributes, such as intelligence and morality, and on the other ‘abnormal’ traits, such as stupidity and criminality.¹⁷ Consequently, the application of physiognomic principles hinged upon the reduction of complex (individual) expressions to simple (typical) emblems. A person could be identified by his or her facial features and then translated into a specific type of character; for example, according to physiognomic principles, a high forehead and full lips signified a person of

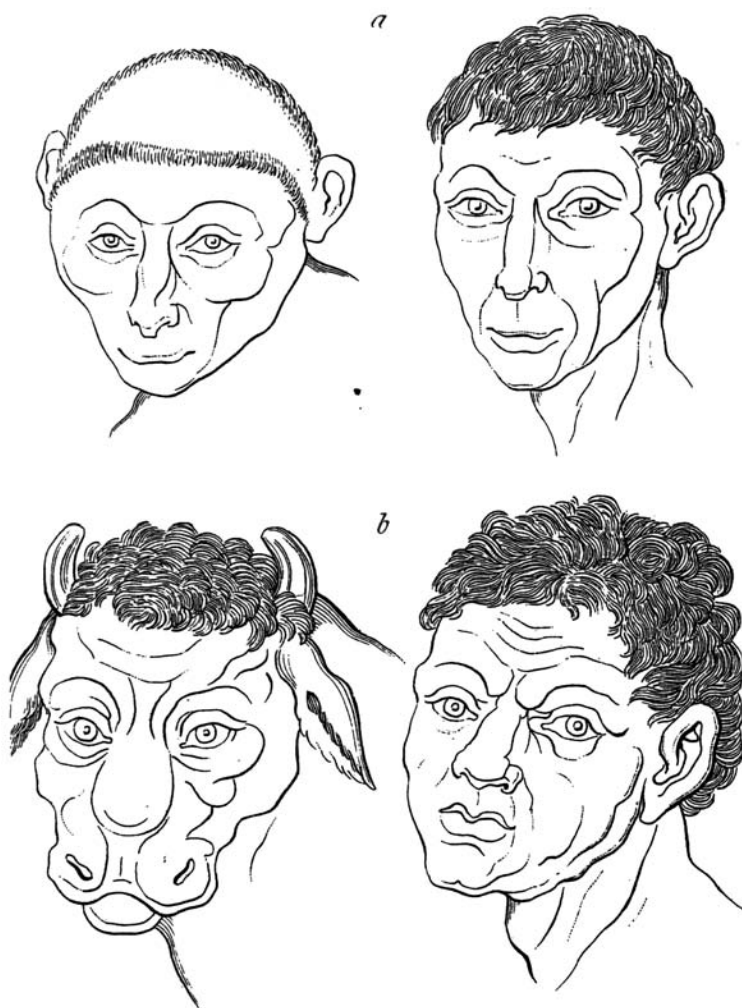


Figure 2. Johann Caspar Lavater, 'Four heads; comparison of man with monkey and man with bull', in *Essays on Physiognomy: designed to promote the knowledge and love of mankind*, translated by Thomas Holcroft (London, William Tegg & Co., 1848), Pl. XII. From the author's personal collection.

intellect with a propensity for the pleasures of the physical life. Put together, these classifications mark out a hierarchy of description: animality was linked above all to the function and structure of the whole human body and provided the lowest order of description, morality was found in the motions of the heart and was the middle order of description, and the intellect corresponded to the head and was the highest order of description.

To discuss highness and lowness in this form, almost as localized physical attributes, implied a progressive explanation of the development of creatures that made explicable the distinctions 'from the insect to man'. Yet such an explanation was not consistent with the moral framework constructed around Lavater's physiognomic teachings. This presented an interesting problem for, as I have indicated, the physiognomic practice of the nineteenth century was founded on a double-edged appeal, towards religion on the one hand and science on the other, as Lavater sought to stamp his teachings with the social might of the former and the intellectual prestige of the latter. Intent on acquiring something of the authority of a mainstream science – such as physiology – for his physiognomic practice, Lavater had contempt for those people who tried simply to read external appearance without grasping the internal animating spirit. The implication was clear: the practice of physiognomy was reserved for the specially initiated because it was an elite activity with particular skills that could not be learned by just anyone. It was also a practice that drew on the discourses of religion and science to make its points. This was not an uncommon position at a time when the traditional authorities of natural philosophy and natural theology were starting to be questioned by the new progressive ideas of transformation and scientific naturalism (and later evolution).¹⁸ Yet I want to suggest that the teachings of physiognomy (in the form proposed by Lavater) do not easily resolve the tension between the idea of a practical science for amateur enthusiasts and a specialized science for the skilled professional.

Lavater had a hugely optimistic faith in the positive powers of physiognomy: he believed his teachings could assist the progress of both individual and society, and so he proclaimed the practical benefits of this science of character and sought to convince the general public accordingly. Suppose we take the example of a man in the company of a stranger, he contended; the man will invariably 'observe, estimate, compare, and judge him, according to appearances, although he might never have heard of the word or thing called physiognomy; [there is] not a man who does not judge of all things which pass through his hands, by their physiognomy; that is, of their internal worth by their external appearance'.¹⁹ This proposition – that everyone practises physiognomy without necessarily knowing its principles – underlines the cultural power of Lavater's ecumenical teachings, but at the same time undermines the idea of a physiognomist as a practising scientist (or even artist) with select and specialized skills. Physiognomy colours all human activity and determines our relations with others, Lavater argued, because we make quite profound judgements about an individual instinctively (based on his or her appearance) without considering the reasons for doing so. Taken in their most positive light, then, the teachings of physiognomy constituted something like a self-help manual that educated an

individual about his or her own character – or that of another – and so fostered a knowledge of the self as a moral being that, in Lavater's opinion, could not fail to influence the natural development of social structures, customs and habits.

The problem was that the authority of physiognomy in a religious context was not sufficient to validate it in a scientific one. There is no doubt that physiognomy aroused some attention in the philosophical and scientific communities of the time due largely to the burgeoning interest in emotional expression; yet a number of leading thinkers (such as Alexander Bain and Charles Darwin) rejected a physiognomic account of expression in favour of a physiological understanding of the function of expression.²⁰ Advanced as a science of character, designed to discover certain fundamental principles of human nature, physiognomy worked against the grain of these new scientific ideas in that it offered what was, at once, a utopian and a practical form of observation, albeit from a limited empirical sample. The difficulty was that 'doing physiognomy' was held to be both a specialist and a populist activity, requiring the exclusive training of a professional as well as the comprehensive life-experience of an amateur. A science of character, as conceived on physiognomic principles, presented an account of the internal substance of an individual based on a series of largely intuitive responses to human action and behaviour. For example, this person has angular and knotty protuberances on the forehead and so has a vigorous, ardent and persevering character; that person has a nose that arches near the forehead, which denotes a person capable of command, rule and destruction; and the other person has very fleshy lips and so sensuality and indolence are the prevailing characteristics.²¹ These sort of common-sense intuitions appear to stand in for characterizations of the nature of mankind: any explanation of an individual must satisfy these characterizations, and yet they are simply descriptions that detail actual appearances rather than internal processes. So what was presented by a science of character based on physiognomic principles was not a consistent, systematic or substantial description of human nature. Rather, it offered an account of individuals based on a series of intuitive descriptions of the physical appearance of a select series of individuals.

To be sure, physiognomy assumed as fact that people judge appearances and took this as evidence of the underlying patterns that could be intuited from facial expressions; and it did so through an inductive method that made it impossible for the standards of expression to be much more than subjective measures of qualitative states. In theory, then, Lavater presented an understanding of expression that reported how individuals responded to each other in a specific environment. But in practice, he was simply stating that a system – in this case the personal selfhood of an individual – was

connected to an environment – in this case the social relations between individuals – without offering an explanation of the inner workings of the system. The negative consequences of this approach to human understanding can be conveyed by looking at an essay in *The Gentleman's Magazine* (1808) that suggested written references could be replaced by the physiognomic mode of interpreting the character of prospective employees via their faces:

The giving of characters, a duty which is seldom faithfully performed, either from pique or want of discernment, might have been dispensed with, each applicant carrying a certificate in his, or her forehead, eyes, nose, or chin, and would have been readily supplied with places, according as their masters had a confidence in one feature more than another.²²

The evil effects of physiognomy are all too evident here. Discrimination, or seeing difference, was the means through which physiognomy held to an essentialist method because it provided a means of reducing the differences of individual appearances into generalizations about those appearances. To fix difference was to see prejudicially, assigning individuals to groups or classes depending on the degree of normality (or abnormality) in their facial features.

The knowledge of expression and character derived from physiognomy was predicated on practical attitudes and widely held beliefs that were generally held to be reliable even though they were hard to justify. Thus, common-sense intuition was central to the practice of physiognomy: it underpinned the appeal to scientific knowledge that Lavater made in his teachings, and also meant that physiognomy shared meanings with the emerging language of self-help guides. These responses were personal and subjective, anecdotal and circumstantial; they may have been the product of an essentialist and determinist philosophy but could these responses be placed in the frame of scientific thought? It would be hard to find a stronger statement against Lavater's physiognomy – as a science of character – than that presented by Lewis Wolpert, who gives a robust defence of science as an unnatural mode of thought: 'doing science ... requires one to remove oneself from one's personal experience and to try to understand phenomena not directly affecting one's day-to-day life, one's personal constructs'.²³ That is to say, the conditions of everyday thought and action do not provide necessary conditions for scientific theorizing; hence Wolpert contends that common-sense intuition is incompatible with the activity of 'doing science', whereas physiognomy represents an attempt to use intuitive responses in the service of personal development and social betterment.

I have been suggesting that Lavater wanted physiognomy to serve a number of different agendas: it underpinned daily existence and yet required specialist training and expertise to be used successfully; it relied on a form of common-sense knowledge as the mechanism for physiognomical perception but not every individual could intuit the correspondences between the external form and the internal properties of humankind; and, finally, it exploited all the authority of religious orthodoxy while attempting to position itself within the remit of the sciences (with a rough approximation to an empirical methodology). In fact, Lavater's physiognomy relied on three basic assumptions: firstly, that a distinction can be drawn between subjects who look and objects who are looked at; secondly, that the appearances of individuals can be divided into the virtuous and the vicious; and thirdly, that there are essential categories for understanding human nature. These three assumptions had hugely negative ramifications, as the actions of determining the type of character as animal, intellectual or moral, and the notion of 'certificating the face' have shown. The populist ideals that prompted Lavater to create and advertise physiognomy as a practical science of character did not produce accurate observations or reliable comparisons of individuals. But it was very much the foundation of physiognomy on the intuitive principles of common sense that explained its widespread success – and eventual failure – as a popular science for all. Unfortunately, the recent reappearance of physiognomic ideas in debates about profiling, selection and social engineering suggests that our willingness to posit a link between appearance and character has not been diminished by the legacy of twentieth-century history.

SECTION II

Contested Knowledges

‘SUPPOSED DIFFERENCES’: LYDIA BECKER AND VICTORIAN WOMEN’S PARTICIPATION IN THE BAAS

Susan David Bernstein

In the February 1869 issue of *Belgravia* magazine, an essay entitled ‘Women and Men’ opens with a trio of questions: ‘What is a woman? What is a man? Are women men?’ The writer then takes issue with a particular ‘strong-minded woman’, Lydia Becker, who had campaigned for women to appear on the voting register in Manchester, and finally arrives at the heart of the article: ‘Although Miss Becker appears to have had her innings in the registration court at Manchester, her grand field-day ... was at Hull, when at the meeting of the British Association for the Advancement of Science she attracted one of the most crowded audiences, by reading a paper with this extremely grotesque title: “On the supposed Differences in the Minds of the two Sexes of Man.”’¹ As it happened, Becker’s title was slightly, though significantly, different: ‘On some supposed Differences in the Minds of Men and Women with regard to Educational Necessities’. While *Belgravia* dismisses Becker’s paper as ‘singularly illogical’ and ‘based on a delusion’,² the prominence given to its substance, including the three propositions that form the kernel of Becker’s argument, is noteworthy. Take, for instance, the second proposition: ‘That any broad marks of distinction which may at the present time be observed to exist between the minds of men and women collectively, were fairly traceable to the influence of the different circumstances under which they passed their lives, and could not be proved to inhere in each class in

virtue of sex.¹³ What accounts for the sensation of Becker's talk with 'one of the most crowded audiences' at the sessions? What was the status of women at conferences and in lecture halls where topics of science were addressed, and where the public understanding of science was shaped? How did Becker herself come to occupy this position on the podium, and how did she understand the significance of her contributions to this association?

The *Belgravia* article details the first of five lectures Becker presented at the BAAS's annual meetings between 1868 and 1874. Listed as a subscriber or a member from 1864 to 1889, Becker stands out in the Association's annual reports as the most visible woman participant in its first half-century. In 1868 and 1869, Becker presented two papers, the only woman to deliver talks in two consecutive years in two different sections in the nearly 40-year history of the annual meetings. These addresses encompass the vectors of her entwined commitments to women's rights and botany. Her first BAAS paper explores contemporary gender ideology in relation to science education for women, and the second paper pursues the etiology of a hermaphroditic flower; the first paper maintains that women are capable of the same intellectual pursuits as men, the second lecture demonstrates this assertion. While today Becker is typically noted for her prominent role in the Victorian feminist movement, in the 1860s she also published a book on botany, wrote another on astronomy, established the Manchester Ladies Literary Society and corresponded with Charles Darwin for several years. Since it was in this decade that Becker's efforts in scientific studies and feminist activism emerged and merged, her early involvement with the annual BAAS meetings indicates the intersection of Becker's botanical interests and gender politics. Becker's presence at and engagement with the conferences illuminate the complex trajectory of women entering into scientific debates, whether in the lecture hall, reading societies, formal scientific publishing or informal correspondence. As Becker's case demonstrates, the social organization and gendered power arrangements of her time structured the institutional practice of science. Becker's dual attentions in her BAAS addresses to gendered knowledge and to hermaphroditic plants likewise demonstrate the faltering of a binary of science (or nature) against society; her botanical analyses of hermaphrodites and her observations of the exogenous factors defining gender distinctions are imbricated positions.

In her 1867 address inaugurating the Manchester Ladies Literary Society, Becker highlights the importance of intellectual dialogue: 'Curious facts in natural history . . . become a source of perpetual interest if we are united with others in the prosecution of such studies, if we are sure of a sympathizing audience to whom we may impart them, and if we know that they will be recorded and preserved as a contribution to the sum of what is known on the subject.'¹⁴ Would the BAAS section meetings offer such a 'sympathizing

audience' for a female botanical researcher? If it were necessary to found a reading society of her own to explore scientific topics, how would Becker fare in an international association in which women's presence and participation were limited and controversial?

The BAAS was in the vanguard in terms of its admission of women to annual lectures, but full incorporation of women into all aspects of the Association was a contested process that took over a century if we consider that Kathleen Lonsdale became its first woman president in 1966.⁵ When the BAAS was founded in 1831, women were rarely active and full members of scientific societies.⁶ Becker's 1869 essay 'On the Study of Science by Women' observes that women were currently barred from the Royal Society, the Royal Geographical Society and the Royal Horticultural Society, although there they were admitted to the gardens. Becker chronicles at length the enforcement of 'scientific disabilities of women' that characterized most learned societies, a discrimination which extended to withholding awards for significant contributions, such as Caroline Herschel's discovery of five comets.⁷

In their history of the BAAS, Morrell and Thackray note that the presence of women at the meetings provided both social appeal and financial sustenance for the Association's success. Women were initially admitted, but only to the evening assemblies, such as *conversazioni*, ceremonial dinners and soirees, all deemed 'popular' occasions, where brief lectures and demonstrations might occur, with the logic that they would enhance the overall entertainment value.⁸ This arrangement promoted the annual summer conferences as an attractive venue for a family holiday and as a marriage market clearing-house. Eminent men of science such as Joseph Hooker and Charles Babington met their future wives (two in Hooker's case) at the BAAS, while Darwin recounts in a letter an enjoyable week he and his wife spent at the 1846 Southampton meetings.⁹

The admission of women to entertainment events at the BAAS was but the thin end of the wedge, for once they were in attendance women's interests in contributing to the sectional papers became evident. The sale of 'ladies' tickets', in effect from the 1830s to 1919, provided a way to monitor the number of women present.¹⁰ From 1832 William Buckland, an early BAAS president, opposed the admission of women to the sections where participants presented their work: 'Everybody whom I spoke to on the subject agreed that if the Meeting is to be of scientific utility, ladies ought not to attend the reading of the papers as it would overturn the thing into a sort of Albemarle dilettanti meeting instead of a serious philosophical union of working men. I did not see Mrs. Somerville, but her husband decidedly informed me that such is her opinion of this matter.'¹¹ Buckland's snipe about an 'Albemarle dilettanti meeting' alludes to the popular evening lectures

delivered at the Royal Institution in Albemarle Street, with similar events at the BAAS designed to supply this form of entertainment to families.

Mary Somerville was a complicated case in point regarding women's access to the sectional meetings, given her prestige as a scientific writer in Victorian Britain. Somerville managed to secure the respect of men of science in part because she also maintained the traditional domestic role of wife and mother whose professional activity involved writing in her home rather than speaking in public. Somerville's absence from the 1832 meetings may be attributed to limited funds and to exhaustion, as well as to her tendency to promote women's participation in science through 'quiet persistence' rather than 'public confrontation'.¹² Somerville's silence on the matter of women at the BAAS did allow for interpretations such as Buckland's of her absence as an argument for barring women from the sections.¹³ To have women present at evening lectures to accentuate the conference's social aspects was one thing, but to have them at sectional meetings where serious scientific work went on was a different matter altogether. Nevertheless, the presence of women directly affected the reporting of science. In his talk on marsupials, Richard Owen felt obliged to report findings on reproduction 'as delicately as possible' due to the attendance of 'Mrs Buckland and lots of ladies, mostly Quakeresses'.¹⁴ In the early years of the Association, women were restricted at sectional meetings to separate galleries or cordoned-off spaces.¹⁵

During the 1840s women gradually filled the lecture halls, even occasionally giving reports in the section meetings, and gained admission as members in 1848, but this begrudging inclusion on the part of the officers was largely due to financial necessity. Even so, women's names in the annual subscription lists remained rare throughout the nineteenth century. In 1868, the year of Becker's controversial lecture, 600 ladies' tickets were sold out of the total attendance of 1,856; Becker was one of only two women listed as members or subscribers. Details about women's contributions each year must be garnered from accounts of the meetings in various periodicals and from letters. In the annual reports, women were not altogether invisible in the sections before Becker's debut in 1868. In the Statistics section, Mrs Davies Gilbert reported on agricultural education in the early 1840s.¹⁶ Although the annual report does not clarify the manner in which the material was delivered, the *Athenaeum* indicates in 1841 that her letter was read at the meeting, and again in 1844 the *Athenaeum* notes that she sent in a paper which someone else read.¹⁷ So, while some women may have managed to attend the sectional meetings, it seems likely that at this stage the rare contributions made by women were actually read by men. In 1844 Richard Owen presented Madame Jeanette Power's experiments on cephalopod marine organisms, while the 1846 Zoology

and Botany section included an extract from Mrs Whitby's letter on the cultivation of silkworms. For those women appearing as contributors in section meetings in these decades, frequently their fathers or husbands are listed as members, and in these cases their chaperons surely cushioned their emergence in the public sphere.

The *Belgravia* article spotlights the controversy that erupted around Becker's 1868 lecture, yet she faced difficulties in order to present this paper. Many women attending the BAAS travelled and stayed with family members, but Becker arrived unchaperoned. Money was required not only for admission but for travel and accommodation; for a woman on her own, financial resources were scarce. Becker had to be resourceful: she persuaded the editor of a Manchester newspaper to send her as a correspondent, and her coverage of the annual conference for the *Manchester Examiner and Times* afforded her the means to attend many other BAAS meetings as well.¹⁸ In the 1868 annual report, Becker is the only woman listed to offer a paper in any section.

The purpose of the sectional gatherings was to promote wide-ranging discussions among a diverse group of people, as this 1838 comment suggests: 'the members generally know a great deal about the things discussed; knowledge of high quality is largely dealt out, and largely digested by great bodies of men who are not known authors, or professors, or doctors.'¹⁹ This inclusive study of science anticipated Becker's bid to make the exchange of scientific knowledge available to all. At the first meeting of her ladies' reading society, Becker deplored the inaccessibility of scientific learning for 'half of the human race', as she recommended that 'maids, old and young, should graduate in the School for Science, rather than in the School for Scandal'.²⁰ Becker's first BAAS paper advocating science education for women disputes innate intellectual habits based on gender. She argues that differences of mind that appear correlated with gender are in fact a matter of social conditions. To this end, Becker offers three propositions, often quoted verbatim in the numerous periodicals that reported on this controversial lecture: 'That the attribute of sex does not extend to mind That any broad marks of distinction that may exist at the present time between the minds of men and women collectively, are fairly traceable to the influences of the different circumstances under which they pass their lives That in spite of external circumstances which tend to cause divergence in tone of mind . . . between men and women, it is a matter of fact that these do not differ more among persons of opposite sexes than they do among persons of the same.'²¹

The extensive coverage in the media makes it possible to grasp the rhetorical scene of Becker's inaugural delivery at the BAAS, as she encountered lively support and snide dismissals in print and inside the hall.²² *The Times* observed, 'Miss Becker presents herself to an Association open, as appears by her presence,

to women as well as men; she excites an exceptional degree of interest, and obtains an animated discussion. Several papers on important subjects by the less favoured sex, as it must be called on this occasion, are hurried over or neglected that many of the lady's hearers may have their say with her.²³ The *Pall Mall Gazette* reviewed five different newspapers on the subject of Becker where the responses ranged from sarcasm to applause: 'The *Star* is of opinion that the meeting of the British Association would not have been held in vain if it had produced nothing but Miss Becker's paper.' This 'animated discussion' included both women and men: 'The *Daily News* remarks that "both sexes of man" seem to have entered warmly into the discussion of Miss Becker's paper, but it ended in an entire rout of the advocates of feminine inferiority.'²⁴

The standard practice of the section was to present all the papers and then conclude with a discussion open to the audience. But 'the furore excited by Miss Becker's paper was so great' that the speaker following Becker was unable 'to secure the attention of the section'.²⁵ The *Englishwoman's Review* noted that audience participation revolved around Becker's paper and the relative educations of girls and boys. The article includes parenthetical descriptions of lively reactions to the various remarks made by commentators including '(Hear, hear)' in reply to the contention that education should be geared to each sex, and '(Laughter)' following a woman's comment that current female education produced 'as empty-headed a creature as could be found on the face of the earth'. At the same time, the chairman of the session was greeted with applause when he argued that because the life's work of men and women differed, so should their educations.²⁶ It does appear that Becker had ample opportunity for rebuttals and that she argued forcefully not only for equality of education but also for co-education, given that 'the isolation of the sexes was prejudicial to both'. The animated engagement of the audience emerges repeatedly; one reporter remarked, 'Miss Becker, whose observations had been frequently interrupted by laughter and applause, resumed her seat amid loud cheers'. Clearly her debut on a sectional podium at the BAAS realized Becker's own faith in debate. One member even proposed that the BAAS council establish a committee 'to consider how the scientific education of women can be promoted' and this resolution was carried in the meeting with only one dissenting vote.²⁷ Becker's success had further repercussions: she was invited to speak at the Hull Literary Institute and at the Nottingham Philosophical Society in the following months, and later in 1869 she published a full exploration of the question of women's training in science, 'On the Study of Science by Women'.²⁸ She went on to give four more lectures at the BAAS from 1869 to 1875; in 1871 she addressed a packed hall on gender inequality in employment practices, and the periodical press once again captured the spirited responses of her interlocutors.²⁹ While not the

kind of recognition she sought, Becker was invited to be a patroness at the closing ball that summer, again indicating the obstacles created by contemporary gender conventions for a female intellectual.³⁰ Nevertheless, the 1875 conference marked a milestone when seven women delivered papers, a number neither duplicated nor surpassed until the next century.³¹

Where Becker's 1868 paper on gender and intelligence argues for narrowing the 'supposed' gaps between men and women by offering science education to girls and mixed classes for both genders, her 1869 address explores the mingling of male and female portions of flowers. Whether her views of nature's flexible arrangements were shaped by her political beliefs, or vice versa – or most likely a complex interaction – her botanical studies present intriguing parallels with her arguments for gender equality. Becker's talk, 'On Alteration in the Structure of *Lychnis diurna*, observed in connexion with the development of a parasitic fungus', also marked a watershed because it was unusual for women to deliver papers outside the Social Statistics section. One might view her presentation in the Biology section even as a consequence of the power of Becker's talk the previous year, as she embodied her argument championing women's intellectual capacity to study science.³² Yet 20 years later, reflecting on what would be her last BAAS meeting, Becker regretted that the organization had continued its exclusionary policy of 'systematic discouragement of scientific study by women'.³³

Becker's 1869 paper draws directly upon her correspondence with Darwin. Not only did they correspond about botanical matters, she also sent him the abstract of her 1869 address and later sought his advice about publishing her findings.³⁴ As early as May 1863, Becker had sent Darwin *Lychnis* seed specimens from hermaphroditic plants along with her detailed observations, material that may have contributed to Darwin's study of purple loosestrife (*Lythrum salicaria*), first published in 1864.³⁵ To launch her Manchester Ladies Literary Society in early 1867, Becker had requested that Darwin submit his paper, 'On the Sexual Relations of the Three Forms of *Lythrum Salicaria*'.³⁶ Following the first meeting of the Society, Becker sent Darwin her own substantive analysis about the varied arrangements of stamens and pistils she had noted in *Lythrum*. Becker did not concur with Darwin's theory, and she details her divergent reading in her BAAS paper, as conveyed in the annual report: 'A few of these flowers had been submitted to Mr. Charles Darwin, and he had suggested that the pollen being destroyed at an early period, the pistil was developed in compensation. But though this explanation appeared probable at first sight, further examination of the facts did not seem to sustain it. The writer believed the influence exerted by the pollen to be of a much more subtle and surprising character than this.'³⁷ It is worth highlighting here Becker's rejection of a compensatory pistil (regarded as the

'female' organ of plants) in response to the destruction of the pollen produced by the 'male' part of the flower. Instead of Darwin's interpretation, Becker focuses on the 'subtle and surprising character' of the pollen infected by a fungus. The advantages of hermaphroditism in nature, where male and female organs contained within one flower make it possible for individuals in the same species to fertilize each other and thus be twice as productive in a more diverse way than with unisexual reproduction, resonate suggestively with Becker's argument about co-education.

Analogies between discourses may be strained, but even so Becker's abiding interest in hybridity and plant hermaphroditism may be fruitfully juxtaposed alongside her continued arguments about gender equality and co-education, particularly directed to the study of science. In arguing against intrinsic 'supposed differences' between men's and women's minds, Becker drew many analogies from the study of nature. In 'On the Study of Science by Women', the publication expanding upon her controversial 1868 talk, Becker builds her case by delineating the process of biological taxonomy:

When a naturalist seeks to group a number of individuals into a distinct class, he fixes on some character ... distinguishing them from other individuals. When he finds such a group distinctly defined, he calls it a species. But when he finds two individuals differing very widely from each other, yet so connected by intermediate forms that he can pass from one extreme to the other without a violent break any where in the species, he considers them to be of one and the same kind. If we apply this principle as an illustration of the variety of human intellects, taking the conventional masculine type of mind as one end of the scale, and the conventional female type as the other, we shall find them connected by numerous intermediate varieties, distributed indiscriminately among male and female persons; that what is called a masculine mind is frequently found united to a feminine body, and sometimes the reverse, and that there is no necessary nor even presumptive connection between the sex of a human being and the type of intellect and character he possesses.³⁸

Believing that domestic tasks should not be limited to women, Becker later commented that every boy 'should be taught to darn his own socks and cook his own chops'.³⁹ Becker's vision of gender parity in the domestic realm carried over to the public sphere as well. She cites instances of women cross-dressing as men who 'maintained the secret of their sex for years'⁴⁰ and thus successfully led military and medical careers. Although she does not name James Barry, the renowned military doctor who served across the British empire, the press was

filled with stories about him after July 1865 when it was disclosed following his death in London that he was in fact a woman.⁴¹ Becker's interest in gender flexibility in culture echoes her fascination with hermaphroditism in nature, and as such formulates the crux of her bid to make women esteemed participants at its meetings. Similarly, Becker expressed to Darwin great enthusiasm over his concept of pangenesis.⁴² Although this hypothesis met with severe criticism when published in 1868, Becker's attraction to the idea that every cell in an organism contributed to the reproductive process parallels her view of scientific research as a collaborative enterprise without a single originator. Extrapolating from structures in nature, Becker advocates a kind of social, political and intellectual hermaphroditism, thus anticipating Virginia Woolf's 1929 celebration of the androgynous mind in *A Room of One's Own*.

Although Becker established the Manchester Ladies Literary Society to afford women access to scientific studies, she clearly championed women's full participation in science on egalitarian grounds rather than the complementary logic of botany as a 'ladies' science'. Shteir has observed that Becker's work 'defeminised' botanical discourse in the 1860s, of a piece with what Shteir calls 'the contestational practice of the early suffrage movement' where women vied for the same professional status and opportunities afforded men.⁴³ Becker's 1868 and 1869 lectures might also be understood as performances to disrupt the accepted role of women in the BAAS, despite the fact that the ladies' tickets lingered until 1919, the year after British women over thirty with education and property qualifications were enfranchised. The media coverage of Becker's inaugural BAAS talk makes clear that she did not encounter a wholly 'sympathizing audience', but the widespread reports on her lecture demonstrate that Becker's words were distributed far beyond the ears of the hundred or so original auditors.⁴⁴ The BAAS served many purposes for Becker as an independent woman: varied travels for the summer meetings which took her from Manchester to Montreal, an intellectual community where she might test her ideas, a venue for networking where she met many prominent men of science. Becker used this latter resource in her advocacy of women's rights: she sent pamphlets on women's suffrage to Professor Leone Levi of Norwich and Professor Charles Babington of Cambridge, whereas her correspondence to Darwin after 1868 appears on stationery imprinted with 'Manchester National Society for Women's Suffrage'.⁴⁵ Becker believed that scientific knowledge should be within the reach of all who desired it, and her BAAS lectures collectively signify her efforts to make this a reality. As Lightman has noted in his work on popularizers of science, lecture halls were one of many 'sites of contestation' of varied meanings of science; we might view the nineteenth-century lecture halls as sites of contestation of varied meanings of gender as well.⁴⁶

A FAIR TRIAL FOR SPIRITUALISM?: FIGHTING DIRTY IN THE PALL MALL GAZETTE

Elisabeth Wadge

THE type of faith or Spiritual reliance,
Used to be 'DANIEL in the den of Lions.'
But since a certain case in Chancerie,
'Lyons in DANIEL'S den,' it ought to be.¹

In early May 1868, the correspondence pages of the *Pall Mall Gazette* (PMG) were the scene of a heated debate between two of the most well known figures of the day, Professor John Tyndall and Mr Daniel Dunglas Home. Each man was the recognized leader of his field, but their fields were apparently diametrically opposed. Tyndall was head of the Royal Institution and the natural heir to the late Michael Faraday as experimental philosopher and materialist scientist; Home was celebrated across Europe and America as a gifted spiritualist medium, whose physical phenomena were unsurpassed. Their topic was a séance proposed in 1861, at which Faraday was to have examined spiritualist phenomena produced by Home, but which had not ultimately taken place. The two men began a stiffly polite dialogue within the correspondence columns, but rapidly degenerated into personal attacks and self-vindicating reasoning. By the time they called a truce in late May, the debate had clearly demonstrated the strength of the barriers constructed between

established scientific practice and areas of investigation still regarded as beyond the scientific pale. Nevertheless, the increasing sub-division of the sciences within and across their traditional boundaries was creating a troubling uncertainty over what might be a fit subject for scientific inquiry, at a time when the very notion of orthodoxy in a rapidly shifting field was itself in flux. The energy invested in this debate, and the level of national publicity afforded it, indicate that the process of designating a cultural movement such as spiritualism as 'scientific' or 'unscientific' was far from neutral, and mobilized a range of intellectual and social anxieties.

The specific context for this debate was significant. At that time, Home was defending a legal action that was fast becoming a *cause célèbre*, moving from the legal sections to the front pages of various daily newspapers.² It was alleged that he had exercised undue influence over a widow, Mrs Jane Lyon, with the result that in late 1866 she gave him sums totalling £60,000. This enormous amount was given upon the apparent posthumous instructions of Mrs Lyon's late husband, who intimated that she should adopt Home as her son. When she later regretted these gifts, she sued Home for their return and both parties ended up in court. The nature of the alleged influence was unprecedented, since it involved a dead man expressing his wishes through the unconscious agency of the living. The legal point was whether Home had exercised improper influence, although behind it lurked the spiritual question of whether it was possible for the late Charles Lyon to have contacted his widow through Home. Religious and legal concerns collided, in the courtroom and in the reports that filled newspapers in late April and late May.

The *Lyon v. Home* case was expounded in a series of reports themselves simply reflecting public interest in Home's phenomena. The PMG initially noted that 'a curious case, which has been talked about for some time, was opened yesterday before Vice-Chancellor Giffard' (21 April, p.5). The case aroused strong feelings in the courtroom: Home's departure 'occasioned an outburst of hissing and hooting on the part of the crowd', who hustled him to his cab (22 April, p.6). (Indeed, Home claimed later that week that an attempt was made upon his life by two assailants with a stiletto blade (25 April, p.7).) Fascinating details of Home's life emerge from the official statements (especially those reported on 29 April, p.8). He and Jane Lyon had met at the Sloane Street Spiritual Athenaeum in October 1866, and comforting messages from her late husband had immediately been forthcoming; further meetings corroborated Charles Lyon's continuing existence. In her own home, a séance with Daniel revealed that 'he is to be your son; he is my son – therefore yours'; Home confirmed that her adoption of him 'was the will of the spirit' and, 'under the influence ... of Home's spiritual powers and ascendancy', she concurred (21 April, p.5). Lyon's counsel noted Home's alleged lack of

control over phenomena – ‘They would not happen when he wished, and his will had nothing to do with them’ – but skirted around their truth by asserting he could prove ‘the existence of *pretensions* by which that case of spiritual ascendancy could be maintained’.³ Where Jane Lyon asserted that their relationship was that of mother and child, Home countered that she began legal proceedings only when he refused to marry her (22 April, p.6). Far from being under his control, it was alleged that poor health and financial dependence left Home ‘completely under her influence’.

The counsels for the plaintiff and the defence painted their respective clients as the victim without control over the course of events.⁴ Lyon was allegedly manipulated by Home into following her husband’s wishes, and did what she did to please Charles. Home, according to character witnesses Gerald Massey and Georgiana Houghton, was under the ‘tyranny’ and ‘very potent influence’ of Lyon, who ‘acted very imperiously towards him’; he was used by spirits, Charles Lyon included, to further their will, not his own (28 April, p.9). Both sides made use of material evidence, alleging that any letters in Jane Lyon’s handwriting had been written at Home’s dictation, and any letters in his hand had been written at her or her husband’s instigation (29 April, p.9). The plaintiff’s counsel concluded that ‘unless he could prove that there was a spiritual manifestation, [Home] must be convicted as an impostor’, thus neatly backing him into a corner: either Home proved he was a genuine medium, in which case he had exercised undue influence and must return the money, or without such proof he stood revealed as a manipulative impostor and must return the money. Vice-Chancellor Giffard felt it advisable to delay giving judgement until the next term (1 May, p.9).

The following day’s editorial summarizes this singular case, highlighting some possible conclusions: that ‘there are enough fools in the world to keep the knaves alive’, that ‘if neither spirits nor mediums can arrange a system to command public confidence, society should ... insist upon some spiritual treaty’, and that ‘the persons who act as [spirits] agents must be responsible for the consequences of their behaviour, even if they are nothing but agents, simply because we cannot get at the principals’. The writer suggests that Home could have used Lyon’s money to found ‘an institution for the due investigation of spiritual phenomena’. To prevent frauds capitalizing on his success, he should be ‘carefully examined by the first experimental philosophers of the day, until the laws of these anomalous phenomena have been satisfactorily determined. Surely Professor Tyndall would find lodgings for him in the Royal Institution, and every man of science would be glad to subscribe a moderate sum to keep him as the most singular of recorded curiosities. If he stands the inquiry, he would be cheap at £30,000 from the nation’ (2 May, p.10). It is at this point that the matter shifts from the law courts to another arena, that

of the correspondence columns. The mention of his name encourages Tyndall to point out that the Royal Institution had indeed already been involved with spiritualism, under the auspices of his mentor, Faraday: Faraday had agreed to meet Home for just such a scientific investigation but, neither party having been happy with the circumstances proposed, the planned séance was cancelled (5 May, p.3). The situation in 1868 was further complicated by the fact that Faraday, having died in 1867, was unavailable for comment on his original decision, and so Tyndall commented in his name. The two original protagonists had conducted tentative negotiations in 1861 through intermediaries (Robert Bell for Home, and Sir James Emerson Tennent for Faraday), and now Faraday had Tyndall to speak for him. Home and Tyndall's communication through a national newspaper indicates the real bone of contention: less that justice may or may not have been done in 1861, but that it should be seen in 1868 to have been done. The argument was not over the truth or otherwise of spiritualism, but over the wisdom of Faraday's decision not to investigate, and how this affected the reputations of the figures involved.

The irony of Faraday making a posthumous appearance in the press a year after his death was presumably not lost upon the *PMG*'s readers. Earlier, Faraday had investigated table-turning, mediumship's domestic corollary; noting how great was public desire for his professional opinion, he created a simple test involving loosely connected layers of cardboard and sandpaper attached to a lever, which indicated the relative motion of hand and table, strongly suggesting involuntary muscular action. The apparatus was displayed at Newman's, the philosophical instrument maker in Regent Street, and the results published in the *Times*. Faraday scorned those who

refer the results to electricity and magnetism – yet know nothing of the laws of these forces; or to attraction – yet show no phenomena of pure attractive power; or to the rotation of the earth, as if the earth revolved around the leg of a table; or to some unrecognized physical force, without inquiring whether the known forces are not sufficient.

He lamented the level of ignorance that allowed these quasi-explanations to flourish unchecked, and exhorted spiritualists to 'resign expectation [and] be led by the facts and the truth of nature'.⁵ Eight years before the proposed séance with Home, therefore, Faraday published his last word on the subject, only to be revived by Tyndall to win an argument 15 years later.

Faraday allegedly continued to return through mediums to recant his earthly opinions, to the delight of many who had found his 1853 exposure of table-turning too simplistic.⁶ Other hardened sceptics apparently followed suit. In 1875 a Chicago justice of the peace claimed to have received a message from

the spirit of a Supreme Court judge, overturning the decision he had made while alive which a lawyer was at that very moment attempting to use as a precedent.⁷ Re-evaluation of a decision was apparently no longer the province of succeeding generations: it seemed a judgement could now be revoked at any point, leaving little ground for certainty in argument. Or decisions apparently from the dead could continue to influence the living, in ways not hitherto encompassed within the legal term *mortmain*. This was the basis both for Jane Lyon's case and for John Tyndall's controversy with Home. The scientific debate unfolded within the interval between the hearings and the verdict, and this coincidence of timing suggests deeper parallels. Both discussions, in court and in correspondence, focused upon the question of influence and the legitimacy of its extent.

The start of the correspondence is Tyndall's letter to the PMG's editor, noting that Faraday had acceded to an invitation to see Home's phenomena, 'claiming only the right to test the "manifestations" by his own methods' and to publish the results whatever they might be (5 May, p.3). A reply from Home the following day clarifies that the séance did not take place because Faraday requested as a condition of attending that Home supply a programme of events in advance. Since Home asserts his 'performances' are beyond his control, this is an impossible demand. Home suggests another meeting, a 'patient and candid investigation', in words reminiscent of another kind of challenge:

It will give me the same pleasure to meet Professor Tyndall and any two gentlemen he shall designate. On my side I will have at least two gentlemen I will meet Professor Tyndall and these gentlemen when and where they please, and under such circumstances as they may decide on. (6 May, p.3)

This is neither the language of the laboratory nor of the courtroom; it is the language of the duelling-ground. The testing becomes a question of honour, involving credibility as a gentleman as well as a professional. With this letter the two trials of Home, the scientific and the legal, begin to converge, since both are intimately concerned with his public reputation.

Tyndall then calls upon Home to produce the letter proving that Faraday refused to meet Home in 1855 due to religious scruples (7 May, p.3). As letters between Faraday and Mr Cox (Home's then host) indicate, this opportunity was actually declined on the grounds of Faraday having 'lost too much time about such matters already'.⁸ However, Tyndall's tone deteriorates in describing how Faraday saw spiritualism as 'a compound of knavery and delusion', 'clothed in the garb of religion' (7 May, p.3). Quoting this opinion without

comment effectively repeats the original insult, calling Home a fraud without stepping out from Faraday's shadow. Tyndall also asks Faraday's original supporter in 1861 to publish his letter of refusal, to clear up 'all apparent differences between Mr. Home and myself'. Without overtly responding to Home's challenge, Tyndall thus calls up his own witnesses in the public trying of Home's honesty.

Home makes no reply to this before Tyndall again figures in the correspondence columns (9 May, p.6). This time, his letter incorporates those of 'seconds' on each side: William Wilkinson, a lawyer fighting an action by Lyon alongside Home, in a related but subsidiary case; and on his own side letters from Sir James Emerson Tennent and Faraday. The inclusion of Wilkinson is significant because of his involvement in the legal situation: in publishing this letter from 'one of the most distinguished "spiritualists" in England', Tyndall casts doubt upon Wilkinson's impartiality in both the legal and the scientific settings simultaneously. Tyndall begins by observing that it is 'a critical point in the history of English law and of English common sense', and closes with the note that he leaves 'these facts to settle in the public mind, and more especially in the legal mind'. This explicit linking of the legal situation, where Home's honour is at stake, with the scientific matter automatically makes the scientific situation a matter of honour too. Faraday, in his letter here included, also refers to his honour: previous investigations have been 'consistent with the self-respect that an experimental philosopher owes to himself', but he has 'found nothing worthy of attention'. He declines to meet 'any whose minds are not at liberty to investigate' or to debate with 'those who have already made up their minds in a direction contrary to [his] own'. The former is a scientific question, the latter a question of pre-judging value, or prejudice. In identifying others who have decided contrary to his decision, Faraday implies that he has already reached a decision. This exchange hardly demonstrates the open-minded impartial attitude one might expect from a natural philosopher, either on Faraday's part or on Tyndall's.

Home's response to these letters-within-letters is apparent astonishment, as he never saw Faraday's original set of requests; he queries whether Faraday's tone is that 'of a humble student and inquirer' or 'of a mind far gone in pre-judging the question'. Following his 1853 investigation of table-turning, Home observes, Faraday took every development of phenomena as a personal affront against his publicized judgement. Emphasizing the human element inherent in any such scientific interaction, Home suggests that 'hydrogen, or any long suffering gas, or even the spectrum analysis, would have felt insulted by being submitted to such conditions' (11 May, p.3).⁹ Faraday's spirit, and by extension Tyndall's, is not 'according to the true scientific method'. The development here is that Faraday's reputation can be challenged: not only is

he painted as prejudiced against spiritualism as others are in its favour, but he also no longer guards the true scientific method. Home, the medium, is the one claiming to know what this is, rather than men of the Royal Institution.

In the subsequent lull, editorializing takes over. The PMG reports the *Telegraph's* view that Tyndall is justified in publishing details of the 1861 proposed séance to save Faraday from the imputation of a prejudiced mind. The writer states, 'The truth of the doctrine professed by Messrs. Home, Wilkinson, and Co. does not yet appear capable of positive proof or disproof' (15 May, p.3), despite the current legal case with just this capability as its sub-text. The writer notes many things one cannot know for certain, including the motivation of disembodied spirits. Without the possibility of positive proof or disproof (which surely means that any scientific inquiry will fail), the balance of probability is all that can be evaluated, as in a trial with numerous conflicting witnesses. The writer then shies away from his uncertainty, evoking the sanctity of religious belief to avoid the whole vexed question, as Faraday had done in 1855, and as Giffard cannot in 1868.

The next instalment is lengthy but straightforward. Like Tyndall's it comes from the Athenaeum Club, but not from a scientist: Francis T Palgrave is an art critic, and is not only disinterested but also uninterested, except insofar as spiritualism has been for him a 'devastator of agreeable conversation'. The editor publishes his letter under the heading 'Science *versus* Spiritualism', thus drawing boundaries which Palgrave's letter does not really follow (16 May, p.6). Palgrave has no axe to grind with Tyndall – indeed, he goes out of his way to express admiration for Faraday and his fellow experimental philosophers – yet he takes issue with how both Faraday and Tyndall have behaved towards Home. Putting aside any declared partisans, he notes that 'to the general or undecided public the verdict of Professors Faraday and Tyndall, yea or nay, after a fair and full trial, would be conclusive'. The idea of a fair trial resonates because of the preceding legal case, and it recurs a dozen times throughout Palgrave's letter, albeit regarding the scientific ramifications. Palgrave's point is that Faraday's manner and assumptions made his proposed séance an unfair trial, and allowed Home to refuse a perhaps conclusive test on the grounds of prejudice. He notes that in 1861, 'Faraday was to act more or less as a judge in what other paragraphs in his own letter describe as a scientific investigation'. Even if he had doubts about the outcome, Faraday's mere involvement at any level should have constrained him to a position of equality and civility; 'a more judicial spirit' in the 'advocate of science' might have resulted in a fair and full trial, and a definitive verdict representing a victory for truth, at all events. Palgrave's use of legalistic terms, and transfer of them to the scientific context, subtly reminds readers

of the awaited verdict and of the open-mindedness that, combined with vigilance, ought to underpin all human judgements.

Tyndall's reply (18 May, pp. 2–3) follows Palgrave's pregnant blending of two discourses by explicit reference to 'recent events' reviving critical interest in spiritualism. 'In the public courts of England men with heavy scientific appendages to their names had testified' regarding spiritualist phenomena, testimonies then repeated and amplified. This reference reunites the shadowy figures of Palgrave's rhetoric with their material counterparts, and demonstrates how certain professional specialisms translated into new fields, into new discourses. Disagreeing with Palgrave's view of Faraday's motives, Tyndall defends Faraday's 'repugnance' and 'strong contempt' because of the sheer validity of his opinion: 'He had not the slightest hope of adding to his own knowledge by further investigation. He regarded the necessity of even discussing such phenomena ... as a discredit ... to the education of this age.' Asserting that Faraday had wished merely to apply 'the methods which have made his name immortal', Tyndall claims the right to deal with Home 'as I now deal with Nature', to conduct the aborted 1861 séance, outstripping the caution of Faraday himself.

A new correspondent enters the columns on 19 May, lowering the temperature and appealing to all three correspondents in a sensible delineation of the question. George Henry Lewes fulminates against the mischievous view that scientists refuse spiritualists' challenges, commenting that 'it is not the investigation they decline, but the séance where they are not permitted to investigate'. He notes his own startling experiences, when scepticism had been kept covert:

One of the Eumenides informed me that she died six years ago, in the Jewish persuasion, aged twenty-five, leaving seven children. Shakespeare declared that he was the author of an article in the last *Quarterly*. My sister (I never had one) assured me that she watched over me, and read my works with interest.

Taking a carefully neutral tone, Lewes observes that Home must know how people perceive his phenomena, and must feel keenly any injustice regarding this perception. Under such circumstances, no degree of offence in Faraday's letter should have dissuaded him from the original meeting – 'To have convinced Faraday would have been a triumph' – or from the meeting now proposed by Tyndall. Accusations of prejudice against Faraday miss the point, Lewes believes: 'cases are always prejudged But evidence triumphs. No amount of bias can resist certain tests.' The interesting shift here is in the association of bias with the scientific, not spiritualist, viewpoint, and in

the possibility that the evidence might favour either side. If spirits are as willing to appear for scientists as 'when transfers of property are to be made, or when relationships to rich old ladies are to be disclosed' (clear references to the Lyon case), then the 'very warrantable suspicions which keep men of science aloof' will be dispelled. While noting that the 'valuable time' of scientists should not be wasted in futile inquiries, Lewes yet maintains that a proper inquiry would not be futile, and undertakes to supply such men of science as are necessary (19 May, p.3). The question of value underpins Lewes's views – the value to each party of visibly undertaking a full and fair trial, and the value of what each party would be staking on such an enterprise.

Palgrave soon returns (21 May, p.3) to defend himself against Tyndall's attacks concerning his last letter, in a perfectly poised letter – 'I have rarely seen so much irony attempted' – that Tyndall must have found infuriating. His theme remains the same: that Faraday's options were only 'to refuse a trial, or to give a fair one in the common, judicial sense – a trial, in a word, in which the judge does not begin by defining the verdict'. Faraday's deviations from impartial investigation and respectful tone in his 1861 letter, Palgrave notes, were uncharacteristic; he was it seems far from believing that his scientific accomplishments removed him 'from human fallibility, nor claiming exemption from ordinary rules of judgement', an implicit contrast being drawn here with Tyndall's behaviour. Palgrave welcomes Lewes's offer of assistance in any future test-séance, finding him 'personally and scientifically so well qualified' for the task by reason of his measured yet vigilant attitude and perhaps also his attainments in the theory and practice of science. Palgrave focuses on the 'delicate' act of publishing posthumous letters, thus fixing a man eternally in the position he adopted at one specific moment. For a gentleman, only 'high and indisputable public utility' justifies publishing that which 'reflects strongly on the living'. He continues thunderously: 'But to publish it at a time when the character of the living (however little we may be disposed to estimate it) happens to be the subject of legal inquiry, with the avowed intention of biasing the mind of the judge, and this in order to support one's own case in a literary controversy, appears to me an act to which an epithet more forcible than 'unadvised' might with propriety be affixed.' Palgrave pinpoints the personal nature of Tyndall's attack upon Home, which mirrors the evaluation of Home's honour in the legal case, and the timing of the one to coincide with the other. Both contests will decide whether Home is, in the public's eyes, honest and trustworthy, and since a medium relies entirely upon reputation, an *ad hominem* attack is particularly effective.

At this point, when both discourses and both trials are so inextricably linked in its letters pages, the PMG devotes its leader article to 'Science and Spiritualism' (22 May, p.1). The writer rehearses across 40 lines the debate

to date, rather disingenuously lamenting the 'parliamentary mode of address' that has kept readers enthralled. He also laments the nature of Faraday's initial letter, which was (if Faraday intended a serious meeting) 'injudicious': 'It does not follow that because a man is a great philosopher he is able to write a good lawyer's letter.' However, Faraday's desire not to 'waste his valuable time and energies on such a wild-goose chase' is understandable, and his declared viewpoint a warning to spiritualist correspondents. Clearly distinguishing between Faraday and his successor, the writer notes that Tyndall is now in May 1868 a challenger, rather than being, as Faraday was in June 1861, the challenged; Faraday's original language and stance were suitable in 1861, but Tyndall's adoption of them in 1868 ignores the change in context and thus prejudices a proper inquiry. Tyndall's closing offer of a test-séance is, however, worthwhile, since he would use 'the ordinary scientific methods used for the investigation of nature'; far from finding Tyndall's disbelief offensive, the writer suggests that it is 'the very reason why the challenge is given', just as in court a doubtful case is tried upon every point.

The final sally is the publication by Tyndall of a letter from one of the original protagonists, Sir James Emerson Tennent himself, replacing Faraday's challenge letter in its context. With Home's approval, Sir James apparently presented a condensed account of a séance to Faraday, omitting any phenomena where conditions might preclude full investigation; Faraday was reluctant to participate in a test-séance, since despite his pre-eminence in experimental observation and design his limited experience of 'mere mechanical agency' meant he was not certain of detecting fraud. Any 'corroboration of error' would not be in the 'interests of true science, of which he was one of the recognized trustees', and therefore he did not feel 'safe in acceding' without the conditions that so offended Home (25 May, p.4). This rather surprising twist clearly indicates the risks to Faraday of tangling with mediums, and the low value he perceived in the outcome of an investigation. This is the PMG's last published word on the scientific matter.

At 11 o'clock on 22 May, Vice-Chancellor Giffard began to give his verdict on the *Lyon v. Home* case, a brief Law report appearing in the same issue of the PMG as the leader article. Referring to precedents set by Lords Eldon and Cottenham, Giffard ruled that Jane Lyon was 'subject to hallucination, or undue influence' when making her gifts, and that the money should be returned to her (22 May, p.8). A subsequent item notes that since her testimony was demonstrably unreliable and unhelpful, Home would not have to pay all costs. Nevertheless, since 'the onus of supporting the gifts and deeds rests entirely on the defendant', and since 'he has not made or proved such a case', it is declared that 'that the gifts and deeds are fraudulent and void' (23 May, p.7). That day's *Times* also carries a lengthy report on 'this celebrated

case, which has ... excited public attention to an extent quite unprecedented in the annals of the proceedings of the High Court of Chancery';¹⁰ it gives details of the precedents used by Giffard. The Eldon ruling had indicated that a court could permit monetary transfer only as 'an act of pure volition uninfluenced'. The Cottenham ruling had highlighted the need to watch 'with great jealousy' all financial transactions involving 'relations in which dominion may be exercised by one person over another'. Sidestepping the question of whether Home had indeed 'channelled' Charles Lyon – 'I know nothing of what is called "spiritualism" otherwise than from the evidence before me' – Giffard was nonetheless 'satisfied that the statements ... that the transactions had nothing to do with spiritualism were not according to the fact'. In other words, Jane Lyon had believed Home's claim to be conveying her dead husband's wishes, and had acted accordingly. In two hours and 20 minutes, Giffard spelled out the grounds for Home's legal failure and damaged his professional reputation irreparably. Home's prosecutors in court and in the PMG won in each case, both public opinion and common sense reportedly turning against him. While Lyon did not emerge spotless from the witness box, the damage to her reputation presumably had few professional ramifications. Home, by contrast, was ridiculed ceaselessly, on contradictory grounds: he was disreputable and no gentleman whether his spiritualist phenomena were genuine or fake. After lodging an appeal with the Lord Chancellor against Giffard's decision, he left England for Germany and did not return for some time.¹¹

Throughout April and May 1868, Home's professional and personal lives were put on trial in two separate contexts that nevertheless had significant connections. Whereas scientists had previously avoided involvement, during the court proceedings the extent of their unwitting engagement became clear. Tyndall's explicit linking of the legal and scientific trials was only one of the connections, and its chronological coincidence masked a fundamental parallel between the situations of spiritualist and scientist, one which Tyndall himself would have strongly resisted. In the legal trial, Home was asked to prove a negative – that he had not unduly influenced Lyon – just as in a test-séance any scientist would be required to prove a negative – that Home's manifestations were not attributable to fraud. Faraday's recognition of this led to his avoiding possible corroboration of fraud by avoiding an experimental séance, but Home was unable to avoid his challenge in the courts. Similarly, the parallels between Home and Tyndall were uncomfortably close. The medium claimed he summoned up Charles Lyon's spirit, which spoke through him alone for the good of those left behind, and to confirm his adopted son's status; the scientist posthumously published the pronouncements of his mentor for the benefit of the general public, implicitly asserting his own position as Faraday's sole successor. If Tyndall was indeed Faraday's rightful heir, and it

was valid for him to adopt the attitudes of seven years previously, then the scientific establishment's dismissive attitude towards spiritualism was unchallenged. Otherwise, Tyndall was guilty of performing the same movement of appropriation of the dead as Home, albeit for motives of professional standing and not financial gain; otherwise, the need for proper investigation – a full and fair trial – would persist, and return to haunt the Royal Institution.

Unlike Faraday who, aware that his practical expertise did not extend to testing mediums, refrained, Tyndall expected that his usual methods would produce a reliable result, confidently transferring his professional training out of its accustomed channels and asserting its validity across intellectual boundaries. This translation into a new setting appeared to work for Tyndall, whereas Home was not so lucky: with a hostile crowd and a judge anxious to avoid abetting fraud, his character witnesses and unparalleled professional reputation counted for little against a specific and immanent day of judgement. Tyndall's timing, and his overt linking of the two trials, implied that Faraday's judgement and Giffard's should accord, the 1861 dismissal counting against Home in 1868. In addition, while spiritualists argued along the lines of fairness, scientists argued on the grounds of worth; the former was an appeal to justice, the latter an appeal to value. The pre-judgement made on spiritualism's ultimate value mitigated against any fair evaluation of it, although perhaps Tyndall and Faraday failed to recognize the questionable validity of their *cui bono* argument, one that they would surely never have made in relation to other curious phenomena.

Small wonder, then, that spiritualists felt his trial had been neither fair nor full. Here, Home's and Tyndall's situations polarized. The legal situation demanded a single verdict on a given day, based only on evidence presented in that courtroom, while full and fair scientific testing could incorporate discussion, re-evaluation, redesign and repetition. A full and fair legal trial would submit all available evidence to an impartial judge for evaluation of the inherent probabilities; a full and fair scientific trial could be devised and conducted by a specifically qualified scientist, with a good chance that inconclusive results could lead to endless deferral of a definite conclusion. As shown by the arguments between William Crookes and William Carpenter in periodicals of the 1870s, and the embarkation of the Society for Psychical Research into full-time investigation in 1882, a definite conclusion was indeed elusive; it evaded capture within the lifetimes of Tyndall and Home, as well as of many of the original psychical researchers. Despite the judgement handed down on 22 May, by Vice-Chancellor Giffard, the absence of any official scientific investigation in 1868, as in 1861, meant that in the public mind the verdict on Home and his fellow mediums remained, at best, 'not proven'.

‘THIS IS OURS AND FOR US’:
THE MECHANIC’S MAGAZINE
AND LOW SCIENTIFIC CULTURE
IN REGENCY LONDON

James Mussell

In his pamphlet *Observations on the Education of the People*, Henry Brougham links scientific education with a stable working populace. For Brougham, science was a unity, and reflection upon it would reveal an ordered world, functioning correctly. ‘The more widely science is diffused’, he writes, ‘the better will the Author of all things be known’.¹ Yet, this view of science, predicated as it was upon a unified Nature, was actively critiqued in the early nineteenth century. Cheap, mass-market periodicals such as the *Mechanic’s Magazine* emerged from a combination of technological innovation, philosophical radicalism and entrepreneurial opportunism, to provide a textual space for an alternative scientific culture. These titles foregrounded dialogue, preventing the ‘thematic finalization’ necessary to disseminate unified Nature as a final signified.² Their textual community, lying outside of ‘high’ scientific discourse and yet at times engaging with it, allowed members to negotiate and appropriate, in a dialogic exchange, the contested signs of the industrial age.

This chapter seeks to recover this rival scientific discourse and, by exploring its foundation in the textual community that supported it, identify its codes, constructions and participants. The chapter is organized into three sections: the first considers the foundation of the *Mechanic’s Magazine* and

the strategies employed by the editors to carve out a readership from within the reading audience of the new 'mass' journals such as the *Mirror of Literature*. The second examines the consolidation of this audience and its discursive and spatial relationship with the newly formed London Mechanics' Institution. The third concludes with an analysis of the science of the *Mechanic's Magazine*: by describing how this material is inscribed into the form of the journal, it posits a marginal scientific culture founded on contemporaneity, dialogue and the contingency of invention.

The *Mechanic's Magazine* was begun in 1823 by the journalist and political economist Thomas Hodgskin (1787–1869) and the Scots patent agent and author Joseph Clinton Robertson (1788–1852). Hodgskin was an ex-naval officer who, after being court-martialled in 1812, attended the University of Edinburgh. In 1815, he left the University and, on the recommendation of the radical political activist Francis Place, travelled through Europe. His correspondence with Place brought him to the notice of the Benthamite circle. On his return, Hodgskin turned down Mill's offer to reside at Ford Abbey, choosing instead to write up his travels in Edinburgh. Here he made a range of acquaintances, from the political economist John Ramsay McCulloch to the phrenologist and radical George Combe and the poet and essayist William Hazlitt. Yet, Hodgskin's literary career stalled: his book, *Travels in Germany*, only sold half of its 1,000 copies and his increasing political radicalism in the strict conservative climate of Edinburgh meant he could only find a market for travel writing and occasional hack work. Depressed both emotionally and financially, Hodgskin returned to London to take a position as a Parliamentary reporter on the *Morning Chronicle* that Place had secured for him.

It was probably during his second period in Edinburgh that Hodgskin met Joseph Robertson. In 1821, Robertson published a *Lives of Eminent Scotsmen* under the auspices of the Society of Ancient Scots but was forced to leave the city shortly afterwards.³ By this time, he had already begun the *Percy Anecdotes* in collaboration with the Yorkshireman Thomas Byerley. Issued in 44 parts from 1820–3, these were incredibly successful and were reputed to have sold more than 260,000 copies in the first four years. Robertson's collaboration with Byerley not only gave him experience in serial publication, it also provided an opportunity to work with one of the pioneers of the cheap weekly press. In 1822, Byerley began the *Mirror of Literature, Amusement and Instruction* with the publisher John Limbird and it was this model of publishing and editorial practice that Robertson would adopt for the *Mechanic's Magazine*.

The *Mirror* consisted of miscellaneous extracts and reviews arranged in close double columns, with at least one woodcut in each number. Jon P Klancher claims, it became 'the standard text among 1820s "cheap" journals, its initial

issues reputed to sell 80,000–100,000 copies and widely imitated in the next ten years'.⁴ Its form, the 16-page octavo, drew upon both the traditions of the literary weekly and the radical press and, as Jonathan R Topham suggests, 'clearly represented a publishing format with potential for exploiting the cheap market which the literary entrepreneurs of the Strand conceived to exist'.⁵ By merging this form with extracted material from prohibitively priced books, the *Mirror* sought to 'afford the most useful Instruction and amusement at the least price possible'.⁶ Limbird marketed the *Mirror* on its broad appeal: a reviewer quoted in an advertisement recommends it 'to the perusal of youth, and the patronage of the rich, it being pregnant with instruction and amusement to all ranks, from the peer to the cottager'.⁷

The success of the *Mirror* indicated to Robertson the existence of a large market for cheap reading, and so the *Mechanic's Magazine* was set up to capture a portion of it. As Francis Place commented retrospectively in his notebooks:

Under these circumstances it seemed reasonable that a work on art and mechanical science would command a sale of a very considerable number and at least produce as much money as would cover the expenses of conducting it and give some profit, perhaps a considerable profit, to the undertakers of the work.⁸

Robertson and Hodgskin sought to carve out from the 'mass' readership of the *Mirror* their own reading audience, distinct from the 'mass', that would sustain the periodical production of the *Mechanic's Magazine*. In their prospectus, which was reprinted in the first number of the *Mechanic's Magazine*, the editors claim 'there is no periodical Publication of which that numerous and important portion of the community, the Mechanics or Artisans, including all who are operatively employed in our Arts and Manufactures', can say, 'This is ours and for us'.⁹ As yet, the editors can only address the 'Mechanics and Artisans' as an absent reading community, an extra-textual potentiality. Bakhtin claims that social awareness is only created relationally with other groups.¹⁰ Therefore, to bring this audience into existence, the editors must bring the mechanics into their text, in order to dialogize them against other more established communities of readers.

The readers that the editors wished to attract were a distinct social group with a strong sense of self-identity. I J Prothero describes the mechanics as 'the skilled artisans, the better-off journeymen who belonged to trade societies'.¹¹ These were men who had learnt their trade after a long period of apprenticeship and claimed their skills demarcated them from the common labourer. They worked for wages rather than day rates and because their

hard-earned skills were of value to the community they believed it was their legal right to support themselves through their pay. Although there were social differences both vertically in terms of hierarchy and horizontally across trades, they shared a sense of identity and value in a wider social context. To reach such readers, the editors refined their selection and production of material, seeking a text which would, if not reflect their world, at least be sufficiently recognizable to interact with it.

Initially unable to comprehend the mechanics as a reading audience, only as specific readers to be targeted within a wider, already existing reading audience, the early *Mechanic's Magazine* employs the same types of articles, the same grammar, as the *Mirror* but tailors the content specifically for mechanics. In the opening number, the editors claim they will 'comprehend a digested selection from all the periodical publications of the day, both British and Foreign, and from all new works, however costly, of whatever may be more immediately interesting to the British artisan', including 'a due portion of that lighter matter, which those who toil most stand in need of, to relieve and exhilarate their minds'.¹² Thus, the first number opens with a biography and portrait of Watt and, among the 'lighter' material, has a piece of verse entitled 'An Address to a Blacksmith on his Birth Day'. The woodcuts are by Matthew Urlwin Sears, the same engraver who illustrated the *Mirror*. The second number repeats this miscellaneous format: a short story called 'The Basket Weaver' sits by Hodgskin's economic analysis of the Spitalfields Acts.

As the *Mechanic's Magazine* shares its format with the *Mirror*, it too presents the world as 'collage, a strangely constructed sequence of verbal and graphic images'. Klancher claims these readers experience a world mirrored in 'discontinuous shards with no visible principle of continuity' into which they are plunged 'without a panoptic vantage on the textual whole'.¹³ However, in the *Mechanic's Magazine*, each fragment of social representation is articulated to the same addressee, providing an underlying sense of unity. Yet, this addressee remains stubbornly extra-textual, only ever entering as empty target. Klancher recognizes that 'audiences cannot be understood taxonomically; they are not simply distinct sectors of the cultural sphere'. Instead they:

are mutually produced as an otherness within one's own discourse. In order to form the mode of reception for one audience, the writer has to produce, at the same time, another audience-text relationship, and this exerts an internal pressure against which he defines his relation to other readers.¹⁴

Robertson and Hodgskin achieve this through the insertion of readers' contributions into the miscellaneous context. In the very first number they state

that 'communications from intelligent Mechanics, and from all others who may take an interest in the diffusion of useful information on any of the subjects embraced by this publication ... will be thankfully received, and have every attention paid to them'.¹⁵ By the second number, they claim to have received a contribution from 'Mathew Spiers' and answer him in their new column 'To Our Readers and Correspondents'. Regardless of the authenticity of the letter, a model of dialogue is set up within the text providing both a representation of a member of the reading audience and a textual demonstration (in the form of reader's comments regarding the magazine) of the actual practice of reading.

These contributions are a different form of writing which contrasts with that of the rest of the journal. Bakhtin, while recognizing the individuality of utterances, notes that each sphere of language use generates '*relatively stable types of these utterances*' and labels them 'speech genres'.¹⁶ In the discourse of the text, each contribution 'is a link in the chain of speech communication' responding to what has already been said and pre-empting what is to come.¹⁷ Bakhtin writes 'each speech genre in each area of speech communication has its own typical conception of the addressee, and this defines it as genre'.¹⁸ Although readers' contributions are nominally addressed to the editor, they actually address the author of a previous utterance who is usually, but not always, another contributor. By invoking a past utterance, the contributor insists on both the contemporaneity of that utterance, allowing it to span numbers, and invokes the author textually as addressee. Therefore, these contributions from readers are an active dialogizing form which interacts and contrasts with the more passive operations of the miscellany. Whereas the other content of the *Mechanic's Magazine* signifies the absent reader, labelling them outside the text, the contributions from readers actually embody the addressee within their discourse, generating a dual presence and a genuine textual community.

The interplay between these two different types of content, these two different speech genres, shaped both the reading audience of the *Mechanic's Magazine* and the content that it covered. In its first volume, the *Mechanic's Magazine* is not a stable, monolithic text but a shifting textual field as the editors interrogated their audience and responded to their immediate historical moment.¹⁹ In its opening numbers, the editors manipulated the balance between genres, introducing divisions and sub-headings, in an attempt to further define exactly who their readers were and what material was appropriate for each. From the third number, readers' contributions are freely mixed with extracts and original articles. In the fifth number, they are divided into 'Discoveries and Inventions', 'Social Talk', 'Domestic Medicine' and 'Domestic Economy.' The latter of these categories suggests that even though

the *Mechanic's Magazine* is aimed at male artisans, these readers are imagined in a domestic setting. This is reiterated by the presence of fiction and verse, the 'lighter' material which is to provide relaxation for the mechanic, and by implication his family, at the end of the working week.

This material is explicitly gendered by the editors. In the sixth number of the *Mechanic's Magazine*, they provide the following correspondence to 'R N', a reader who has complained of the 'lighter' elements of the journal:

We could provide a round robin from all the mechanic's [sic] wives and daughters in the kingdom to prove that he is wrong in his objections to the amusing part of our miscellany. Let the same remark suffice for 'John Lignum' who says, he would rather see us 'without a tail (tale).' How does John know that we wear that old fashioned appendage [sic]?²⁰

The editors are attempting to provide reading to satisfy more than just the scientific interests of the mechanics through the introduction of a range of discursive voices, organized into gendered categories, which reflect the social heteroglossia through which they live their lives.

However, by the tenth number of the *Mechanic's Magazine*, the editors, despite their vigorous defence, have removed the poetry and fiction from the text. From the twelfth number until the end of the first volume 'Domestic Economy,' 'Domestic Medicine' and 'Social Talk' appear intermittently, before disappearing entirely from the second volume. The reason for this re-evaluation of the reading audience is the formation of the London Mechanics' Institution, first proposed in the *Mechanic's Magazine* of 11 October 1823. The proposal, written by Hodgskin and Robertson and edited by Francis Place, stresses the need to keep the Institution independent:

The education of a free people, like their property, will always be directed more beneficially for them when it is in their own hands. When government interferes it directs its efforts more to make people obedient and docile than wise and happy.

This is then reiterated in the terms of broader class conflict:

The people only want to have the means of educating themselves left in their pockets untouched by the tax-gatherer, and there is no doubt but they will employ those means more for their own advantage than they can possibly be employed by men who, for the very reason that they belong to the upper classes, can know little or nothing of what the lower classes need, nor what is fitting for them.²¹

Any difficulty in mobilizing a class which 'have long been accustomed only to stir in any combined and general measure, when led on and diverted by some members of the aristocracy of wealth and rank' is countered by the fact that it is better to bear the difficulties than 'seeming to be in league with the parties of whom they so loudly complain to work their own degradation'.²²

The rhetoric and emphasis of independence is crucial to Robertson and Hodgskin's idea of the Institution. It was to be a place where the mechanics could meet for mutual instruction. It was, in other words, to be a physical embodiment of the same principles that underscore the textual space of the *Mechanic's Magazine*: instead of contributors' voices in dialogic exchange with utterances from other social languages, the Institution would have actual voices exchanging and 'speaking' the wider social world. The discussion, unencumbered by the dominating presence of other classes, would be free to appropriate or reject that discourse, either making it their own or defining themselves against it. The proposal makes this explicit when it states that the mechanics must 'make such an institution *their own and for them*', echoing the address in the first number of the *Mechanic's Magazine*.²³

The history of the London Mechanics' Institution is well known and given elsewhere.²⁴ As early as 22 November 1823, correspondence began to appear in the *Mechanic's Magazine* questioning the legitimacy of the middle class supporters of the Institution (notably Brougham) and the language they used to describe the mechanics. The editors, although at this point still supportive of the Institution, were obliged to print the criticism, often using it as a pretext to defend it further. These discussions and the large amount of letterpress used to report on the progress of the Institution forced out the 'lighter' and 'domestic' elements of the *Mechanic's Magazine*. The editors redefined their audience, locating them not in their homes but in the dialogic space surrounding the Institution. The contributions, previously unmarked voices interacting with explicitly gendered material, were now gathered around the strictly male space of the Institution.

The presence of the interested middle classes, no matter how philanthropic their motives, meant that the science taught in the Institution could not be of the type, fused with social intercourse, that animated the dynamic core of the *Mechanic's Magazine*. In his *Labour Defended Against the Claims of Capital*, Hodgskin writes:

Mechanics' institutions will teach men the moral as well as the physical sciences. They excite a disposition to probe all things to the bottom and to supply the means of carrying research into every branch of knowledge ... [the labourers] may care nothing about the curious researches of the geologist or the elaborate classification of the

botanist, but they will assuredly ascertain why they of all classes of society have been involved in poverty and distress.²⁵

The science diffused through lectures in the Institution only taught the abstract 'curious researches' in an attempt to prevent research being carried into all branches of knowledge. The courses were, according to Barnes and Shapin, 'to be scientific; 'pure' rather than 'applied'; factual rather than theoretical or speculative; and 'simplified' in presentation.' It was 'a rarefied, atomized, antitheoretical version of scientific knowledge designed precisely to constrain its recipients and stultify their imagination'.²⁶ This knowledge, in Bakhtin's terms, is an authoritative discourse:

The authoritative word demands that we acknowledge it, that we make it our own; it binds us, quite independent of any power it might have to persuade us internally; we encounter it with its authority already fused to it It is, so to speak, the word of the fathers.²⁷

Delivered in the form of a lecture, with the audience silenced, this form of scientific knowledge demands unconditional acceptance, not discussion and transformation. It is this knowledge that Brougham seeks to popularize: a number of unquestionable proofs of design in Nature which in turn justify the rational organization of society.

It is not so much the content of the knowledge that is important but the unquestioned acceptance of the final signified – the proof of design – which is constantly reiterated in the rhetoric of the scientific popularizers. For these middle class educators, reflection upon this link would cultivate pleasure and obedience among the working classes. Brougham writes:

The possession of knowledge, the having a pursuit of a nature at once so gratifying and so pure as that of learning, must produce the same effect upon the working classes that the possession of wealth does upon the rich; it gives them a direct *interest* in the peace and good order of the community, and renders them solicitous to avoid whatever may disturb it.²⁸

Even Dr Birkbeck, whose mechanics' class at Glasgow was considered the precedent for all the mechanics' institutes, constantly reiterated this bourgeois ideology. Not only is knowledge power, he says, quoting what has become the motto of the *Mechanic's Magazine*, but also it is 'wealth, is comfort, is security, is enjoyment, is happiness. It has been found so completely to mingle with human affairs, that it renders social life more endearing, has

given morality more uprightness, and politically has produced more consistent obedience'.²⁹

As Anne Secord has shown, the working class rejection of bourgeois ideology did not also entail a rejection of science.³⁰ The audience in the lecture did not passively accept what was being taught but instead responded to it internally. Bakhtin and Medvedev maintain that 'a scientific work never ends; one work takes up where the other leaves off. Science is an endless unity. It cannot be broken down into a series of finished and self-sufficient works'.³¹ Yet, this closure is essential for authoritative discourse as 'its semantic structure is static and dead, for it is fully complete, it has but a single meaning, the letter is fully sufficient to the sense and calcifies it'.³² The science that is actually diffused in this context lies on the border of authoritative, and what Bakhtin calls internally persuasive, discourse.

Such discourse is 'affirmed through assimilation, tightly interwoven with "one's own word"'. It is creative and productive, awakening new words from within, and 'does not remain in an isolated and static condition'.³³ The mechanics already possessed a scientific language. A contributor to one of the *Mechanic's Magazine's* imitators, the *Mechanic's Weekly Journal*, objects to Birkbeck's assumption of their scientific ignorance:

In all the manufactures I ever was in, every instruction necessary was freely given to those that could or would take it; and, for my part, I have taken great pains in instructing young men in mathematics, drawing, and making calculations – for all our works are carried on by drawings – every thing is made as plain as possible, – the work is completed by the working men, and set to work by them.³⁴

This knowledge gave them an internal world of signs with which to dialogize the discourse of the popularizers and make it their own. Bakhtin claims, consciousness 'awakens to independent ideological life precisely in a world of alien discourses surrounding it' and that when 'thought begins to work in an independent, experimenting and discriminating way, what first occurs is a separation between internally persuasive discourse and authoritarian enforced discourse, along with a rejection of those congeries of discourses that do not matter to us, that do not touch us'.³⁵ The consciousness that is generated by this contact is what Susan Sheets-Pyenson calls a 'low' scientific culture, an alternative culture alongside that of the dominant 'high' culture, and it is this that is expressed textually by the form of the *Mechanic's Magazine*.³⁶

As the editors believed the Institution would be a site for discussion between mechanics, usually on scientific matters, they foregrounded the speech genre that reflected this: the readers' contributions. When Robertson

turned against the Institution, ostensibly due to its class bias, he retained the contributions as the defining feature of the *Mechanic's Magazine*. Robertson eagerly encouraged his contributors: his prefaces to each successive volume praise them at length to ensure the continuous flow of free copy. It is this structure, which evolved with the *Mechanic's Magazine*, that permits and enacts a textual space for a rival scientific discourse in opposition to that articulated from within the Institution.

The dialogism inherent in the exchange of contributors ensures a certain type of scientific discourse. It is anonymous yet multi-authored, with no one contributor able to close the dialogue. Unlike contemporary 'high' science periodicals like Alexander Tilloch's *Philosophical Magazine* that consisted of a series of papers by named authors, articles in the *Mechanic's Magazine* are usually short, pseudonymous responses. This both denies the authors any extra-textual sources of authority and prevents any finalization in the traditional sense. In a scientific paper, the author seeks to close the subject according to their speech will: the thematic (scientific) finalization coincides with the compositional finalization in order to convince the reader of its truth. Contributions to the *Mechanic's Magazine* are utterances in response to former utterances, and it is the change of speaking subjects that signals the finalization. Whereas the scientific paper seeks to unite thematic finalization with compositional finalization, the contributions in the *Mechanic's Magazine* are only finalized when another author begins, thereby building dialogue into scientific textual discourse.³⁷

The dialogic structure of the *Mechanic's Magazine* enacts internally persuasive discourse. 'The internally persuasive word', Bakhtin writes, 'is either a contemporary word, born in a zone of contact with unresolved contemporaneity, or else it is a word that has been reclaimed for contemporaneity; such a word relates to its descendents as well as to its contemporaries as if *both* were contemporaries; what is constitutive for it is a special conception of listeners, readers, perceivers'.³⁸ This precludes thematic finalization in two ways. Firstly, as each utterance invokes the one before and, as a response could occur at any time in the future, is never closed, the *Mechanic's Magazine* enacts an eternal contemporaneity without beginning or end. In this context, all truths become provisional and contingent and any sense of progress is made through the accumulation of utterances rather than their relevance or correctness. Secondly, the *Mechanic's Magazine* unites its readers with its writers, representing them as one community defined by the form of their interaction and existing in the present. This leads to a degree of equivalency between utterances which foregrounds method over final result. For instance, on 16 October 1824, 'Piger' inquires why the multiplication of two negative numbers results in a positive product. The mathematical and theoretical approaches that correspondents

use to establish a definite proof range from geometry to algebra, to practical examples and, despite each writer reaching the desired conclusion, the debate continues. In this dialogic space, no method has precedence over any other and even after the generation of a required result, it is still not considered absolute truth.³⁹

The *Mechanic's Magazine*, then, represents a textual embodiment of a scientific culture which, because of the principles of dialogue enacted by the text, lies counter to the officially sanctioned science promulgated from the ideological space surrounding the Mechanics' Institution. In the dialogic core of the *Mechanic's Magazine*, the various signs of the wider social world, including those of other scientific cultures, are constantly exchanged, appropriated and revalued, in a context of eternal contemporaneity. W H Brock states that if 'there were various curricular strategies available to protect the fabric of society of late Georgian and Early Victorian society . . . they all had in common the appeal to unchanging law'.⁴⁰ It is precisely this idea of 'unchanging law' that is challenged by the form of the *Mechanic's Magazine*: the final signifieds necessary for its demonstration are fragmented by the shifting values provisionally established by readers' contributions. The text of the *Mechanic's Magazine*, predicated on dialogue not permitted in the dominant scientific codes of the popularizers, provided a textual space for an alternative scientific culture.

HOW DID THE CONSERVATION OF ENERGY BECOME 'THE HIGHEST LAW IN ALL SCIENCE'?

Ted Underwood

Because science was becoming a professional enterprise in the nineteenth century, an inquiry into the 'centrality' or 'marginality' of scientific ideas at mid-century entails two separate questions. First, why did a given idea influence other researchers or fail to influence them? Where professional networks made themselves effective gatekeepers of credibility, this becomes in the first instance a question about the functioning of those networks. But there was also a popular audience for scientific ideas, and another way to assess the perceived 'centrality' of a scientific discourse would be to ask how well it was known to this broader community, and why they regarded it as fundamental or incidental to the scientific enterprise. By the later nineteenth century, this is less a question about the validation of competing claims than about the apportionment of finite public awareness. The universe of authenticated science was growing large; in the general-interest monthlies and nascent magazines of 'popular science', new ideas competed for attention with hagiographic surveys of past achievement. Many claims regarded as credible by researchers were nevertheless ignored by the reading public. For a new idea to be celebrated as a groundbreaking discovery, it needed to possess not only credibility but visible social implications. Indeed, popularizers often promoted scientific ideas by converting statements about nature into statements about human society. In the case

of mid-nineteenth-century enthusiasm for 'the correlation and conservation of forces', British readers perceived a new idea as central to science because they conflated it with older ideas that had already become central to the way they thought about their industrial economy.

By the early 1850s, the idea we call 'the conservation of energy' or 'the first law of thermodynamics' had been expressed in terms a twenty-first-century physicist would recognize. It had been known since the seventeenth century that the *vis viva* of moving bodies, the quantity mv^2 , was conserved in elastic collisions. But in many other processes *vis viva* seemed to be destroyed by friction and lost as heat. In the 1840s, research on 'the mechanical equivalent of heat' by J P Joule and others suggested that there was a fixed ratio of conversion between heat and mechanical power, so that even in friction, *vis viva* might not be absolutely destroyed. Hermann von Helmholtz enlarged the significance of *vis viva* further in 1847 by showing that a system of masses whose motions were governed by attractive or repulsive central forces would always conserve a dynamic quantity proportional to $1/2mv^2$. Bodies would lose *vis viva* by moving against vectors of force and thereby increasing 'the sum of the tensions' between them (conceived as the integral of force across distance); conversely, any loss of 'tension' would produce a corresponding increase in the *vires vivae* of the bodies involved.¹ As W.J.M Rankine restated the idea in 1853, every loss of 'actual energy' is a gain in 'potential energy', and *vice versa*.² The 'energy' conserved would encompass not just the phenomena of gravitation, macroscopic motion and heat, but also electricity and magnetism and any other forces as yet undiscovered (as long as those forces varied only with the distance between centres of mass).

In less than a decade, the newly-discovered principle was hailed as the foundation and capstone of science. Herbert Spencer called it 'the sole truth which transcends experience by underlying it';³ for Michael Faraday, it was 'the highest law in physical science which our faculties permit us to perceive.'⁴ By the 1860s articles on the topic were proliferating not just in magazines of popular science but in general-interest monthlies and weeklies (*All the Year Round*, *Once a Week*, *Good Words* and *Cornhill Magazine*, for instance) where they rubbed shoulders with serialized fiction.⁵ In an introduction to an American anthology on the topic designed for an audience of non-specialists, Edward L Youmans proclaimed that 'the conservation of forces' was 'the highest law in all science – the most far-reaching principle that adventuring reason has discovered in the universe.'⁶ The model of commutable and transmutable agency implied by energy conversion reshaped representations of the human body, and had long-lasting echoes in late nineteenth-century fiction.⁷ All this enthusiasm contrasts instructively with

the history of the second law of thermodynamics, which was articulated only about five years later, but took two decades longer to move to the forefront of public consciousness.⁸

But enthusiasm for the first law – called variously the ‘circulation’, ‘transformation’, ‘correlation’, or ‘persistence of force’ – coincided with deep confusion about its content. Both Faraday and Spencer, for instance, thought the ‘force’ conserved was the instantaneous product of mass and acceleration, rather than the integral of force across distance. Faraday therefore inferred that Newton’s inverse-square law could not be a complete description of gravitation, since bringing two bodies nearer would seem to create, and moving them apart would seem to destroy, force. He used this apparent anomaly to predict a ‘gravelectric effect’ that would be produced when the force of gravity was annulled by distance.⁹ Spencer not only shared this definition of ‘force’, but spoke about ‘the persistence of force’ in terms which reveal confusion about the very notion of a dynamic conservation principle – equating it with the ‘persistence’ (or rather, constancy) of gravitational force that has to be assumed whenever weight is used as an indication of mass.¹⁰

Faraday was writing in 1857, and Spencer in 1862. On a timeline of discovery this falls rather late; even Crosbie Smith’s account of the formulation of the first law, which emphasizes gradual conquests of credibility and not simply date of initial publication, places the turning point in 1852–53. By that date, William Thomson and W J M Rankine had articulated a new language of ‘actual’ and ‘potential energy’, and were receiving praise from a newly-elected president of the British Association for the Advancement of Science (BAAS). ‘From now [1853] on’, Smith writes, ‘the new terminology of energy would replace Thomson’s “principle of mechanical effect” and Helmholtz’s “law of conservation of force” as well as various older versions of conservation of *vis viva*.’¹¹ But though the inevitability of this replacement is clear to us now, and may in 1853 have been clear to well-connected members of the BAAS, it was by no means clear to the Victorian reading public – or to older researchers like Faraday. Even writers like John Tyndall, who did understand that the conserved quantity was proportional to mv^2 and not to ma , continued to use the word ‘force’ for both quantities until the 1870s, or made a distinction between ‘force’ and ‘moving force’ that was easily lost on less mathematically-minded readers. As a consequence, articles about ‘the circulation of force’ in periodicals read by a non-specialist audience continued to promulgate a mistaken understanding of the conserved quantity well into the 1860s. Anne Gilchrist, for instance, agreed with Faraday that the conservation of force proved that Newton’s inverse-square law is an incomplete description of gravity.¹² James Hinton defined ‘force’ even more vaguely as change. ‘Is the quantity of force in nature (that is, of change or tendency to change)

always the same? Science answers this question in the affirmative. The amount of force does not vary.¹³ The word 'force' was ambiguous, but a large number of Victorians were substantively, and not just semantically, confused.

The confusion is not particularly surprising. Force and power are predicates that can be experienced directly and assigned to tangible objects; force is (speaking informally) the weight one struggles to lift; power, the amount of weight an agent can lift in a given time. There were niches in the English language for these concepts to fill. The integral of force across distance, by contrast, is a property of systems, not of an agent; its practical significance as 'duty' or *effet dynamique* only became clear in the late eighteenth century – and then primarily to engineers. It is thus understandable that the new conservation principle should have been popularized as a transformation or circulation of 'force'. But the elusiveness of the conserved quantity we now call 'energy' makes it all the more surprising that the first law should have been acclaimed as an epoch-making discovery. What did Victorians think had been discovered? Not, evidently, the conceptual or practical importance of $1/2mv^2$ in particular. One might hypothesize that nineteenth-century readers were indiscriminately attracted to conservation principles – as instances of the divine Providence revealed in a self-sustaining world. David Gooding, for instance, has argued that Faraday's resistance to mechanical models of force was founded on his view of nature as a self-poised dynamic economy, which in turn was founded on theology.¹⁴ But though this argument is persuasive in Faraday's case, it would be difficult to generalize it to explain the widespread enthusiasm for 'the circulation of force' in the late 1850s and 1860s. The notion of a self-sustaining natural economy had become controversial in the wake of works by Charles Lyell and Thomas Chalmers.¹⁵ By mid-century, in fact, some Christians felt obliged to show that Nature's poise was subject to decay, and dependent on occasional renovation by God. Smith has argued that William Thomson's thermodynamic research seeks for this reason to demonstrate progression and decay in nature.¹⁶ In short, the notion of balance was not a straightforwardly reassuring one at this historical juncture; it would not in itself have provided a reason for Presbyterians and uniformitarians, Sandemanians and radical agnostics to unite in agreement that 'the circulation of force' was a peculiarly central and fundamental law.

But though writers were by no means agreed about the existence or temporal orientation of divine Providence, Victorian writings on the first law do share one recurrent quasi-religious theme. The agnostic John Tyndall expressed it eloquently in a series of lectures delivered at the Royal Institution in 1862, and later reprinted in *Heat as a Mode of Motion* (1863):

Leaving out of account the eruptions of volcanoes, and the ebb and flow of the tides, every mechanical action on the earth's surface, every

manifestation of power, organic and inorganic, vital and physical, is produced by the sun. His warmth keeps the sea liquid, and the atmosphere a gas, and all the storms which agitate both are blown by the mechanical force of the sun. He lifts the rivers and the glaciers up to the mountains; and thus the cataract and the avalanche shoot with an energy derived immediately from him.¹⁷

The parallelism and personification generate a sermonic cadence, but in Tyndall's unorthodox sermon the solar orb usurps God's role as mover and sustainer of the world. The rhapsody continues, however, for another four pages, and it is on the whole less interested in challenging Christianity than in defining sunlight as a point of origin where natural production and human work are fused:

The clover sprouts and blossoms, and the scythe of the mower swings, by the operation of the same force. The sun digs the ore from our mines, he rolls the iron; he rivets the plates, he boils the water; he draws the train. He not only grows the cotton, but he spins the fibre and weaves the web. There is not a hammer raised, a wheel turned, or a shuttle thrown, that is not raised, and turned, and thrown by the sun.¹⁸

The light revealing the world and the labour shaping it are two aspects of a single protean power. The sun, as the source of that power, binds the natural order to the order of economic production – personifying natural force as a worker, and elevating work to the status of an ontological principle. Anson Rabinbach has named this system of belief *productivism*: 'the belief that human society and nature are linked by the primacy and identity of all productive activity, whether of labourers, of machines, or of natural forces.'¹⁹ As Rabinbach has shown, a productivist interpretation of work as natural force undergirds much nineteenth- and early twentieth-century social theory, from the writings of Karl Marx and Friedrich Engels to new sciences of ergonomics and efficiency.²⁰

Victorian writers perceived the 'circulation of force' as a peculiarly important discovery in large part because it seemed to ratify this existing productivist interpretation of work. Evidence for the connection can be found first of all in writers who stated forthrightly that the significance of the new doctrine lay in its power to physicalize the social realm – and particularly, to link labour to sunlight. Herbert Spencer, for instance, made 'The Correlation and Equivalence of Forces' a lynchpin of his philosophy because it dramatized the congruence of social and natural processes:

Based as the life of a society is on animal and vegetal products; and dependent as these animal and vegetal products are on the light

and heat of the sun; it follows that the changes going on in societies are effects of forces having a common origin with those which produce all the other orders of changes which have been analysed. Not only is the force expended by the horse harnessed to the plough, and by the labourer guiding it, derived from the same reservoir as is the force of the falling cataract and the roaring hurricane; but to this same reservoir are eventually traceable those subtler and more complex manifestations of force which humanity, as socially embodied, evolves.²¹

For Spencer as for many other writers, the point of correlating forces to each other lay less in the quantity conserved than in the associated implication that physical, vital, mental and economic forces all shared a connate origin. In introducing his often-reprinted anthology on the topic, Youmans echoed Spencer's emphasis on the first law's power to unify the social and physical realms. Since 'in the dynamical point of view there is a strict analogy between the individual and the social economies', even the forces of 'morality and liberty' should perhaps be correlated to physical force.²²

But productivism did not have to involve such direct Spencerian correlations between social and natural force. Even writers who were cautious about those correlations emphasized the identity of the physical forces revealed in nature and in economic production. And both scientists and journalists used the sun as the symbol of that identity. In the *Proceedings of the Royal Society of Edinburgh* for 1852, William Thomson stressed that 'Heat radiated from the sun ... is the principal source of mechanical effect available to man.'²³ His observation was widely echoed. Herman von Helmholtz observed that 'all force, by means of which our bodies live and move, finds its source in the purest sunlight'; Justus Liebig stressed that the power of the working body was 'derived in the first instance from the sun'.²⁴ An anonymous article in *Chambers's Journal* interpreted the first law as a proof that 'the source of all labour is the sun. All the labour done under the sun is really done by it.' Other periodicals ran comparable articles on 'The labour of the sunbeams' or 'What we owe to the sun'.²⁵

Correlation is not causation. Though popular articles on the conservation of energy consistently stress the identity of natural force and labour, this need not necessarily imply that ideas about work were responsible for the perceived centrality of thermodynamic science. It could just as well imply, conversely, that thermodynamic science gave rise to new theories of work. This is in fact Rabinbach's view: productivism arose from 'the conceptual revolution ushered in by nineteenth-century scientific discoveries, especially thermodynamics'.²⁶ It thus becomes important to show that a fascination with the solar origin of labour preceded anything one

can call thermodynamic science in Britain. In *A Treatise of Astronomy* (1833) – frequently republished throughout the rest of the century as *Outlines of Astronomy* – John F.W Herschel digressed for a moment to make the following observation about the sun:

The sun's rays are the ultimate source of almost every motion which takes place on the earth. By its heat are produced all winds, and those disturbances in the electrical equilibrium of the atmosphere which give rise to the phenomena of terrestrial magnetism. By their vivifying action vegetables are elaborated from inorganic matter, and become, in their turn, the support of animals and of man, and the sources of those great deposits of dynamical efficiency which are laid up for human use in our coal strata. By them the waters of the sea are made to circulate in vapour through the air, and irrigate the land, producing springs and rivers.²⁷

In referring to the 'great deposits of dynamical efficiency which are laid up for human use in our coal strata', Herschel deploys an English calque for *effet dynamique*, J.N.P Hachette's term for the product of weight and height.²⁸ The passage thus reveals the influence of an engineering tradition, and a dimension of measurement, that would eventually figure importantly in thermodynamic research.²⁹ But it would be an exaggeration to call Herschel's train of reasoning in this passage even incipiently thermodynamic. His concern is not to correlate quantities of heat and motion, but to locate the ultimate source of both – and especially, of the economically useful motion latent in wind, water, food and coal.

Herschel's interpretation of coal as stored sunlight was not in itself novel. Writers began describing 'pit-coal' as 'light that has been imprisoned five thousand years' soon after Jan Ingenhousz linked the respiration of plants to the sun in the late eighteenth century.³⁰ But the growing prominence of coal-fired steam engines in the British economy gave those speculations new social significance in the 1830s. In the same year Herschel published his *Treatise of Astronomy*, Thomas Carlyle developed a similar idea in *Sartor Resartus*:

As I rode through the Schwarzwald, I said to myself; That little fire which glows star-like across the dark-growing (*nachtende*) moor, where the sooty smith bends over his anvil, and thou hopest to replace thy lost horse-shoe, – is it a detached, separated speck, cut-off from the whole Universe; or indissolubly joined to the whole? Thou fool, that smithy-fire was (primarily) kindled at the Sun; is fed by air that

circulates from before Noah's Deluge, from beyond the Dogstar; therein, with Iron Force, and Coal Force, and the far stronger Force of Man, are cunning affinities and battles and victories of Force brought about: it is a little ganglion, or nervous centre, in the great vital system of Immensity.³¹

Why does the smith's fire risk being seen as a 'detached, separated speck, cut-off from the whole Universe'? These are not common terms of contempt for a village smith. But they do make sense if the passage is seen as defending industrial work generally against an accusation of detachment from nature.

In the early nineteenth century, the question of whether industrial work was or was not 'detached' from nature closely concerned many people other than Romantic poets and philanthropists. Protection of agriculture against foreign competition had for several decades been justified by a notion that agriculture was – because of its connection to nature – the most productive way of employing a given portion of capital. In agriculture alone, 'nature labours along with man; and though her labour costs no expense, its produce has its value, as well as that of the most expensive workmen.' In manufacture, by contrast, 'nature does nothing; man does all; and the reproduction must always be in proportion to the strength of the agents that occasion it.'³² Adam Smith, who wrote these words, did not himself advocate protective measures against the importation of corn, but his words were widely cited for that purpose, and from the 1790s through the first two decades of the nineteenth century, economists who criticized the protection of agriculture found that they first had to combat a widespread assumption that human labour cooperated with nature only in agriculture, and was most genuinely productive there.³³ In the new American republic Alexander Hamilton spent several pages attacking Smith on this topic.³⁴ The French economist J-B Say devoted much of his *Traité d'Économie Politique* (1803) to a description of nature's role in industrial work: 'The elasticity of steel allows us to form springs that operate clocks; the weight of bodies serves us the same purpose; we turn to our profit all the laws of the physical world. We are almost always working in concurrence with nature.'³⁵ British pamphlet wars on this topic in the first decade of the nineteenth century (involving William Spence and Thomas Chalmers, James Mill and the *Edinburgh Review*) culminated in a debate between T.R Malthus and David Ricardo over the Corn Law of 1815. Malthus defended a theory of rent that represented it as an immediate compensation for the productive power of nature, revealed uniquely in land; Ricardo countered that rent was founded on scarcity, not on the absolute value of nature's work. Nature's powers were engaged equally in all production, and became visible in agriculture only because different portions of land

were unequally productive. Rent was therefore founded on 'an imperfection, compared with the [universally diffused] natural agents by which manufacturers are assisted.'³⁶

This history of controversy is latent in Carlyle's assertion that every smithy-fire is '(primarily) kindled at the Sun'. Carlyle takes the 'Iron Force' of the smithy as a metonym for industrial production, and uses the connection between 'Coal Force' and sunlight to imply that industrial work participates as actively as agriculture in the 'great vital system of Immensity'. I do not mean to say that *Sartor Resartus* is a brief against the corn tariff. But Carlyle's 'natural supernaturalism' quietly assimilates an argument about the naturalness of manufactures that had once been a polemical claim advanced by industrialists and Whiggish economists. The quietness of this assimilation is not a sign that the argument has lost its political force; it is a sign of success. Defenders of the corn laws in the 1830s and 1840s argued increasingly on a prudential basis, saying less and less about nature's unique cooperation with human labour in the tilled field.³⁷ Their opponents had captured that high ground of abstract principle, convincing the British public that industry, like agriculture, drew its productive power from the sun. Though the social significance of the connection between industrial power and sunlight was often left tacit, the connection appealed particularly to rhapsodists of industry like Carlyle and to industrialists like the railway engineer George Stephenson (1781–1848). A number of people who knew Stephenson reported his enduring fascination with the idea that his trains were powered by the sun. Because our only record of the fascination is anecdotal, it is difficult to put an exact date on it, but the statements attributed to Stephenson suggest that the scientific ideas shaping his belief were pre-thermodynamic. Samuel Smiles reports the following conversation with William Buckland, which took place in 1845:

'Now Buckland', said Mr. Stephenson, 'I have a poser for you. Can you tell me what is the power that is driving that train?' 'Well, said the other, I suppose it is one of your big engines'. 'But what drives the engine?' 'Oh, very likely a canny Newcastle driver'. 'What do you say to the light of the sun?' 'How can that be?' asked the doctor. 'It is nothing else', said the engineer: 'it is light bottled up in the earth for tens of thousands of years, – light, absorbed by plants and vegetables, being necessary for the condensation of carbon during the process of their growth, if it be not carbon in a different form, – and now, after being buried in the earth for long ages in fields of coal, that latent light is again brought forth and liberated, made to work, as in that locomotive, for great human purposes'.³⁸

This conversation was filtered through both Buckland and Smiles before appearing in print in 1857, so it is difficult to date specific turns of phrase. The argument put in Stephenson's mouth is, however, very credibly the kind of thing a sixty-four-year-old engineer who had taught himself science as a young man might have said in 1845. Stephenson's argument – that plants imbibe light, and that coal is therefore in effect condensed or bottled light – has little relation to British science of the 1840s, but is entirely in keeping with photochemical theories that were popular at the beginning of the century.

Since Carlyle, Herschel and Stephenson were concerned to underline the continuity of a causal chain, and not to correlate one quantity with another, a twenty-first-century historian would hesitate to describe their statements as precursors of a conservation principle. But Victorian scientists and writers unanimously gave these men credit for anticipating (however intuitively and roughly) the fundamental significance of the 'persistence' or 'correlation' of forces. This again suggests that the new doctrine was perceived as significant because it ratified an older, qualitative connection between labour (especially industrial labour) and the sun. William Thomson acknowledged Herschel's description of the sun as a source of 'mechanical effect'.³⁹ Spencer and the physiologist William Benjamin Carpenter acknowledged George Stephenson in even stronger terms. 'Looking at this matter from the standpoint afforded by the "correlation" doctrine', Carpenter wrote, 'we cannot but feel an increased admiration of the intuitive sagacity of that remarkable man George Stephenson, who was often laughed at for propounding in a somewhat crude form the very idea which we have just been endeavouring to present under a more philosophical aspect'.⁴⁰ At different moments, Tyndall described both Herschel and Carlyle as if they had played John the Baptist to later discoverers. A footnote to the concluding peroration of *Heat as a Mode of Motion* remarks that 'The germ, and much more than the germ, of what is here stated is to be found in a paragraph in Sir John Herschel's *Outlines of Astronomy*'.⁴¹ In 'Personal Recollections of Thomas Carlyle' (1890), Tyndall quotes the 'Schwarzwald' passage from *Sartor Resartus* at length, remarking that 'such passages – and they abound in his writings – might justify us in giving Carlyle the credit of poetically, but accurately, foreshadowing the doctrine of the Conservation of Energy'.⁴²

From a twenty-first-century perspective, I wish to emphasize, these remarks are bad history of science. The dimension of 'energy' itself, with its combination of sweeping generality and mathematical specificity, now seems to be the important innovation associated with this episode in intellectual history. To describe Carlyle or Stephenson as forerunners of energy conservation would be to confuse a conservation law with one of its peripheral and

contingent consequences (a connection between sunlight and terrestrial industry). But as we have seen, Victorians did not necessarily think of 'the circulation of force' as a 'conservation law' at all. Even the subset of writers who were clear about the mathematics of the conserved quantity often seemed to care more about the fact that it could unify natural and economic production. When Tyndall writes that Carlyle's vaguely qualitative link between a smithy-fire and sunlight poetically anticipates 'the doctrine of the Conservation of Energy', he cannot be thinking of that doctrine as an assertion of the importance of any particular dynamic dimension. The significance of energy conservation seems to depend instead on its implication that the sun, in Tyndall's words, 'not only grows the cotton, but ... spins the fibre and weaves the web.'⁴³

A final piece of evidence suggesting that solar productivism contributed importantly to the enthusiasm surrounding thermodynamics lies in the emergence of a subtle priority struggle surrounding John Herschel's 1833 remark that "The sun's rays" are the ultimate source of almost every motion which takes place on the earth.⁴⁴ By 1851, Herschel had begun to suspect that his passing remark had acquired new importance; in reprinting his *Outlines of Astronomy* that year, he underlined his claim to priority by adding a footnote – 'So in the edition of 1833'⁴⁵ – after the sentence about the connection between sunlight and the 'dynamical efficiency' of coal. In editions published after 1857 a further sentence is added to the footnote: 'The celebrated engineer Stephenson is generally, but erroneously, cited as the originator of this remark.'⁴⁶ Perhaps Herschel was tired of reading about Stephenson's 'intuitive sagacity'. The message appears to have reached Samuel Smiles, because he eventually deleted a sentence that, in the 1857 edition of his biography, had described Stephenson's idea as 'a most striking and original one'.⁴⁷ Herschel's possessiveness about his remark of 1833 reveals a growing awareness that it had become something worth fighting over. On the one hand, this shows that a connection between nature and industry glimpsed in the 1830s had acquired a novel prestige in the 1850s through its connection to experiments on 'the correlation of forces'. But it also suggests, conversely, that Victorians perceived 'the correlation of forces' as a peculiarly important and central doctrine in part because it seemed to ratify justifications of industry that had been articulated 20 years earlier.

I have been arguing that productivist celebrations of industrial work as an expression of natural force played an important role in promoting the first law of thermodynamics to the stature of 'the highest law in all science', a pre-eminence that was granted to it by many British and American writers in the 1850s and 1860s. I have not advanced an analogous claim about the earlier process in which a smaller number of researchers formulated pieces of this

doctrine and made them credible to immediate colleagues. It is reasonable to discuss these processes in partial separation; they took place in different decades, in different kinds of periodicals and lecture halls, and required different strategies of persuasion. But it is worth observing, in passing, that a fascination with the link between sunlight and labour is present in researchers like Thomson, Helmholtz, Liebig and Carpenter, as well as in later and more popular writers. It is possible that a productivist emphasis on the naturalness of industrial work will turn out to have played a role in motivating researchers themselves, as well as in permitting those researchers to enlist the enthusiasm of a wider audience. Some 40 years ago Thomas Kuhn observed that the various people who could be described as discoverers of energy conservation tended to share a 'concern with engines' and a pre-disposition 'to see a single indestructible force at the root of all natural phenomena'.⁴⁸ But energy conservation was not the first place where those two nineteenth-century concerns had come into contact. In the 1830s, writers like Carlyle had linked the steam engine to the universally-diffused force of sunlight, in order to show that industry and agriculture drew their productive power from the same sources. The economic history behind such claims suggests that the nineteenth-century pre-disposition to see a single force at the root of all phenomena may have been not only (as Kuhn hypothesized) a residue of *Naturphilosophie* but an affirmation of an emerging industrial system. It is possible that such an affirmation contributed to thermodynamic researchers' sense of the importance of their own discoveries. Certainly, it is what mattered most to the broader public, which was pleased to learn that 'all the labour done under the sun is really done by it.'⁴⁹

‘SCRIPTURAL GEOLOGY’, *VESTIGES OF
THE NATURAL HISTORY OF CREATION*
AND CONTESTED AUTHORITY
IN NINETEENTH-CENTURY
BRITISH SCIENCE¹

John M Lynch

The past two hundred years have seen naturalistic and often materialistic modes of scientific inquiry (as advocated by, for example, John Tyndall and Richard Dawkins) superseding the teleological and theistic methods used by earlier non-specialists. This hardening of the scientific viewpoint (though not absolute, particularly in physics) is interesting when one considers the concomitant increase in the acceptability to the general public of evolutionary explanations, the increase in the popularization of what was seen as ‘good science’, and the parallel reduction of extra-epistemic factors in evolutionary theorizing. In this essay I briefly examine two episodes within the development of a professional Victorian geology: the rejection of ‘scriptural geology’ by the emerging geological community and the controversy over the 1844 publication of *Vestiges of the Natural History of Creation*. Both of these episodes nicely illustrate the boundary work that went into delineating the emerging community from those that used, and what would become, sidelined modes of discourse. As a part of this delineation – and over the course of the early part of the century – geology was characterized as a practical, specialized, active, ‘masculine’ endeavour that was amenable to Christianity. Such rhetoric

allowed the community to distance themselves from both Biblical literalists and the amateurs who in many ways founded the field, thus moving the adjudication of scientific claims out of the public sphere into the hands of what would eventually become the scientific profession.²

I

Contrary to popular opinion, Christians prior to the early nineteenth century were not doctrinally bound to the idea of a young earth in accordance with the calculations of Archbishop James Ussher of Armagh. Michael Roberts has convincingly shown that few educated Christians followed such a recent chronology even at the time Ussher was writing in 1658. Instead, many supported a Chaos Restitution model in which an initial chaos of undefined duration was followed by a recent reordering in six days and a global Deluge. While the Deluge was deemed to be responsible for the geological strata and humankind was thus viewed as a recent creation, few educated individuals believed in a literal six-day creation, and as awareness of the vastness of geological time grew, the period of initial chaos was greatly extended. Thus, prior to 1800, while investigations were giving scientific evidence for an ancient earth – and natural philosophers were generating naturalistic theories to explain the origin and form of the earth – Anglican theologians were already accepting a non-literal reading of Genesis.³

During the winter of 1803–04, the Scottish theologian and later founder of the Free Church, Thomas Chalmers, presented a series of lectures which outlined a reconciliation of the apparent incompatibility between the Genesis account of creation and the findings of the developing science of geology. He argued that the language of scripture allowed for an indefinite gap between the first and second verses of Chapter I, which in turn allowed for a time in which geological formation could occur before the traditional six-day creation – which, in this view, represented a restoration of the whole earth after periods of activity and eventual devastation. The ‘Gap theory’, as it became known, aimed to show that Genesis and geology could live side-by-side, once one was willing to interpret the scriptures to allow for the apparent age of the earth.

Chalmers’ theory received explicit support from such geologists as William Conybeare and William Buckland – who eventually recanted his previously held belief in a global Deluge – before being modified by John Pye Smith in 1839. Smith abandoned the idea of a worldwide Deluge in favour of a regional tranquil flood, and posited that 6,000 years ago, God destroyed and flooded a portion of the earth, subsequently restoring and repopulating it as the Biblical Eden. The original creation occurred prior to this point in time, and thus

Chalmers' worldwide re-creation became localized to a smaller geographical area. This theory received support from such respected individuals as Adam Sedgwick, William Whewell and John Herschel, yet was denounced by the few remaining (but very vociferous) Anglican literalists who remained wedded to a recent creation of the universe despite mounting evidence for deep geological time. Though not themselves geologists, these keen but biased amateurs saw fit to comment on the ability of geologists within their sphere, and felt that the findings of science were becoming increasingly atheistic.⁴

Remarkably enough, these scriptural geologists, as they became known, were not beyond modifying scripture to suit their own exegesis. Andrew Ure allowed for an extra day of creation that occurred after the Deluge, and Hugh Miller noted that Granville Penn interpreted verses of Genesis that conflicted with his view as (in Miller's words) 'mere idle glosses, ignorantly or surreptitiously introduced into the text by the ancient copyists'. This ability to pick and choose which parts of the text to treat literally led Miller to exclaim:

It need not surprise us that a writer who takes such strange liberties with a book which he professes to respect, and which he must have had many opportunities of knowing, should take still greater liberties with a science for which he entertains no respect whatever, and of whose first principles he is palpably ignorant.⁵

This raises an interesting point. Even in their theology – what should surely have been their strongpoint – the scriptural geologists were suspect. If anything, they were fighting a rearguard action against those mainstream theologians who sought, in the mind of the scriptural geologists at least, to compromise the divine word.⁶ They saw that if the earth was populated prior to the Edenic creation, then there must have been death, and if there was death, then biblical themes of original sin and redemption through the sacrifice of Christ were called into question. At the heart of the matter were the historicity of Christ, his redemptive actions, and the future of humankind. Such extra-scientific concerns would lead James Mellor Brown to attack Buckland for his quasi-traitorous act – as a churchman – of denying the ubiquity of the Biblical Deluge:

This affords another illustration of men who pull down the bulwark, but disclaim any intention of endangering the citadel. The Trojan Horse, drawn within the walls of the devoted city by friendly hands, is a standing emblem of men acting under the unsuspecting guidance of the Evil One.⁷

While geologists like Buckland were allegedly acting unwittingly for the 'Evil One', Mellor Brown felt that no particular expertise was required when

examining scientific matters. Rather, the natural world could be understood by reasoning from the pages of Scripture. Thus, revelation was exalted over natural theology, and Mellor Brown was swimming against the stream of natural theology which was becoming particularly prevalent in British thought.

The reaction of the geological community to these individuals inhabiting the scientific borderlands can probably best be summarized by a statement made by Charles Lyell in 1827:

We cannot sufficiently depreciate the interference of a certain class of writers on this question ... While they denounce as heterodox the current opinions of geologists, with respect to the high antiquity of the earth and of certain class of organic beings, they do no scruple to promulgate theories concerning the creation and the deluge, derived from their own expositions of the sacred text, in which they endeavour to point out the accordance of the Mosaic history with phenomena which they have never studied, and to judge of which every page of their writings proves their consummate incompetence.⁸

To those engaged in practical, empirical study of nature, the scriptural geologists and their ilk threatened to present science as antithetical to Christian piety – a charge which the early geologists denied, and indeed, given the roots of historical geology, proves meaningless. The tension between the two groups is particularly highlighted in the relationship between the scriptural geologists and the British Association for the Advancement of Science (BAAS). Founded in 1831, the BAAS was an organization of significant cultural power, counting as it did nobility, gentlemen of leisure, engineers, and members of the emerging scientific profession within its ranks. The scriptural geologists saw the Association as advocating theologically unsound and even heretical ideas, particularly in its members' willingness to assign science and religion to separate spheres of knowledge. This view led the likes of Frederick Nolan, William Cockburn and Henry Cole to attack the Association and prominent Broad Church scientists such as Adam Sedgwick who would, while addressing the Geological Society of London in 1830, excoriate Ure for his book, *A New System of Geology*, which he termed a 'monument of folly'.⁹ In this work, Ure explicitly claimed the utility of a young earth interpretation of geology, while advocating a single Ice Age, which was a natural consequence of a global Deluge. While Ure was a competent chemist, Sedgwick felt that his geological theorizing was worthless, and furthermore endangered the work of the GSL's members:

The goodly pile, gentlemen, which many of you have helped to rear, after years of labour, has been pulled down and reconstructed; but with

such unskilful [sic] hands that its inscriptions are turned upside down; its sculptured figures have their heads to the ground, and their heels to the heavens; and the whole fabric, amid the fantastic ornaments by which it is degraded, has lost the beauty and the harmony of its old proportions.¹⁰

The following year, Sedgwick famously recanted his belief in the ubiquity of the 'philosophical heresy' of adopting 'the diluvian theory, and [referring] all our superficial gravel to the action of the Mosaic Flood'. Sedgwick's recantation occurred for simple evidential reasons; there was no evidence of 'man, and the works of his hands' in these deposits. Thus, for Sedgwick, in this case at least, evidence took precedence over biblical exegesis. Sedgwick's apostasy, at least as far as the scriptural geologists were concerned, was completed with his *Discourse on the Studies of the University* (1833), in which he praised the attempts of Chalmers to reconcile science and religion, while noting that 'to confound the ground-works of philosophy and religion is to ruin the superstructure of both.'¹¹

One of the most prominent Anglican critics of geology was William Cockburn, Dean of York. The arrival of the BAAS at York in September 1844 gave Cockburn an opportunity to directly attack the perceived infidelity of the BAAS geologists. In delivering a paper, which was described by one observer, the publisher Robert Chambers, as 'a kind of indecent oddity', Cockburn prompted Sedgwick to rise and captivate 'the audience for an hour and a half, alternatively charming them by his vast learning, and throwing them off their gravity by the most amusing and grotesque illustrations.' Sedgwick saw the work of the BAAS members to be collecting facts and not forwarding hypotheses, and it was Cockburn's ideas that were 'irrational guesses and absurd hypotheses'. Feeling that the charges of antiscripturalism were dead, Sedgwick refused to react to Cockburn's later challenges for public debate, and the era of scriptural geology began to lurch to a close.¹²

II

A few weeks after the York meeting, an anonymous work, *Vestiges of the Natural History of Creation*, carried evolutionary thinking from the streets into the drawing rooms of polite society. It has long been recognized that the publication of *Vestiges* was, in many ways, a watershed in the development of Victorian science. Studies over the past 40 years have culminated in Secord's magisterial *Victorian Sensation*, a work to which the reader is directed for a full exposition of the work and its author, the aforementioned Chambers. In *Vestiges*, Chambers outlined a hugely synthetic theory of change, one that viewed creation as an ongoing process ruled over by material laws put in

place at the beginning of time. Thus, Chambers felt he was providing a scientific theory that would be welcomed by the likes of the member of the BAAS, particularly when compared with the religious dogmatism of Cockburn. After witnessing the York meeting, Chambers had some expectation that Sedgwick would support his theories, but this was clearly not the case. Sedgwick had previously referred to transmutation as 'no better than a phrenzied dream' and *Vestiges* gave him no reason to change his opinion. In the *Edinburgh Review*, Sedgwick launched an eighty-five page assault on 'the filthy abortion'. As Secord notes, the voice that Sedgwick creates in the review is distinctly Christian and masculine, yet one that was not beyond contradicting its previous pronouncements. Given the progressive nature of Chambers's ideas, Sedgwick, like Buckland, had to shift his previous belief in progress within the fossil record, adopting instead the attitude that progress was apparent. This *volte-face* would be noted by Chambers.¹³

Sedgwick was not the only prominent gentleman of science who attacked *Vestiges*. William Whewell weighed in with a slim volume entitled *Indications of the Creator*, which largely consisted of extracts from previously published works. A forty-five page preface to the second edition of 1846 echoed some of the points Sedgwick made against Cockburn. Chambers was faulted for bringing 'dogmatic' ideas to the public before they had been examined and refuted by scientists; 'hypotheses which have thus been advantageous to science have been tentative hypotheses admitted into the mind for trial and rejected if the facts were found to contradict them; not dogmatic hypotheses published to the world'. Whewell emphatically argued that science could not answer questions of ultimate origins, as an 'impassable abyss separates us from the origin of things'. Thus, he believed that humility was the preferred stance for an investigator, who must face the fact that 'the chain of existing causes does not, in any case, conduct us back to its origin' and 'we cannot obtain from science a complete view of the history of the universe.' In a letter to Fredrick Myers, Whewell proclaimed:

If the mere combining chemistry, geology, physiology, and the like, into a nominal system, while you violate the principles of each at every step of your hypothesis, be held a philosophical merit, because the spectator is seeking a wilder law than gravitation, I do not see what we, whose admiration of the discovery of gravitation arises from its truth, and the soundness of every step to the truth, have to do, except seek another audience.

Herein lies the heart of the problem. Whewell believed that the public's acceptance of the ideas put forth in *Vestiges* did not reflect well either on the

readership's state of mind or the ability of the scientific community to explain 'good science' to them.¹⁴

Faced with such a reception, Chambers worked hard to win his professional audience over, and *Vestiges* did not remain a static target for critics. He made significant changes to his ideas, often after surreptitiously consulting authorities such as George Fownes, Edwin Lankester and William Carpenter.¹⁵ By 1853 and the tenth edition, *Vestiges* was a radically different beast, both in appearance and content, from previous incarnations, and it was this version that led to what was probably the most virulent published reaction to *Vestiges* – the review by Huxley in the *British and Foreign Medico-Chirurgical Review*. The 29-year-old Huxley was at this time still finding his feet in the scientific establishment and, experiencing some difficulty in obtaining a professional scientific position, was supporting himself as a journalist and popular lecturer. He thus appears to have directed at least some of his ire at the still anonymous Chambers.¹⁶ It was in this edition that Chambers replied to many of his critics by demonstrating that many of the authorities he used were also the authorities of contemporary science. In attacking Sedgwick, he not only pointed out that the geologist had once held the general position that he himself advocated, but also that, being 'neither an anatomist or naturalist' Sedgwick could not be accepted as an expert in such areas.¹⁷ Thus, central to his defence were his views on authority and professionalism. Chambers was a talented amateur in a period which was still largely the age of the amateur with few professional scientists. There was no clear distinction between 'science' and 'non-science' in this period, and those who considered themselves professional scientists often disagreed on the distinction between the two spheres. As *Vestiges* went through successive revisions, movements within the community were leading to increased calls for professionalization, and by the tenth edition the negative reaction of those such as Huxley was, as Hodge notes, largely in response to Chambers's status as an anonymous amateur who was attempting to bring scientific issues directly to the public without the mediation of the professionals who existed at that time.¹⁸

While Chambers accepted his own ability to popularize ideas, and deplored the limited vision of specialists, he still believed that specialists were the sole authorities within their sphere (in this matter he distrusted Sedgwick), and felt that popularizers were not necessarily to be trusted. This is wonderfully illustrated by his reaction to Miller, who had been 'officially patronized and applauded' despite the fact that 'twenty chapters from a mere working geologist and litterateur like Mr Miller could not stand against' any of the ideas of Louis Agassiz. Needless to say, in this instance, Chambers felt that Agassiz's observations supported his theories – despite the fact that

Agassiz himself had little time for *Vestiges*, which he saw as being ‘supported only by antiquated assertions’.¹⁹

Chambers saw himself as a natural philosopher rather than as a scientist. Throughout his writings in *Chambers’s Edinburgh Journal*, he seldom used the term ‘science’, preferring ‘natural philosophy’, which cloaked himself in an older, more inclusive tradition.²⁰ This allowed him, as an amateur (as were most natural philosophers), to believe that he could synthesize the writings of the professionals in a manner that was not only novel but in a form that would educate the masses. Like his critics Miller and Huxley, Chambers functioned as a popularizer. Yet, while Chambers was castigated for his efforts, Miller was accepted owing to his obviously more pious attitude to nature and its lessons, and Huxley, as part of the emerging scientific elite, could effectively delineate boundaries and thus maintain his status as respected scientist and trusted popularizer.

Indeed, the popular nature of *Vestiges* caused problems for Charles Darwin. While his reading of Sedgwick’s attack in the *Edinburgh Review* may have left Darwin feeling confident in aspects of his theory, he was worried about the fate of his work if it should be used by a popularizer such as the anonymous Vestigenarian.²¹ In the letter which would be later used by Lyell and Hooker to establish his precedence over Wallace as discoverer of natural selection, he asked Asa Gray:

Not to mention my doctrine; the reason is, if anyone like the Author of the *Vestiges* were to hear of them, he might easily work them in, & then I [should] have to quote from a work perhaps despised by naturalists & this would greatly injure any chances of my views being received by those alone whose opinions I value.²²

This too was the fear of many of the early ‘Darwinists’ such as Huxley – that their developing ideas would be taken by a popularizer and brought into disrepute. It was clear that Darwin and his supporters had to maintain control over the dissemination of their ideas if they themselves were to successfully overcome the opposition of individuals like Sedgwick and Whewell who had so strenuously opposed Chambers. These considerations would be seen in the treatment of such individuals as Henry Charlton Bastain and St George Jackson Mivart in the years following the publication of *Origin of Species*.²³

Vestiges, far from reflecting atheistic ideology, clearly posits a unitary impersonal divine being as responsible for the natural laws that brought everything into being.²⁴ In so doing, it projects a deistic image of a universe created by a beneficent and wise Creator, an image in accordance with the accepted viewpoint of many eighteenth-century natural philosophers to whom

the existence of natural law exalted rather than diminished God. Yet there was a tension with more traditional views. In the first edition, Chambers noted that 'the ordinary conceptions of Christian nations on this subject are directly derived from Scripture, or, at least, are in conformity with it', yet 'there is not the least appearance of an intention in that book to give philosophically exact views of nature.' Chambers believed that universal law was simpler, and had greater beauty, than the exceptions demanded by those who clung to literal belief in Scripture. For him, the Creation 'flow[ed] from commands and expressions of will, not from direct acts'. While few at the time were Biblical literalists, Chambers's words would still have proved discomfiting, if only because of their directness, and he eventually removed any discussions of the scriptural implications of creation by natural law from the sixth edition and onwards.²⁵

As far as moral philosophy was concerned, Chambers's admiration for Bentham puts him strongly in the Utilitarian camp, which was very much in decline and strongly opposed by liberals such as Sedgwick and the dons at Cambridge. Natural philosophy and Benthamite utilitarianism all declined together. Faced with the decline of natural philosophy and the increasing specialization of science with its search for law, Chambers desired a mechanism by which he could reconcile his beliefs with scientific observations. Far from being atheistic, he was merely a relic of a past time, and was in a sense – perhaps unconsciously – trying to rationalize what amounted to pre-nineteenth-century beliefs.²⁶

III

Just as Chambers in many ways exemplified a slightly out-moded worldview, so too did the scriptural geologists in their backlash against geological developments in the early 1800s. As has been pointed out elsewhere, few of these critics had any first-hand experience in field geology, most showed little presence in the emerging professional societies, had comparatively few scientific publications, and would not have considered themselves to be 'geologists' (though the term 'scriptural geology' appears to have originated with the 1826 publication of George Bugg's two volume work of that title). James Moore notes that these individuals were 'largely pre-professionals or members of the older professions – classically educated and genteel laymen, versed in polite literature; clergymen, linguists and antiquaries – those, in general, with vested interests in mediating the meaning of books, rather than rocks, in churches and classrooms.' At a time when individuals were beginning to professionalize and attempt to become the sole authority on interpretation of the natural world, the scriptural geologists harkened back to a day when the educated

layman was seen as an equal partner in such discourse. It is clear that, as Martin Rudwick writes, the reaction of the scriptural geologists was 'in part a cultural [one] to the social and cognitive exclusion of all but self-styled experts from an area of speculation that, in the heyday of theories of the earth, had been open to all.' By the admittedly lax standards of the day, the scriptural geologists were still largely dilettantes – educated men, in a few cases with some scientific background, but not by any means conversant with the ideas and methodologies being used by those who were attempting to professionalize geology. To modify a statement by John Hedley Brooke, one could be a reader of geology, one could think about the relations between geology and faith, one could be a patron of safe geology, one could be an author and a teacher of geology – without actually doing any geology. Most of the scriptural geologists did not do any geology, and this, as much as anything else, seemed to irk the geologists.²⁷

It is clear that Chambers suffered from the same 'social and cognitive exclusion' that Rudwick highlights, and the anonymous author was not perceived as doing science. Facts and observations came first, theories only followed later; as Rudwick notes in another context; 'Whether their attitude to theorizing was cautious and skeptical ... or enthusiastic and confident ... [the geologists] all deferred to what they called the "Baconian" foundations of their science, at least by insisting rhetorically on the primary significance of first-hand observation in the field.' Geology was a masculine, robust, activity. The act of geologizing was seen to be a manly, rough endeavour that involved direct interaction with the world around the observer. Geology was actively experienced, not witnessed second-hand through the writings of others. Robert Dick wrote, 'When I need to know what a rock is, I go to it. I hammer it; I dissect it. I then know what it really is', and Edward Forbes felt that 'a man, to be a true geologist, must have a body as well as a soul. No mincing town-dandy or sickly bookworm is likely to thrive in the profession.' Chambers, a 'sickly bookworm' as a youth and somewhat physically handicapped as an adult, attempted to redefine himself as a man of science, published 13 papers between 1843 and 1864. Supplemented by larger works such as *Ancient Sea Margins* (1848), Chambers began to receive what Secord sees as grudging notice by the scientific community to which he was a previous 'hanger on', perhaps tainted by his interest in phrenology. Such recognition was, alas, worthless to the Vestigenarian who was officially distanced from Chambers, and would always remain a dilettante in the eyes of geologists.²⁸

Whether perceived as dilettante or respectable amateur geologist, Chambers continued to attend BAAS meetings. Just as Sedgwick had his day at a BAAS meeting – vanquishing Cockburn in York – so to would Huxley in debating Wilberforce in Oxford 16 years later. A 75-year-old Sedgwick was in

attendance as President of the geological section, and Huxley's presence at the debate is owed to encouragement by Chambers that very morning. An iconographic episode, seen by later generations to mark the triumph of scientific reason over religious rhetoric, the incident marks – as well as any other – the ascendancy of the new young professionals and the eventual passing of the era both of the Oxbridge churchmen and the enthusiastic amateur.²⁹

‘THIS HOUSE IS A TEMPLE OF RESEARCH’¹: COUNTRY-HOUSE CENTRES FOR LATE VICTORIAN SCIENCE

Donald L Opitz

The present chapter considers sidelined sites of science in the late-Victorian period, precisely the moment during which many historians have characterized the sciences as having transformed into specialized professions based in academic and government-sponsored research facilities, like the Cavendish Laboratory, Cambridge and the Royal Botanic Gardens, Kew. The growth and character of these facilities have received much historical attention; yet, surprisingly, their forerunners have not. If we agree that a significant institutional shift took place during this period, we might ask what were those forerunner institutions and what became of them?

Several histories of Anglo-American scientific institutions have contributed to a ‘canon’ of nineteenth-century science as a progress story of a shift from ‘amateur’ to ‘professional’ science in which the former involved individual, leisurely endeavours and the latter professional teams, working in ‘Big Science’ at new research institutions.² Contemporary late-Victorian discourse championed state-supported research while bemoaning the inadequacy of private initiative.³ More recent laboratory studies, in turn, have tended to celebrate specialized, extra-domestic sites – museum, laboratory, observatory – while sidelining ‘private’ domestic spaces.⁴ The image has

emerged of 'domestic' science as quaint and idiosyncratic, although various scholars are succeeding in dismantling this skewed representation.⁵

One of the best documented cases of a nineteenth-century amateur tradition, located at the private estate of John William Strutt, third Baron Rayleigh, Terling Place, in Essex, has helped create this idiosyncratic imagery. One of Rayleigh's biographers identified him as 'the last of the great "sealing-wax and string" individual researchers' in physics, while another noted the death of his son, also a physicist, as an end to 'the investigator working independently in his own laboratory'.⁶ Considering the Terling laboratory in a broader social context gives a rather different image. Terling Place participated in a wide network of scientific country houses – with vital linkages to newly built and singularly-devoted sites – sustaining a knowledge economy supported by private, patrician resources and values.⁷ I will sketch the character and extent of one such network in which Rayleigh centrally participated, consider how its perspective reconciled scientific and evangelical religious beliefs, and analyse its influence in professional developments that integrated aristocratic, 'amateur' values with extra-domestic 'professional' interests. This influence challenges the notion of an amateur science supplanted or sidelined by professionals and, as we will see, offers an alternative framework for understanding late-Victorian institutional developments in science.

Science as an aristocratic practice enjoyed a long tradition that shared features with other forms of learning among a leisured class.⁸ Agriculture provided one important impetus for the country squire to acquire and apply scientific learning.⁹ By the mid-nineteenth century, however, an increasingly utilitarian interest in the mechanical arts, and the economic promise of industrialization, characterized a new gentry deriving from both the traditional aristocratic order and the rising professional elite. Investment in railroads, overseas ventures and manufactured goods created unprecedented economic opportunities, and the application of scientific knowledge in the industrial project gave science, a new relevance beyond leisurely pursuit. William Parsons, third Earl of Rosse, joined the Royal Commission for the Great Exhibition of 1851 believing that it would encourage engineers and men of science to make their work more relevant to industry and manufacturing.¹⁰ He advocated, in his presidential address to the Mechanical Science Section of the BAAS in 1857, the advancement of this science devoted to 'questions of practical engineering': 'It is obviously the interest of public men, no less than of private individuals.'¹¹ Rosse's advocacy, combined with his own engineering and astronomical practice at Birr Castle, underlined a marked shift in science's status among the 'leisure' class that challenges Martin Wiener's controversial thesis that Britain's industrial spirit owed its decline to a disinterested aristocracy.¹²

Thriving among the aristocracy was a wide network indeed of country houses infused in scientific thought and practice, intimately linked across broad distances through tightly-knit social relations. Housing the Rayleigh scientific circle were three key estates: his own Terling Place; Whittingehame, home of his brother-in-law Arthur James Balfour (afterwards first Earl of Balfour) in East Lothian; and Hatfield, home of Balfour's maternal uncle, Robert Gascoyne-Cecil, third Marquis of Salisbury, in Hertfordshire. Each estate comprised tenant farms, manor house, sculptured gardens and servants, and served a vital public role as a paternalistic seat around which local village life revolved.¹³

Rayleigh planned his extensive, private laboratory at Terling Place shortly after he inherited the estate in 1873, having already designed and built a smaller laboratory at nearby Tofts, his temporary residence after marriage. Following his mentor, James Clerk Maxwell, he understood that the word *laboratory* 'denotes a place to work at experiments and connotes a place, full of articles not wanted at present and liable to noxious fumes.'¹⁴ Rayleigh placed the laboratory in the conservatory wing and stable lofts, where several separate rooms with ventilation systems for exhausting chemical fumes could be accommodated. He specialized compartments for distinct kinds of work – desktop study, chemical preparations, photography and spectroscopy. His use of his manor house's space was by no means limited to these places, as he found the basement floor, a stable foundation for an interferometer and the long basement tunnel, ideal for acoustics experiments. One set of experiments on sound perception placed the observers in the adjacent master bedroom in isolation from the sound sources. Tubes fed through the walls conveyed the sound to the observers' ears.¹⁵ Except for his accomplishments as Director of the Cavendish Laboratory in Cambridge (1879–84), he produced the bulk of his 446 papers here.¹⁶ [Figure 1]

Rayleigh's marriage to Evelyn Balfour, sister of his Cambridge friend Arthur Balfour, inducted him into a scientific, aristocratic family. The Balfour seat at Whittingehame was home to a private museum of fish, bird, insect and fossil specimens collected on and near the estate by an industrious team of eight young siblings passionate about natural history. These collections launched the naturalist careers of Frank Balfour in embryology and Alice Balfour in entomology.¹⁷ Throughout their lives, the siblings and their spouses (especially Henry Sidgwick, husband of Eleanor Balfour) often converged at their large house, commandeering various rooms as workspaces. A letter by Arthur Balfour, written in January 1878 records a typical holiday scene here:

At present this house is a 'Temple of Research' ... Frank, though he supposes himself to be taking a holiday, is I believe preparing his book



Figure 1. Rear view of Terling Place, Essex, from a photograph taken in 1905. The conservatory on the left provided passage to the laboratory. By permission of a private owner.

for the press (in the smoking-room), Eustace (in the billiard room) is drawing the illustrations for *his* book, Henry Sidgwick is finishing his article for the *Encyclopaedia* and writing a paper for the Metaphysical Society in *his* sitting-room, while I, in mine, am working very hard at my ‘immortal work’.¹⁸

Frank Balfour’s *Elasmobranch Fishes* appeared in publication later that year, and Arthur Balfour’s ‘immortal’ book *A Defence of Philosophic Doubt* followed in 1879.¹⁹ Arthur’s dislike of experimental work (‘drudgery’) propelled him towards philosophical study, but he readily lent a hand in his brother-in-law’s experiments at Terling Place and other sites, even serving as a research subject in Rayleigh’s experiments on colour perception. These, in turn, informed Balfour’s philosophy.²⁰

Like Terling Place, Salisbury’s Hatfield House – the third site for country-house science in this network – boasted a chemical and physical laboratory constructed in a converted wine cellar [Figure 2]. There, Salisbury conducted experiments on electricity, magnetism and spectroscopy, when an otherwise busy political career permitted. Significantly, German physicist Heinrich Kayser cited Salisbury’s spectroscopic work as the first to establish that a gas of low temperature could emit a bright light spectrum.²¹

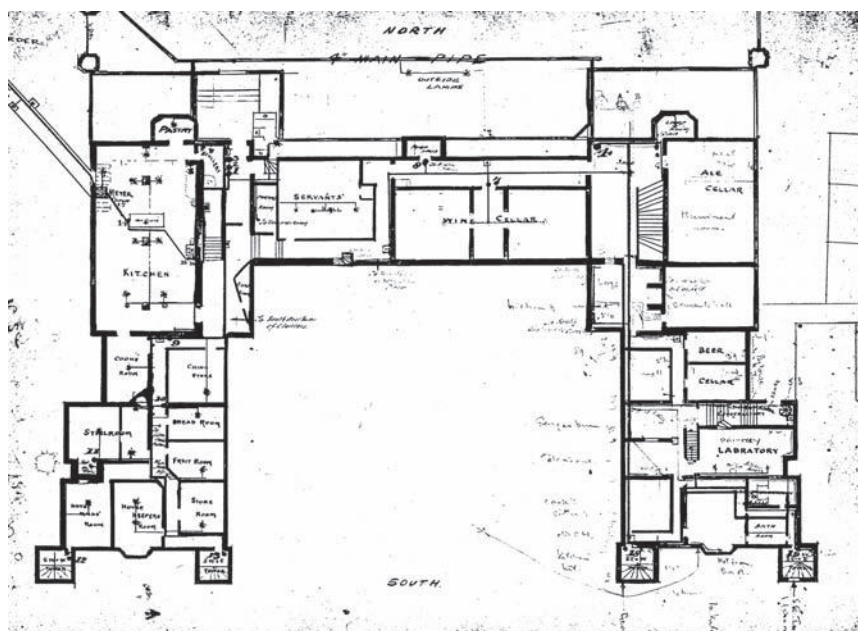


Figure 2. 1869 Plan of the Hatfield House basement, showing the ‘laboratory’ in the lower right. Courtesy of Robin Harcourt Williams. ©The Marquess of Salisbury, Hatfield House.

His scientific work had its mishaps, too: on one occasion, as his daughter recorded, ‘the household was roused by a loud explosion, and its master issued from the laboratory, covered with blood and severely cut about the face and hands, to explain to his terrified family – with evident satisfaction at the accurate working of chemical laws – that he had been experimenting with sodium in an insufficiently dried retort.’²²

This country-house circle extended still further, including Eustace Balfour’s father-in-law, George Campbell, eighth Duke of Argyll, BAAS President and participant in the post-Darwinian debates on evolution, who practised natural history at his Scottish country home, Inveraray Castle.²³ Friends of Rayleigh, including Maxwell and William Thomson – members of what Crosbie Smith has termed a ‘North British’ alliance of physicists – also adapted spaces for experiments at their town and country houses.²⁴ Another set of acquaintances, the Parsons’ of Birr Castle, Ireland, offer the strongest case of Victorian country-house science. Led by Rosse, family members and hired servants carried on an influential industry of astronomical, microscopic, photographic and engineering works. As friends and, later, in-laws of Rayleigh, they effectively linked Ireland to the British network.²⁵

Together, this extensive, interconnected social economy linking Terling, Whittingehame, Hatfield, Inveraray and Birr provided spaces for practical work, intellectual abstraction and social intercourse. The close association of the families inhabiting and visiting these houses sustained a shared value system, a fundamental, defining feature of which was an undercurrent of evangelical, religious faith.²⁶ The network's vision for professional identity-formation contrasted with a more middle-class and Non-conformist perspective, and its key figures participated in the 'professionalization' process by offering an identity that blended elements of the 'professional' with those of the gentlemanly 'amateur'.

The roots of this aristocratic circle's religious perspectives lay in the family backgrounds. 'To begin fearlessly, I am an Evangelical in heart & soul', wrote Rayleigh's Anglican grandmother Marianne Vicars to her daughter. 'It is indeed a sect everywhere spoken against ... it is a sure sign to me that they hold the *Truth*.'²⁷ Her son's death in the Crimean War produced a potent evangelical martyr under the pen of Catherine Marsh.²⁸ Her daughter, Clara Vicars, followed her teachings, upholding the importance of atonement, personal discipline and familial devotion. After Clara's marriage to John Strutt, second Baron Rayleigh, she implemented a regime of daily family prayer and closely involved herself in her children's upbringing. The religious, aristocratic couple educated their children in a home-based system in which science and theology commingled. The family's 'book-room' housed both the prayer books and the first experimental apparatus of their son, the future physicist. In his more mature years, it also served as the site of his first private laboratory.²⁹

The third Baron Rayleigh's in-laws added to the evangelicalism of this network. Lady Blanche Balfour displayed an evangelical zeal in the upbringing of her children, her direction of local parish affairs, and her family's practice of daily prayer and Bible readings. 'Teach me to use my influence over each and all', she would recite, 'that I may guide with the love and wisdom which are from above the religious education of my children.' She directed her children's home education in religion and natural science, viewing each as a means for understanding God's design. 'Broad Church' in outlook, she attended services in Churches of both England and Scotland with her children. Rev. James Robertson, the Whittingehame minister, whom she had personally recruited, found her practice of motherhood and influence on her distinguished children ideal subjects for the evangelical magazine, *Good Words*.³⁰

Embued with this kind of religious home education, the members of these families publicized a message that reconciled science and Anglican theology. The limited circulation of Arthur Balfour's first book *A Defence of Philosophic Doubt* in 1879 prompted criticism by scientific naturalists like Leslie Stephens; it also attracted the admiration of Asa Gray, the Harvard botanist, who desired

to place Darwinian theory on a Christian foundation.³¹ As he prepared his lectures on 'Science and Religion', delivered at Ball College, Gray found, *Defence* to be 'the most masterly essay, I have seen of late years' and sought an introduction to the young philosopher.³² With a much wider circulation and several editions, Balfour's second book *The Foundations of Belief* roused public controversy in the 1890s. Now a well-known Conservative politician, Balfour directly attacked scientific naturalists and made more explicit arguments in favour of Divine intervention and the importance of basing science upon faith. His book convinced the elderly Thomas Huxley to pen a rebuttal, which never appeared in full owing his death.³³

Balfour's maternal uncle Salisbury shared this vision. With practical experience in experimental science, he spoke credibly on scientific matters, though he strongly opposed scientific naturalism. Salisbury presided over the BAAS meeting at Oxford in 1894, just prior to the sensation of Balfour's *The Foundations of Belief*. Biometrician Karl Pearson noted the similarity of the uncle's and nephew's views and their political bearing: 'Mr Balfour's demonstration that naturalism affords no basis for ethics, and Lord Salisbury's attack on science – his new appeal to the argument from design – will go far, in the absence of any prominent theologically-minded Liberal politician, to bring the new bigotry into line with the Tory party.'³⁴ While 'science' was never Salisbury's direct target, he questioned the accuracy of natural selection in light of Kelvin's argument that the earth was much younger than required by Darwin's theory. Salisbury effectively attacked one of the scientific naturalists' fundamental pillars and promoted, instead, theological explanations for evolution and other unsolved mysteries.³⁵

As early as 1884, Rayleigh, also in a BAAS presidential address, reconciled science and Anglican theology while criticising scientific naturalism. 'In his heart', he professed at the Montreal meeting, a scientific worker 'knows that underneath the theories that he constructs, there lie contradictions which he cannot reconcile. The higher mysteries of being, if penetrable at all by human intellect, require other weapons than those of calculation and experiment.'³⁶ More muted about religion than his in-laws, Rayleigh's evangelical religiousness, nevertheless, failed to escape popular attention. In the final months preceding his joint publication, with chemist William Ramsay, of their discovery of argon in late 1894 and early 1895, Rayleigh maintained his usual attendance at his parish's Sunday morning services even while Ramsay pressed onward at Terling. Others knew better of their uneasy collaboration, but a minister proselytised it as an exemplar of Divine will in the evangelical magazine *Morning Watch*: 'That is one of God's laws: He sends forth His servants two by two, for union is strength, and union is love, and God is love.'³⁷

Rayleigh's interest in psychical research also bridged science and theology and bolstered his stance against scientific naturalism. With the Balfours, he participated centrally in the Society for Psychical Research (SPR) which, as Richard Noakes has argued, included an elite response to spiritualism and the 'Sidgwick circle's' rejection of evangelical teachings – though Henry Sidgwick's rather agnostic views were clearly not shared by either the Balfours or Rayleigh.³⁸ Popular media sometimes missed the SPR's distinct experimental methods that damaged spiritualists' credibility. Making this oversight, a reporter noted Rayleigh's 'spiritualism' as a foil for scientific naturalism: 'It is quite possible, that a Cambridge Don, having taught his class the properties of fluids, will invite i[t]s members to come and converse with their departed grandmothers, so that a knowledge of science may not turn them into Materialists.'³⁹ Whether as psychical researcher or religious aristocrat, Rayleigh's reconciliation of science and religion prompted the evangelical Anglican Sir George Stokes, Lucasian Professor of Mathematics at Cambridge, to invite Rayleigh to lecture before the Victoria Institute, a society of Christian scientific men devoted to combating materialism.⁴⁰

Members of this country-house network found no better opportunity to evangelize science alongside their religious values than by assuming and fulfilling 'professional' duties, often at the surprise of their contemporaries.⁴¹ Rayleigh's appointment as a Cambridge Don, which proved effective in advancing the Cavendish Laboratory, nevertheless created an unlikely union of 'Lord and Professor' as noted by *Punch*: '[W]hen before was Lord seen in gown of Don, or Don in robe of Lord?'⁴² Such a blending of identity served important public functions, like the BAAS presidency in 1884. In the Association's first overseas meeting, Rayleigh represented, as Balfour pointed out, 'not only Science but England and the Peerage!'⁴³ Rayleigh's influence may be measured by the persistence with which the physical science community sought his leadership in key professional endeavours – a unanimous vote for his Cavendish directorship followed by his Royal Institution professorship and Royal Society appointment as acting chair of the National Physical Laboratory's executive committee. Such practical, public work fell within his sense of evangelical, aristocratic 'duty'. As an aristocrat fulfilling 'professional' roles, he endowed a professionalizing science with respectability, and, as one onlooker noted, by virtue of his scientific work he did the same for the aristocracy.⁴⁴ [Figure 3] His example offered an alternative to the middle-class professionalism envisioned by scientific naturalists, of whom his evangelical circle was severely critical.⁴⁵

In the natural sciences, Frank Balfour paralleled his brother-in-law's influence. By the early 1880s, Balfour's pre-eminence in evolutionary embryology, experimentally vindicating natural selection, prompted the ageing

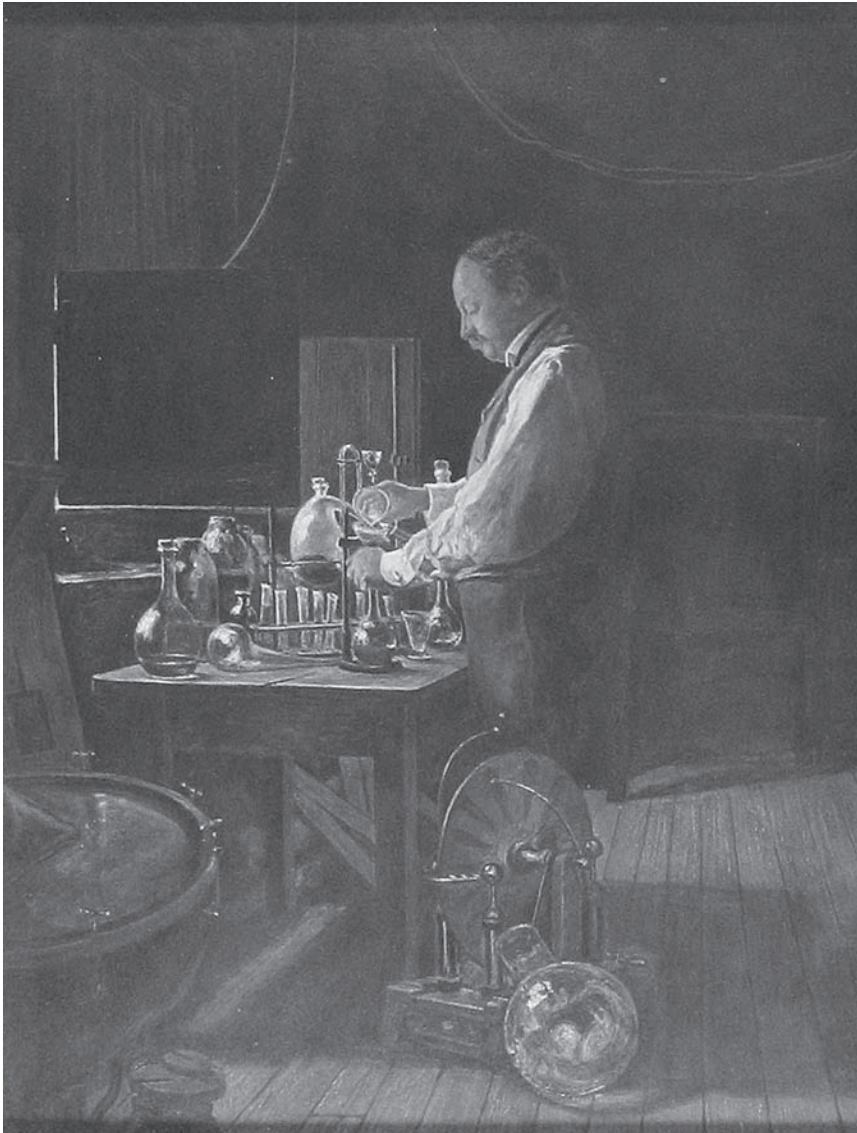


Figure 3. Lord Rayleigh in the Terling laboratory, painted by Sir Philip Burne-Jones, 1888. From a photograph taken by Les Goodey, courtesy of Jonathan Smith. By permission of the Master and Fellows of Trinity College, Cambridge.

Darwin to opine that his younger protégé would ‘some day be the chief of English Biologists’.⁴⁶ Darwin’s ‘bulldog’, Huxley, concurred; he felt that ‘the future of zoological science in this country was very safe in his [Balfour’s] hands.’⁴⁷ Representations of Balfour consistently idealized his appearance and

abilities, revealing virtually no flaw except perhaps his left-handedness.⁴⁸ Of his teacher and friend, American palaeontologist Henry Osborn reminisced, 'I felt that, I was in the presence of a superior being, of a type to which I could not possibly attain' – an impression not atypical of the reverence Balfour garnered.⁴⁹ His charm played no small role in attracting enrolments in his morphology classes at Cambridge that steadily increased nearly ten-fold within a span of six years (1875–81).⁵⁰ His success prompted colleagues to successfully advocate the creation of a professorial chair for him at Cambridge. At the very basis of his popularity was his gentlemanly character.

Idealizations of Balfour survived his own death through the memories of his wide circle of friends, underlining the power of his polite identity.⁵¹ The very tragedy of his fall in 1882 while climbing the never-before-ascended peak, Aiguille Blanche de Peuteret, in the Swiss Alps, only added to his heroism. A memorial fund attracting world-wide subscriptions sustained an ongoing school of research, 'the Balfour Studentships', at Cambridge. Also at Cambridge, largely owing to his family's contributions, a zoological library and women's biological laboratory were founded, and Hildebrand's bronze bust of him in climbing attire secured his permanent visual presence.⁵² [Figure 4] Even for the SPR, Balfour's noble spirit added further purpose to research in spirit communications.

Rayleigh's and Balfour's blended identities as 'amateurs' and 'professionals' reflected broader social transitions in science.⁵³ By the end of the nineteenth century, 'amateur' science straddled two distinct images, both of which needed to reckon with professionalizing discourse. Private, domesticated initiative received celebratory praise, as from Kelvin, who stated in 1898 that 'the best of experimental physics in this country has been undertaken by wealthy amateurs.' Nevertheless, advocating for government sponsorship, he and his peers also acknowledged that their private initiative was insufficient for establishing a national science of the first rank: 'We must not fold our hands and wait till they give results which are of great practical good.' The debates surrounding the founding of the National Physical Laboratory (eventually built in Bushy House, formerly a royal palace) seemingly revisited those sparked by Charles Babbage's 1830 essay 'On the Decline of British Science'. Despite often disparaging rhetoric, amateur practice remained centrally important to the advancement of British science during this late period.⁵⁴

To return – which sites for science preceded academic- and state-sponsored spaces and what became of them? The discourse of nineteenth-century professionalization has implied a sidelining of private, amateur science, and indeed their domestic moorings. However, the centrality of scientists' homes for experiment, field work and intellectual intercourse into the twentieth century recommends an alternative conclusion, that domestic sites continued to

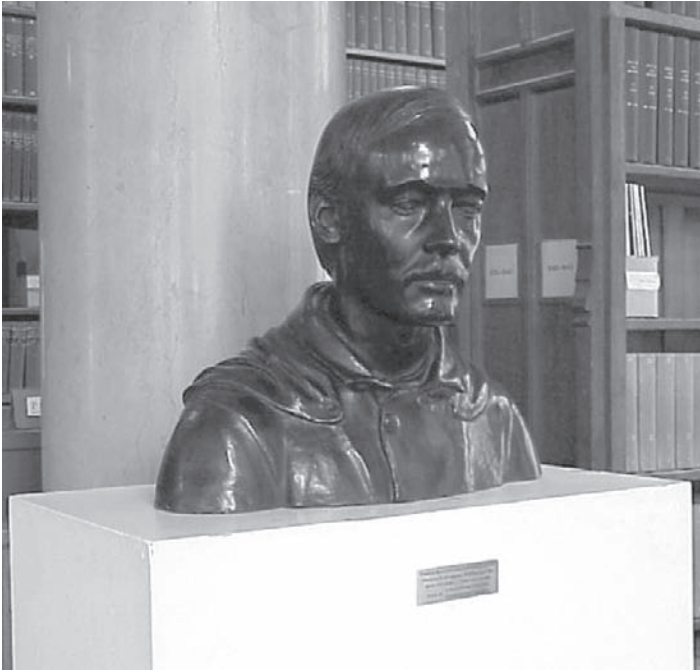


Figure 4. Bronze bust of Francis Maitland Balfour in climbing attire, sculpted by Adolf von Hildebrandt, in Florence, 1883. Presented by John Willis Clark and George Horace Darwin to the Cambridge University Museum of Zoology. From a photograph taken by Frances Wei, courtesy of Clair Castle. By permission of the Balfour and Newton Library, Cambridge.

sustain a scientific economy alongside the gradual development of ‘professional’ institutions. Extensive country-house networks, like that in which Rayleigh, Salisbury and the Balfours participated, provided influential, alternative visions for professionalizing science than those advocated by scientific naturalists. The evangelical, private practice associated with this aristocratic circle represents an important, domestic tradition extending beyond the Victorian period – even as agricultural depression and political reform severely curtailed the aristocracy’s power and income.⁵⁵ As new, ‘professional’ research sites increasingly dominated the institutional landscape in science, we must consider how the relationships between domestic and extra-domestic research spaces varied and yet remained vitally important.

SECTION III

Entering The Modern

FRESNEL'S PARTICULAR WAVES: MODELS OF LIGHT AS CATALYTIC MODES OF WORLDMAKING IN EARLY MODERN TIMES

Bernd Klähn

Introduction: Visual Worldmaking

Leading, primarily, a visual existence, man's main orientations are focused on light and perception of light. The optical channels of a human being are superior to the rest of his sensory inputs, as long as quantity and complexity of data are under question. With regard to linguistic design, metaphors of light hold an eminent position in human discourse, connected especially to fields of knowledge, learning, moral quality and aesthetic perfection. As a consequence, they stand out in all ideological terrains of human self-constitution, concerning basic religious concepts, their narrative transformations as well as rationalistic sublimations of these models, during modernity's development towards the age of enlightenment.

This essay deals with the interrelationship between scientific—especially physical—theories and the ideological groundwork of a given culture. Centring on influences emerging from pre-formal concepts of reality, the problem will be raised if any explicit physical theory will succeed in keeping a constitutive distance from such pre-scientific forms of worldmaking.¹ It will need to be asked if modern quantifying approaches to nature fall under

the clandestine seizure of intersubjective and intracultural dynamics, importing a basic matrix of anthropomorphic worldmaking into those formal and quantified versions of reality we use to call sciences.²

Complying with such a project gives good reasons to keep a secure distance from positions which take the social construction of all forms of reality and worldmaking for granted. It is not the structuring idea of this paper to underline the presumable irrelevance of scientific hopes for objective truths – a tendency one might call ‘epistemic relativism’.³ But what is feasible here is the awareness of an intraoccidental cultural dichotomy, leading to a situation where discussions about the ‘two cultures’ may bring about fruitful results concerning a basic understanding of natural and social sciences and humanities. What is most interesting for the essay at hand is that the idea of a permanent and unbridgeable hiatus between scientific and social-philosophical thinking appears to be without solid ground. Even in modern times these ‘two cultures’ have been one;⁴ and it will be a main intention of this paper to underline the hidden connection between scientific and social-philosophical worldmaking, by taking a critical look at a development within modern optics, when the upheaval of classical concepts by an intellectual outsider unveiled the implicit socio-philosophical pre-conceptions of all those who firmly believed in the unassailable solidity of scientific reasoning.

Yet in contrast to well-known interpretations of epistemological change within scientific thinking,⁵ the following considerations emphasize that internal paradigmatic confrontations within physical and optical theoretical development do not suffice to explain the relevance of these conflicts with regard to their implicit but persistent manifestations in other cultural areas. These parallels do not only betray a strong connection between strategies of thinking in different fields, but also underline overlaps with social forms of organization, prominent within modernity’s evolution. Long-term tensions within social development seem to appear simultaneously within modern physics, touching upon questions of allonomy and autonomy, inner and outer authority, also discernible in changes between different systems of political power.

Looking back into the history of modern and antique optics, this essay will endeavour to highlight how the main strands of this development converge in the achievements of Augustin Fresnel, whose ingenious and autonomously forwarded insights into the nature of light give an illuminating idea of the interweaving of physical model-making and (unconscious) processes constituting social forms of subjective self-awareness. In the course of this ‘tour de Fresnel’, it will have to be illustrated that modern sciences – central or marginal – do not change arbitrarily from one established paradigm to a

new, more successful one, but are bound up in a kind of epistemological entanglement with an overall ideological development: a socio-cognitive confederacy scientists normally do not bother to register.⁶

Occidental Optics: From Harmony to Autonomy and/as Interaction

Antique Physics: Circling in Celestial Harmony

The subsequent line of thought will survey basic patterns of antique world- and self-understanding, focusing on pre-physical concepts of light and their effects on modern culture. Leaving aside – in a first step of simplification – all the special complexities of pre-Socratic philosophy, Plato's concept offered an appropriate and exemplary point of departure for understanding antique philosophy and its imprint on Europe's cultural development.⁷ In his harmonically closed cosmos, well-rounded, abstract ideas secured the world's permanent idealistic constitution, never prone to the pressures of progress. Functioning as a symbolic, almost iconic, representation of this harmonic perfectionism, the circle took the role of a geometric prototype, illustrating such a perfect, self-hermetic but also self-transgressing harmony, conceived of without end or beginning.

Heavily leaning on this fundamental model of cyclical holism, Plato's philosophy looked for circles, cycles and repetitions in all forms of reality. On this track, celestial mechanics moved into the scope of ancient interest, presenting an almost ideal arrangement composed of permanence, cyclical repetition and perfectly circular forms, structures and processes.⁸ Heaven and sky, representing God's power and presence, induced Greek astronomers to erect classical astronomy on the groundwork of spherical calculations and prognostications.⁹

According to the perspective of physical analysis, optics and celestial mechanics function as basic elements within such a constitutive theoretical fabric. Both fields of investigation converge on the ideal typology of celestial motion. Only there, eternal circles seem to be secured, while the cardinal characteristics of terrestrial regions are straight lines or curvatures, as well as varying motions, continuously changing their basic items. Within antique ideology, terrestrial science is a contradiction in terms, as long as general findings are at issue. As a result of this ideologically pre-formed situation, and notwithstanding an enormous mathematical competence, there are no terrestrial physics in antique times dealing with mechanical bodies and their interactions. This missing idea of physical interaction gives an indication of one important centre of antique worldmaking: astronomical movements are supposed to be free of

interactions. Celestial bodies are moving on their own, following the principle of eternal harmony and divine autonomy.

For Plato, there was only one exception, showing man transgressing his own singular existence and reaching out for permanent constructive results: within the field of political organization, human communities could be apt to outlast the transitoriness of human individual life, offering a place on earth where unchangeable laws may hold. Thus, adapting antique astronomy to human needs did not yield early versions of terrestrial physics – but political philosophy. Staging some kind of antique ‘elementary particle physics’, where the elements in question were human beings well-equipped with rationality, political philosophy enabled antique ideology to register and emulate the ideal essence of divine harmony.

This tight connection between models of interaction and anthropomorphic activities became one of the typical peculiarities of occidental self- and world-awareness. Already in antique times we come across those anthropomorphic traits that will always be virulent in later stages of occidental models of interaction, even in highly technomorphic stages of theoretical physics. Concepts of interaction – and this is an early analytical result of this essay – are deeply anchored in human self- and world-understanding. Whenever denying this pre-structured epistemological fundament, modern physics (involuntarily) undermines self-generated epistemological structures, without making critical and creative use of it.

Focusing on the development of modern optics, and especially Fresnel’s contribution to it, subsequent considerations will concentrate on the question in which way this ‘anthropomorphic physicalism’ has marked (or has been marked by) modern ideologies of power. Or in what respect eventual epistemological innovations have induced a splitting up of this nexus, liberating scientific thinking from the implicit coercion of philosophical and social contextualizations.

Descartes’s: Rainbows and Isolated Rational Subjects

Whatever approach to a deeper understanding of modern times is consulted, the interplay between reformation, geographic exploration, Renaissance and emerging natural sciences – associated with the names of Luther, Columbus, da Vinci and Galileo – is usually interpreted as a constitutive (or at least symptomatic) mode of this epoch. René Descartes’s, fulfilling the role of a brilliant actor in this network of intertwined causes and effects, occupies the position of an eminently competent protagonist, in whose philosophy and scientific determination these different strands converge.

Descartes's rationalism introduced the autonomous subject into the pivotal zone of world and historymaking, attaching this radically subjectified version to internal self-awareness and self-certainty, exclusively based on the immediate presence of reason. Installing his 'Cogito, ergo sum' as a programmatic subjective motto for a radical change from outer authority of antiquity (cosmological harmony) to the inner authority of modern times (rational self-awareness of the subject),¹⁰ Descartes's concept prepared the ground for subsequent conflicts between world- and self-understanding in the centuries to come. Questions concerning the relationship between intersubjectivity and power acquired the status of touchstones for Descartes's new ideology of the rationally founded subject. Remarkably, Descartes's subjectified forms of rationality included a swing towards scientific worldmaking: his main opus, *Discours de la méthode*, was published in one volume with two treatises on optical questions and problems of celestial mechanics. For an outstanding modern thinker like Descartes, there was an evident union between rationalistic self-awareness and basic theory-making in the fields of optics and mechanics.

As to his optical research, Descartes was a man of completion, bringing classical optics (based on rays of light) to an appropriate end, than taking a vanguard position within optical sciences. He seemed satisfied with the elimination of the antique hiatus between earth and sky, offering a very elegant trigonometric form for the law of refraction. Furthermore, he set out to explain the colours of the (semi-circular) rainbow by applying the law of refraction, centring his argumentation on spherical drops of water.¹¹ Differing greatly from antique ideas of the polis, where every individual had to fulfil a special task in favour of a harmonious totality, Descartes's raindrop-individuals were only bound to the laws of refraction and reflection, realized within their singular and autonomous spheres of innerness, giving rise to a pluralistic phenomenon, the dispersion of light. The almost chaotic interplay of drops only increased the phenomenon's intensity and secured the circular structure in the sky.

Undeniably, Descartes's subjective rationalism had a strong influence on his physical models. Rational autonomy of singular elements, the basis of his *cogito*, determined his modes of interpretation and the choices of themes. The problem of interactions between his elementary 'subjects' did not move into his centre of discussion. Choosing topics like refraction and colours of rainbows, Descartes stayed in a theoretical terrain, where the interaction between single drops and elements of light seemed totally irrelevant to the effects observed. He was addicted to an extreme model of (non-)interaction between individuals, highly esteeming the creativity of singular elements, while the overall effect resembled a mere macroscopic reflex of

these elementary enactments. Under this perspective, a rainbow was just a two-dimensional semi-circle, the individual drop, on the other hand, a three-dimensional complete sphere, including and anticipating the whole colourfulness of the rainbow. In Descartes's opinion, interaction, even as cooperation, was not a relevant concept for modernity.¹²

The Diffraction of Light: Newton's Absolutist Model of Interaction

Isaac Newton's *Philosophiae naturalis principia mathematica*¹³ are famous as a turning-point study in the fields of gravitation and dynamics – and also as a successful attack on the antique dichotomy between heaven and earth. There is only one physical science left in Newton's *Principia* – the physics of sky and earth, comprised in a single theoretical approach, based on terrestrial experiments.¹⁴ The leading principle behind this concept is Newton's law of gravitation. Aside from the fact that Descartes's formal idea of rationality has been substituted by Newton's quantitative concept of mass, Newton's 'gravitating subjects' are almost identical with Descartes's rational subjects. But here, modern minds part. While equality remains Descartes's *ultima ratio*, Newton's gravitating subjects are determined by the amount of mass they consist of. In Newton's world, bigger masses induce bigger effects.

Starting from this simple variation of the equality principle, Newton favoured a similarly clear concept of interaction. Bodies interact in pairs, attracting each other in proportion to their masses. Focusing on astronomical problems which could be understood as two-body-problems, Newton introduced a mode of intersubjectivity into Descartes's isolated milieu of rational subjects.

Seventeen years later, in his *Opticks*¹⁵, Newton illustrated basic interactions between light and mechanical bodies by applying his explicitly corpuscular conception¹⁶, consisting of two essential levels of description. First, particles of light were subject to forces from their environment similar to gravitational effects. Second, these forces were omnipresent, but their manifestation remained confined to boundary situations. The light corpuscles themselves, according to Newton's model, had different sizes, resulting in different colours whenever they got separated and regrouped – conforming to their masses – by outer forces.¹⁷

Even here, comparisons with background concepts concerning social interactions arise. Newton has re-confined Descartes's autonomous light-subjects to the influence of external forces. He has even promoted a divergent tendency within: in his world of optical elements, the competences of individual particles differ, as does their ability to evade such incursions. In Newton's model, colours of light – like differences within society – stand for

enormous individual differences. This inequality is the essential reason for different optical effects, discernible in interactions of light with mechanical bodies. Descartes's starting point, the elementary identity of reasonable subjects, is left behind.

This opto-philic comment on socio-political conditions converged in those considerations Newton and his contemporaries applied to a 'modern' optical phenomenon, not discussed by antique authors: the diffraction of light. As an elementary phenomenon, it may easily be observed whenever light passes a solid body producing a shadow. Coloured fringes, for the first time systematically described by Grimaldi,¹⁸ appear at the borders of these shadows, especially when extremely small objects are used. This new phenomenon which could not be explained by geometrical optics and the classical laws of reflection and refraction, became a touchstone and a neo-logical platform for a modern typology of optical models, comparable to the change in optical theory some hundred years later, when absorption and transformation of light initiated the quantum revolution of optics at the beginning of twentieth century.

Just like Grimaldi and other experimentalists before him, Newton illuminated very small objects with beams of sunlight.¹⁹ He observed colourful fringes framing the whole shadow and, in addition to it, he registered a broadening of the shadowy zone. But in spite of his experimental precision, he pointedly denied what generations of interested people had seen before: even in the shadow there were thin illuminated stripes, giving every indication that part of the light beam entered the dark zone. Radically averse to any wave theory of light, Newton almost hysterically disputed this effect:

The Waves, Pulses or Vibrations of Air, wherein Sounds consist, bend manifestly, though not so much as the Waves of Water. For a Bell or a Cannon may be heard beyond a Hill which intercepts the sight of the sounding Body, and Sounds are propagated as readily through crooked Pipes as through straight ones. But Light is never known to follow crooked Passages nor to bend into the Shadow.²⁰

Evidently, he was desperately trying to save a model of light based on gravitational interactions between light corpuscles and massive mechanical bodies, thus underlining the predominance of external control over singular and different forms of individuality.²¹ It is a cardinal idea of this essay that Newton's irritation concerning a wave theory of light may plausibly be accounted for by deep-rooted objections to socio-political and intersubjective concepts, implicitly connected with these forms of scientific world-making – an aspect which becomes more plausible, when Augustin Fresnel's

role within this process of constructing a modern theory of light moves to the fore.

Fresnel's Radical Wave Theory of Light: The Liberation of the Autonomous Subject Within Nineteenth-Century Optics

Looking back at the history of modern wave theories of light, Christiaan Huygens and his model of light propagation in the form of non-periodic wave impulses plays a major role. Though the missing periodicity did not allow an attempt at a coherent explanation of colours, he was the first to support the idea that the shock wave fronts of light propagation might be composed of an infinite number of elementary wave fronts, superposing each other to form a macroscopic effect.²² With such a conception at hand, Huygens went back to a Cartesian idea, advancing his 'light subjects' to almost identical actors and constructors of effects produced by light.²³ But physics had to wait for the French Revolution till a French engineer eliminated Newton's theory of light within some years – while bringing Huygens' promising, but defectively constructed, ideas of light propagation to a consistent end.

The French Revolution, a decisive turning-point in occidental history, banned the idea of absolutist interaction – as unilateral domination – from the minds of the enlightened public. Augustin Fresnel may be regarded as the eminent enactor of these new concepts within physical optics, rejecting external manipulation and outer control from the spiritual groundwork of optical worldmaking. His basic conviction centred on a radical expansion of Huygens' wave-like model. Starting from the supposition that light consisted of equally constructed elementary waves interfering in the process of light propagation, he denied any control exerted on these waves from outside. Colours, interference patterns and all other optical phenomena were, according to Fresnel, the results of a non-disturbing and undisturbed interaction between these elementary waves. In his eyes, they superposed, but never changed, their true elementary character.²⁴ With Fresnel's theory of light, enlightenment returned to its original source and true home: optics.

Fresnel's fascinating and autonomous achievements have to be measured against the flabbergasting talents of another creative genius – and not only in the fields of optics: Thomas Young. Undoubtedly, Young was the first to show that Newton's experimental findings could be explained in a perfectly consistent manner, by applying a wave-concept of light. His famous double-slit experiment is still a milestone in modern optics, proving the undulatory theory in a simplified and straightforward device.²⁵ But, aside from the fact that Young was an unsteady guest in physical optics, never interested in staying too long in a field of research when the main aspects had been demonstrated, he

was still – paradoxically – a steady follower of Newtonian concepts. His double-slit-experiment forced light to pass two slits simultaneously, so that the binary interaction of two elementary waves behind the slit could be observed. There was no idea of a pluralistic ensemble of waves in Young's conception. This revolutionary idea had to wait for Fresnel, with his open-minded approach to difficult problems.

It is important to stress this point, for the critical historical evaluation tends to reduce Fresnel's work to the secondary achievement of a busy worker, rounding out the grandiose idea of a genius (Young) with interesting, but not at all basic, supplements.²⁶ In the light of the perspective taken in this essay, the opinion is diametrically opposed to this monolithic way of ignoring the grandiosity of Fresnel's undertaking. Yet the following analyses also have to show why Fresnel's conception cannot simply be included into a sequence of highlights, forming a straight line of progress within modern physics. They mark a new peripheral starting point in modern optics, not at all compatible with classical Newtonian nor genial Youngian considerations.

As an engineer for road- and bridge-construction, Fresnel had only rudimentary knowledge of optical phenomena and could not refer to relevant textbooks: they did not exist. Being totally involved in directing and supervising street-building-projects in France, Fresnel led an isolated life, looking for intellectual diversion in different fields. Starting in philosophy and theology, he switched over to optics later on, totally uninformed, in the beginning, about any relevant optical theory of his time. After the political turmoil of Napoleon's return from Elba in 1815, he finally succeeded in 1816 to present his autochthon ideas to the French *Académie des Sciences* – to be informed by Arago that Thomas Young had preceded him, more than a decade ago.²⁷

But Fresnel soon realized that he had only been crossing Young's and Newton's optical tracks. His 'traditional' approach at the beginning of his research was to assume that a special wave, the so-called 'diffraction wave', was produced as rays of light passed a body's edge and was mainly responsible for the emergence of colourful effects. This initial theory was still anchored in mechanical thinking, but has already turned its back on Newton's gravitational absolutism. In a first step, Fresnel substituted a long-distance force by a local impetus, a singular collision between light wave and mechanical body. This is what Thomas Young took for granted, too, when he developed and explained his double-slit-experiment. It is instructive to realize here that a change from particle to wave concepts did not eliminate the dominant modes of mechanics; it merely tuned them down from long-range to short-range interactions.

Finally, in the case of light-body-interactions, Fresnel altered his concept totally, starting from a supposition that had never been put forward before. In this early stage of his research, he made up his mind to assume that all effects of diffraction – colour, broader shadows, striped areas etc. – were direct and self-produced results of the light's wave character and quite independent of any pre-supposed mechanical interaction between a massive body and extremely small light particles. Clearing the terrain during a short orientational phase, Fresnel left aside all absolutist residual components, lending total structuring verve to light alone – light waves produced colours simply by interacting with one another, without changing the state they were in. The significance of a mechanical body contracts to a residual presence. It does not exert any influence on passing light waves, but only absorbs a part of the light's total wave field. This passive elimination without any further effect on the rest of the light wave results – under special conditions – in a local and stationary inference pattern, a pattern, usually not observable because of its quickly changing and fluctuating dynamics.

This liberated interference model of light accounted for the emerging structures of the well-composed colourful whole by claiming the superposition of identical autonomous elements. Without any manipulation from external forces, Fresnel had adapted a physical model of interaction to the changed socio-political conditions and intersubjective interactions of nineteenth century – a model, never before applied to physics. Even a genius like Thomas Young, working on the margins of accepted scientific theories, did not concoct such a radical break with classical modern physics. Such a 'liberal' model of interaction was devoid of any compatible ideological fundament before the French Revolution, being dependent on the humus of bourgeois individualism – and a marginally implemented mind, like Fresnel's, deeply enmeshed in practical observations and activities, philosophical meditations and the simple intention, to solve problems within a given matrix of ideas. Which is not to say that scientific findings and theoretical constructions are secondary cognitive products, emerging after decisive changes in political and ideological areas. But it will be difficult to maintain that these socio-philosophical changes and their fostering of a liberated rational subjectivity are only coincidental within physics's gradual disavowal of Newton's absolutist particle-theory of light.

In this final version, still holding valid in contemporary classical wave optics,²⁸ Fresnel conceived of physical interactions between light elements in the form of a hyper-complex field of superpositions. This complexity could be accounted for by the extreme similarity of the interacting elements. Without this volume of identical elementary waves no interference patterns could be discerned, no explicit and static order would emerge. So Fresnel's

theory of light, strangely but consequently affiliated with an implicit theory of social interaction, returned to Descartes's radical concept of rational equality. But the Cartesian isolationism of 'lone (rational) wolfs' got a new intersubjective impetus by Fresnel's principle of interfering elementary waves. This interplay of identically pre-conditioned actors means that equally constructed elements may be able to produce new macroscopic structures, without changing each other. The autonomous subject of modern times has never been more radically installed than in Fresnel's optical conception.

By this idea of non-manipulating interaction, Thomas Hobbes' fixing of individuals as each other's rationally instructed murderers had been radically infiltrated. Identity of interests and preferences is a positive component, implicitly introduced by Fresnel into his physical model. Interests – the difference within similarity, physically speaking: the different wavelengths of light – admit for interactions that do not affect the rational self-definition of the single subject. Even interactions with other interests (i.e. wavelengths) are possible and do produce interference patterns. But these patterns, resulting from difference, have no durability, no static value. They are states of fluctuation in a dynamic interplay between different (social/physical) actors, not at all interested in one another's transformation or even elimination.

As is true of other concepts of light in occidental history before, Fresnel's optical theories give the opportunity to construct, or disclose, a fascinating relationship with socio-semantic considerations. The implicit connection between elementary light waves, being capable of omni-directional interactions with one another and thus generating overall structures without limiting their freedom, concedes to modern optics a role on the stage of a 'second Enlightenment', a kind of camouflaged report on the theory and praxis of social processes. In this case, the special attraction of physics, especially optics, lies in its potential for simplification and radical reduction, starting, as with all theories of enlightenment and modern rationality, from simple elements and their interactions. It is this basic rationalistic inkling for immediately sensuous and at the same time theoretically simple phenomenological approaches which pushes physical models – often against the totally averse self-understanding of their inventors – into the boundary regions between social and physical world-orientation.

Transitions to Quantum-Optics and Their Socio-Political Background Logic

At the end of nineteenth century, modern physics approached another crisis, once again, ensuing from unexplained optical phenomena. These phenomena concerned, using Einstein's famous formulation, 'the production and

transformation of light',²⁹ i.e. a phenomenological frame of minor interest for previous generations of physicists who were more interested in 'pure' optical cases. But leaving 'pure' optics in favour of transformational phenomena was a representative trend in occidental culture near the end of nineteenth century. Discussions about a pure reasonableness of subjectivity had long since been abandoned; the actual trajectory of cultural preference was leading from Nietzsche, Bergson, William James and Freud into the psychic, non-rational interiority of the subject. Here we find modernist alternatives to rationality and formalized versions of innerness: associations, drives, emotions, dreams, hallucinations etc., endowing the subject with new, non-rational faculties – and problems.

Even theories of light answered this challenge with new forms of model-making, turning their attention towards processes taking place inside illuminated objects – questions, modern optics had scrupulously conserved for more than four hundred years under the cover of professional secrecy. Brachial interactions with rigid bodies had focused diverging scientific inclinations since Descartes's formulation of the law of refraction. Now, the modernist penchant for the dynamics of innerness functioned as a catalytic tool in favour of processes taking place in interior layers of an illuminated body.

This is not the place to keep track with the particulars of this development, but the following details should be pointed out: Einstein's interpretations of the photoelectrical effect centre on the 'granular energetic structure' of light. This structure is no longer conceived in a Newtonian fashion as gravitational, but energetic, thus transgressing the immanent disciplinary borderlines of physics. Einstein's innovative idea conceives of light as composed of packages of energy, so-called light quanta, being able to eject electrons out of a given material.

It is astonishing to note the empirical adequacy of this model and its correlation with a changing concept of subjectivity. While the interactive equality of 'light subjects' was (following a sublimated Cartesian fashion) a self-evident pre-condition for Fresnel, Einstein started from the assumption that these elements could be able to act very differently, depending on basic data and qualities (for instance the frequency of light). So the intracultural scenario had changed completely. While Newton had believed in the power of massive bodies over light, Einstein shifted his attention to light quanta dissecting a given mechanical material. The upgrading of subjectivity in the sense of idiosyncratic perseverance and activity could no longer be denied. In an almost 'Marxian sense', Einstein's light quanta were able to change the material world – which is more than the aesthetic satisfaction offered by a Fresnelian interference pattern.

Subjects – and this is an implicit consequence of Einstein's ideas – are no longer ultimately free in a Cartesian sense. They act within a partial,

modernist realm of freedom, partly bound by coercive restraints, partly capable of inaugurating instantaneous singular achievements (like light quanta, ejecting electrons out of metals). Fresnel's continuum of equally pre-structured subjects, interfering freely, gets replaced by a discontinuous field of interactions, where subjects remain in interzones, incalculably dislocated. Freedom as a determining factor, integrated into the rationality of any singular subject in moral self-responsibility, is partially dispersed. The local precision of subjective self-definition undergoes an ideological process of divergence in quantum physics.

Conclusion

Looking back from this quantum-theoretical position to Fresnel's specific achievement in modern physics and implicit political thinking, it may become clear that his theory of interference is the result of a grandiose convergence of individual peripheral conditions (within a given social and historical situation) and the untypical ways of approaching an unsolved problem with the eccentric verve of a singularly creative mind. Which does not imply cultural and ideological incompatibility, but instead supports the assumption, as I have argued, that human worldmaking cannot be dissected into 'sciences' and 'humanities' without ignoring a fundamental union between both spheres within the groundwork of anthropological self- and world-orientation. Here we find a second, unbroken leitmotiv: in optical theories and the logic of their intertwined confrontations and phases of peacefulness, the history of modernity as a history of (inter)subjectivity is being retold in a clandestine version.

And Fresnel's example shows even more, especially for those who hope that marginalized thinking might be able to counteract the dominant streams of cultural development: marginalized sciences do not persist in a permanent form. In the course of time, centre and periphery tend to exchange their positions, supporting the idea that centralism and marginalism as ideological prototypes of late-modern hermeneutical worldmaking are oscillating on a dynamical axis. Their social and historical success seem to be dependent on cultural factors. So it might reasonably be claimed that scientific progress is not primarily nurtured by 'truth', explanatory success or style, but by similarity to actual forms of social interaction. In other words: sciences exist in a metaphorical relationship to society.

This essay tried to outline the implicit socio-philosophical structures patterning the process of change from Newton's authoritarian concept of interaction to Fresnel's libertarian idea of autonomous superposition of elementary light waves. Starting with the energy of an overworked man looking

for intellectual compensation, Fresnel's undertaking shifted from an individual marginal exercise to a project correlated with parallel developments in society and subjective self-understanding. In the end, he stood in the centre of modern optics, an amateur who had started his studies 'in complete isolation from the scientific world'.³⁰

REPOPULATING HEAVEN: VICTORIAN LUNAR STUDIES AND THE ANXIETY OF LONELINESS

David Clifford

Setting aside any question of 'faith and doubt', superficially associated with the history of the mid-nineteenth century, there was as much a tendency as ever during that period for people to look heavenwards with their questions. The author of an 1856 article in *Chambers's Journal* noted that:

Countless books have been written, and countless discussions held upon [the Moon]; professors have had more to say about it than about anything else in the circle of the universe: they never will let the moon alone; they take the attitude of her mountains, the depths of her caverns, the breadth of her plains ... It must be confessed, that we have treated the moon somewhat lightly; made her the common subject of conversation; and expressed our opinions upon her very freely.¹

If anything characterizes nineteenth-century attitudes towards science, it is its increasing popularization by just such a plethora of books and discussions, to say nothing of the enthusiasm of writers and publishers such as Robert Chambers himself. The scale of space attracted a readership negotiating its way between traditional biblical motifs, and models that corresponded with emerging astronomical data, and it is unsurprising to find that in his own popularizing volume, Chambers expressed one in terms of the other.²

The author of the above *Chambers's* article, however, correctly identified a trend among popularizers during the middle decades of the nineteenth century. Science writers and their readers shifted their attention beyond the Earth beneath their feet and into space, our satellite, the neighbouring planets, and what might be found there. This interest in space and, to a limited degree, being in space (if not space travel, which I will discuss below), emerges from a complex array of anxieties relating to a cosmological decentring of humankind. This trope is now a commonplace among histories of Victorian culture, and the complexities of the so-called science–religion debate, unhelpfully oversimplified in much earlier historical study but discussed with greater care and sympathy in the last three decades or so,³ should be regarded as a starting point when considering how Victorian writers and readers of astronomical texts sought to reconcile competing ideological influences. Even if traditional archetypes of heaven were figurative, they unquestionably occupied a conceptual space that was above, rather than on, Earth, and they were populated by beings superior to humanity. The nineteenth-century fascination with the extra-terrestrial combined with a relatively firm belief that, like traditional concepts of heaven, it remained beyond mortal reach, presents a useful means of exploring how Victorian stargazers resisted the depopulation of this space, as if an excess of evidence of its uninhabitability might endanger the angels. This essay will address a selection of the instances in which the popularity of writing on astronomical matters, and especially on the Moon, intersected with the values and priorities associated with historically traditional conceptions of space, and humankind's place in it. It will consider some of the ontological consequences most pronounced at this intersection, in particular the need to reconcile evidence of empty and uninhabitable space with the traditional interpretation of heaven as a populous, watchful and ultimately protective place.

I wish first to distinguish this interest in space and what might be found there from other fields of nineteenth-century scientific enquiry, by observing that its greater capacity for speculation and conjecture offered a relief from the oppressively empiricist absolutism of the laboratory, or even of the solidity of what a geologist found under his hammer, or what the biologist scooped out of a pond and observed in the sunlight. Speculation and conjecture drove and drives all science, to be sure, but space had at this time yet to shift from a concept perceived only as impossibly distant and immeasurable, to one that offered up its secrets as regularly and reliably as, say, anatomy or chemistry. If Isaac Newton's mechanical universe was considered the apotheosis of scientific reasoning, the idea of actually travelling to any part of that universe was discussed primarily to be dismissed, or even ridiculed. The generally perceived impossibility of venturing beyond the Earth was the very reason

discussions on the matter were so attractive, perpetuating that perception in the process. There were elements of heaven and its population, even under scientific scrutiny, which straddled traditional and modern creation tropes. These, I shall argue, allowed for a gentler transition from an anthropocentric and teleological ontology to one in which humankind contemplated its place in a more lonely, random and apparently undirected universe. Similar anxieties accompanied other scientific ideas in circulation – uniformitarian deep time, for example, or evolutionary theory, or solar entropy⁴ – but the observational (or in the case of solar entropy, mathematical) basis of these was grounded less conjecturally. I will argue that the Moon was a reassuring space onto which Victorian writers and readers of popular astronomy sought to superimpose Earth-like qualities, while still embracing all the evidence pointing to its lifelessness, through its status as a safely distant and mysterious place.

And to conclude my codicils to the following argument, I should make clear that I do not suggest that space and its astronomical bodies were in any sense a ‘new’ pre-occupation among the period’s scientifically curious. Marjorie Nicolson described in 1948 the variety of texts since classical times which documented humankind’s ambitions to reach the Moon.⁵ Bishop Francis Godwin wrote his popular and much-translated *The Man in the Moone* around 1585 (it was not published until 1638, after his death), in which a marooned sailor is carried to the Moon by migrating birds. Cyrano de Bergerac’s *Histoire Comique ou Voyage dans la Lune* (1650) was likewise very successful, and in 1687, near the end of her career, Aphra Behn produced her comedy *The Emperor in the Moon*, a drama upon which the influence of Lucian’s *Icaromenippus* (ad 150), purportedly the earliest story of lunar exploration, is evident. Following Behn’s satirical tradition, Daniel Defoe wrote and published *The Consolidator* in 1705, in which the narrator is taken to the Moon by a large, feathered flying machine and meets there The Man in the Moon, with whom he discusses the business of human society, politics and trade.⁶

But the Moon provided these earlier writers with the distance required by satirists, and their targets were more pointed for the consequent defamiliarization. Defamiliarization, indeed, was among nineteenth-century writers’ uses of the Moon, but much less, if ever, for specifically satirical purposes. Thoughtful Victorians would have scorned sympathy for a pre-Copernican model of the solar system. Yet a study of scientific texts from the nineteenth century, both academic and, perhaps more importantly, popular, indicate a residual attachment to a geocentric model of the universe with its concentric spheres for the fixed stars, the planets, the sun and the Moon, if only to serve as a paradigm for earthly (and so human) pre-eminence. Traces of

this continued attachment to ancient cosmologies can be detected in the continued use, throughout this period, of the preposition 'in' – as in the spatial location, 'in the Moon'. This was the idiomatic form adopted by those writers of the seventeenth and eighteenth centuries cited above, and was the common form until the twentieth century (the idiom continued so far, at least, as the title of H G Wells's 1900 novel *The First Men in the Moon* – indicating the durability of this relationship between concept and language even among more radically-minded Victorians). The traditional explanation for locating oneself or anything else 'in' the Moon is that it is perceived as a two-dimensional shape on its geocentred sphere; something (The Man, usually) is 'in' it if it is enclosed within the disc. The fact-based (if not necessarily accurate, in terms of our own knowledge) studies of the Moon that I will look at in this essay gradually conferred a third dimension upon the traditional two-dimensional idea, located it in three-dimensional space, and allowed the sphere on which it sat – eventually – to fade into mythology.

I

As consumption of scientific knowledge during this period increased among a lay readership, and as the priorities of scientific enquiry began tentatively to be detached from those of natural theology, the modernity of science acquired its own status. Separating 'modern' from ancient thinking followed the Enlightenment tradition but was self-evidently precarious: Lord Monboddo's proto-evolutionary ideas, that orang-utans were primitive versions of humankind and could be taught to speak, were regularly held up to ridicule during the first half of the nineteenth century. Thomas Burnet's geological theories, in which he constructed a model of the Earth's physical history that accorded with Biblical accounts of creation, were likewise regarded with scorn (though Stephen Jay Gould has argued that, in spite of dogma, Burnet's account was actually well grounded in terms of contemporary scientific thinking).⁷ On the matter of space – the possibility that the Moon was inhabited and that it might one day be possible to travel there – Whiggish Victorians could look to the seventeenth-century bishop John Wilkins to affirm their own superior knowledge. Wilkins proposed, quoting Kepler, that future generations would discover the art of flying and would use it to reach the Moon soon after, speculating in his essay on the inhabitants they would find there. Indeed, in 1823, *The Retrospective Review* returned to Wilkins's book, reassuring its readers that they had 'occasion to laugh a little...at some of the speculations in which the worthy prelate indulged himself.' Writing on the Moon half a century later, one reviewer commented that 'to think of a trip to the moon is to think of Bishop Wilkins [with] a sigh

for the good old prelate; and, if you like, a smile at his wonderful incredulity. That worthy man's projects for reaching the "lunary world" are ... frankly foolish."⁸ In an essay on Wilkins in *All the Year Round* written between these two articles, another author is at least a little less unkind:

At one time we are told that the absence of an atmosphere and water would render life on [the Moon] impossible, at other another time astronomers suggest the possibility of vapour and atmosphere different, perhaps, from that to which we are accustomed, but by no means incapable of supporting a mooncalf. As to the passage thither, indeed, no practicable means have ever been suggested ... This, therefore, remains a prophesy [of Wilkins's] unaccomplished, and is likely to remain so.⁹

Pertinent to this discussion however is the observation that, although the question of an atmosphere, water or life on the Moon was open, the prospect that anyone might one day find a way of overcoming the Earth's gravity and go there was not. Objectivity is foreclosed by relative certainty.

Accounts of travelling into space in this period routinely disregarded any discussion of how such travel might be possible.¹⁰ In this respect, their fidelity to Baconian empiricism is admirable, and accounts, perhaps, for the dismissal of Wilkins's proposition, based as it was on such unscientific fancy. The writers of the articles in *Chambers's* and the *British Quarterly*, for example, both lift their readers onto the Moon by effort of imagination, and once there recount only what is within the accepted limits of scientific theory. Maximillian Schele de Vere, a professor at the University of Virginia, wrote a similar essay in 1855 for *Putnam's Magazine*, and employed the same means of transport for his 'trip to the moon' before listing known facts and theories about landscape, geology, temperature and the condition of the sky viewed from its surface.¹¹ For all their basis in observable fact, however, most such essays required imaginative speculation if only in terms of what was visible from the Moon's surface; yet none would follow Wilkins in imagining the animus behind the actual journey, which was as much a recurrent basis for scorn among Victorians as for his practical (and, so far as is yet discovered, unworkable) means of doing so. By combining modern empiricism, scorn for ancient speculation, and perhaps even anxiety over discussing autonomously-directed journeys to heaven, consumers of such astronomical essays imagined versions of 'heaven' which dovetailed with pre-existing forms: revealed by learned men with arcane knowledge whose word one took in good faith, but a heaven that remained impossibly distant and, ultimately, a matter of speculation.

II

These imagined experiences of what it was to be on the Moon are set against a backdrop upon which the plurality of worlds – the question of whether or not the Earth was alone in the universe in hosting intelligent life – was debated most fiercely. This is, as is well known, a debate which stretches back to ancient Greece, but was revived in Britain in the nineteenth century first by the charismatic Presbyterian divine Thomas Chalmers, whose *Astronomical Discourses* first electrified their audiences as lectures in 1817, and then sold in their thousands on publication. Chalmers strongly advocated the plurality of worlds, on the grounds that God would not scatter such an extraordinary quantity of planets about the universe and leave them needlessly unpopulated when they might accommodate Christian souls. Multiple worlds also served, for Chalmers, to humble Earth's humankind in any ambitions that they alone were God's chosen people. This debate continued for years, beyond Chambers's imposition of an evolutionary model onto the nebular hypothesis in *Vestiges* in 1844, until 1853, when William Whewell, Professor of moral philosophy and Master of Trinity College, Cambridge, published *Of the Plurality of Worlds: An Essay*. Whewell vigorously repudiated the idea of multiple worlds, arguing that God could hardly reveal himself through Christ on every planet on which so much intelligent life teemed – it was too much to expect of the one true Saviour to tour the universe, being endlessly crucified in order to save populations with whom other Christians could not possibly communicate. In response, the noted science writer (and noted Presbyterian) David Brewster first reviewed the essay critically in *The North British Review*, and followed this with a stiff rebuttal of Whewell's argument in *More Worlds than One: The Creed of the Philosopher and the Hope of the Christian* (1854).¹²

Whewell and Brewster pursued this debate, often quite acrimoniously, for several years. Many others joined in, offering their own judgment on the theological basis for or against plurality. The topic followed the fate of *Vestiges* by entering drawing room and other domestic conversations, even satirized by Trollope in 1856 in *Barchester Towers*.¹³ But the debate over the plurality of worlds, and the dispute between Whewell and Brewster, has been the subject of other studies and is not my focus here.¹⁴ The plurality of worlds debate was essentially metaphysical rather than scientific even if it drew heavily on scientific data. The debate on whether the universe was infinitely dotted with bustling populations, themselves perhaps debating whether the cosmos was teeming with life, or was rather filled to its edges with empty, sterile rocks encircling wasteful suns, centred on worlds far beyond the Earth and its satellite. To that extent it shared with discussions on the Moon a need

for the dispute never to be settled: the assumption of impossibility concerning any future journey to the Moon applied infinitely to the planets beyond our nearest neighbour.

Yet the Moon itself was giving up its secrets to advances in technology. As mirrors became more accurate, larger, and relatively less expensive – popular science periodicals such as astronomer Richard A Proctor's *Knowledge* (from 1881) carried advertisements for hobby telescopes, with mirrors of four inches and upwards, in almost every weekly issue – so telescopes grew, and proliferated, with the consequent increase in articles on astronomy. Some writers of these regarded the quest for greater magnification itself as faintly ludicrous. In Putnam's 1855 article, again, Schele de Vere credits astronomers such as 'the munificent Earl of Rosse [William Parsons, 3rd Earl], who compels the chaste Goddess to come down within the familiar distance of three hundred miles, even to bold Ireland!' with providing the information on which his readers fed so hungrily. (Rosse's massive Parsonstown 'Leviathan' telescope, finished in 1847 with its 4-ton, 72-inch mirror, was the largest telescope in the world until the early twentieth century.) 'Anoki ben Ierah' ('Child of the Moon') scoffed respectfully at the giant telescope of Parsonstown in *Fraser's Magazine*; and by the 1870s, relative distance by magnification was reckoned to bring the Moon within a range of forty miles.¹⁵ The result was that – although 'forty miles will not enable us to distinguish men or women in the moon; nor even to detect mammoths or mastodons [...] make out castles or coliseums however numerous such erections might happen to be' – imagined visits to the surface of the Moon could claim to be grounded in observable 'fact'.

Such facts, straightforward enough in terms of Newtonian mechanics, often exploited readers' delight in imagining the spectacular and unfamiliar, combined with a certainty that such visions would remain confined to the imagination:

That side of the moon which is turned from us, has a night of nearly fifteen days; the stars only, and planets, shine on its ever dark sky ... with its dark, unbroken, night, a true and literal 'fortnight', it is the observatory of the moon, the best of the whole planetary system.¹⁶

Straight before us, midway between the horizon and that part of the heavens directly above our heads, we perceive a vast and mysterious object – a monster-planet faintly illumined, like the moon by daylight, and enveloped in a floating veil of many shades. This planet is nearly fourteen times larger than our satellite; it neither rises nor sets, but remains ever in the same station, while it is the firmament which seems to revolve around it ... This extraordinary and gigantic object is called the Earth.¹⁷

Circles, we may hardly call them craters, not merely as large as English counties or German principalities, but reaching 100 miles or even half as much more, in breadth, are far from rare; their cavities so depressed, and their boundaries so lofty, that, planted within them, Alps, or even in places Andes, would disappear.¹⁸

Such popular scientific expositions, with their combination of bedazzlement, 'truth' and absolute distance from personal experience, were a perfect replacement for the retreating cosmological narratives of heaven which had always shared those properties.

Various other studies have explored the degree to which some scientists attempted to wrest ownership of those narratives from their theological origins, but this is a precarious and uncertain area of study while so many 'scientists' held strong religious views themselves.¹⁹ Writers of all leanings presented evidence or recounted information that to some extent would reinforce their own ideological standpoint. But it remained the case that the foundation for this, in an age of runaway empiricism, was the establishment of accurate scientific data, with telescopes of whatever size were available.

In this respect, the Moon remained on the front line of the plurality of worlds debate. The tradition held that a body of its size hanging in space so close to Earth would, intuitively, be similar to Earth in all other respects, and the assumption of people living there was not regarded as absurd. In much the way that is still the case with contemporary expeditions to Mars, Victorian selenography was conducted with the faint optimism that we would find a neighbour, or past evidence of one, in this apparently isolated region of the solar system. Although by the mid-century, evidence pointed to the Moon's present lifelessness, as most of those writing accepted, many observers persisted in projecting a version of Earth, or its life-sustaining qualities, onto our satellite, and on the basis of these formed their conclusions on what they would expect a 'terrestrial' moon to be like. The anxiety of loneliness, in this sense, is aligned with the anxiety of death, and perhaps importantly, death in which the traditional purpose of heaven was giving way to empty space, much larger and much emptier than had previously been believed, populated by dead rocks. A paradox was thus created for astronomers seeking to include theology in their observations. A universe in which only the Earth was populated by sentient life was isolated and cosmically insignificant, raising questions about God's purpose of making the pinnacle of his creation, humankind, such a vanishingly small fraction of it; but a model of creation in which more than one planet was populated could no longer claim to have humankind at its centre.

Whewell held the torch for those upholding the anthropocentric model of the universe even where its scale diminished humanity physically if not spiritually, and aggressively pressed the latest astronomical data to his service. The moon could not be inhabitable if revelation was to make logical sense, he claimed, as he argued against the basic components for hospitability being absent from the Moon's surface. Modern telescopes, said Whewell, would enable astronomers to detect structural lunar surface changes if there had been any: not merely large buildings but the equivalents of terrestrial events such as forest clearances, or anything similar to the Great Fire of 1666. The 'seas', he observed, were not 'level and smooth, as water would be; nor uniform in their colour, but marked with permanent streaks and shades, implying a rigid form'. He asserts the certainty of water or any atmosphere being absent by the absence of any light distortion at the Moon's edges as stars pass behind it, a conclusion based on repeated observation throughout the century.²⁰

The presumption that the other planets and heavenly bodies would share similarities with Earth – that they had, for example, water, air, continents, and perhaps a variety of life – was hardly absurd when Earth's inhabitants had had only the one model to study closely. An author named George Leigh wrote a short essay for *Once a Week* entitled 'Are the Planets Inhabited?', in which he judged each of the known planets (though not the Moon) up to the recently discovered Neptune in terms of their fitness for human habitation. Leigh concluded that:

The existence of atmospheres round the other planets of our system being so highly probable, we have good grounds for believing that they are suited for the habitation of beings like ourselves ... The proofs that the globe we inhabit was expressly designed as a dwelling for us abound so thickly, that for any person to maintain that it was formed by a fortuitous concourse of atoms, is preposterous ... Can we doubt, then, that the Creator who formed this globe created the others for a like purpose? That their inhabitants resemble us physically is only a reasonable supposition.²¹

This anxiety for the nebular hypothesis and its implied evolutionary corollary echoes Whewell, a decade earlier, though to the opposite conclusion, indicating the conundrum presented by the need to interpret astronomical discovery in theological terms. Divine fiat, rather than the hateful development hypothesis admired by such as Herbert Spencer, G.H. Lewes and George Eliot, created the Earth and all life as far as Whewell was concerned. But for George Leigh (like Brewster) this could only logically result in the perfectability of creation.

III

The need to impose habitability, or even hints of it, onto the rest of the cosmos at times took priority over the due care normally given to observation. Although the great majority of articles on the Moon and space inclined for that body's present lifelessness, the search for possible Earth-like conditions was abandoned only slowly and reluctantly. Depopulating heaven was one thing in a physical sense, but to render it uninhabitable as well was to call into question the very point of most of what God had created. Any trace of atmosphere or water might indicate life-sustaining qualities for some manner of being, and the attention given to this need at times even distorted the empirical astronomy that was valued so highly. On 2 January 1857, in the public heat of Whewell and Brewster's argument, an occultation of Jupiter by the Moon could be clearly witnessed across much of England. The planet disappeared behind the dark limb of the six-day old Moon and emerged from the bright limb fifty-five minutes later. Reports by several of the observers were published in the *Monthly Notices of the Royal Astronomical Society* for January that year.²² These were dense and mathematical, but the occultation was discussed shortly afterwards, stripped of excess complexity, in the periodical press. One of these appeared in *Chambers's Journal* for March 1857, concentrating on one detail in particular. Upon emerging:

The bright border of the moon ... crossed the soft green face of the planet, not with a clear sharply cut outline like that which had been presented as the disc passed into concealment; it was fringed by a streak or band of graduated shadow [... William] Lassell described it as offering to his practised eye precisely the same appearance that the obscure ring of Saturn presents.

The same phenomenon was seen at observatories across the country, so an early assumption by one astronomer that it was a misalignment of his telescope could be dismissed. 'Can science read the meaning of this shadow-fringe inscription?' asked *Chambers's* author provocatively:

The sagacious Plumian professor of astronomy at Cambridge, Professor [James] Challis, seems to have been the first to hit upon the true interpretation of the riddle. [He] has long suspected that the ['seas'] are really shallow basins filled by a sediment of vapour which has settled down into those depressions; in other words, he conceives that there are fog-seas, although there are no water-seas, in the moon [... He] was able to satisfy himself that the planet actually did come out from

behind a widely gaping hollow of the moon's surface – at the bottom of a lunar fog-sea, seen edgeways, so to speak.²³

Although an accomplished astronomer, Challis is unfortunately often remembered as the man who did not *quite* discover Neptune in 1846. His determination to detect vapour, some trace that might reignite the hope of supporting life, on the Moon's surface may also have prevented him from first-guessing the existence of Jupiter's ring system, otherwise undetected until 1979.

Nineteenth-century astronomical studies had brought the Moon, and so the most familiar part of heaven, closer. While never actually representative of heaven it had always possessed its own mystery, a combination of its traditional mythology and the widely understood nature of its impossibly unbridgeable distance. Aligned with ideas about the possible population of bodies beyond Earth was a continued anxiety regarding the continued 'development' of life on Earth, the entropy of the sun, the death of the universe. The idea that the history of each planet would roughly mirror that of the Earth, with different starting and end-points, inspired H.G Wells's *The War of the Worlds*, with its armies of evolutionarily advanced but dying Martians looking to a younger planet to rejuvenate their species. Richard A. Proctor, one of the century's foremost popularizers of science, proposed that the Moon had once been identical to Earth, but owing to its size had cooled at a far greater rate – and represented the state of the Earth as it might be in 300 million years.²⁴ Other planets and celestial bodies took their place on the scale of development, but always under the assumption that the geological development and decline of the planets followed a similar general trajectory. This was exactly the embodiment of the development hypothesis that Whewell feared so much. If anything, Whewell's physically unpopulated heaven preserved the metaphysical existence of the angels more convincingly than did their substitution by extra-terrestrials, but the weight of popular opinion was not with him. As in many fields of science during Victorian times, its popular dissemination enabled a secular interpretation of observed data considerable influence. What is fascinating about these popular discussions on the habitability of the Moon and planets is the degree to which interpretation drew on traditional cosmologies, and what that dependence says about the conflict with which they embraced the emerging paradigms of the universe.

‘YOU SHOULD GET YOUR HEAD
EXAMINED’: FREUDIAN
PSYCHOANALYSIS AND THE LIMITS
OF NINETEENTH-CENTURY SCIENCE

Peter Naccarato

As science turned its focus towards ‘man’, new disciplines emerged with the aim of utilizing scientific methods to understand and regulate human behaviour. Rather than understanding this scientific inquiry into human behaviour as a unique historical phenomenon with an identifiable moment of origin, we must recognize its continuity with a long tradition through which it found momentum yet against which it would struggle for authority. The search for knowledge about human existence and behaviour clearly did not begin with the emergence of science as a discrete discipline in the eighteenth century; instead, through a complex network of intellectual and social reconfigurations, it was incorporated into an emerging disciplinary structure and found legitimization through its relation to science. During this time of change in how knowledge was produced, valued and circulated, a range of new disciplines contended for authority over self-knowledge. To understand how psychoanalysis would eventually come to function within this network of disciplines and ultimately to undermine the concept of science itself, I want to consider briefly how several of them developed and how they come to contribute to Freud’s psychoanalytic project.

As philosophy became focused on metaphysical questions explored primarily through philosophical speculation and reason, physiologists continued to use scientific methods to observe and measure bodily responses as a means of establishing the basic foundations for sensory physiology. Despite their attempts to understand all perceptual phenomena in terms of mechanistic bodily operations, however, physiologists were increasingly confronted with a factor that they seemed unable to explain. The basic problem was accounting for discrepancies in observations and records made by different scientists. Throughout the early decades of the nineteenth century, scientists would continue to work to answer these crucial questions, which challenged the accuracy of their own methodology. At the same time, others began to question this method and to employ alternative strategies. By 1860, Wilhelm Wundt was devising experiments that would allow him to explore this problem. Rather than understanding the personal equation as 'an observational difficulty which had to be overcome', Wundt 'saw the troublesome observational errors as providing a handle on how the mind works'.¹ Not merely a problem to be solved, Wundt believed that he had found a window on the mind itself.

Another mode for the production of self-knowledge that would play an important role in the establishment of psychoanalysis was psychiatry, which was considered throughout much of the nineteenth century as 'a stepchild of medicine'.² Only with advances in neurology that allowed psychiatry to assert that 'all insanity and other mental phenomena must have organic correlates',³ did it gain recognition as a medical specialty. Led by Wilhelm Griesinger and his followers, the somatic school of psychiatry 'banished all discussion of psychology and philosophy, concentrating exclusively on organic brain pathology',⁴ Griesinger's book, *Mental Pathology and Therapeutics*, published in 1845, moved psychiatry towards the somatic approach which would dominate its future development.

As neurology focused on 'organic damage to, or malfunctioning of, the nervous system because of disease, injury, or inherent flaw',⁵ a simultaneous movement in psychiatry concentrated on the identification and classification of mental disorders. This work was advanced, most dramatically with the first publication of Emil Kraepelin's *Compendium* in 1883 and the subsequent elaborations of future editions. In these texts, Kraepelin divided insanities into two major groups: the dementia praecox patients for whom there was little hope for improvement and the manic-depressive patients for whom recovery was expected. With the Kraepelin classification system fully developed and neurology 'reach[ing] its first peak as a branch of medical science',⁶ a new dynamic psychiatry was in place by the end of the nineteenth century.

As the experimental methods of psychiatry and psychology were employed in the development of a set of systematic procedures for the treatment of

mental disorders, psychotherapy emerged as a field within which 'trained professional[s] employ[ed] systematic psychological procedures to help a client change troublesome thoughts, feelings and behaviour patterns'.⁷ This transition from experimental research to therapeutic method and treatment marked the establishment of a critical bridge across which the 'sciences of man' could connect the discourse of science – with its rigorous standards of experimentation and methodology – with the discourse of medicine, focused on treatment, intervention and cure. At the end of the nineteenth century, Sigmund Freud would find himself ready to cross that bridge.

Rather than accepting psychoanalysis as a radical break in an otherwise continuous history of ideas, I am concerned here with how Freud's project was made possible by a historically specific organization of knowledge through which it was initially authorized and yet to which it would ultimately pose a significant challenge. It is by exploring the specific historical and disciplinary conditions through which his psychoanalytic project was constructed and has circulated that we can understand the influence of psychoanalysis on the disciplines from which it emerged, on those to which it would offer a share in the scientific production of self-knowledge, and on the disciplinary structure itself.

In disciplinary terms, psychoanalysis initially emerged as Freud worked in the space between several established disciplines, including neurology, psychiatry and psychology. Although his primary training was in neurology, Freud would become increasingly interested in the contribution of psychology towards an understanding of mental illness and the functioning of the mind. At the same time, most of his theoretical speculations on the functioning of the mind would grow out of his own clinical observations. In looking for both treatments for his patients and explanations for their conditions, Freud found himself confronting a disciplinary crisis. In terms of medical explanations for mental illness, neurology offered little more than mechanistic speculations about lesions, toxins and other unidentifiable physiological determinants. In terms of treatment, psychiatry's primary prescriptions were diet and rest. In working towards theories of the causes of mental illnesses, Freud was obviously influenced by his strong neurological training yet 'his thoughts were being more and more diverted to psychology.' At this point of disciplinary ambiguity, Freud's 'first endeavour was naturally to reconcile his ... interests'.⁸

One of his major attempts at such reconciliation was his 'Project for a Scientific Psychology', in which he 'attempted a grand physiological theory according to which psychology would be put on a firm neurological basis'.⁹ Although he sent a draft to Wilhelm Fliess in January 1896, 'The Project' was never published in Freud's lifetime. The document provides important insight into Freud's struggle to construct a bridge between neurology and psychology.

His purpose was stated clearly in the opening sentences: 'The intention is to furnish a psychology that shall be a natural science: that is, to represent psychical processes as quantitatively determinate states of specific material particles, thus making those processes perspicuous and free from contradiction.' (1. 295) Following in the mechanistic tradition, Freud understood all mental operations in terms of neuron stimulation, discharge and equilibrium. In the third section of his work, Freud hoped to use the hypotheses, he 'derived directly from pathological clinical observation' (1. 295) as a basis for 'the giving of a mechanical representation [of psychical processes]'. (1. 371) In doing so, he could use his own clinical observations to construct a theory of normal mental processes while also firmly establishing a neurological foundation for psychology.

During the same period in which he worked on 'The Project', Freud's association with Josef Breuer would influence his thinking concerning the relation between neurology and psychology while also advancing his commitment to developing new methods of treatment. In June 1892, Freud and Breuer began drafting their 'Preliminary Communication', which would comprise the first section of *Studies on Hysteria*. At the same time, they prepared the case material that would be included in that volume. Published in 1895, *Studies on Hysteria* provides additional insight into the disciplinary crisis Freud and Breuer confronted as they worked to synthesize their commitment to finding new therapeutic methods of treatment with their desire to use these clinical experiences as the grounds for theoretical speculation concerning the causes and treatment of neuroses and other mental diseases. These theoretical speculations reflect their continuing struggle to combine their neurological training and background with their increasing belief in the psychic roots of hysteria and other neurological disorders.

The text itself reveals the disciplinary boundaries that Freud and Breuer were confronting. Its four main sections are comprised of a coauthored theoretical statement titled 'On the Psychical Mechanism of Hysterical Phenomena', the five case studies upon which the theoretical speculations are based, Breuer's own speculations on hysteria called only 'Theoretical', and Freud's theoretical contribution, 'The Psychotherapy of Hysteria'. One primary division between case studies (psychiatry) and theoretical speculation (psychology and neurology) is explicit. In their preface to the first edition, Breuer and Freud provide a chronology for the text, suggesting that from 'a new method of examining and treating hysterical phenomena' came a number of 'theoretical conclusions'. (2. xxix) This also became the framework within which James Strachey later considered the impact of this text on the history of psychoanalysis. Strachey's introduction echoed a strong disciplinary division, suggesting that 'an inquiry into the bearing of the *Studies* upon a

subsequent development of psychoanalysis may be conveniently divided into two parts.' He focused first on the extent to which 'the technical procedures described in the *Studies* and the clinical findings to which they led pave the way for the practice of psychoanalysis' and secondly, on how 'the theoretical views propounded here [were] accepted into Freud's later doctrines'. Although he does recognize such a division as 'necessarily an artificial one', it nonetheless served as the basis for discussing the text and its contribution to psychoanalysis. (2. xvi)

In addition to the explicit separation of case studies from theoretical speculations and hypotheses, a second disciplinary division is identifiable within the pages of the *Studies*. Some have argued that the ongoing dispute between neurology and psychology, which unfolds in the text, was one important reason for the failure of Breuer and Freud's professional and personal relationship.¹⁰ The preface to the first edition of the *Studies* addresses these theoretical differences:

If at some points divergent and indeed contradictory opinions are expressed, this is not to be regarded as evidence of any fluctuation in our views. It arises from the natural and justifiable differences between the opinions of two observers who are agreed upon the facts and their basic reading of them, but who are not invariably at one in their interpretations and conjectures. (2. xxix–xxx)

However, as James Strachey points out in his introduction, despite this clear acknowledgement of the theoretical differences between Breuer and Freud in the preface, they are 'far from prominent' (vol. 2, p. xxi) in the text itself. Before discussing how the theoretical differences between Breuer and Freud indicate an ongoing negotiation of the disciplinary boundary between neurology and psychology, I want to consider further the implications of Strachey's observation.

It seems significant that while these theoretical differences can clearly be addressed in the preface to the text and in later reflections upon it, they cannot be articulated directly in the text itself. This underscores an important process through which disciplinary rules are enforced and boundaries are maintained. Because the establishment of a specific set of discursive practices is a primary means by which disciplines construct and maintain themselves, the role of the text is crucial. A text that claims a specific disciplinary affiliation, by employing the 'tools, methods, procedures, exempla, concepts and theories' unique to that discipline, necessarily garners authority from this association while simultaneously upholding the identity of the discipline.¹¹ In the case of Breuer and Freud, their collaborative text must itself fall within a disciplinary category or it must account for whatever disciplinary borders it

intends to cross. As the chapter divisions indicate, their text does bring together two different kinds of work, namely clinical observation and treatment with theoretical speculation. Although it contributes to more than one discipline, by acknowledging its dual focus, the text nevertheless functions to strengthen and maintain each of them. The more ambiguous confrontation between psychology and neurology, however, is more threatening.

Both Breuer and Freud had been strongly influenced by the School of Helmholtz and one of its prominent members, Ernst Brücke; consequently, 'much of the underlying theory in the *Studies on Hysteria* is derived from the doctrine of that school that all natural phenomena are ultimately explicable in terms of physical and chemical forces.' (2. xxii) This physiological model is predominant throughout the *Studies* because the mental apparatus is understood as carrying a charge of energy and Breuer explores the distinction between 'free' and 'bound' psychical energy. (2. 193–5) Echoing the neurological terminology that Freud would struggle with in his 'Project for a Scientific Psychology', the 'Preliminary Communication' outlines a mechanistic process, in which the psychical energy generated by traumatic experiences, when not accompanied by an adequate reaction and, thus denied 'the normal wearing-away processes by means of abreaction', becomes the source of hysterical phenomena. (2. 11) Throughout the text, in fact, the concepts of cathexis, affect, and the displacement of psychical energy – all of which would become familiar psychoanalytic concepts – exhibit their initial physiological connotations. Despite this apparent grounding in neurology and physiology, the influence of psychology is also very strong.

In introducing his theoretical contribution to the book, Breuer explained that 'little mention will be made of the brain and none whatever of molecules. Psychical processes will be dealt with in the language of psychology; and, indeed, it cannot possibly be otherwise.' (2. 185) This makes quite explicit the disciplinary distinctions that are at play in the text and also in the theoretical differences between Breuer and Freud. At a moment when Freud was pre-occupied by his theories of cathected neurons and neuronically inertia, when he was 'engaged in devising a complicated structure of hypotheses, intended to make it possible to describe mental events in purely neurological terms', Breuer was focused on 'treating the subject of hysteria on purely psychological lines'. With Freud 'at a half-way stage in the process of moving from physiological to psychological explanations of psychopathological states', the disciplinary tension not only between Breuer and Freud but also in Freud's own thinking underscores the intricate relationship between psychoanalysis and disciplinarity. (2. xxiv)

However, rather than accepting Strachey's chronology, which places Freud in the middle of a journey from physiology to psychology, I want to

suggest a different perspective on how Freud's psychoanalytic project engaged these disciplinary concerns. While Strachey argues that basic psychoanalytic concepts like cathexis and psychic energy gradually lost their physiological association as Freud became increasingly interested in psychology, I believe that the disciplinary ambiguity that arose around these specific concepts anticipates the ways in which psychoanalysis in general came to challenge the very disciplinary structure from which it emerged. Despite efforts to ground psychoanalysis in one discipline or the other, Freud's neurological background continued to influence his basic theories and models of the psychic apparatus even as he claimed to repudiate this field in favour of psychology. Eventually, Freud would directly challenge such disciplinary distinctions as he absorbed the mechanistic traditions of neurology into his emerging psychoanalytic psychology.

The question of how psychoanalysis should be positioned within the disciplinary structure continued to unfold as Freud, his therapeutic techniques and his developing model of the mind gained recognition within the scientific and medical communities. While many colleagues did not accept Freud's increased emphasis on the role of sexuality as the cause of neurosis or the complicated model of the psychic apparatus that he constructed through his work with dreams, there was increased interest in his therapeutic techniques and his clinical successes. As his clinical experiences, his work on dream interpretation, and his own self-analysis continued, Freud used the insights he gained from these practices to continue developing his psychoanalytic theories of the human mind, both normal and abnormal. The psychic processes that he was detecting through his analytic work became 'central not only to his treatment of patients but to his use of psychoanalysis as the investigative method, by which he made his major psychological discoveries'.¹² With *The Psychopathology of Everyday Life* (1904), *Jokes and Their Relation to the Unconscious* (1905) and *Three Essays on the Theory of Sexuality* (1905), Freud put forward theories that would add a whole new dimension to psychology. While its identification as a part of psychology did connect psychoanalysis with a well-established discipline and provide it with an intellectual space under the general rubric of science, questions concerning its intellectual and disciplinary identity were not resolved.

Despite its connection to psychology, psychoanalysis was initially rejected by the scientific community primarily because the methods of analysis and interpretation upon which its theoretical speculations were based did not meet established standards of scientific practice. More specifically, psychoanalytic research was labelled unscientific because 'the psychoanalyst does not construct a situation in which he or she can control variables and manipulate them one at a time to measure their impact and so establish causal connections'.¹³

While Freud believed strongly that his therapeutic technique was firmly grounded on a set of 'technical rules' as well as 'a number of ... scientific theories' (11. 222), he was forced to confront from early in his career the 'outlawing of psychoanalysis by scientific circles'. (12. 138) Despite the judgement of the scientific community, however, Freud continued to argue for the scientific status of psychoanalysis. When understood in terms of the disciplinary, intellectual and cultural authority that science could confer upon psychoanalysis, Freud's emphasis on this point was warranted.

Freud directly answered the challenge to the scientific status of psychoanalysis in the opening sentences of *Instincts and their Vicissitudes*, in which he worked to define the processes through which sciences are established and scientific work is undertaken:

We have often heard it maintained that sciences should be built upon clear and sharply defined basic concepts. In actual fact no science, not even the most exact, begins with such definitions. The true beginning of scientific activity consists rather in describing phenomena and then in proceeding to group, classify and correlate them. Even at the stage of description it is not possible to avoid applying certain abstract ideas to the material in hand ... such ideas ... will later become the basic concepts of the science. (14. 117)

Not surprisingly, Freud suggests that this was precisely the trajectory according to which psychoanalysis had developed. From his early clinical experiences and observations, Freud worked to define, classify and explain the psychic phenomena that he had uncovered. In using such observations as the basis for establishing a lexicon of psychoanalytic concepts and ideas, Freud believed that he followed the natural progression of any science. As subsequent clinical experiences either verified these theoretical speculations or prompted modifications in them, psychoanalysis matured to the point at which its 'basic scientific concepts', formulated with 'increased precision', became 'serviceable and consistent over a wide field'. (14. 117) This, Freud argued, is the fundamental process through which definitions are acquired and scientific knowledge is advanced.

Ten years later, in his *Autobiographical Study*, Freud again returned to the question of how sciences are constituted and how the development of psychoanalysis conforms to this model. Again explaining that 'the basic ideas or most general concepts in any of the disciplines of science are always left indeterminate at first and are only explained to begin with by reference to the realm of the phenomena from which they were derived', Freud argued that it would be impossible for psychoanalysis – or any other natural science – to begin with

any level of completeness or certainty. Rather, it is the very nature of any science that is based on observation to 'work out its findings piecemeal and to solve its problems step by step'. (20. 58) In comparing the development of psychoanalysis with that of zoology, botany, biology and physics, Freud actively asserted the scientific status of his work.

In working to position psychoanalysis within the bounds of science, Freud was concerned not only with the intellectual and cultural value of his work but also its useful contribution to this dominant discourse. Freud clearly recognized the authority that science assumed within the current intellectual climate, and he believed that it could be broadened through the insights of psychoanalysis into the human mind. In this scenario, psychoanalysis benefited as it gained validity and prestige through its association with science and, at the same time, the ability of science to exert its influence over an even broader range of ideas was made possible as it embraced Freud's psychoanalytic psychology. While this disciplinary manipulation is clearly situated within the historically specific framework of the modern disciplinary structure through which science assumed its authority, for Freud, it marks a natural progression in which the superior methods, techniques and principles of science guaranteed that it would emerge as the dominant source of knowledge. In linking psychoanalysis with science, then, Freud could both attain a certain level of intellectual validity and authority while also rehearsing within his own work concerning the superiority of science.

Freud's thoughts concerning the relationship between science and psychoanalysis are most thoroughly discussed in lecture 35 of his *New Introductory Lectures on PsychoAnalysis* titled 'The Question of a *Weltanschauung*'. In trying to provide a definition for this 'specifically German concept', Freud suggested the following:

In my opinion, then, a *Weltanschauung* is an intellectual construction that solves all the problems of our existence uniformly on the basis of one overriding hypothesis, which, accordingly, leaves no question unanswered and in which everything that interests us finds its place. (22. 158)

The question, then, is whether or not psychoanalysis leads to a particular *Weltanschauung*. The answer, according to Freud, is that while psychoanalysis, 'as a specialist science, a branch of psychology – a depth psychology or psychology of the unconscious' (22. 158), cannot be the basis for its own *Weltanschauung*, it does function as a crucial element in the developing scientific *Weltanschauung*. As such, its major contribution to science is 'in having extended research to the mental field'. Arguing that without psychology,

'science would be very incomplete', Freud established the framework within which he would first argue for the absolute superiority of science as the only viable *Weltanschauung* and then position psychoanalysis, with its insights into the human mind, as essential if science is to achieve completion. (22. 159)

In confirming the position of science, Freud quickly dismissed the 'three powers which may dispute [its] basic position', namely art, philosophy and religion. Discounting art as 'almost always harmless and beneficent ... not seek[ing] to be anything but an illusion ... mak[ing] no attempt at invading the realm of reality', Freud bluntly verified the inferior status imposed upon art within the modern hierarchy of disciplines. In a similar way, philosophy is first credited with 'behav[ing] like a science and work[ing] in part by the same methods', but it is then quickly accused of 'clinging to the illusion of being able to present a picture of the universe which is without gaps and is coherent, though one which is bound to collapse with every fresh advance in our knowledge.' By 'overestimating the epistemological value of our logical operations and by accepting other sources of knowledge such as intuition', philosophy goes astray and departs from its association with science. Of these three, Freud concluded, it is religion that 'alone is to be taken seriously as an enemy'. (22. 160)

The primary reason why religion poses such a threat to the scientific *Weltanschauung* is that in the past, 'when there was scarcely such a thing as science', religion constructed its own *Weltanschauung* that, 'although profoundly shaken, persists to this day'. In considering why religion has played such a crucial role in human history, Freud explained that it has served three important functions; first, 'it satisfies the human thirst for knowledge' and, in doing so, becomes a direct rival of science. Second, 'it soothes the fear that men feel of the dangers and vicissitudes of life, when it assures them a happy ending and offers them comfort in unhappiness.' (22. 161) In this function, religion is superior to science. Third, 'it issues precepts and lays down prohibitions and restrictions' and, in this regard, is 'furthest away from science'. In trying to understand the 'remarkable combination in religion of instruction, consolation and requirements', (22. 162) Freud concluded that 'the religious *Weltanschauung* is determined by the situation of our childhood.' (22. 164) In other words, 'religion originated from the helplessness of children' and its contents reflect the 'survival into maturity of the wishes and needs of childhood'. (22. 167) Not surprisingly, it is through psychoanalytic insights into the role of childhood wishes, fantasies and experiences in the development of the individual that this critique of religion is made possible. In other words, the progression from religion to science is advanced by psychoanalysis.

With religion marked as emerging during 'the ignorant times of the childhood of humanity', the movement towards a scientific *Weltanschauung* necessarily marks the 'passage from childhood to maturity'. (22. 168) In intellectual terms, this transition is marked by the movement away from faith, with 'the prohibition against thought issued by religion to assist in its self-preservation', towards reason, which will 'prove to be the strongest uniting bond among men and lead the way to other unions.' (22. 171) While Freud recognized that the battle between religion and science continued to be waged, he looked optimistically to the future: 'Our best hope for the future is that intellect – the scientific spirit, reason – may in process of time establish a dictatorship in the mental life of man.' Recognizing that science is still very young, admitting the fact that 'the path of science is indeed slow, hesitating, laborious', and acknowledging the doubt which may arise as conjectures and hypotheses are challenged or modified in light of ongoing observation and experimentation, Freud nevertheless concluded that 'things are not looking so bad in the business of science.' (32. 173–4) And insofar as psychoanalysis is defined as 'a part of science ... [that] can adhere to the scientific *Weltanschauung*', (22. 181) its future, according to Freud, is equally ensured.

While Freud never wavered in his opinion that psychoanalysis was a science, the scientific community was not so easily convinced. The fact that the scientificity of psychoanalysis continues to be debated and that its assimilation under the rubric of science has never occurred, provides important insight into the disciplinary and intellectual ramifications of this debate. Just as the scientific community insisted that if psychoanalysis was to assume scientific authority, it must conform to its standards and adopt its rules, proponents of psychoanalysis challenged science to reconsider its own parameters in order to include the therapeutic and theoretical techniques of psychoanalysis within its scope. Rather than accepting and reproducing this oppositional model, I am interested in understanding how these debates expose the processes through which the disciplinary structure is constructed and maintained in the early twentieth century, and how the 'confrontation' between science and psychoanalysis challenges this system.

One of the major reasons why the scientific status of psychoanalysis was challenged was because its system of analysis and observation failed to conform to the standard scientific method. Unable to validate its conclusions in any objective way, psychoanalysis asked its supporters to 'take it on faith'.¹⁴ It was typical for scientists who were doubtful about psychoanalysis to attack 'the scientific fragility of observations made in all clinical settings', to expose its rejection of 'traditional scientific methodology', or to denounce its 'scientific sparseness' and its 'scientific weaknesses'.¹⁵ Because psychoanalysis 'relies upon techniques that do not admit of the repetition of observation, that have

no self-evident or denotative validity, and that are tinged to an unknown degree with the observer's own suggestions',¹⁶ it cannot assume scientific authority. Maintaining a rigid definition of 'scientific' to include 'information only when it has been secured through procedures that are repeatable and involve techniques that make it possible to check on the objectivity of the reporting observer',¹⁷ many in the scientific community denied psychoanalysis any further consideration.

Some scientists began to conduct studies in which the scientific status of basic Freudian concepts could be tested through more traditional methods of observation and experimentation. One of the most famous studies was conducted in the early 1940s by the psychologist Robert Sears, who was retained by the Social Science Research Council to 'do a survey of the objective studies of psychoanalytic concepts'.¹⁸ In working towards 'the development of objective techniques for studying the kinds of behaviour to which Freud applied himself', Sears' final report collected much of the experimental work in psychology that had employed more traditional scientific methods to confirm or reject a wide range of basic Freudian theories. In his concluding remarks, Sears admits that although psychoanalysis 'deals with many things other sciences have ignored ... [that] must be incorporated into the general body of scientific knowledge' he simultaneously acknowledges that 'how to do this is a puzzler.' Because his study produced some positive and some negative results, he concludes that while psychoanalysis is, in fact, 'a science of personality', it must, in the final analysis, be recognized as 'not a good science'.¹⁹

While attempts at verifying or disproving Freudian psychoanalysis through the use of scientifically acceptable experimental techniques continued through the mid-twentieth century,²⁰ others have explored how the therapeutic techniques and theoretical speculations of psychoanalysis might offer science a means of expanding its focus and redefining its methodology. Rather than force psychoanalysis to conform to a rigid definition of science or reject its scientificity because of its inability to do so, some scientists argued that Freud challenged the scientific community to revise its own self-definition to include those techniques of observation, experimentation and analysis that had proved so effective in psychoanalysis. In his review of Sears' survey, Heinz Hartmann welcomed this 'extremely valuable' book, which 'gives a clear and objective account of some of the research done in this field by psychologists during the last few years'.²¹ Rather than challenge Sears' conclusion that 'psychoanalysis is, by the criteria of physical sciences, not a "good" science', Hartmann questioned the accuracy of these criteria:

I should like to add that for many important problems of psychology, [psychoanalysis] is the best method available at present and that the

verification by other methods of so many heretofore disbelieved analytical findings should lead to a higher evaluation of analysis itself as a scientific *method*.²²

From this perspective, it is not the extent to which psychoanalytic techniques conform to a pre-determined scientific method which determines the scientific status of psychoanalysis; instead, it is the success of psychoanalysis in treating patients, curing symptoms and deducing sound theoretical models from these clinical experiences that demands a re-consideration of how science and its approved methodologies are defined. By both exposing the arbitrariness of scientific standards and challenging the exclusivity of the scientific method, psychoanalysis called into question, the disciplinary and intellectual authority of science as well as the entire disciplinary structure that was so firmly grounded in it.

This disciplinary ambiguity was heightened as scientists, psychologists and medical doctors worked to understand how psychoanalysis had an impact upon the practice of medicine, the training of doctors and the disciplinary divisions between medicine and psychology. Again underscoring the intricate connection between disciplinarity and professionalization, serious questions soon arose concerning whether or not medical training should include a background in psychoanalysis or, more generally, to what extent psychoanalytic theories about the human mind would affect all branches of the medical sciences. With a traditional separation between the medical treatment of the body and the psychiatric treatment of the mind well-established, medical doctors speculated as to how psychoanalysis might challenge this division. Should psychoanalysis be classified as a branch of psychology, thus maintaining the gap between the care for the body and the study of the mind? Or would its development of new psychiatric techniques and its insights into the relationship between physical symptoms and psychic causes challenge this seemingly natural split?

While many argued that the introduction of psychic phenomena 'threaten[ed] to disturb the homogeneity of medicine', others believed that 'science c[ould] not close its eyes to phenomena only because it c[ould] not master them with the usual test methods.' Rather than excluding psychoanalysis because its subject matter did not conform to standard scientific and medical methods, those 'method[s had to] be adjusted to the nature of the subject matter'.²³ By doing so, scientists might begin to recognize that 'it is artificial to separate mental diseases from physical diseases, or mental processes from physical processes.' In recognizing the 'permanent interrelation between them', scientists could begin to understand psychoanalysis as more than just 'an important new method or as an isolated discovery', but, instead,

as 'the beginning of a new phase in biological and medical thinking'.²⁴ Acknowledging and permitting such intellectual revision could function in two ways. First, it could call into question the stability and authority of the disciplinary structure through which these separate categories are constructed and their separation is maintained. Second, the assertion of a 'permanent interrelation' between the psychical and the physical, despite possible challenges to the disciplinary structure, could also work to continually re-inscribe the binary between them. On this and many other occasions, psychoanalysis would need to negotiate its own participation in both of these processes. Consequently, the early intellectual and cultural history of psychoanalysis demonstrates the extent to which the hierarchical division of knowledge into distinct disciplines is an ongoing process that must always adjust to the development of new methods and techniques, the crossing of disciplinary boundaries, or the appearance of potentially new disciplines. As the mechanism through which knowledge is validated and authorized, this disciplinary structure must maintain its own authority by ensuring that all sources of knowledge production are contained within its categories. By challenging the traditional definition of science and the scientific method, by bridging the gap between the theoretical science of psychology and the medical practice of psychiatry, by offering links between the mind and the body, and by constructing a complete theory of the human mind that could potentially influence a vast range of disciplines, psychoanalysis exposed the limits of nineteenth-century science while garnering its own intellectual and cultural authority from it.

SCHOLARS, SCIENTISTS AND SEXUAL INVERTS: AUTHORITY AND SEXOLOGY IN NINETEENTH-CENTURY BRITAIN

Heike Bauer

This essay investigates the shifting centrality/marginality axis of scientific authority in nineteenth-century Britain by comparing two key works of the newly-developed discipline of sexology: *Psychopathia Sexualis* (1886–1902) written by Richard von Krafft-Ebing and *Sexual Inversion* (1897) written by John Addington Symonds and Havelock Ellis. Examining the production and dissemination of the two works highlights the contested authorial positions within the new discipline. It reveals that the boundaries between the role of sexologist and that of the sexological case study of the (male) ‘sexual invert’ were fluid. While the concept of ‘sexual inversion’, which was developed by Karl Heinrich Ulrichs, who claimed that same-sex sexuality is a form of gender inversion, theoretically included forms of female ‘sexual inversion’, in this context, it refers to male same-sex love.² The psychiatrist, Krafft-Ebing developed new sexual taxonomies and published them in 12 revised editions of his *magnum opus* on sexuality. Ellis, scientist and literary scholar, is today better remembered for his involvement in the first study in English of same-sex sexuality than its initiator Symonds, a Renaissance scholar, literary critic and self-identified ‘sexual invert’. By comparing the production of *Psychopathia Sexualis* and *Sexual Inversion* and their impact on British culture, I hope to unveil some of the forces that shaped the battle for sexological authority in late nineteenth-century Britain. I argue that, unlike in European

branches of sexology, which were firmly anchored within scientific tradition, British sexology was partly indebted to the literary realm. This in turn influenced the way British sexologists tried to establish their authority in the new discipline. The question of centrality versus marginality is, therefore, closely tied in to definitions of science, literature and sexual identity.

Psychopathia Sexualis had been written originally as a medical reference book for use in the courtroom. It aimed to record and understand human sexuality. What was so original in Krafft-Ebing's work was not only that for the first time, sexual behaviour was theorized in a scientific text, but that the subjects could articulate their own narratives, at least in part. Through often sexually explicit case studies, Krafft-Ebing attempted to find new taxonomies for sexuality. His authority was derived from the analysis of subjective accounts, rather than from anonymous empirical study. For the reader, this offered the possibility of identifying and also, crucially, identifying with various sexual identities and preferences. This may help to explain why *Psychopathia Sexualis* has remained in print to this day – the latest German edition was published in 1997; the most recent English edition in 1999.

The publishing history of *Psychopathia Sexualis* clearly shows its popularity. Moreover, it reflects Krafft-Ebing's continued interest in sexuality and sexual practice. He regularly updated the work, displaying an increasing acceptance of different sexualities. Krafft-Ebing revised *Psychopathia Sexualis* 12 times between 1886 and 1902. Each of Krafft-Ebing's revisions was published by the specialist medical publishers, Ferdinand Enke.³ Only a select number of Krafft-Ebing's editions were translated into English, which obscures the development of the work. The American psychiatrist, Charles Chaddock, first translated *Psychopathia Sexualis* into English in 1892, based on the seventh German edition.⁴ Numerous translations of different editions and by different translators were to follow, produced by publishing houses on both sides of the Atlantic, from the highly regarded New York medical publisher Physicians and Surgeons Book Company to London's popular Heinemann house.⁵ The medical book was such a success that after Krafft-Ebing's death, his sons entered a dispute with the publisher Enke about the income generated by the work and about who was to edit the subsequent publications.⁶

By 1886, Krafft-Ebing had made himself a name as a psychiatrist. He held one of the most prestigious chairs in psychiatry at Vienna. He had been awarded a vast number of honorary fellowships and received distinguished professional accolades.⁷ Tellingly, while *Psychopathia Sexualis* was used in German courtrooms, it was not acknowledged as an authoritative 'scientific' work in Britain and the study of sex remained partly censored. While Krafft-Ebing was revered as a psychiatrist, his sexological work was not highly estimated in mainstream

British medical circles, as his obituary in the *British Medical Journal* in 1903 reveals:

His name was brought into somewhat unfortunate prominence by his book entitled *Psychopathia Sexualis* ... The Book has gone through many editions, but this questionable popularity has been due rather to the curiosity of the public than to the appreciation of the medical profession. Krafft-Ebing, however, made many valuable contributions to neurology for which his name must be held in honour.⁸

The obituary makes the distinction between Krafft-Ebing's credentials as a neurologist and his popular acclaim as a sexologist. *Psychopathia Sexualis* is described as a popular, rather than a scientific, work and as Krafft-Ebing was at the centre of the new discipline, this pushed it to the borders of medical science.

Havelock Ellis and British Sexology

Sexology's marginal role in British science was reinforced by its partly literary antecedents. Ellis began as a teacher and literary critic, although to gain scientific credibility, he embarked on a medical degree at the University of London. He does not seem to have much enjoyed it, and in 1889, he graduated with the Licentiate of the Society of Apothecaries, the lowest possible medical diploma, which had only recently been introduced. Ellis did not take up practising medicine; instead he decided what he called to apply 'the scientific spirit ... to life', and embarked on his sexological studies.⁹ Both Ellis and Krafft-Ebing had received medical training and, not being themselves homosexual, could claim to approach the subject with scientific objectivity. Unlike Krafft-Ebing, however, Ellis did not display particular interest in male 'inversion' as a sexological subject (although he later analysed the lesbianism of his wife Edith Lees) and unlike his German colleague, his claim to medical authority was somewhat spurious. Ellis devoted his subsequent sexological studies mainly to issues related to 'heterosexuality'. Ellis was proud of the leading role he played in contemporary scientific and literary developments. When he moved to London in 1879, he almost immediately started to publish literary-critical essays and articles in the periodical press. One of the most influential of these was an 1883 *Westminster Review* essay entitled 'Thomas Hardy's Novels' that elicited a positive response from Hardy himself.¹⁰ In the late 1880s, Ellis established himself as general editor of two of the most important literary and scientific series of the time: the *Mermaid Series*, which re-published the plays of English Renaissance dramatists, and the *Contemporary Science Series*, which published works

by leading European scientists and sexologists of the time. The first volume in this series was Geddes and Thomson's *The Evolution of Sex*; other volumes included Mantegazza's *Physiognomy and Expression*, Moll's *Hypnotism* and Lombroso's *The Man of Genius*, as well as Ellis's own publications *The Criminal* (1890) and *Man and Woman* (1904).

Towards the end of his life, in 1934, Ellis published a collection of notes and essays entitled *My Confessional*, which supplemented his autobiography *My Life* with further insights into his intellectual production. *My Confessional* includes a characteristic short treatise on contemporary science entitled 'Make room for the Pioneer!' He commented:

In every spiritual and material field we are pushing the known into the unknown, carrying the torch of science to disperse the gloom around us, harnessing the great forces of Nature to our small forces for the immense magnification of our power over the world, penetrating, as none ever penetrated before, into the obscure mysteries of the mind, planning for practical realization schemes of social reorganization which of old were merely the dreams of philosophers or the abortive struggle of madmen.¹¹

This triumphant, Nietzschean account of scientific progress is typical for Ellis's thinking. While he frequently emphasized his intellectual debts to a particular literary tradition, he also clearly saw himself in the centre of new scientific developments.¹² For Ellis, scientific progress went hand in hand with intellectual advancement, whereby the 'torch of science' illuminates both 'spiritual' and 'material' concerns.

Belief in progress as well as in his own role as a philosophical – scientific pioneer characterized Ellis's career. His involvement with the literary and scientific movements of the day make him pivotal in the production and distribution of sexual knowledge in late-Victorian and Edwardian Britain. *Sexual Inversion* was in a sense his apotheosis, as it was arguably the widest reaching of all of his work, though rather ironically, Ellis later dismissed the work's importance. It is crucial to remember that *Sexual Inversion* was part of a wider ranging body of literary and literary – scientific works produced by Ellis over a period of more than thirty years. Like Symonds, Ellis's intermediary role between sexology and literature is typical of British sexology, a discipline that remained very much at the margins of nineteenth-century science.

Symonds, Autobiography and the Literary – Philosophical Tradition

If sexology held a marginal position in British science, the autobiographically inspired literary – philosophical sexological works of Symonds were

marginalized within sexology itself. A man of letters, poet, married 'sexual invert' and sexologist, Symonds describes his life work in his *Memoirs* as follows:

Trying to evade the congenital disease of my moral nature in work, work has drained my nerves and driven me to find relief in passion. The subjects with which I have been occupied – Greek poetry, Italian culture in one of the most lawless periods of modern history, beauty of nature and the body of man – stimulate and irritate the imagination.¹³

Here, Symonds evokes the intricate connections between his scholarly work and his sexual identity – the 'congenital disease' of his moral nature refers to his own 'sexual inversion'. It is apparent from Symonds's *Memoirs* that in fact he did not feel his homosexuality to be a disease, but a creative stimulant that lay behind all of his work. His portfolio includes more than a dozen monographs, numerous articles and reviews in leading periodicals and a series of editions.¹⁴ Moreover, he is the author of two of the first treatises on homosexuality in English, *A Problem in Greek Ethics* (1883) and *A Problem in Modern Ethics* (1891). Symonds's sexological work was inextricably linked to his autobiography as well as to the literary realm. It was perhaps partly due to the fact that Symonds was himself an 'invert' and that he had no scientific training that his authority in the discipline of sexology was belittled by contemporaries, including Ellis.

Symonds's two sexological pamphlets provide insight into the beginnings of British sexology. The first, *A Problem in Greek Ethics*, can be seen as his preliminary study of 'inversion'. Here, he analysed the historical context for male same-sex love, by tracing its origin back to the civilization of ancient Greece. In doing so, Symonds deliberately attempted to fill a gap in sexological scholarship. The treatise opens thus:

For the student of sexual inversion, ancient Greece offers a wide field for observation and reflexion. Its importance has hitherto been underrated by medical and legal writers on the subject, who do not seem aware that here alone in history have we the example of a great and highly developed race not only tolerating homosexual passions, but deeming them of spiritual value and attempting to utilize them for the benefit of society.¹⁵

Symonds begins his investigations of 'sexual inversion' by asserting the need to look at literary – philosophical history. He shows his familiarity with relevant continental scholarship, and emphasizes the need for more than the mere medico-forensic approach of European sexology.

While *A Problem in Greek Ethics* is indebted to Symonds's classical scholarship, *A Problem in Modern Ethics* documents how Symonds's understanding of sexuality partly derived from his study of European sexology. *A Problem in Modern Ethics* might be called the first 'reader' in sexology: a critical anthology of hitherto published sexological scholarship. Unlike the sexologists, who scrutinized the life of a variety of individuals, Symonds now scrutinized the works of the sexologists. From a scholarly perspective, he engaged with the subject that dominated his life:

The underlying preoccupation of my life has been a tyrannous emotion, curbed, suppressed for the most part, but occasionally indulged with spasmodic violence. Literature takes the second place; and for this reason ... I have never been able to take it very seriously.¹⁶

If for Symonds literature indeed had a subordinate role, it nevertheless gave his life the structure, financial backing and the language that was to enable him to write *A Problem in Modern Ethics*. Together with its reprint in the German edition of *Sexual Inversion*, *A Problem in Modern Ethics* provides the clearest insights into Symonds's understanding of his own sexuality, as well as an invaluable critical account of contemporary sexology, and, despite Symonds's ranking of literature in 'second place', *A Problem in Modern Ethics* links sexology and literature as the space where discourses on homosexuality were possible.

A Problem in Modern Ethics is, like the body of works it examines, ostensibly directed 'especially to medical psychologists and jurists', yet Symonds clearly does not intend such an exclusive readership. He writes: 'Those who read these lines will hardly doubt what passion it is that I am writing at [sic].' He continues: 'surely it deserves a name ... inverted sexuality'.¹⁷ Symonds here situates the work and his role as its author within an autobiographical frame. He foresees the reader's reaction to the treatise as similar to his own readings of sexological texts. He had himself gained access to the medico-legal works of European sexologists, but unlike them, Symonds makes references to a lay readership of 'sexual inverts' that his work may attract, making an autobiographical-scholarly claim for authority. Unlike other sexologists, he aims his text not exclusively at the medical profession, but also at the 'inverted' reader.

Symonds said of *A Problem in Modern Ethics* that it was 'one of the few adequate works of scholarship [he could] call [his] own'.¹⁸ Symonds had studied all published material on the subject, as is demonstrated by the opening of the treatise with a bibliography which spans the range of different disciplines and viewpoints on the subject of sexual inversion in Europe.¹⁹ It includes the French forensic physician Tardieu and his countryman, the

psychiatrist Moreau de Tours; the Italians, Mantegazza and Lombroso; as well as the German forensic physicians, Casper and Liman; and, of course, Krafft-Ebing. Significantly, all of the English-language works in the bibliography are literary. They include the tenth volume of Burton's *Arabian Nights* (1885), and two works by Whitman, *Leaves of Grass* (1889–90) and *Democratic Vistas* (n.d.). While there did not, of course, exist a comparable British body of sexological works at the time, it is noteworthy that Symonds had eschewed scientific British works on sex such as Geddes and Thompson's recent biological study *The Evolution of Sex* (1889).

The bibliography reveals that Symonds read German, French and Italian. While his sexological knowledge as a whole was based on the wide spectrum of European sexology, it is the German homosexual activist, lawyer and writer Ulrichs, who emerges as the greatest influence on Symonds. Ulrichs had posited that in the case of the male homosexual, which he called 'Urning', '*anima muliebris virili corpore inclusa*' (a female soul is confined in a male body); for a homosexual woman the formula was '*anima virilis muliebri corpore inclusa*' (a male soul is confined to the female body).²⁰ This theory seems to have appealed to Symonds. He lists 14 of Ulrichs's publications, both under the latter's real name and under his pseudonym *Numa Numantius*. Unlike later sexologists, Ulrichs had no scientific training; instead he wrote literary and autobiographical studies of 'sexual inversion'. None of these texts were published outside Germany, although most of them were available in the library of the British Museum. As Symonds spent relatively little time in London – and by the time of writing *A Problem in Modern Ethics* he had permanently moved with his family to Switzerland – it appears feasible that he actually owned most of Ulrichs's publications himself. He was also in regular correspondence with the German, and once visited Ulrichs in his Italian residence.²¹ This reinforces Symonds's understanding of himself as an 'invert', and it illustrates the particular genealogy of Symonds's sexological investigations.

The fact that Symonds came from outside the medico-legal realm, the realm which provided the background to European sexology, provides a vital clue for understanding how sexological knowledge was disseminated and understood in Victorian Britain. For Symonds, literary writings, especially classical texts, aided his sexological studies because they provided him with a particular history of male same-sex love. At the same time, his own sexological studies provided space for consolidating his scholarly pursuits and personal desires. Symonds played a crucial role in the emergence of British sexology in providing a key link between German sexology and British thinking in the late nineteenth century. He was not only personally acquainted with some of the best-known sexologists and writers of his time,

but he also engaged with their theories. He contributed to the discipline both as a critic and as a 'case', initially in his *Memoirs* and later as Case XVII in *Sexual Inversion*. He was clearly aware of the fact that his intellectual interests were not merely theoretical, but that his life was testimony to his ethical, philosophical and medical beliefs. His autobiography lends authority to his sexological writings.

Sexual Inversion

In 1892, Symonds implored his friend Arthur Symonds to approach Havelock Ellis and ask whether he would consider collaborating with Symonds on a book.²² Ellis, who had recently made himself a name as a sexologist with the publication of *The Criminal* (1890), was then working on *Man and Woman: A Study of Human Secondary Characters* (1894). This, he regarded a preliminary study for what he was later to describe as his 'chief life-work', the seven volumes of *The Psychology of Sex*.²³ Ellis agreed to collaborate with Symonds, and in his autobiography, he recalled how the work was to be shared:

I drew up a scheme of the book with the parts I proposed to be assigned to each author. [Symonds] accepted my scheme, remarking that I had assigned the most important chapter to myself but making no demur. I had no wish to take chief share, but it seemed clear to me that it was on the scholarly and historical side alone that Symonds could properly come into the book.²⁴

Ellis's dismissive tone of Symonds's apparently non-scientific involvement in the project characterizes how, by the time of publication, his attitude had changed towards both his coauthor, who had died before the book went to print, and towards the project itself. In retrospect, Ellis considered Symonds's death serendipitous 'because the significance of a book on inversion would have been greatly discounted by the fact that one of the writers was known to many as personally concerned in the question of homosexuality.'²⁵ For Ellis, the same biographical facts that Symonds had found authoritative undermined Symonds's expertise. Contrary to Ellis's claims, Symonds's contribution made up roughly half of the content of the first edition. He thus helped to produce the first English sexological textbook, yet it was Ellis alone, who, somewhat unjustifiably, was, henceforth, to be known solely as its author.

When Symonds died in 1893, Ellis was, however, left to complete *Sexual Inversion*. Prior to that, the two men, who had never met, conducted their working relationship entirely through letter correspondence. Symonds had

contributed the historical – philosophical background to *Sexual Inversion*, which consisted largely of reprints of his two previous treatises on the subject, *A Problem in Greek Ethics* and *A Problem in Modern Ethics*, and he provided a number of case studies in addition to his own. Ellis undertook the sexological analysis of the materials. With Symonds's death, the project came to a halt and only two years later did Ellis decide to publish. He arranged for translation into German and then first publication in Germany, as the way had already been opened not only by Krafft-Ebing, but also by the Berlin-based sexologists Albert Moll and Magnus Hirschfeld.²⁶ *Sexual Inversion* was translated by the physician Hans Kurella, whom Ellis had met at the International Medical Congress in Rome in 1894. Kurella translated *Sexual Inversion* as *Das Konträre Geschlechtsgefühl*, a term coined by Moll in 1891.²⁷ Ellis objected to this translation, presumably partly because Moll's own book had appeared under the same title.²⁸ Unlike the later English translation *Das Konträre Geschlechtsgefühl* had both Ellis's and Symonds's name on the title.

The situation was different in Britain, where *Sexual Inversion* was first printed in English by Wilson and Macmillan.²⁹ When the book was ready for distribution, Symonds's family put pressure on his literary executor Horatio Brown and demanded that Symonds's name be removed from the work.³⁰ Brown blocked the distribution of *Sexual Inversion*, although a few copies escaped being pulped. Ellis then reworked *Sexual Inversion* by removing all reference to Symonds's name. From the new edition, he completely excluded Symonds's 'homosexuality in Greece' (chapter three of the German edition), as well as two appendices, one a discourse on Roman same-sex love, and the other on 'soldier love and related matters'. Both appendices had been written and signed by Symonds. The abridged edition was then published in Britain in 1897 by the 'Watford University Press'³¹ of the German fraudster Georg Ferdinand Springmühl von Weissenfeld.³² One of the copies of *Sexual Inversion* came into the possession of a small society called the Legitimation League, which advocated a reform of marriage laws to give rights to children born to unmarried parents. The society's secretary, George Bedborough, unknowingly sold a copy of *Sexual Inversion* to a policeman and was arrested and tried for distributing 'obscene' material, under the same act under which Radclyffe Hall's novel *The Well of Loneliness* was prosecuted in 1928. *The Well of Loneliness* was the first English novel to present a literary case study of female sexual inversion,³³ and its trial appeared to be a repeat of the 1897 trial of *Sexual Inversion*, when Bedborough was found guilty, and which resulted in *Sexual Inversion* being banned in England.³⁴ However, unlike Hall's novel, *Sexual Inversion* remains to this day unpublished in Britain. The Philadelphia-based press F. A. Davies, which also published translations

of Krafft-Ebing's *Psychopathia Sexualis*, published *Sexual Inversion* in America, thus making it available in English. Despite the ban, the work became a reference text for late-Victorian notions of homosexuality in Britain.

During the late nineteenth century, sexological authority in Britain was a complex, unstable construct. As we have seen in the assessment of Krafft-Ebing's work, while his role as a sexological authority was acknowledged, the discipline itself was marginal within the sciences. This was reinforced by British sexology's literary tendencies, as British Symonds wrote literary – philosophical treatises, and even the more scientifically oriented Ellis was drawn to poetry and literary criticism. Symonds was, furthermore, marginalized as a sexologist within the discipline itself, partly due to his literary – philosophical approach and partly because of his personal interest. At the beginning of the twentieth century, new disciplines such as psychoanalysis and sociology entered the scientific mainstream and usurped sexology's priority in the analysis of sex. In recent years, post-Foucauldian criticism has been slow in a reassessment of sexology³⁵, however, more currently the posts of sexological authority have shifted. Krafft-Ebing has been vilified and Ellis has been criticized harshly. Symonds on the other hand has undergone a scholarly revival, if on a very small scale. His self-acknowledged homosexuality has moved him towards the centre of studies in the history of same-sex sexuality. Sexological authority thus remains an unstable notion, shifting from and to the centres and margins of contemporary critical discourses. The scholars, scientist and sexual inverts, key figures that made the discipline, constitute the axis of this movement.

UNMASKING IMMORALITY: POPULAR OPPOSITION TO LABORATORY SCIENCE IN LATE VICTORIAN BRITAIN¹

Martin Willis

British science in the early twenty-first century seems largely institutionalized; sophisticated laboratories, commercial companies and governmental organizations control the production and distribution of scientific knowledge to such an extent that it is difficult to imagine that science was not always part of the British hegemony. Indeed, there is a popular, if ahistorical, perception that science has always been practised from within such a power structure. Despite the continued opposition of, among others, animal rights activists and certain scientific ethics lobbying groups, professional science appears insulated against, and unconcerned by, any voices raised in protest. Yet, the rise of the scientific institution in the late nineteenth century reveals that the central and dominant position of professional science did not go uncontested and that public resistance was not as marginalized as it appears today. State-supported (if not funded) scientific laboratories, the most important symbol of professional science in the last quarter of the century, found to their cost that the ideology of institutional science was anathema to some sections of the Victorian public and to try to sideline their contribution to scientific debates would result in voluble opposition.

The founding of the British Institute of Preventive Medicine in 1889 is one of those important moments in the history of the scientific laboratory

that reveals how professional scientific culture was once a site of vigorous and violent conflict over authority. The vociferous antagonism between the Institute's professional scientists and antivivisection campaigners highlights those arenas of cultural representation that were used as touchstones of authority and power. Having first detailed the position of professional scientific institutions in the 1880s, this paper will then show how the animal experimentation proposed by the Institute led to a characterization of laboratory science as both immoral and a danger to the nation. Scientists countered this image by representing the antivivisectionists, in turn, as weak-minded women and working-class agitators. Yet while the antivivisectionists conducted a public campaign through national newspapers and periodicals, the scientific community preferred to talk directly to state-organized institutions. This fight for the dominant voice is one of the key investigations of this paper, an interrogation that will lead ultimately to the question of which position, either of centrality or marginality, the conflicting sides eventually occupied?

Professional Institutions of Science

The creation of a professional culture in the sciences was always fraught with conflict. Despite the success of amateur scientific institutions such as the Mechanics' Institute, professionalization remained a remote possibility through the first half of Victoria's reign. It was not until the 1870s, when the Devonshire Commission was one of several voices recommending state support for scientific developments, that the first signs of professionalization appeared. The Clarendon and Cavendish Laboratories (founded at Oxford in 1872 and at Cambridge in 1873) were the first sites of science to create an atmosphere of professional scientific endeavour. Nevertheless, such professionalism remained unsupported by government, for the state remained resolutely against large-scale funding for scientific institutions, preferring instead to increase funds for individual scientific projects, administered by various organizing bodies over which government had varying degrees of control. This decision did little more than exacerbate the individuality of scientific research, reinforcing the representation of the scientist as an amateur enthusiast, who performed science for science's sake.²

Altering this culture of science was a challenge to those lobbyists who were determined, on one hand, to place science on a more professional footing and, on the other, to gain what they believed to be the proper financial recompense for scientific work. For collectives like the BAAS – one of the foremost defenders of science during the Victorian period – TH Huxley's vision of the scientist as someone, who may 'earn praise but not pudding'³

needed to be immediately addressed. They were opposed, however, by a powerful cultural movement that continued to privilege scientific amateurism as heroic determination to succeed against all odds. Those who wished to could draw upon the work of many of the leading scientists of the period in order to prove that a lack of professionalism had apparently not put paid to discoveries of enormous magnitude. Charles Darwin and William Crookes, for example, were only two among many, who remained private scholars throughout their careers. Further support of the status quo was provided by a signifying system that saw professionalism as restrictive, staid and unimaginative while amateurism was characterized by freedom, excitement and ingenuity. As A W Williamson argued in an address to the BAAS in 1874, independent research proved 'how true and pure a love of science exists in this country and how Englishmen will cultivate it when it is in their power to do so.'⁴

In spite of the combative views of certain reactionary factions, the science lobby began to prevail upon both the state and the public by the final two decades of the nineteenth century. A vital factor contributing to their change of fortune was the challenge to national pride brought about by both France and Germany's determination to strike out ahead of Britain in all matters scientific. From sea expeditions and land explorations to a scientific civil service, continental Europe proved to Britain how vital to national morale and the national economy was the pursuit of science. As Peter Alter stresses: 'During the last third of the nineteenth century ... public discussion increasingly stressed the benefits, indeed the necessity, of applied science in order to achieve economic and industrial progress.'⁵ Certainly by the later 1880s (when the creation of a British Institute of Preventive Medicine was first mooted) 'the old free amateurism was disappearing'⁶ and a new professional scientific class was emerging.

This is not to say that the culture of amateurism was being readily excised from scientific practice. Indeed, the final decade of the nineteenth century is the period when professional and amateur science clashed most often. The laboratory was often at the centre of conflict between new professionalism and the vestiges of amateurism. As a focus of scientific activity – for both scientists and the public – the laboratory was an important site from which scientific culture could be produced and distributed. The professional laboratory had, therefore, to be the antithesis of the amateur laboratories heralded earlier in the nineteenth century. These new spaces had to be dedicated to labour, to transparent research cultures, and to public approval. The private laboratory of the non-professional years had been part of the domestic spaces of individual scientists, their researches had taken place in secrecy, and the individual scientist was checked only by his own conscience and abilities.

To subvert the Frankensteinian image of the laboratory scientist, professional scientists had to reinvent the symbolism of their architecture, their investigative practices, and the methods by which their community represented itself to society.

From the building of the first laboratories in Oxford and Cambridge, the earliest examples of a conscious professionalizing of the scientific laboratory, it was clear that the scientific community was initially unable to make the necessary changes. The Cavendish Laboratory in Cambridge, for example, built a private study and laboratory for its leading Professor of Chemistry. Designing a laboratory interior that continued to pander to the individual scientific 'genius' did not appear to be in keeping with the Cavendish scientists' own boasts of the new 'public' laboratory. For those members of the public who did gain access to the Cavendish Laboratory, there was a great deal in the representation of professional science that differed little from the amateur status apparently left behind. The ground floor was occupied by as many domestic spaces as workspaces: there were several antechambers, an administrator guarded the entrance to the active laboratory rooms, and almost everyone present was under the express invitation of the Laboratory manager (who also happened to be the Professor with the private laboratory). A culture of amateur homosociality pervaded the interior of the Cavendish Laboratory; a culture only reinforced by the prohibition on female visitors, the necessity for entrance to be preceded by written application, and the invitation extended to those entering to await their guide or colleague in the laboratory sitting room.⁷

Ironically, discarding the trappings of amateur science proved as difficult to achieve as substantial state support for professionalization. Yet, the scientific community must have realized that a positive public perception of science was vital. Certainly they had revealed an ability to manipulate the public during the several decades that they had lobbied for a more privileged position within the cultural and financial economy of the state. However, they seemed to set aside those practices as soon as they gained legitimate professional status. This degeneration into a residual amateurism led ultimately to the dissemination of a conflicted set of signifiers from the scientific institution to the general public. Laboratory science appeared professional and state-controlled as well as amateur and unlegislated. This confusion constituted one of the major reasons for the forthcoming conflicts over cultural authority.

(Im)moral Practices

The scientists involved in the creation of the British Institute of Preventive Medicine knew from the beginning that their proposed laboratory would be

contested by certain pressure groups. Before the Chelsea site had been purchased, or the creation of the Institute was certain, scientists attending a meeting with Sir James Whitehead, the Lord Mayor of London, became aware that any apparent discussions of a 'Pasteur Institute in London' would cause controversy. Writing to Whitehead on 12 June 1889, the zoologist Edwin Ray Lankester noted that:

There have been two absolutely false letters in the *Pall Mall Gazette* during the week on the subject of Pasteur – written by persons totally ignorant of the subject and evidently prompted by the antivivisection party. That party is very small in numbers but entirely unscrupulous in regard to facts. They make a great deal of noise and do not hesitate to tell the most shameless falsehoods. It will be of very great importance that you should show that you and men like yourself attach more weight to the opinion of Paget, Lister, Roscoe and Huxley than to that of these irresponsible agitators.⁸

Lankester's dismissal of the antivivisection leagues he believed responsible for recent media opposition to the meeting reveals how little regard the scientific community had for its public image. For Lankester, the private administrations of governmental and scientific authorities were of sole importance. The opinions of a non-professional organization (it is likely that the 'false letters' were placed by the Victoria Street Society, antivivisection lobbyists led by Frances Power Cobbe) should not only have been cast aside, they should have been illuminated as false and immoral.

Private letters, regardless of the correspondents' positions of authority, had less effect than the open letters placed by antivivisection groups in the popular press. Less than two weeks later, TH Huxley was so concerned by the growing power of the antivivisection campaigners that he wrote directly to Whitehead while holidaying in Switzerland. In that letter Huxley forcefully asks that Whitehead should not be dissuaded from his course by 'the blind opponents of properly conducted physiological experimentation, who prefer that men should suffer, rather than rabbits or dogs.'⁹ Three days after that, on 28 June, Lankester writes twice to Whitehead, first from the Athenaeum Club to ask whether the Lord mayor 'may be discouraged by the opposition crew',¹⁰ and then from Exeter College, Oxford to note 'that it will be necessary to have a good force of police to guard the entrance to the meeting.'¹¹

The pressure put upon the Institute founders was increasing, leading the scientific community to resort to state-controlled force. Whether Lankester was more concerned for his personal safety, rather than the image of science, is irrelevant, for his view is indicative of the scientific community's

determination to create and control the new laboratory culture. The four letters discussed here provide an important touchstone to the early confrontations between the Institute and the antivivisectionists. The assertion of moral authority becomes central to each group's claims of cultural authority. For the scientific community, it was the antivivisectionists, who were immoral; in their determination to block scientific progress towards, as Huxley states, the improved 'welfare of society'.¹² For the antivivisectionists, scientific experimentation on animals was an immoral practice, and those scientific institutions that privileged such experiments could not expect anything other than public opprobrium.

Throughout 1889 and into the mid-1890s, the antivivisection lobby controlled almost all of the public fora in which cultural representations were formed. The discourse that dominated the public perception of the Institute was that of its opposers, who distributed pamphlets, held quasi-political rallies, and constantly addressed the public from the pages of the periodical press. By 1894 (the year the Institute announced its intention to build a laboratory in Chelsea) *The Lancet* was reporting that 'the opponents of the British Institute of Preventive Medicine must, we may assume, now number 5,000 persons.'¹³ Such a high figure was undoubtedly one of the outcomes of the news media campaign waged by the protestors. In the final three months of 1894, there were more than a hundred letters and articles in national and regional newspapers denigrating the practices of the Institute. A letter in *The Gloucester Chronicle* argued that vivisection 'would be against the national will and conscience',¹⁴ *The Echo* described the vivisection laboratory as 'a living tomb',¹⁵ and *The Star* believed the Institute would be 'a market garden of disease, an Inferno for innocent creatures, a material danger and a moral horror'.¹⁶

The popular image of the scientist and the scientific community was of an inhuman and amoral torturer, a dabbler in dark arts, performed for the most part in the gothic laboratories imagined by various fictions. HG Wells appeared to capture public opinion with the vision of the vivisector in his 1896 novel *The Island of Dr Moreau*, which several contemporary commentators believed to be his own form of support for the antivivisection campaigns. Such a powerful presence in popular culture led the antivivisectionists to reach almost to the edge of plausibility in their determination to discredit the culture of science. Later in 1894, protestors had begun to suggest that human vivisection was next on the scientific agenda. The London-based *Echo* suggested that inmates of London's asylums may become the next experimental materials for diphtheria vaccinations¹⁷ while earlier in the year *The Bristol Mercury* had printed a letter stating that 'if vivisection of animals is allowed ... it is a certainty that vivisection of human beings is by no means distant.'¹⁸

That the popular press gave over valuable print to judgmentalism is indicative of the prime position held by the antivivisectionists in the construction of laboratory culture. Their discourse was so influential because it was the only discourse to which the public had access. Although, as we have seen, scientists such as Huxley and Lankester did oppose the campaigners and were able to articulate that opposition, they did so only for each other in private correspondence. Certainly, this was due to a belief that the scientists involved in the Institute had the ear of those with political power and influence, the 'powers that be'¹⁹ as one commentator described them. The necessity to gain public approval appeared secondary to gaining governmental authority. However, with public opposition reaching the heights suggested by *The Lancet*, those paid politicians responsible for Chelsea and the surrounding boroughs were increasingly being pressurized by their constituents to agitate on their behalf. When the Chelsea MP Mr Whitmore lobbied the Home Secretary with the 'most extensively signed petition since the days of the Chartists'²⁰ the Institute's leading scientists realized that a response was necessary if they were not to lose a grip on state support as well as public opinion.

Rather than leap into public conflict with the antivivisectionists on the battleground of the periodical press, the Institute began to conduct a number of operations designed to bolster its position with the government rather than the public. At the same time the antivivisectionists, believing that popular support for their cause had been achieved, for example in the 100,000-signature petition, also turned their attentions to state authorities. The conflict for the dominant voice in the creation of laboratory culture now became one for control of centralized authority rather than popular representation. The Institute's leading scientists were more comfortable with the type of discourses required to conduct a campaign within the state executive. Lankester's familiarity with the machinations of an Oxford College or the workings of the Athenaeum Club, for example, were more appropriate training grounds for Establishment politics than they were for media manipulation.

Secrecy

The Institute began, therefore, to confront the image afforded it by the antivivisectionists by both gathering its own documentary evidence of public opinion to lay before the state authorities and conducting its own investigations into the antivivisection campaigners themselves. Building a portfolio of written materials was a direct counter to those other writings that had so damaged the reputation of institutionalized science. Indeed, the Institute's decision to oppose one type of narrative with another is a conscious attempt

to reclaim dominance through textual authority. Those narratives of resistance that the antivivisectionists disseminated through the media (the personal letter, the testimony of lay visitors to Pasteur's laboratory, the opinion column) were to be erased by new narratives with traditional objective authority (the official report, the investigative deputation, and the survey). Of course, for the scientific community this was not only a clever tactic but also the natural method of securing truth. Scientific methodology, after all, demanded an objectivity brought about by a reliance only on factual evidence and the writing up of proper experimental procedures.

The compilation of these new texts demanded security. For them to gain proper authority, there should be no opportunity for alternative discourses to oppose them. Therefore, the Institute closed its doors to visitors (both official and unofficial) and hired a series of private detectives and professional researchers to compile the necessary documents. The erection of a series of barricades around the laboratory site at Chelsea did not go unnoticed. Under the bold headline 'More Secrecy!' *The Star* detailed the ejection of a district council party from the land owned by the Institute: 'The councillors were met at the gate by the legal representative of the Institute, who stated that he had instructions to refuse them admission, as their inspection was considered both vexatious and unnecessary.'²¹ Where the professional scientists saw their laboratory as secure, the media represented it as secretive. The same evidence receives very different interpretation when entering the public arena, thereby proving to the scientists that their authority over scientific culture was under threat on territory not their own. Therefore, despite claims by antivivisectionists that the building of a laboratory on the Chelsea embankment would endanger the local community, any signs of increased security (which could be read as a positive reaction to such concerns) become signs of secrecy, denoting the immorality of the imagined experiments behind the closed doors of the Institute. Nevertheless, with the antivivisectionists so astute at manipulating events to reinforce their own position, the Institute was in no position to offer transparency and openness without the fear that this would also be turned against them.

To resort, therefore, to other forms of action more suited to the knowledge-base of the professional scientist, placed the scientific community on stronger foundations and gives scientists greater control over the dissemination of information about their own practices. The two most important documents produced by the Institute were intended for exactly that purpose: the reclamation of authoritative discourse and an intervention in the constructed culture of the laboratory. The first document was a survey of Chelsea residents conducted by A W Whalley in 1894. More a publicity drive than a survey, Whalley did not only note down the answers to his questions, he attempted

to persuade the residents of the benefits of the proposed laboratory. His final report to the Institute clearly betrays his scientific bias as well as the biases of his masculine middle-class ideology:

In most of the flats the residents whom I was able to see were of the female sex and bitterly opposed to the Institute. I only found two who spoke sensibly on the matter. About a dozen were neutral and treated the matter in a commonplace light. These were principally married people.²²

As is clear, those who were not in favour of the Institute lack sensibility. Whalley implicitly suggests that this is more closely related to their gender and marital status than to any particular knowledge of science. To be both a woman and unmarried is to be almost instinctively against the building of the Institute. For Whalley, all the marginal positions of society are lined up together: distrust of science, single, female. That the antivivisection groups were often perceived as comprising exactly these demographics, hints at the root of the prejudice Whalley is happy to condone.

Authority

There is the beginning here of a strategy to move the boundaries of the debate beyond the vivisection controversy to more general lines of cultural authority. Who is speaking becomes more important than what is said. This redefinition of the terms of the conflict becomes clearer in the second document produced by the Institute. In late April 1894, a private detective was employed by the Institute's secretary to report on a forthcoming protest march against the Chelsea laboratory site.²³ While this document performs the primary role of authoritative written narrative for the consumption of various state bodies, its content betrays an inclination to discredit the Institute's critics as working-class agitators of dubious moral character.

Organized by various Labour and Anti-Vivisection Leagues the parade constituted a march through Chelsea (past the site of the Institute) to a meeting point, where several speeches and collections would take place. The unnamed investigator and author of the report, acting as an unaffiliated fellow marcher, detailed first the disorganization of the events of the day. Opening his narrative by commenting on the tardiness of the protestors, 'though announced for one O'clock, it was 2 pm before the assembly was sounded', the first pages of his report continually highlighted the unprofessional nature of the protest. Pamphlets were distributed 'indiscriminately', the marchers were 'mostly children' and the largest banner 'fell down soon

after starting'. In discussion with some of those present, the investigator discovered that 'not everyone of course was interested' in the materials being distributed. One man, who 'had about four quires of pamphlets' was 'taking them home to put in the closet' rather than disseminating them among the public.²⁴

The protest march was characterized as haphazard and chaotic, those attending as largely uninterested or ignorant, and the organizers viewed as self-indulgent amateurs with no sense of organization. The report implicitly linked event coordination with credible belief. The antivivisectionists' inability to form a proper and professional protest acts against their claims of vivisection's immorality. A lack of planning and structure undermines the intellectual and ideological foundations of their protest. For the Institute to shift the debate away from the 'facts' of their experimental practice to the authority and credibility of the protestors marks a significant change, but it does parallel the personalizing of the culture of science that the protestors themselves had begun in the mass media. There they had damned Lister and other Institute scientists as often as they had vivisection itself. Now, the Institute was about to mimic those actions by discrediting the antivivisectionists. However, one significant difference remains; while the protesters had charged individual scientists with a lack of morals, the Institute was attempting to eradicate the protesters' narrative authority. Rather than hint at an ambivalent approach to human morality, the Institute was determined to make the protestors voiceless.

Having begun this process by illuminating the chaos of the march and thereby the 'thinking' of the antivivisectionists, the report next attacked the class affiliations of those involved, moving from criticism based upon what the protestors represented to their personal character. One of the key speakers, Miss Jessie Craigen, was described as 'a stout elderly lady of dark complexion with a stubby beard and a strong moustache, who was sucking oranges, the greater part of the journey.' The march went predominantly through streets, 'where every flat contained a family' and, where 'the public house ... did a roaring trade'. Overall, the crowd looked 'dirty and disreputable' and the majority left the procession 'just in time for the opening of the public house'. The narrative contrived to make a spectacle of the protest as agitation performed by, and for, the working classes within their own territory. Marginalized in this way, the investigator was able to dismiss the march as a localized event that is little more than preliminary entertainment to the usual practice of alcohol consumption. The ignorance of the marchers was reinforced when they passed the site of the Institute 'without any signs of recognition' before adjourning to the Gun Tavern 'for refreshment'.²⁵ The physical degeneration of Jessie Craigen and her fellow protestors was

connected first to their class origins and, ultimately, to their mental capacity. The entire protest was represented as a middle-class vision of the working class equivalent of the Sunday promenade. All notion of a committed and vital protest movement is lost.

Voices in Conflict

For the Institute and its professional scientists such documentary evidence of the protestor's 'real' agenda would only have served to buttress their belief in their own authority. The antivivisectionists were viewed not as a combative group raising their voices in reasonable objection to laboratory science, but a piecemeal rabble of ignorant agitators bent on subversion. To the middle-class professional scientists of the Institute, Lister's decision to maintain dialogue with the state while ignoring popular protest seemed vindicated. Did such internal decisions allow the scientific community to dominate representations of institutional science? Certainly the Institute's committee appeared confident in gaining state support. The rhetoric of their deputation to government ministers is in direct contrast to the tentative opposition made by the representatives of the Chelsea residents and antivivisectionists. While they quietly appeal for consideration of their views, the Institute's members forcefully demand the legal support of the state.

The minutes of a meeting between the Institute and the Board of Trade in June 1894 (two months after the compilation of the report on the protest march) reveals how authoritative the Institute believed itself to be. Sir Henry Roscoe begins by noting that 'the deputation ... is one of exceptional importance [that] represents the whole body of medical opinion in this country.'²⁶ Even more pertinently, Sir John Lubbock concludes with a satirical swipe at the antivivisectionists that betrays a self-assurance in the Institute's domination of the debates: 'Vivisection has no doubt been referred to today but it is after all but a very small part of the work before us, unless indeed vivisection is to be understood as applying to the bacteria (laughter); but I would venture to remind you that although Acts of Parliament may prevent us from destroying the bacteria, they cannot prevent the bacteria from destroying us (laughter).'²⁷ Lubbock's humorous jibe at the antivivisectionists hinted at the connection he perceived between their discourse and the bacteria the Institute was determined to eradicate. Faced with opposition, the Institute tried to put into practice its normal scientific procedures by inoculating itself against an opposing force through the production of a discourse that negated that opposition in the corridors of government. However successful a tactic, it led to a polarization of the conflict over laboratory science into state versus public. Despite winning the battle for governmental

approval, the Institute would never hold the same position outside Whitehall. For a broad cross-section of the public, as well as the popular media, the scientific laboratory was still viewed as the antivivisection lobby wished: an immoral institution of a powerful and invisible science uninterested in lay opinion. Ultimately sidelined by the state, protesters and activists would continue to confront the scientific laboratory from the margins, while the scientific community turned inwards and satisfied itself with the central authorities of government. In taking up such positions, laboratory science and the public were beginning a period of entrenchment that has now lasted through an entire century and is beginning on the next.

Notes

Chapter 1 Margins and Centres

Alex Warwick

- 1 Prince Albert, 'Presidential Address to the BAAS' in George Basalla, William Coleman and Robert Kargon (eds), *Victorian Science* (New York, Anchor Books, 1970), p. 51.
- 2 W.V. Harcourt, 'Inaugural Address to BAAS' in Basalla et al. (eds), *Victorian Science*, p. 32.
- 3 Prince Albert, 'Presidential Address', pp. 51–2.
- 4 Susan Cannon, *Science in Culture: The Early Victorian Period* (New York, Science History Publishers, 1978), p. 167.
- 5 See for example Seymour H. Mauskopf, 'Marginal Science' in R.C. Olby et al., *Companion to the History of Science* (London, Routledge, 1990), pp. 869–85.
- 6 'Introductory Remarks' in M.P. Hanon, M. Osler and R.G. Weyant (eds), *Science, Pseudo-science and Society* (Ontario, Wilfrid Laurier University Press, 1980) p. 4.
- 7 Roger Cooter, *The Cultural Meaning of Popular Science*, (Cambridge, Cambridge University Press, 1984), p. 19.
- 8 Alison Winter, 'The Construction of Orthodoxies and Heterodoxies in the Early Victorian Life Sciences' in Bernard Lightman (ed.), *Victorian Science in Context* (Chicago, University of Chicago Press, 1997), p. 26.
- 9 Robert Young, *Darwin's Metaphor: Nature's Place in Victorian Culture* (Cambridge, Cambridge University Press, 1985), p. 125.
- 10 William Whewell, *The Philosophy of the Inductive Sciences* (London, Parker, 1840), vol. 1, p. 544.
- 11 Richard Yeo, *Defining Science: William Whewell, Natural Knowledge, and Public Debate in Early Victorian Britain* (Cambridge, Cambridge University Press, 1993), p. 5.
- 12 L. Huxley (ed.), *Life and Letters of Thomas Henry Huxley*, 2 vols (London, Macmillan, 1900) vol. 1, p. 100.
- 13 J.B. Morrell and A.W. Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (Oxford, Clarendon Press, 1981)
- 14 Cooter, *Cultural Meaning*, p. 91.
- 15 Adrian Desmond and James Moore, *Darwin* (London, Penguin, 1991), pp. 469–70.

- 16 See Roy M. MacLeod, 'The X Club: A Social Network in Late Victorian England', *Notes and Records of the Royal Society of London* (1970), reprinted in Roy M. MacLeod, *The 'Creed of Science' in Victorian England* (Aldershot, Ashgate, 2000), p. 310.
- 17 See for example Anne Secord, 'Corresponding Interests: Artisans and Gentlemen in Nineteenth-Century Natural History', *British Journal for the History of Science*, 27 (1994), pp. 383–408.
- 18 Yeo, *Defining Science*, p. 33.
- 19 Winter, 'Construction', p. 44.

Chapter 2 'Speakers Concerning the Earth': Ruskin's Geology After 1860

Caroline Trowbridge

- 1 See the *Wellesley Index to Victorian Periodicals* where the number of articles on geology drops after 1859.
- 2 *Geological Magazine*, 1 (1864), p. 175.
- 3 The one exception is W.G. Collingwood who defended the legitimacy of Ruskin's geology in *The Life and Work of John Ruskin* (London, Methuen, 1893), pp. 27–34.
- 4 See Robert Hewison, *John Ruskin: The Argument of the Eye* (London, Thames and Hudson, 1976), p. 176; also John D. Rosenberg who calls Ruskin's scientific experiments 'not science but play' in *The Darkening Glass* (London, Routledge, 1963), p. 180.
- 5 See Robert Hewison (ed.), *New Approaches to Ruskin* (London, Routledge, 1981); Dinah Birch (ed.), *Ruskin and the Dawn of the Modern* (Oxford, Oxford University Press, 1999); Francis O'Gorman, *Late Ruskin: New Contexts* (Aldershot, Ashgate, 2001); Michael Wheeler (ed.), *Time and Tide: Ruskin and Science* (Northamptonshire, Pilkington, 1996).
- 6 See O'Gorman, *Late Ruskin*, pp. 28, 73; Sharon Aronofsky Weltman, 'Myth and Gender in Ruskin's Science' in Birch (ed.), *Ruskin and the Dawn of the Modern*, p. 154.
- 7 *Quarterly Journal of the Geological Society of London*, 56 (1900), p. lx.
- 8 *The Geological Magazine*, 7 (1900), p. 95.
- 9 *The Geological Magazine* 7 (1900), p. 95.
- 10 E.T. Cook and Alexander Wedderburn (eds), *The Works of John Ruskin* (Library Edition), 39 vols. (London, George Allen, 1903–1912), vol. 26, p. 42. Citations from this edition hereafter will appear in the text by volume and page number alone.
- 11 Alison Winter, 'The Construction of Orthodoxies and Heterodoxies in the Early Victorian Life Sciences' in Bernard Lightman (ed.), *Victorian Science in Context* (London, University of Chicago Press, 1997), pp. 24–50.
- 12 Tim Hilton, *John Ruskin: The Later Years* (New Haven, Yale University Press, 2000), p. 362.
- 13 See O'Gorman, *Late Ruskin*, pp. 74–80.
- 14 O' Gorman, *Late Ruskin*, pp. 221–2.
- 15 Arabella Buckley, *The Fairy-Land of Science* (Stanford, n.p., 1879), p. 119.

- 16 Greg Myers also notes that the second printing sold 31,000 copies before 1905. See Myers, 'Science for Women and Children: The Dialogue of Popular Science in the Nineteenth Century' in John Christie and Sally Shuttleworth (eds), *Nature Transfigured: Science and Literature, 1700–1900* (Manchester, Manchester University Press, 1989), pp. 188–9.
- 17 Margaret Gatty, for instance, was a populariser whose *Parables from Nature* (1855) went through 18 editions by 1882. Each chapter of Gatty's work used interactions in an anthropomorphic but scientifically accurate animal world to demonstrate Christian values. See Alan Rauch, cited in Bernard Lightman, "'The Voices of Nature': Popularizing Victorian Science' in Lightman (ed.) *Victorian Science in Context*, p. 192.
- 18 Frank Turner, *Between Science and Religion* (New Haven, Yale University Press, 1974), p. 24.
- 19 Turner, *Between Science*, p. 11.
- 20 Lightman, "'The Voices of Nature'", p. 192.
- 21 Henry Larkin, 'Extra Physics, and The Mystery of Creation' (London, Hodder and Stoughton, 1878), p. 24. See also Rev. Robert E. Hooppell, 'Materialism: Has it Any Real Foundation in Science?' (London, Rivingtons, 1874); James Hutchison Stirling, 'Materialism in relation to the study of Medicine: An Address to medical students' (Edinburgh and London, n.p., 1868); William Thomson (Lord Archbishop of York), 'Materialistic Theories, a Lecture' reprinted in C.J. Ellicott (ed.), *The Christian Evidence Society and its Lectures* (London, Hodder and Stoughton, 1871).
- 22 See Dinah Birch, 'The Ethics of the Dust: Ruskin's Authorities', *Prose Studies: History, Theory, Criticism*, 12:2 (September 1989), p. 147.

Chapter 3 Swimming at the Edges of Scientific Respectability: Sea Serpents in the Victorian Era

Sherrie Lyons

- 1 C. Lyell, *A Second Visit to the United States of North America*, 2 vols (London, John Murray, 1847), vol. 1, p. 140.
- 2 Jules Verne, *The Complete Twenty Thousand Leagues Under the Sea*, trans. Emanuel J. Mickel (Bloomington, Indiana University Press, 1969), p. 1.
- 3 Martin Rudwick, 'Uniformity and Progression: Reflections in the Structure of Geological Theory in the Age of Lyell' in D. Roller (ed.), *Perspectives in History of Science and Technology* (Norman, University of Oklahoma Press, 1971), p. 214; Peter Bowler, *Fossils and Progress: Paleontology and the Idea of Progressive Development in the Nineteenth Century* (New York, Science History Publications, 1976).
- 4 A great deal of secondary literature on Uniformitarianism exists. A few relevant citations are: Peter Bowler, *Evolution* (Berkeley, University of California Press, 1984); Walter Cannon, 'The Uniformitarian-Catastrophist Debate', *Isis*, 51 (1960), pp. 38–55; Charles Gillispie, *Genesis and Geology: A Study in the Relations of Scientific Thought, Natural Theology and Social Opinions in Great Britain, 1790–1850* (New York, Harper, 1951, reprint 1959); Stephen Gould, 'The Eternal Metaphors

- of Paleontology' in Anthony Hallam (ed.), *Patterns of Evolution* (Amsterdam, Elsevier, 1977), pp. 1–26; S. Gould, 'Agassiz's Marginalia in Lyell's *Principles* or the Perils of Uniformity and the Ambiguity of Heroes', *Studies in the History of Biology*, 3 (1979), pp. 119–138; R. Hooykaas, *Natural Law and Divine Miracle; the Principle of Uniformity, Biology, and History* (Leiden, Brill, 1959); M. Rudwick, 'Uniformity and Progression'.
- 5 James Hutton, *Theory of the Earth*, *Transactions of the Royal Society of Edinburgh*, 1 (1788).
 - 6 Archives, Boston Museum of Science.
 - 7 A large fish related to the cod.
 - 8 'The Sea Serpent', *Niagara Patriot*, 1:18 (18 August 1818), p. 2.
 - 9 'The Serpent Not Taken', *Niagara Patriot*, 1:23 (22 September 1818), p. 2.
 - 10 'The Sea Serpent', *Buffalo Patriot*, 5:225 (6 August 1822), p. 3, reproduced from the *Boston Gazette*.
 - 11 'J. Dawson to C. Lyell, 20 September 1845', *Charles Lyell Scientific Correspondence*, Edinburgh University Library, Box 26.
 - 12 Lyell, *A Second Visit*, vol. 1, p. 131.
 - 13 The group of marine mammals that includes whales, dolphins and porpoises.
 - 14 From *The Illustrated London News*, (28 October 1848), *Charles Lyell Scientific Correspondence*, Edinburgh University Library, Box 26.
 - 15 Nicolaas Rupke, *Richard Owen, Victorian Naturalist* (New Haven, Yale University Press, 1994), pp. 324–332.
 - 16 R. Owen, 'The Great Sea Serpent', *The Annals and Magazine of Natural History* (1848), pp. 458–463, p. 463.
 - 17 A "mythological" giant squid like creature whose detailed description appeared in Erik Ludvigsen Pontoppidan's (the Bishop of Bergen) *The Natural History of Norway* (n.p., 1755).
 - 18 'C. Lyell to G. Mantell, 20 February 1830', in K. Lyell (ed.), *Life and Letters of C. Lyell*, 2 vols (London, John Murray, 1881), vol. 1, p. 262.
 - 19 Lyell, *A Second Visit*, vol. 1, p. 141.
 - 20 Harriet Ritvo, 'Professional Scientists and Amateur Mermaids: Beating the Bounds in Nineteenth-Century Britain', *Victorian Literature and Culture*, 19 (1991), pp. 277–289.
 - 21 Carolyn Kirdahy, 3 June 1993, archivist, Boston Museum of Science, personal communication.
 - 22 Eugene Batachelder (ed.), *A Romance of the Sea Serpent or the Ichthyosaurus and a Collection of Ancient and Modern Authorities* (Cambridge, John Bartlett, 1849), p. 117.
 - 23 G.M. Eberhart, *Monsters* (New York, Garland 1983).
 - 24 W. J. Hooker, quoted in Bernard Heuvelmans, *In The Wake of Sea Serpents*, trans. Richard Garnet (New York, Hill and Wang, 1968), p. 24.
 - 25 Batachelder (ed.), *A Romance of the Sea Serpent*, pp. 135–136.
 - 26 Heuvelmans, *In the Wake*, p. 25.
 - 27 Jack Morrell and Arnold Thackray, *Gentlemen of Science* (Oxford, Clarendon Press, 1981), p. 32.
 - 28 Morrell and Thackray, *Gentlemen of Science*, p. 270.
 - 29 Edward Newman, 'The Great Sea Serpent', *Westminster Review*, 50 (1849), pp. 491–515, p. 492.

- 30 Ron Westrum, 'Knowledge About Sea Serpents', in Roy Wallis (ed.), *Sociological Review Monograph 27* (Keele, University of Keele, 1979), pp. 298–300.
- 31 Newman, 'The Great Sea Serpent', p. 491.
- 32 Ritvo, 'Professional Scientists', p. 287.
- 33 Heuvelmans, *In the Wake*, p. 182.
- 34 Peter Medawar, *The Art of the Soluble* (London, Methuen, 1967), title page.

Chapter 4 The Drugs, the Blister and the Lancet are all laid aside. Hydropathy and Medical Orthodoxy in Scotland, 1840–1900

Alastair Durie

- 1 See Robin Price, 'Hydropathy in England, 1840–1870', *Medical History*, 25 (1981), pp. 269–80; Rees, Kevin, 'Water as commodity: hydropathy in Matlock' in Roger Cooter (ed.), *Studies in the History of Alternative Medicine* (Basingstoke, Macmillan, 1988), pp. 27–44.
- 2 William Chambers, 'Hydropathic establishments' in *Chamber's Journal of Popular Literature, Science and Arts* (1878), p. 562.
- 3 Jane B. Donegan, *Hydropathic Highway to Health: Women and Water-Cure in Antebellum America* (Connecticut, Greenwood Press, 1986), pp. 170–77.
- 4 *The Lancet*, 14 November 1846, p. 537.
- 5 *The Lancet*, 12 March 1842, p. 830.
- 6 William Meikle, MD Thesis, *On Some of the Actions and Uses of Water as a Therapeutic Agent*, (1857), p. 43. University of Edinburgh Archives.
- 7 James Manby Gully, *The Water Cure in Chronic Disease*, (London, J. Churchill 1847), pp. 689–690.
- 8 See, for example, in praise of Dr Macleod, and Ben Rhydding, *A Letter by a Liverpool Merchant to his Brother Merchants and to all who are labouring under the Great English Malady. The Wear and Tear of the System, or an Overwrought Brain* (London, Charles Gilpin, 1851).
- 9 *G[lasgow] M[edical] J[ournal]*, series 3, vol. 5, 1857, p. 96.
- 10 P.S. Brown, 'Social context and medical theory in the demarcation of nineteenth century boundaries', in W.F. Bynum, and Roy Porter (eds), *Medical Fringe and Medical Orthodoxy 1750–1850* (London, Croom Helm, 1987), p. 224.
- 11 *The Medical Mirror*, 1 October 1883.
- 12 Review of *The Sophistry of Empiricism*, 1853, *GMJ Series 3*, vol. 1, p. 234.
- 13 On the general background, see Phillip A. Nicholls, *Homeopathy and the Medical Profession* (London, Croom Helm, 1988).
- 14 For example, *Confessions of an Homoeopathist* (Dublin, S.B. Oldham, 1846).
- 15 See E.S. Turner, *Taking the Cure* (London, Michael Joseph, 1967), pp. 194–96.
- 16 Rowland East, *The Principles and Practice of the Water Cure Popularly Expounded* (London, Glasgow and Edinburgh, George Davidson & Co., 1850), p. 1.
- 17 Rowland East, *Results of the Water Cure* (Glasgow, Gallie, 1848), p. 11.
- 18 J.S. Blackie, *The Water Cure in Scotland. Five Letters from Dunoon* (Aberdeen, George Davidson, 1849) p. 42.

- 19 John Stuart Blackie, *Notes of a Life*, A. Stoddart Walker (ed.) (Edinburgh, William Blackwood, 1900), pp. 149–50.
- 20 J.F. Paisley, *A Proposal for the Settlement of the Homeopathic Controversy* (Edinburgh, n.p., 1852).
- 21 Correspondence and comments, 1859, *The Aberdeen Water Cure Journal*, vol. 1, pp. 66–67.
- 22 Letter to Dr Macleod, 26 October 1847, *The Cold Water Journal*, vol. 1, p. 188.
- 23 John Balbirnie, *Hydropathic Aphorisms. The Simple Treatment of Disease Contrasted with Medical Abuses* (London, Darton, 1856), p. 107.
- 24 'A new cure for intemperance', 21 June 1879, *The Christian News*, p. 12.
- 25 'Introductory address', 1848, *The Journal of Health*, vol. 1, pp. 1–3.
- 26 'Incidental matters', 1849, *The Journal of Health*, vol. 2, p. 99.
- 27 Harry B. Weiss, and Howard R. Kemble, *The Great American Water Cure Craze. A History of Hydropathy in the United States* (New Jersey, The Past Times Press, 1967), pp. 35–7. Candidates had to deposit a thesis, but what form Munro's submission took is not known; perhaps it was an early tract of his.
- 28 Queriest, 27 June 1861, *The Scotsman*, p. 6.
- 29 A. Hunter, *Health, Happiness and Longevity* (Glasgow J. Menzies & Co., 1885), Preface, p. 6.
- 30 Mrs Hunter is described in R. Metcalfe, *The Rise and Progress of Hydropathy in England and Scotland*, (London, Simpkin, Marshall, Hamilton, Kent, 1912), pp. 171–90.
- 31 A. Hunter, *Health, Happiness and Longevity*, pp. 12–13.
- 32 *Chamber's Journal*, 7 September 1878, p. 561.
- 33 A.R. Hope Moncrieff (ed.), *Where Shall We Go? A Guide to the Watering Places of Health Resorts of England, Scotland, Ireland and Wales* (London and Edinburgh, Adam & Charles Black, 1892), p. xxii.
- 34 Rev. Alex Stewart, *Evolution in Relation to Geology, Rudimentary Structures, Design and Christianity* (London and Glasgow, Smart and Allen, 1875). His other publications included *The Physiology of Hydropathy* (Manchester, J. Heywood, 1884), and *Hints on Health to the Overworked* (Manchester, J. Heywood, 1885).
- 35 *Aberdeen Journal*, 1 June 1900, p. 8.
- 36 'The cult of the hydropathic', 28 December 1907, *Glasgow Herald*, p. 9.

Chapter 5 Anna Kingsford: Scientist and Sorceress

Alison Butler

- 1 William Wynn Westcott, 'Man, Miracle, Magic' reprinted in R.A. Gilbert (ed.), *The Magical Mason: Forgotten Hermetic Writings of William Wynn Westcott, Physician and Magus* (Wellingborough, Aquarian Press, 1983), pp. 66–70; p. 67. Read to Golden Dawn members at an unrecorded date.
- 2 Antoine Faivre, 'Introduction' in Antoine Faivre and Jacob Needleman (eds), *Modern Esoteric Spirituality* (New York, Crossroad, 1992), p. xii; and Antoine Faivre, *Access to Western Esotericism* (Albany, State University of New York Press, 1994), pp. 4–6.

- 3 See Peter J. Bowler, *Reconciling Science and Religion* (Chicago, University of Chicago Press, 2001) for an examination of the resurgence of support for religious values in Victorian England in the face of the extreme philosophy of scientific naturalism. Also see Frank Miller Turner, *Between Science and Religion* (New Haven, Yale University Press, 1974) for a detailed study of some of the individuals who opposed the intolerance and dogmatism of scientific naturalism.
- 4 T.G. Ashplant and Adrian Wilson, 'Present-Centred History and the Problem of Historical Knowledge', *Historical Journal*, 31:2 (1988), pp. 253–74; p. 253, p. 267.
- 5 Ashplant and Wilson, 'Present-Centred History', p. 258.
- 6 English translation published in 1881. Anna Kingsford, *The Perfect Way in Diet. A Treatise advocating a return to the natural and ancient food of our race* (London, Kegan Paul, Trench & Co., 1881).
- 7 Anna Kingsford and Edward Maitland, *The Perfect Way, or, The Finding of Christ* (London, n.p., 1882; revised edition 1887; repr. Montana, Kessinger Publishing, 1997), p. 50.
- 8 Kingsford and Maitland, *The Perfect Way*, p. 200.
- 9 Anna Kingsford, *How the World Came to an End in 1881* (London, Field & Tuer, 1884), p. 9.
- 10 Mrs Algernon Kingsford, *Violationism: or sorcery in science* (Bath, n.p., 1887), p. 2.
- 11 Kingsford, *Violationism*, p. 4.
- 12 Anna Kingsford, *Dreams and Dream Stories*, ed. Edward Maitland (London, George Redway, 1886).
- 13 Edward Maitland, *Anna Kingsford: Her Life, Letters, Diary and Work*, 2 vols (London, George Redway, 1896).
- 14 See Alex Owen, *The Darkened Room: Women, Power and Spiritualism in Late Victorian England* (London, Virago Press, 1989) for the definitive study of Victorian mediumship as protest and dissent.
- 15 Kingsford, *Dreams and Dream Stories*, p. 7.
- 16 Kingsford was not alone in her passionate opposition to vivisection. Other articles on the subject were being written around the same time by other female writers, including another graduate from medical school in Paris, Dr Elizabeth Blackwell, *Erroneous Method in Medical Education* (1891), and Mary J. Hall-Williams, MD, 'From the Human and Scientific Standpoint' (1895).
- 17 Kingsford, *Violationism*, p. 1.
- 18 Kingsford, *Violationism*, p. 1.
- 19 Anna Kingsford, *Pasteur: His Method and its Results* (Hampstead, n.p., 1886), p. 28.
- 20 Maitland, *Anna Kingsford*, p. 182.
- 21 For a full account of this incident see Maitland, *Anna Kingsford*, pp. 182–248.
- 22 Maitland, *Anna Kingsford*, vol. 2, p. 259.
- 23 Maitland, *Anna Kingsford*, vol. 2, p. 261.
- 24 Maitland, *Anna Kingsford*, vol. 2, p. 246.
- 25 Ellic Howe, *The Magicians of the Golden Dawn* (London, Routledge & Kegan Paul, 1972; repr. York Beach, ME, Samuel Weiser, 1978), p. 40.
- 26 Maitland, *Anna Kingsford*, vol. 2, p. 246.
- 27 Maitland, *Anna Kingsford*, vol. 2, p. 268.
- 28 Maitland, *Anna Kingsford*, vol. 2, p. 247.
- 29 Diary entry from Kingsford dated 14 April 1887, reproduced in Maitland, *Anna Kingsford*, vol. 2, p. 292.

- 30 Stephan Ronan (ed.), Iamblichus, *De mysteriis Aegyptiorum – On the Mysteries*, trans. Thomas Taylor and Alexander Wilder (Hastings, Chthonios Books, 1989), vol. 3, ch. 6, pp. 112–14; vol. 14, ch. 133, pp. 3–8.
- 31 Owen, *The Darkened Room*, p. 6.
- 32 S.L. MacGregor Mathers, *The Kabbalah Unveiled* (London, George Redway, 1887).
- 33 See Bowler, *Reconciling Science*.

Chapter 6 A Science for One or a Science for All? Physiognomy, Self-Help, and the Practical Benefits of Science

Lucy Hartley

An earlier version of part of this essay appeared in Chapter 1 of my book, *Physiognomy and the Meaning of Expression in Nineteenth-Century Culture* (Cambridge: Cambridge University Press, 2001); reprinted with permission.

- 1 For the importance of self-regulation of the individual, see Roger Smith, *Inhibition: History and Meaning in the Sciences of Mind and Brain* (Berkeley, University of California Press, 1992). State regulation of the individual was inevitable, operating according to a strict opposition of rational against irrational, the latter being the jurisdiction of the mental institution. Foucault has written at length on the ramifications of this opposition, especially in *Madness and Civilization: A History of Insanity in the Age of Reason*, trans. Richard Howard (London, Routledge, 1967); see also Andrew Scull, *Museums of Madness: The Social Organization of Insanity in Nineteenth-Century England* (Harmondsworth, Penguin, 1979).
- 2 The best studies of phrenology are Roger Cooter, *The Cultural Meaning of Popular Science: Phrenology and the Organisation of Consent in Nineteenth-Century Britain* (Cambridge, Cambridge University Press, 1985), and David de Giustino, *Conquest of the Mind: Phrenology and Victorian Social Thought* (London, Croom Helm, 1975); the most notable studies of physiognomy are John Graham, *Lavater's 'Essays on Physiognomy': A Study in the History of Ideas* (Berne, Peter Lang, 1979), Christopher Rivers, *Face Value: Physiognomical Thought and the Legible Body in Marivaux, Lavater, Balzac, Gautier, and Zola* (Wisconsin, University of Wisconsin Press, 1994), and also Graeme Tytler, *Physiognomy in the European Novel: Faces and Fortunes* (Princeton, Princeton University Press, 1982).
- 3 Contemporary debates about the purpose of science are described in Richard Yeo's *Defining Science: William Whewell, Natural Knowledge, and Public Debate in Early Victorian Britain* (Cambridge, Cambridge University Press, 1993).
- 4 See Karl Figlio, 'Theories of Perception and the Physiology of Mind in the Late Eighteenth Century', *History of Science*, 12 (1975), pp. 177–212; L.S. Jacyna, 'Immanence or Transcendence: Theories of Life and Organization in Britain, 1790–1835', *Isis*, 74 (1983), pp. 311–29; and Roger Smith, 'The Background of Physiological Psychology in Natural Philosophy', *History of Science*, xi (1973), pp. 75–123.
- 5 The prefix 'pseudo' is derived from the Greek *pseudes* meaning false: it suggests a lack of genuineness, something purporting to be what it is not; hence the phrase 'pseudo-science' implies a counterfeit or pretended science.

- 6 Alison Winter, 'The Construction of Orthodoxies and Heterodoxies in the Early Victorian Life Sciences' in Bernard Lightman (ed.), *Victorian Science in Context* (Chicago, University of Chicago Press, 1997), pp. 24–50; p. 25.
- 7 The most relevant of the increasing number of studies of medicine and the human sciences are Adrian Desmond, *The Politics of Evolution: Morphology, Medicine, and Reform in Radical London* (Chicago, University of Chicago Press, 1989); Christopher Fox, Roy Porter, and Robert Wokler (eds), *Inventing Human Science: Eighteenth-Century Domains* (London, Routledge, 1995); and Rick Ryland, *Victorian Psychology and British Culture 1850–1880* (Oxford, Oxford University Press, 2000). An excellent collection of primary materials can be found in Jenny Bourne Taylor and Sally Shuttleworth (eds), *Embodied Selves: An Anthology of Psychological Texts, 1830–1890* (Oxford, Clarendon Press, 1998).
- 8 For a comprehensive account of the classical tradition of physiognomy, see Elizabeth C. Evans, 'Physiognomics in the Ancient World', *Transactions of the American Philosophical Society*, 59 (1969), pp. 5–97.
- 9 The critical reception of physiognomy in the nineteenth century is discussed by Ellis Shookman, 'Pseudo-Science, Social Fad, Literary Wonder: Johann Caspar Lavater and the Art of Physiognomy', and by Judith Weschler, 'Lavater, Stereotype, and Prejudice', in Ellis Shookman (ed.), *The Faces of Physiognomy: Interdisciplinary Approaches to Johann Caspar Lavater* (Columbia, Camden House, 1993), pp. 1–24 and pp. 104–25 respectively.
- 10 Mary Cowling, *The Artist as Anthropologist: The Representation of Type and Character in Victorian Art* (Cambridge, Cambridge University Press, 1989), p. xix. See note 2 for details of these studies.
- 11 For discussion of the eighteenth-century intellectual context for Lavater's famous work, see Maria Allentuck, 'Fuseli and Lavater: Physiognomical Theory and the Enlightenment', *Studies in Voltaire and the Eighteenth Century*, 15 (1967), pp. 89–112; and Fanny Price, 'Imagining Faces: The Later Eighteenth-Century Sentimental Heroine and the Legible, Universal Language of Physiognomy', *British Journal of Eighteenth-Century Studies*, 6 (1983), pp. 1–16.
- 12 Further details of this publication history can be found in John Graham's article, 'Lavater's Physiognomy in England', *Journal of the History of Ideas*, 22 (1961), pp. 561–72.
- 13 *The Encyclopaedia Britannica*, 4th edn, 20 vols (Edinburgh, Archibald Constable & Co., 1810), XVI, p. 439.
- 14 Johann Caspar Lavater, *Essays on Physiognomy; for the Promotion of the Knowledge and Love of Mankind; Written in the German Language by J.C. Lavater, and abridged from Mr. Holcroft's Translation* (London, G.G.J. & J. Robinson, n.d.), vol. 1, p. 10.
- 15 Lavater, *Essays on Physiognomy*, vol. 1, pp. 16–17.
- 16 Lavater, *Essays on Physiognomy*, vol. 1, pp. 10–11.
- 17 Daniel Pick's *Faces of Degeneration: A European Disorder, c. 1848–1918* (Cambridge, Cambridge University Press, 1989) remains one of the best works on this subject.
- 18 The 'classics' on this subject are John W. Burrow, *Evolution and Society: A Study in Victorian Social Theory* (Cambridge, Cambridge University Press, 1966); William Coleman, *Biology in the Nineteenth Century: Problems of Form, Function, and Transformation* (New York, John Wiley, 1972); and Charles C. Gillispie, *Genesis and Geology: A Study in the Relations of Scientific Thought, Natural Theology, and*

Social Opinion in Great Britain, 1790–1850 (Cambridge, MA, Harvard University Press, 1951).

- 19 Lavater, *Essays on Physiognomy*, vol. 1, p. 17.
- 20 See my recent book, *Physiognomy and the Meaning of Expression in Nineteenth-Century Culture* (Cambridge, Cambridge University Press, 2001).
- 21 The examples are Lavater's own, taken from studies 'Of the Forehead', 'Of the Nose', and 'Of the Mouth and Lips' in *Essays on Physiognomy*, vol. 1, pp. 51, 61 and 64 respectively.
- 22 Anon., 'The Projector, No. XC', *The Gentleman's Magazine*, LXXVIII (1808), p. 1085. Compare with an earlier issue on the same theme in *The Gentleman's Magazine*, LXXI (1801), p. 124.
- 23 Lewis Wolpert, *The Unnatural Nature of Science* (London, Faber, 1992), p. 17.

Chapter 7 'Supposed Differences': Lydia Becker and Victorian Women's Participation in the BAAS

Susan David Bernstein

My appreciation to Rebecca Stott and Barbara T. Gates for their comments on this essay.

- 1 *Belgravia*, 7 (February 1869), p. 61.
- 2 *Belgravia*, p. 61.
- 3 *Belgravia*, p. 62.
- 4 Helen Blackburn, *Women's Suffrage: A Record of the Women's Suffrage Movement in the British Isles with Biographical Sketches of Miss Becker* (London, Williams & Norgate, 1902), p. 38.
- 5 Patricia Phillips, *The Scientific Lady: A Social History of Women's Scientific Interests, 1520–1918* (London, Palgrave Macmillan, 1991), pp. 206–7. Phillips cites several landmarks in the history of women's inclusion in the BAAS: in 1853, a 'Miss Bowlby' was the first woman admitted to full membership; in 1913, the botanist Ethel Sargent was the first woman president of a section; and in 1914, E. R. Saunders of Newnham College was one of the first female members of the Council.
- 6 Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (London, Royal Historical Society, 1984), p. 149.
- 7 Lydia Ernestine Becker, 'On the Study of Science by Women', *Contemporary Review*, 10 (January–April 1869), pp. 386–404; p. 393.
- 8 Morrell and Thackray, *Gentlemen of Science: Early Years*, pp. 150–5.
- 9 W.H. Brock, 'Advancing Science: The British Association and the Professional Practice of Science' in Roy MacLeod and Peter Collins (eds), *The Parliament of Science: The British Association for the Advancement of Science, 1831–1981* (Middlesex, Science Reviews Ltd, 1981), pp. 89–117; p. 95.
- 10 Brock, 'Advancing Science', p. 95.
- 11 Morrell and Thackray, *Gentlemen of Science: Early Years*, p. 150; original emphasis.
- 12 Kathryn A. Neeley, *Mary Somerville: Science, Illumination, and the Female Mind* (Cambridge, Cambridge University Press, 2001), pp. 203–5.

- 13 Neeley, *Mary Somerville*, pp. 203–5.
- 14 Morrell and Thackray, *Gentlemen of Science: Early Years*, p. 155.
- 15 Jack Morrell and Arnold Thackray (eds), *Gentlemen of Science: Early Correspondence of the British Association for the Advancement of Science* (London, Royal Historical Society, 1984), p. 301.
- 16 Joan E. Parker, 'Lydia Becker's "School for Science": a challenge to domesticity', *Women's History Review*, 10 (2001), pp. 629–50; p. 637.
- 17 'The Eleventh Meeting of the BAAS', *Athenaeum* (21 August 1841), p. 651; 'Fourteenth Meeting of the BAAS', *Athenaeum* (28 September 1844), p. 884.
- 18 Referred to in a letter dated 9 August 1868, just a few weeks before the BAAS meeting was held. See *Women's Suffrage Collection from Manchester Central Library* (Manchester, Adam Matthew Publications, 1995), reel 2.
- 19 Morrell and Thackray (eds), *Gentlemen of Science: Early Correspondence*, p. 451.
- 20 Blackburn, *Women's Suffrage*, p. 35.
- 21 *Report of the British Association for the Advancement of Science* (Norwich, n.p., 1868), p. 156.
- 22 Articles appeared in *The Times* (27 August 1868), *Pall Mall Gazette* (27 August 1868; 29 August 1868; 5 September 1868; 8 September 1868), *Lancet* (5 September 1868; 9 October 1869), *Saturday Review* (5 September 1868).
- 23 <http://historyonline.chadwyck.co.uk/pfto> (Palmer's Full Text Online), 27 August 1868, p. 6.
- 24 *Pall Mall Gazette* (27 August 1868), p. 2.
- 25 'Miss Becker's Paper Read Before the British Association for the Advancement of Science', *Englishwoman's Review*, 3:9 (October 1868), pp. 48–55.
- 26 Parker, 'Lydia Becker's "School for Science"', p. 635.
- 27 Parker, 'Lydia Becker's "School for Science"', p. 634.
- 28 Becker, 'On the Study of Science by Women', p. 387. See Parker, 'Lydia Becker's "School for Science"', p. 635.
- 29 *Athenaeum* (19 August 1871), p. 249.
- 30 Audrey Kelley, *Lydia Becker and the Cause* (Lancaster, Centre for North-West Regional Studies, University of Lancaster, 1992), p. 51.
- 31 Six of these papers were delivered in the Statistics section; at least three addressed the woman question in some fashion. Apparently a controversy ensued about whether to abolish the section because of this influx of female presenters, yet well into the twentieth century the vast majority of papers delivered by women occurred in this section. For details of the papers, see the annual reports of the BAAS.
- 32 The *Athenaeum's* coverage of the 1869 Biology section runs the complete abstract for Becker's talk. See *Athenaeum* (11 September 1869), p. 342.
- 33 Becker, *Women's Suffrage Journal* (October 1889), p. 25; quoted in Parker, 'Lydia Becker's "School for Science"', p. 637.
- 34 Becker's address was noted in *Journal of Botany, British and Foreign*, 7 (1869), pp. 291–2, within a larger report on the BAAS's Exeter meeting, pp. 282–94. See also *Darwin Correspondence*, 14 October 1869 (DAR 160:117) and 29 December 1869 (DAR 160:118) in the Darwin Correspondence Archive at the University of Cambridge.
- 35 See *Darwin Correspondence*, May 1863 (DAR 160.108, DAR 160.1:106). Also see Charles Darwin, *The Different Forms of Flowers on Plants of the Same Species* (London, John Murray, 1877).

- 36 *Darwin Correspondence*, 22 December 1866 (DAR 160:113).
- 37 *Report of the British Association for the Advancement of Science* (Exeter, n.p., 1869), p. 106.
- 38 Becker, 'On the Study of Science by Women', p. 387.
- 39 Quoted in Parker, 'Lydia Becker's "School for Science"', p. 641.
- 40 Becker, 'On the Study of Science by Women', p. 387.
- 41 For a recent biography, see Rachel Holmes, *Scanty Particulars: The Life of Dr James Barry* (London, Viking, 2002).
- 42 *Darwin Correspondence*, 13 January 1869 (DAR 160:116). Becker writes, on stationery imprinted with 'Manchester National Society for Women's Suffrage', that Darwin's chapter on pangenesis in *The Variation of Animals and Plants Under Domestication* (1868) 'seems like a revelation'.
- 43 Ann Shteir, 'Elegant Recreations? Figuring Science Writing for Women' in Bernard Lightman (ed.), *Victorian Science in Context* (Chicago, University of Chicago Press, 1997), pp. 236–55; p. 251.
- 44 This figure is speculation based on the report that 2,004 people attended the 1869 BAAS which consisted of 28 sectional meetings. See MacLeod and Collins, *The Parliament of Science*, p. 280.
- 45 Shteir's 'Elegant Recreations?' quotes Babington's letter declining to sign a suffrage petition: "I hope that after this matter is settled you will return to the gentle science of Botany." See *Darwin Correspondence*, 13 January 1869 (DAR 160:116).
- 46 Bernard Lightman, 'Science and the Public: The Contested Meanings of Science in Victorian Britain,' at conference 'Wrestling with Nature: From Omens to Science', University of Wisconsin, 26–28 April 2001.

Chapter 8 A Fair Trial for Spiritualism?: Fighting Dirty in the *Pall Mall Gazette*

Elisabeth Wadge

- 1 *Punch: Or, The London Charivari* (9 May 1868), p. 207.
- 2 Since the *Pall Mall Gazette* (hereafter PMG) carries the Tyndall—Home debate, it will remain my focus during the following discussion; all date and page references will be included parenthetically within the text.
- 3 Parallels with the Wilde trials, on charges of 'posing as a sodomite' [sic], are interesting in that the *pretension* formed the basis for the charge; emphasis added in both quotations.
- 4 For the plaintiff, Mr W.M. James QC, Mr Druce QC, Mr Fischer; for the defendant, Mr Henry Matthews QC, Mr Fitzroy Kelly, Mr Charles Walker.
- 5 *Times* (30 June 1853), p. 8. For a spiritualist's response, see J.C. Perry, 'Professor Faraday and Table-Turning', *Times* (4 July 1853), p. 8.
- 6 'The Spirits Again', *Every Saturday* (2 November 1872), pp. 489–91; see also the pamphlets written through T. Cushman Budington, published by the Star Publishing Company, Massachusetts, in the 1880s.
- 7 *Times* (8 January 1875), p. 10.

- 8 Frank A.J.L. James (ed.), *Correspondence of Michael Faraday* (London, Institution of Electrical Engineers, 1991-), vol. 4 January 1849–October 1855; 10 April 1855, p. 854.
- 9 Home may be following *Punch*'s lead here, since its Science Gossip section puns that a 'Scientific Ghost-story will shortly appear ... founded on Spectrum Analysis'; (9 May 1868), p. 202.
- 10 *Times* (23 May 1868), p. 11.
- 11 *Spiritual Magazine*, 3 (1868), p. 380, p. 478.

Chapter 9 'This is Ours and For Us': The *Mechanic's Magazine* and Low Scientific Culture in Regency London

James Mussell

- 1 Henry Brougham, *Observations on the Education of the People Addressed to the Working Classes and Their Employers* (London, Richard Taylor, 1825), p. 32.
- 2 M.M. Bakhtin and P.N. Medvedev, *The Formal Method in Literary Scholarship*, trans. A.J. Wehrle (Baltimore, John Hopkins University Press 1994), cited in *The Bakhtin Reader*, ed. by Pam Morris (London, Edward Arnold 1994), pp. 175–8.
- 3 David Stack, 'Nature and Artifice': *The Life and Thought of Thomas Hodgskin* (Suffolk, The Boydell Press 1998), p. 78 and 'Place Manuscripts', British Library Add MSS 27823 fo.242.
- 4 Jon P. Klancher, *The Making of English Reading Audiences 1790–1832* (Wisconsin, University of Wisconsin Press, 1987), p. 79.
- 5 Jonathan R. Topham, 'Thomas Byerley, John Limbird and the Production of Cheap Periodicals in Regency Britain', *Book History*, 8 (2005), p. 89.
- 6 Anon., in Joseph C. Robertson (ed.), *Trades Newspaper*, 1 (17 July 1825) (London: John Limbird, 1825), p. 14.
- 7 Anon., *Trades Newspaper*, p. 14.
- 8 Place Manuscripts, BL Add MSS 27823 fo.240.
- 9 Anonymous [Joseph Clinton Robertson and Thomas Hodgskin], 'To the Mechanics of the British Empire,' *Mechanic's Magazine*, 1 (30 August 1823) (London: Knight and Lacey, 1823), p. 16. Any further citations of the *Mechanic's Magazine* will be abbreviated to MM and should be considered authored anonymously by the editors unless otherwise noted. Note also that although the *Mechanic's Magazine* changed its title from the singular *Mechanic's* to the plural *Mechanics* after 25 September 1824, the singular is used throughout this chapter.
- 10 Klancher, *The Making of English Reading Audiences*, pp. 12–13 follows M.M. Bakhtin, 'Discourse in the Novel,' *The Dialogic Imagination*, ed. Michael Holquist, trans. Caryl Emerson and Michael Holquist (Austin, University of Texas Press, 1987), pp. 284–5.
- 11 I.J. Prothero, *Artisans and Politics in Early Nineteenth Century London: John Gast and his Times* (London, Methuen, 1979), pp. 4–5, pp. 20–8.
- 12 Anon. [Robertson and Hodgskin], p. 16.
- 13 Klancher, *The Making of English Reading Audiences*, p. 79.
- 14 Klancher, *The Making of English Reading Audiences*, p. 12.
- 15 MM (30 August 1823) (1823b), p. 16.

- 16 M.M. Bakhtin, 'The Problem of Speech Genres,' in *Speech Genres and Other Late Essays*, ed. Caryl Emerson and Michael Holquist, trans. Vern W. McGee (Austin, University of Texas Press, 1986), p. 61. Emphasis in the original.
- 17 Bakhtin, *Speech Genres*, p. 65.
- 18 Bakhtin, *Speech Genres*, p. 65.
- 19 Mark Parker, *Literary Magazines and British Romanticism* (Cambridge, Cambridge University Press, 2000), p. 10, p. 15.
- 20 MM (4 October 1823) (1823c), p. 96.
- 21 MM, 1, (11 October 1823), pp. 99–100.
- 22 MM, 1, (11 October 1823), p. 100.
- 23 MM, 1, (11 October 1823), p. 101 and see MM, 1 (30 August 1823), p. 16.
- 24 See, for instance, Thomas Kelly, *George Birkbeck* (Liverpool, Liverpool University Press, 1957), pp. 76–145.
- 25 Thomas Hodgskin, *Labour Defended against the Claims of Capital* (1825), cited in Kelly, *George Birkbeck*, p. 99.
- 26 Barry Barnes and Steven Shapin, 'Science, Nature and Control: Interpreting Mechanic's Institutes', *Social Studies of Science*, 17 (1977), pp. 31–74, p. 48.
- 27 Bakhtin, *The Dialogic Imagination*, p. 342.
- 28 Anonymous [Henry Brougham], 'Thoughts on Popular Education by a Member of the Church of England' in *Edinburgh Review*, 43 (1825), p. 248. Emphasis in the original.
- 29 Cited in Kelly, *George Birkbeck*, p. 95.
- 30 Anne Secord, 'Science in the Pub: artisan botanists in early-Nineteenth Century Lancashire', *History of Science*, 32 (1994), p. 299.
- 31 Bakhtin and Medvedev, *The Formal Method*, p. 176.
- 32 Bakhtin, *The Dialogic Imagination*, p. 299.
- 33 Bakhtin, *The Dialogic Imagination*, p. 345.
- 34 W_D_, 'On the London Mechanic's Institute,' *Weekly Mechanic's Journal or Artisans Miscellany of Inventions, Experiments, Projects, and Improvements in the Useful Arts*, 1 (13 December 1823) (London: Westley and Parrish 1824), p. 47.
- 35 Bakhtin, *The Dialogic Imagination*, p. 345.
- 36 Susan Sheets-Pyenson, 'Popular Science Periodicals in Paris and London: the Emergence of a Low Scientific Culture 1820–1875', *Annals of Science*, 42 (1985), pp. 549–572.
- 37 Bakhtin, *Speech Genres*, pp. 76–7.
- 38 Bakhtin, *The Dialogic Imagination*, p. 346.
- 39 See "Minus," MM (16, 30 October; 20 November; 4, 11 December 1824), p. 53, p. 87, p. 132, p. 138, p. 166, p. 179.
- 40 W.H. Brock, 'British Science Periodicals and Culture 1820–1850', *Victorian Periodicals Review*, 21 (Summer 1988), p. 53.

Chapter 10 How did the Conservation of Energy Become 'The Highest Law in All Science'?

Ted Underwood

- 1 Hermann von Helmholtz, 'On the conservation of force; a physical memoir', in John Tyndall, and William Francis (eds), *Scientific Memoirs, Selected from the*

- Transactions of Foreign Academies of Science, and from Foreign Journals: Natural Philosophy* (London, Taylor & Francis, 1853), p. 122.
- 2 William John Macquorn Rankine, 'On the general law of the transformation of energy', *Philosophical Magazine*, 4:5 (1853), p. 106.
 - 3 Herbert Spencer, *First Principles* (London, Williams and Norgate, 1862), p. 258.
 - 4 Michael Faraday, 'On the conservation of force', in *Experimental Researches in Chemistry and Physics* (London, Taylor & Francis, 1859), p. 447.
 - 5 'Force and matter' in *All the Year Round*, 21 July 1866, pp. 35–8; J. Carpenter, 'What we owe to the sun' in *Once a Week* 15 (1866), pp. 410–14; William Thomson, and P.G. Tait, 'Energy' in *Good Words* 3 (1862), pp. 601–07; James Hinton, 'Force' in *Cornhill Magazine* 4 (1861), pp. 409–20. See also, 'Sun force and earth force' in *The Popular Science Review* 5 (1866), pp. 327–36; 'The labour of the sunbeams' in *Recreative Science* 3 (1862), pp. 56–60.
 - 6 Edward L. Youmans, *The Conservation and Correlation of Forces* (New York, Appleton, 1865), p. xii.
 - 7 Mark Seltzer, *Bodies and Machines* (New York, Routledge, 1992), pp. 29–31.
 - 8 For contemporary testimony to this delay, see Henry Adams, *The Degradation of the Democratic Dogma* (New York, Macmillan, 1919), pp. 142–43.
 - 9 See Geoffrey Cantor, 'Faraday's search for the gravelectric effect' in *Physics Education*, 26 (1991), pp. 289–93.
 - 10 Spencer, *First Principles*, p. 253.
 - 11 Crosbie Smith, *The Science of Energy: A Cultural History of Energy Physics in Victorian Britain* (Chicago, University of Chicago Press, 1998), p. 140.
 - 12 Anne Gilchrist, 'The indestructibility of force' in *Macmillan's Magazine*, 6 (1862), p. 337.
 - 13 James Hinton, 'Force' in *Cornhill Magazine*, 4 (1861), p. 414.
 - 14 David Gooding, 'Metaphysics versus measurement: the conversion and conservation of force in Faraday's physics' in *Annals of Science*, 37 (1980), pp. 1–29.
 - 15 M. Norton Wise, with the collaboration of Crosbie Smith, 'Work and waste: political economy and natural philosophy in nineteenth-century Britain' in *History of Science*, 27 (1989), pp. 263–301; see also pp. 391–449 and 28 (1990), pp. 221–61. See especially 27 (1989), pp. 400–10.
 - 16 Smith, *The Science of Energy*, pp. 100–25.
 - 17 John Tyndall, *Heat Considered as a Mode of Motion* (New York, D. Appleton, 1864), p. 446.
 - 18 Tyndall, *Heat Considered*, p. 447.
 - 19 Anson Rabinbach, *The Human Motor: Energy, Fatigue, and the Origins of Modernity* (New York, Basic Books, 1990), p. 3.
 - 20 Rabinbach, *The Human Motor*, pp. 69–83, see also pp. 179–237.
 - 21 Spencer, *First Principles*, pp. 282–83.
 - 22 Edward L. Youmans, 'Introduction' in Edward L. Youmans (ed.), *The Correlation and Conservation of Forces* (New York, D. Appleton, 1865), p. xxv. See also p. xxvii, and p. xli.
 - 23 William Thomson, 'On the sources available to man for the production of mechanical effect' in *Mathematical and Physical Papers* 1 (Cambridge, Cambridge University Press, 1882), p. 510.
 - 24 Herman von Helmholtz, 'On the interaction of physical forces', John Tyndall (trans.), in Youmans (ed.), *The Correlation*, p. 240; Justus Liebig, 'The connection and equivalence of forces' in Youmans (ed.), *The Correlation*, p. 397.

- 25 'The source of labour', *Chambers's Journal* 4:3 (1866), pp. 554–57; 'What we owe to the sun' in *Once a Week* 15 (1866), pp. 410–14; 'The labour of the sunbeams' in *Recreative Science*, 3 (1862), pp. 56–60. See also P.G. Tait, and William Thomson, 'Energy' in *Good Words* 3 (1862), p. 605.
- 26 Rabinbach, *The Human Motor*, p. 3.
- 27 John F.W. Herschel, *A Treatise of Astronomy* (London, Longman, 1833), p. 211.
- 28 J.N.P. Hachette, *Traité élémentaire des machines* (Paris, Klostermann, 1811), p. 1.
- 29 For a discussion of this tradition's development, see I Grattan-Guinness, 'Advances in engineering mechanics and instruction in France, 1800–30' in *Annals of Science*, 41 (1984), pp. 1–33. For an account of its contribution to thermodynamics, see D.S.L. Cardwell, *From Watt to Clausius: The Rise of Thermodynamics in the Early Industrial Age* (Ames, Iowa State University Press, 1989), pp. 150–89.
- 30 Adam Walker, *A System of Familiar Philosophy in Twelve Lectures*, 2 (London, 1802), p. 98. For an earlier example, see Jean Senebier, *Mémoires physico-chymiques sur l'influence de la lumière solaire pour modifier les êtres des trois règnes de la nature, et sur-tout ceux du règne végétal* 2, (Geneve, Chiral, 1782), p. 244.
- 31 Thomas Carlyle, *Sartor Resartus: The Life and Opinions of Herr Teufelsdröckh*, in Mark Engel, and Roger L. Tarr (eds) (Berkeley, University of California Press, 2000), p. 55.
- 32 Adam Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations*, in R.H. Campbell, A.S. Skinner, and W.B. Todd (eds) 1 (Oxford, Oxford University Press, 1976), pp. 364–5.
- 33 Ronald L. Meek, *The Economics of Physiocracy: Essays and Translations*, (Cambridge, MA, Harvard University Press, 1963), pp. 345–63.
- 34 Alexander Hamilton, *The Reports of Alexander Hamilton*, in Jacob E. Cooke (ed.) (New York, Harper, 1964), p. 121.
- 35 Jean-Baptiste Say, *Traité d'Économie Politique* (Paris, Deterville, 1803), pp. 39–40. My translation.
- 36 See T.R. Malthus, *An Inquiry into the Nature and Progress of Rent* (London, John Murray, 1815); and David Ricardo, *The Works and Correspondence of David Ricardo*, in Piero Sraffa (ed.), vol. 1 (Cambridge, Cambridge University Press, 1951), pp. 69–75.
- 37 Anna Gambles, *Protection and Politics: Conservative Economic Discourse 1815–52* (Woodbridge, Royal Historical Society at the Boydell Press, 1999), pp. 25–26. See also pp. 56–57.
- 38 Samuel Smiles, *The Life of George Stephenson, Railway Engineer*, 2nd edn. (London, Murray, 1857), pp. 484–5.
- 39 Thomson, 'On the sources', p. 510.
- 40 William Benjamin Carpenter, 'The phasis of force' in *National Review*, 4 (1857), p. 390. See also Spencer, *First Principles*, p. 283.
- 41 Tyndall, *Heat*, p. 446n.
- 42 John Tyndall, 'Personal recollections of Thomas Carlyle' in *New Fragments* (New York, D. Appleton, 1892), p. 386.
- 43 Tyndall, *New Fragments*, p. 447.
- 44 Herschel, *Treatise of Astronomy*, p. 211.
- 45 John F.W. Herschel, *Outlines of Astronomy* (London, Longman, Brown, 1851), p. 237.
- 46 Herschel, *Outlines of Astronomy*, p. 260.

- 47 Samuel Smiles, *The Life of George Stephenson, Railway Engineer*, p. 485. The sentence has been removed from, Samuel Smiles, *The Life of George Stephenson, and of his Son Robert Stephenson* (New York, Harper, 1868), p. 468 – and also from subsequent editions.
- 48 Thomas Kuhn, 'Energy conservation as an example of simultaneous discovery' in *Critical Problems in the History of Science* (Madison, University of Wisconsin Press, 1959, rpt 1969), p. 323, see also p. 337.
- 49 'The source of labour', pp. 555–6.

Chapter 11 'Scriptural Geology', *Vestiges of the Natural History of Creation* and Contested Authority in Nineteenth-Century British Science

John M. Lynch

- 1 I would like to thank David Clifford for his helpful comments on an earlier draft of this essay, portions of which have appeared as the introductions to Lynch, J.M. (ed.). 2000, *Vestiges and the Debate Before Darwin*, vols 7, Bristol, Thoemmes Press, and Lynch, J.M. 2002, *Creationism and Scriptural Geology, 1817–57*, vols 7, Bristol, Thoemmes Press. I would also like to thank the attendees at my colloquium ('Anti-Evolutionism and Contested Authority in the Natural Sciences since 1800') held at the University of Minnesota in January 2004 for their thoughtful comments and feedback on some of this material.
- 2 On physics, see Peter J. Bowler, *Recognising Science and Religion* (Chicago, University of Chicago Press, 2001). On popularization, see Joel S. Schwartz, 'Robert Chambers and Thomas Henry Huxley, science correspondents: the popularization and dissemination of nineteenth-century natural science' in *The Journal of the History of Biology* 32 (1999), pp. 343–83 and Bernard Lightman, 'Marketing knowledge for the general reader: Victorian popularisers of science' in *Endeavour*, 24 (2000), pp. 100–06. For boundary work, see T. Gieryn, 'Boundary work in professional ideology of scientists' in *The American Sociological Review* 48 (1983), pp. 781–95. On defining professionals, see Jack B. Morrell, 'Professionalisation', in R.C. Olby, G.N. Cantor, J.R.R. Christie, and M.J.S. Hodge (eds), *Companion to the History of Modern Science* (London, Routledge, 1990), pp. 980–89.
- 3 See Michael Roberts, 'Geology and Genesis unearthed' in *Churchman* 112 (1998), pp. 225–55, in which he examines the writings of John Ray and contemporary theologians, poets and naturalists. Supporters of this model included such theologically orthodox individuals as the natural philosophers Edmund Halley, Thomas Burnet, William Whiston and John Woodward, and the poets John Milton, Edmund Spenser, John Dryden and Alexander Pope.
- 4 While it would be difficult to present an exhaustive list of members of this movement, the following are good British exemplars: Granville Penn (1761–1844), George Bugg (1769–1851), Andrew Ure (1778–1857), Henry Cole (1792–1858), Thomas Gisbourne (1758–1846), Samuel Best (1802–73), James Mellor Brown (1796–1867), Fowler de Johnstone (n.d.), George Young (1777–1848), George Fairholme (1789–1846), William Rhind (1797–1874), William Cockburn

- (1773–1858), Frederick Nolan (1784–1864), William Martin (1772–1851), Sharon Turner (n.d.), Joseph Sutcliffe (n.d.), William Kirby (1759–1850), William Brande (1788–1866), William Martin (1772–1851), Walter Foreman (n.d.), Robert Fitzroy (1805–65), and John Murray (1786–1851). There has been little published work examining the scriptural geologists; see C.C. Gillispie, *Genesis and Geology* (Cambridge, MA, 1951, rpt. Harvard University Press, 1996); Milton Millhauser, 'The scriptural geologists: an episode in the history of opinion' in *Osiris*, 11 (1954), pp. 65–86; Nicolaas Rupke, *The Great Chain of History* (Oxford, Clarendon Press, 1983), pp. 42–50, and the introduction to Lynch, *Creationism*. For a slanted interpretation which sees many of these individuals as 'competent' within geology and not driven by theological concerns, see Terry Mortenson, *The Great Turning Point: The Church's Catastrophic Mistake on Geology Before Darwin* (Green Forest, AR, Master Books, 2004).
- 5 Roberts, 'Geology and Genesis'; Hugh Miller, *The Testimony of the Rocks; or Geology in its bearings on the two theologies, natural and revealed* (Edinburgh, n.p., 1857), pp. 412–14. In this work, Miller describes these individuals as 'antigeologists'.
 - 6 Thus, perhaps in the 'queen of sciences', the scriptural geologists were also destined to be sidelined.
 - 7 James Mellor Brown, *Reflections on Geology, Suggested by the Perusal of Dr Buckland's Bridgewater Treatise* (London, James Nisbet, 1838), p. 24.
 - 8 Charles Lyell, 'Review of "Memoir of the Geology of France"' in *Quarterly Review*, 36 (1827), pp. 482–3.
 - 9 John Willis Clark, and Thomas McKenny Hughes, *The Life and Letters of the Reverend Adam Sedgwick* 1 (Cambridge, Cambridge University Press, 1890), p. 362.
 - 10 Horace B. Woodward, *The History of the Geological Society of London* (London, Longman, 1908), p. 86. The Geological Society would appear to have felt that Sedgwick's treatment was 'appropriate'. Ure and John Murray were the only scriptural geologists of note, who were members of the GSL at a time when expertise in geology was not necessary for membership – see Martin Rudwick, 'A year in the life of Adam Sedgwick and company, geologists' in *Archives of Natural History*, 15 (1988), pp. 243–68.
 - 11 Adam Sedgwick, *A Discourse on the Studies of the University* (London, J.W. Parker, 1883), p. 104. Sedgwick's attackers included Henry Cole, William Sewell, and the *British Critic*, which also objected to Sedgwick's willingness to admit Dissenters to Cambridge.
 - 12 Chambers's article in *Chambers's Edinburgh Review* is quoted in Clark, and Hughes, *The Life and Letters* 1, pp. 77–78; Further details on the York confrontation and subsequent disputes are given in Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (London, Royal Historical Society, 1984), pp. 243–45, and James A. Secord, *Victorian Sensation* (Chicago, Chicago University Press, 2000), pp. 232–33.
 - 13 Sedgwick, *A Discourse*, p. 23; Adam Sedgwick, 'Vestiges of the Natural history of Creation' in *Edinburgh Review* 165 (1845), pp. 1–85; Secord, *Victorian Sensation*, p. 243; Sedgwick's attack would continue in his fifth edition of *A Discourse* in 1850.
 - 14 William Whewell, *Indications of the Creator* (London, n.p., 1845), pp. 9–25 *passim* (emphasis in the original). Myers's letter quoted in Richard Yeo, *Defining Science* (Cambridge, Cambridge University Press, 1993).

- 15 See James A. Secord's edition of *Vestiges of the Natural History of Creation and Other Evolutionary Writings* (Chicago, Chicago University Press, 1994), for a summary of changes, and Secord, *Victorian Sensation* for a discussion of same.
- 16 For further discussion of Huxley's reaction, see Secord, *Victorian Sensation*, pp. 500–04.
- 17 Robert Chambers, *Vestiges of the Natural History of Creation*, 10th edn (London, John Churchill, 1853), p. xlix; surprisingly, this lead Huxley to defend Sedgwick, an individual for whom he had no love.
- 18 M.J.S. Hodge, 'The Universal Gestation of Nature: Chambers, *Vestiges* and *Explanations*' in *The Journal of the History of Biology*, 5 (1972), pp. 127–51.
- 19 Chambers, *Vestiges*, p. xxxix; Louis Agassiz, *Introduction to the Study of Natural History* (New York, Greeley & McElrath, 1847), p. 25.
- 20 Gov Hutchinson, *Robert Chambers's Vision of Science: The Diffusion of Scientific Ideas to the General Reader in Early-Victorian Britain*, unpublished Ph.D Thesis, Temple University, 1980, p. 79.
- 21 In an interesting coincidence, 1844 marked not only Sedgwick's attack on scriptural geology and the publication of *Vestiges*, but also the completion of Darwin's private *Essay* which would, 15 years later, transmute into his *Origin*.
- 22 'Letter of 5 September 1857', reproduced in Frederick Burkhardt & Sydney Smith (eds), *The Correspondence of Charles Darwin* 6 (Cambridge, Cambridge University Press, 1985—).
- 23 For Bastain, see James E. Strick, *Sparks of Life* (Cambridge, MA, Harvard University Press, 2000). For Mivart, see John M. Lynch, *Darwin's Theory of Natural Selection: British Responses, 1859–1871* 4 (Bristol, Thoemmes Press, 2001), pp. xvi–xx, and John M. Lynch, 'Mivart, St George Jackson', in Bernard Lightman (ed.), *Dictionary of Nineteenth-Century British Scientists* (Bristol, Thoemmes Press, 2004), pp. 1411–15.
- 24 To what degree Chambers himself believed in such a divine being is an open question.
- 25 Robert Chambers, *Vestiges of the Natural History of Creation* (London, John Churchill, 1844), p. 154, see also p. 156.
- 26 On Cambridge and Sedgwick, see Martha McMackin Garland, *Cambridge Before Darwin: the ideal of a liberal education, 1800–60* (Cambridge, Cambridge University Press, 1980), and David Oldroyd, 'Adam Sedgwick: a confident mind in turmoil' in Peter Harman and Simon Mitton (eds), *Cambridge Scientific Minds* (Cambridge, Cambridge University Press, 2002), pp. 64–78.
- 27 James R. Moore, 'Geologists and interpreters of genesis in the nineteenth century' in David C. Lindberg & Ronald L. Numbers (eds), *God and Nature* (Berkeley, University of California Press, 1986), pp. 322–50; Martin J.S. Rudwick, 'The shape and meaning of earth history' in Lindberg & Numbers (eds), *God and Nature*, pp. 298–321; John Hedley Brooke, 'The history of science and religion: some evangelical dimensions' in David N. Livingstone, D.G. Hart, and Mark A. Noll (eds), *Evangelicals and Science in Historical Perspective* (New York, Oxford University Press, 1998), pp. 17–40.
- 28 Michael Shortland, *Hugh Miller and the Controversies of Victorian Science* (Oxford, Clarendon Press, 1996), p. 29; Rudwick, 'A year in the life', p. 249; On Baconianism, see Jonathan Smith, *Fact & Feeling* (Madison, Wisconsin, University of Wisconsin Press, 1994), and Richard Yeo, 'An idol of the market-place: Baconianism in

nineteenth-century Britain' in *History of Science*, 23 (1985), pp. 251–98. On Chambers's handicap see Milton Millhauser, *Just Before Darwin* (Middletown, CT, Wesleyan University Press, 1959); Secord, *Victorian Sensation*, p. 391.

- 29 Clark and Hughes provide no mention of Sedgwick's reaction to the encounter between Huxley and Wilberforce.

Chapter 12 'This house is a Temple of Research': Country-House Centres for Late Victorian Science

Donald L. Opitz

Research for this chapter benefited from a dissertation enrichment grant of the National Science Foundation, SES-0094442 (2001). The author would like to thank the Balfour and Newton Library (University of Cambridge), British Library, Imperial College Archives, and Rt. Hon. Baron Rayleigh for kind permission to access and quote from manuscript materials; and Richard Bellon and Sally Gregory Kohlstedt for their helpful comments and suggestions.

- 1 'A.J. Balfour to E. Talbot, Whittingehame, 2 January 1878', in Kenneth Young, *Arthur James Balfour: The Happy Life of the Politician, Prime Minister, Statesman and Philosopher, 1848–1930* (London, G. Bell and Sons, 1963), p. 39.
- 2 Dorinda Outram, 'New Spaces in Natural History', in N. Jardine, J.A. Secord, and E.C. Spary (eds), *Cultures of Natural History* (Cambridge, Cambridge University Press, 1996), pp. 249–65; and Mary Jo Nye, *Before Big Science: The Pursuit of Modern Chemistry and Physics, 1800–1940* (London, Prentice Hall, 1996).
- 3 Peter Alter, *The Reluctant Patron: Science and the State in Britain, 1850–1920* (Oxford, St. Martin's Press, 1987), pp. 214–45.
- 4 Peter Galison and Bruce Hevly (eds), *Big Science: The Growth of Large-Scale Research* (Stanford, Stanford University Press, 1992); Peter Galison and Emily Thompson (eds), *The Architecture of Science* (Cambridge, MA., MIT Press, 1999); Frank A.J.L. James (ed.), *The Development of the Laboratory: Essays on the Place of Experiment in Industrial Civilisation* (New York, American Institute of Physics, 1989).
- 5 Soraya de Chadarevian, 'Laboratory Science versus Country-House Experiments: The Controversy between Julius Sachs and Charles Darwin', *British Journal for the History of Science*, 29 (1996), pp. 17–41; Allan Chapman, *The Victorian Amateur Astronomer: Independent Astronomical Research in Britain, 1820–1920* (New York, John Wiley & Sons, 1998); Simon Schaffer, 'Physics Laboratories and the Victorian Country House' in *Making Space for Science: Territorial Themes in the Shaping of Knowledge* (Basingstoke, Macmillan, 1998), pp. 149–80.
- 6 John N. Howard, 'Principal Scientific Contributions of John William Strutt, Third Baron Rayleigh' in Rutherford Aris, T. Ted Davis, and Roger H. Stuewer (eds), *Springs of Scientific Creativity: Essays on Founders of Modern Science* (Minneapolis, University of Minnesota Press, 1983), pp. 163–87, p. 182; Sir William Gavin, *Ninety Years of Family Farming: The Story of Lord Rayleigh's and Strutt & Parker Farms* (London, Hutchinson, 1967), p. 70.
- 7 Donald Luke Opitz, 'Aristocrats and Professionals: Country-House Science in Late-Victorian Britain', Ph.D. thesis, University of Minnesota (2004).

- 8 Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago, University of Chicago Press, 1994); Morris Berman, "Hegemony" and the Amateur Tradition in British Science', *Journal of Social History*, 8 (1974–5), pp. 30–50.
- 9 Sir E. John Russell, *A History of Agricultural Science in Great Britain, 1620–1954* (London, George Allen & Unwin, 1966); J.D. Sykes, 'Agriculture and Science', in G.E. Mingay (ed.), *The Victorian Countryside*, 2 vols (London, Routledge & Kegan Paul, 1981), vol. 1, pp. 260–72.
- 10 Jeffrey A. Auerbach, *The Great Exhibition of 1851: A Nation on Display* (New Haven, Yale University Press, 1999), p. 29.
- 11 Hon. Sir Charles Parsons (ed.), *The Scientific Papers of William Parsons, Third Earl of Rosse, 1800–1867* (Bradford, The Country Press, 1926), p. 61, p. 63.
- 12 Martin J. Wiener, *English Culture and the Decline of the Industrial Spirit, 1850–1980* (Cambridge, Cambridge University Press, 2004).
- 13 Mark Girouard, *The Victorian Country House* (New Haven, Yale University Press, 1979); Jessica Gerard, *Country House Life: Family and Servants, 1815–1914* (Oxford, Blackwell, 1994).
- 14 'J.C. Maxwell to J.W. Strutt, Glenlair, 8 July 1871', in Robert John Strutt, fourth Baron Rayleigh, *Life of John William Strutt, Third Baron Rayleigh*, aug. edn. by John N. Howard (Madison, University of Wisconsin Press, 1968), pp. 59–60.
- 15 Rayleigh, *Life*, pp. 149–65, pp. 302–6; John William Strutt, Baron Rayleigh, *Scientific Papers*, 6 vols (Cambridge, Cambridge University Press, 1899–1920), vol. 1, pp. 347–63.
- 16 Arthur Schuster, 'John William Strutt, Baron Rayleigh, 1842–1919', *Proceedings of the Royal Society, Obituary Notices of Fellows of the Royal Society*, 98 (1920–1), pp. i–l.
- 17 Donald L. Opitz, "'Behind Folding Shutters in Whittingehame House": Alice Blanche Balfour (1850–1936) and Amateur Natural History', *Archives of Natural History*, 31 (2004), pp. 330–48.
- 18 'A.J. Balfour to E. Talbot, Whittingehame, 2 January 1878' in Young, *Arthur James Balfour*, p. 39.
- 19 F.M. Balfour, *A Monograph on the Development of Elasmobranch Fishes* (London, Macmillan, 1878); Arthur James Balfour, *A Defence of Philosophic Doubt, Being an Essay on the Foundations of Belief* (London, Macmillan, 1879).
- 20 Lord Rayleigh, *Lord Balfour in His Relation to Science* (Cambridge, Cambridge University Press, 1930); James Alistair McGeachie, "'Mr. Balfour's Apologetics": Excoriation and Accommodation in the Intellectual Formation of Arthur James Balfour', Ph.D. thesis, University of Cambridge, (1995), pp. 217–8; Rayleigh, *Life*, pp. 174–8; Balfour, *A Defence of Philosophic Doubt*, pp. 249–59.
- 21 Marquis of Salisbury, 'On Spectral Lines of Low Temperatures', *Philosophical Magazine*, 45 (1873), pp. 241–5; H. Kayser, *Handbuch der Spectroscopie*, 8 vols (Leipzig, S. Hirzel, 1900–34), vol. 1, p. 204n; H. McL[ead], 'Robert Arthur Talbot Gascoyne Cecil, The Marquis of Salisbury, K.G., 1830–1903', *Proceedings of the Royal Society of London, Obituary Notices*, 75 (1905), pp. 319–25.
- 22 Lady Gwendolyn Cecil, *Life of Robert Marquis of Salisbury*, 4 vols (London, Hodder and Stoughton, 1921–32), vol. 1, pp. 175–6.
- 23 N.C. Gillispie, 'The Duke of Argyll, Evolutionary Anthropology, and the Art of Scientific Controversy', *Isis*, 68 (1977), pp. 40–54.

- 24 Crosbie Smith, *The Science of Energy: A Cultural History of Energy Physics in Victorian Britain* (Chicago, University of Chicago Press, 1998).
- 25 Lord Rayleigh, 'Some Personal Reminiscences of Sir Charles Parsons' in *Scientific Papers and Addresses of the Hon. Sir Charles A. Parsons* (Cambridge, Cambridge University Press, 1934), pp. xv–xxviii; W. Garrett Scaife, *From Galaxies to Turbines: Science, Technology and the Parsons Family* (Bristol, Institute of Physics, 2000).
- 26 David W. Bebbington, 'Science and Evangelical Theology in Britain from Wesley to Orr' in David N. Livingstone, D.G. Hart, and Mark A. Noll (eds), *Evangelicals and Science in Historical Perspective* (Oxford, Oxford University Press, 1999), pp. 120–41.
- 27 Marianne Vicars, 'Mother's Thoughts', n.d., Terling Place Archives.
- 28 Catherine Marsh, *Memorials of Captain Hedley Vicars, Ninety-seventh Regiment* (London, J. Nisbet, 1856).
- 29 Rayleigh, *Life*, pp. 15–6, pp. 408–9.
- 30 James Robertson, *Lady Blanche Balfour: A Reminiscence* (Edinburgh, Oliphant, Anderson and Ferrier, 1897), pp. 5–7, pp. 62–3; Janet Oppenheim, 'A Mother's Role, A Daughter's Duty: Lady Blanche Balfour, Eleanor Sidgwick, and Feminist Perspectives', *Journal of British Studies*, 34 (1995), pp. 196–232.
- 31 A. Hunter Dupree, *Asa Gray, 1810–1888* (Cambridge, Belknap, 1959).
- 32 'A. Gray to [T.W.] Church, 11 November 1879', British Library, Balfour Papers, Add. Mss. 49838/74–5.
- 33 Arthur James Balfour, *The Foundations of Belief, Being Notes Introductory to the Study of Theology* (London, Longmans, Green, and Co., 1895); Bernard Lightman, "'Fighting Even With Death": Balfour, Scientific Naturalism, and Thomas Henry Huxley's Final Battle' in Alan P. Barr (ed.), *Thomas Henry Huxley's Place in Science and Letters: Centenary Essays* (Athens, GA., University of Georgia Press, 1997), pp. 323–50.
- 34 Karl Pearson, 'Politics and Science' in *Chances of Death and Other Studies in Evolution*, 2 vols (London, Edward Arnold, 1897), vol. 1, pp. 148–9; L.S. Jacyna, 'Science and Social Order in the Thought of A.J. Balfour', *Isis*, 71 (1980), pp. 11–34.
- 35 Salisbury, 'Presidential Address' in *Report of the Sixty-fourth Meeting of the British Association for the Advancement of Science* (London, John Murray, 1895); Brian C. Shipley, "'Had Lord Kelvin a Right?" John Perry, Natural Selection, and the Age of the Earth, 1894–1895' in Cherry Lewis and Simon J. Knell (eds), *The Age of the Earth: From 4004 BC to AD 2002* (London, Geological Society, 2001), pp. 91–105.
- 36 Rayleigh, 'Presidential Address', *Scientific Papers*, vol. 2, p. 333.
- 37 Rayleigh, *Life*, pp. 209–19; Rev. J.P. Struthers, 'Argon', *The Morning Watch* (March 1895), Terling Place Archives.
- 38 Richard Noakes, 'Cranks and Visionaries: Science, Spiritualism, and Transgression in Victorian Britain', Ph.D. thesis, University of Cambridge, (1998); Frank Miller Turner, *Between Science and Religion: The Reaction to Scientific Naturalism in Late Victorian England* (New Haven, Yale University Press, 1974), pp. 38–67; Bart Schultz, *Henry Sidgwick: Eye of the Universe: An Intellectual Biography* (Cambridge, Cambridge University Press, 2004), p. 43, p. 82, pp. 247–8.
- 39 News clipping marked 'W.M.N.' (19 December 1879), Terling Place Archives.

- 40 'G.G. Stokes to J.W.S. Rayleigh, Lensfield Cottage, 23 April 1900', Terling Place Archives; David B. Wilson, 'A Physicist's Alternative to Materialism: The Religious Thought of George Gabriel Stokes', *Victorian Studies*, 28 (1984), pp. 69–96.
- 41 Stefan Colini, *Public Moralists: Political Thought and Intellectual Life in Britain, 1850–1930* (Oxford, Clarendon Press, 1993).
- 42 *Punch*, 76 (1879), p. 273; R.T. Glazebrook, 'The Rayleigh Period' in *A History of the Cavendish Laboratory, 1871–1910* (New York, Longmans, Green and Co., 1910), pp. 40–74; Simon Schaffer, 'Rayleigh and the Establishment of Electrical Standards', *European Journal of Physics*, 15 (1994), pp. 277–85.
- 43 Rayleigh, *Life*, pp. 139–40.
- 44 Clipping from *Cooperative News*, Terling, (5 July 1919), Terling Place Archives.
- 45 Ruth Barton, "'Huxley, Lubbock, and Half a Dozen Others": Professionals and Gentlemen in the Formation of the X Club, 1851–1864', *Isis*, 89 (1998), pp. 410–44.
- 46 In M. Foster, 'Introduction', in M. Foster and Adam Sedgwick (eds), *The Works of Francis Maitland Balfour*, 4 vols. (London, Macmillan, 1885), vol. 1, p. 23. Balfour's authoritative *A Treatise on Comparative Embryology*, 2 vols (London, Macmillan, 1880–1), largely formed the basis of this opinion.
- 47 T.H. Huxley to A. Dohrn, 24 September 1882, Imperial College Archives, Huxley Papers, 13/293–4.
- 48 M. Foster, 'Francis Maitland Balfour', *Proceedings of the Royal Society of London, Obituary Notices*, 35 (1882), p. xxi.
- 49 Henry Fairfield Osborn, *Impressions of Great Naturalists: Reminiscences of Darwin, Huxley, Balfour, Cope, and Others* (London, Charles Scribner's Sons, 1924), p. 103, p. xi.
- 50 Roy MacLeod, 'Embryology and Empire: The Balfour Students and the Quest for Intermediate Forms in the Laboratory of the Pacific' in Roy MacLeod and Philip F. Rehbock (eds), *Darwin's Laboratory: Evolutionary Theory and Natural History in the Pacific* (Honolulu, University of Hawaii Press, 1991), pp. 140–65.
- 51 H. Blackman, 'A Spiritual Leader? Cambridge Zoology, Mountaineering, and the Death of F.M. Balfour', *Studies in the History and Philosophy of Biological and Biomedical Sciences*, 35 (2004), pp. 93–117.
- 52 Marsha L. Richmond, "'A Lab of One's Own": The Balfour Biological Laboratory for Women at Cambridge University, 1884–1914', *Isis*, 88 (1997), pp. 422–55; J. Stanley Gardiner, 'The Zoological Department, Cambridge', Balfour and Newton Library Pamphlet (Cambridge, University of Cambridge, 1934).
- 53 The issue of *Journal of the History of Biology*, 34 (2001) with articles by Adrian Desmond, Richard Bellon, John C. Waller, and Samuel J.M.M. Alberti is devoted to this theme.
- 54 Rayleigh, *Life*, p. 228; Alter, *The Reluctant Patron*, p. 221; Jack Morrell, 'Individualism and the Structure of British Science in 1830', *Historical Studies in the Physical Sciences*, 3 (1971), pp. 183–204.
- 55 David Cannadine, *The Decline and Fall of the British Aristocracy* (New York: Vintage, 1999).

Chapter 13 Fresnel's Particular Waves: Models of Light as Catalytic Modes of Worldmaking in Early Modern Times

Bernd Klähn

- 1 Pointing out that 'pre-scientific and scientific discourse provides the proto-languages of the sciences', Gerald Holton, in *The Advancement of Science and Its Burdens* (Cambridge, Cambridge University Press, 1986), has clearly outlined the contours of projects moving in this or similar directions. The present essay sets out to illustrate within the aura of optical theories and their historical development, what kind of specific details and general positions may exemplify that 'scientific activity is, and has to be, part of a larger cultural metabolism'. (p. 251)
- 2 Arthur I Miller, *Imagery in Scientific Thought: Creating Twentieth-Century Physics*, (Cambridge MA, MIT Press, 1986) has presented a fascinating treatise on this question, by concentrating on the theme of visual thinking. Though the starting point in this essay may be rather similar, the results diverge considerably: where Miller postulates 'new vistas of imagery', this paper concentrates on the question if other, still more elementary, modes of worldmaking might take over the role of cognitive leitmotifs in scientific constructions of reality. See Miller p. 2; also pp. 309–12.
- 3 Representing the hard science advocates in this 'Science Wars', Alan Sokal, and Jean Bricmont in their *Fashionable Nonsense. Post-Modern Intellectuals' Abuse of Science* (New York, Picador, 1999) try primarily to disavow any intellectual activity in this direction as a consequence of 'mystification, deliberately obscure language, confused thinking, and misuse of scientific concepts'. But summing it all up under the rubric of 'intellectual practices', they fall prey to their own method: scientists are doing a similar job in a comparable milieu. (p. xi)
- 4 Arkady Plotnitsky, *The Knowable and the Unknowable: Modern Science, Non-classical Thought, and the 'Two Cultures'* (Ann Arbor, University of Michigan Press, 2002), p. 200.
- 5 Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, University of Chicago Press, 1960).
- 6 This is near to Michael Lynch's point of view; he points out that 'the content of the debate is philosophical', whereby he takes a position blurring any precise epistemological borderline between sociology and natural sciences deliberately. See Michael Lynch, 'Is a science peace process necessary?' in Jay A Labinger & and Harry Collins (eds), *The One Culture? A Conversation about Science* (Chicago, University of Chicago Press, 2001), pp. 48–60.
- 7 The subsequent reflections refer to Plato's *Politeia*, especially those chapters, where he concentrates on questions concerning human soul, the structure of the polis, and their relations to cosmological harmony. See Plato, *Politeia/Der Staat. Werke in 8 Bänden. Bd. 4* (Darmstadt, WBG, 1990), pp. 303–59.
- 8 Gerald Holton describes the Greek situation very insistently, already alluding explicitly to 'the style of thought and of the time', fostering 'criteria of pre-selection'. See: Gerald Holton, *Thematic Origins of Scientific Thought: Kepler to Einstein* (Cambridge, Harvard University Press, 1988) pp. 75–98.
- 9 Plato, pp. 37–97.

- 10 René Descartes, *Discours de la méthode* (Leyden, Ian Maire, 1637). Quotations from René Descartes, *Discours de la méthode* (Paris, Garnier-Flammarion, 1966), p. 60.
- 11 Descartes, *Discours*, pp. 185–99.
- 12 His critical remarks about the aesthetic appearance of a city, which can only be adequately composed by one architect, are in accordance with this statement: ‘souvent il n’y pas tant de perfection dans les ouvrages composés de plusieurs pièces, et faits de la main de divers maîtres, qu’en ceux auxquels un seul a travaillé. Ainsi voit-on que les bâtiments qu’un seul architecte a entrepris et achevés ont coutume d’être plus beaux et mieux ordonnés que ceux que plusieurs ont tâché de racommoder.’ (Descartes, *Discours*, p. 41)
- 13 Isaac Newton, *Philosophiae naturalis principia mathematica* (London, Pepys, 1687).
- 14 A lucid synopsis of Newton’s argumentation may be found in: Banesh Hoffmann, *Relativity and Its Roots* (New York, Freeman, 1983), pp. 31–33.
- 15 Isaac Newton, *Opticks* (London, Smith & Walford, 1704).
- 16 Newton, *Opticks*, pp. 270–3.
- 17 Newton, *Opticks*, pp. 318–24.
- 18 Francesco Maria Grimaldi, *Physico-mathesis de lumine, coloribus et iride* (Bologna, n.p., 1665).
- 19 ‘Grimaldo has inform’d us, that if a beam of the Sun’s Light be let into a dark Room through a very small hole, the Shadows of things in this Light will be larger than they ought to be ... and that these Shadows have three parallel Fringes, Bands or Ranks of colour’d Light adjacent to them.’ (Newton, *Opticks*, p. 317).
- 20 Newton, *Opticks*, pp. 362–63.
- 21 Newton’s rejection of an undulatory model of light is excessively pronounced, inducing him to attack a supporter of wave concepts – like Huygens – in extremely polemic ways (Newton, *Opticks*, pp. 363–65). Prominent members of the ‘wave group’ within the Royal Society, like Robert Hooke, even made him hold back the results of his optical research for decades. *Opticks* was published one year after Hooke’s death; it had been finished for more than twenty years, but Hooke’s harsh critique on some of Newton’s concepts made him desist from publishing it. See, Emilio Segrè, *From Falling Bodies to Radio Waves. Classical Physicists and Their Discoveries* (New York, Freeman, 1984), pp. 73–5.
- 22 Christiaan Huygens, *Traité de la lumière* (The Hague, n.p., 1690).
- 23 Still one of the best introductions into the conceptions and results of Huygens’ optical research is Vasco Ronchi, *Histoire de la lumière* (Paris, Armand Collin, 1956), pp. 196–213.
- 24 Augustin Fresnel, *Oeuvres complètes* (Paris, H. de Senarmont & Co., 1866), pp. 161–65.
- 25 G. Peacock, and I. Leitch (eds), *The Miscellaneous Works of the Late Thomas Young*, 1 (London, n.p., 1855), p. 80.
- 26 The physicist Emilio Segrè is a rare exception in his high praise of Fresnel’s achievement in modern physics: ‘If Faraday may be compared to Michelangelo, Fresnel would be a Benvenuto Cellini.’ (Segrè, *From Falling Bodies*, p. 96)
- 27 Segrè, *From Falling Bodies*, pp. 91–2.
- 28 Max Born, *Optik* (Berlin, Springer, 1972).
- 29 Albert Einstein, ‘Über einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtspunkt’ in *Annalen der Physik* 17 (1905), pp. 132–48.
- 30 Segrè, *From Falling Bodies*, p. 91.

Chapter 14 Repopulating Heaven: Victorian Lunar Studies and the Anxiety of Loneliness

David Clifford

- 1 'A new view of an old subject' in *Chambers's Journal* 5 (1856), p. 163.
- 2 'The mind fails to form an exact notion of a portion of space so immense [as the solar system]; but some faint idea of it may be obtained from the fact, that, if the swiftest race-horse ever known had begun to traverse it, at full speed, at the time of the birth of Moses, he would only as yet have accomplished half his journey'. Robert Chambers, *Vestiges of the Natural History of Creation* (London, John Churchill, 1844), pp. 1–2.
- 3 Of particular interest in relation to this essay is: John Hedley Brooke, 'Natural theology and the plurality of worlds: observations on the Brewster-Whewell debate' in *Annals of Science*, 34 (1977), pp. 221–86. But among a brief representative sample include also Robert M. Young, 'The impact of Darwin on conventional thought' in Anthony Symondson (ed.), *The Victorian Crisis of Faith* (London, SPCK, 1970), pp. 13–35; John C. Greene, 'Reflections on the progress of Darwin studies' in *The Journal of the History of Biology* 8 (1975), pp. 243–73; David L. Hull, *Darwin and his Critics* (Chicago, University of Chicago Press, 1973); Peter J. Bowler, *The Non-Darwinian Revolution: Reinterpreting a Historical Myth* (Baltimore, Johns Hopkins University Press, 1988).
- 4 For this latter, see especially Gillian Beer, '“The death of the sun”: Victorian solar physics and solar myth' in *Open Fields: Science in Cultural Encounter* (Oxford, Oxford University Press, 1996), pp. 219–41.
- 5 Marjorie Nicolson, *Voyages to the Moon* (New York, Macmillan, 1948).
- 6 Daniel Defoe, *The Consolidator* ed. by M. Seidel, M.E. Novak, and J.D. Kennedy (New York, AMS Press, 2001).
- 7 James Burnett, Lord Monboddo, *On the Origin and Progress of Language* 6 (Edinburgh, A. Kincaid & W. Creech, 1773–92); Thomas Burnet, *The Theory of the Earth* (London, W. Kettilby, 1691); Stephen Jay Gould, *Time's Arrow, Time's Cycle: Myth and Metaphor in the Discovery of Geological Time* (Harmondsworth, Penguin, 1987).
- 8 John Wilkins, *The Discovery of a New World; or, A Discourse tending to prove, that 'tis probable there may be another habitable world in the Moone. With a Discourse concerning the possibility of a Passage thither* (London, n.p., 1638); Anon, 'The discovery of a New World, &c.' in *The Retrospective Review* 8 (1823), pp. 335–42; Anon, 'The Lesser Light' in *The British Quarterly Review* 60 (1874), pp. 338–67.
- 9 Anon, 'Dr Wilkins's Prophetic Dreams' in *All the Year Round* 5 (1861), pp. 582–85.
- 10 The obvious exception to this statement is Jules Verne's *De la terre à la lune* (1865). But Verne's post-civil war satire on Yankee military adventurism was read in the emerging tradition of fantasy travel narrative rather than a feasible ambition for human endeavour.
- 11 Maximillian Schele de Vere, 'A Trip to the Moon' in *Putnam's Monthly Magazine* 5 (1855), pp. 337–45.

- 12 Thomas Chalmers, *A Series of Discourses on the Christian Revelation, viewed in connection with the Modern Astronomy* (Glasgow, J. Smith & Son, 1817); William Whewell, *Of the Plurality of Worlds: An Essay* (London, n.p., 1853); David Brewster, *More Worlds Than One: The creed of the philosopher and the hope of the Christian* (London, John Murray, 1854).
- 13 “Are you a Whewellite or a Brewsterite, or a t’othermanite, Mrs Bold?” said Charlotte, who knew a little about everything, and had read about a third of each of the books to which she alluded’. Anthony Trollope, *Barchester Towers* (Harmondsworth, Penguin, 1994) p. 160.
- 14 See in particular, Brooke, ‘Natural Theology’; Michael J. Crowe, *The Extraterrestrial Life Debate, 1750–1900* (Cambridge, Cambridge University Press, 1986), pp. 265–351; David Lewis, *On the Plurality of Worlds* (Oxford, Basil Blackwell, 1986); Rodney D Holder, *God, the Multiverse and Everything* (Aldershot, Ashgate, 2004). Of interest with regards the earlier debate is Steven J. Dick’s *Plurality of Worlds: The Origins of Extraterrestrial Life Debate from Democritus to Kant* (Cambridge, Cambridge University Press, 1982).
- 15 Maximillian Schele de Vere, ‘A Trip’, p. 338; ‘Anoki ben Ierah’, ‘The Moon’ in *Fraser’s Magazine* 2 (1868), pp. 375–90; ‘The Lesser Light’, p. 340. Rosse’s telescope was unmatched for resolution by even higher levels of magnification or by larger mirrors until the mid-twentieth century.
- 16 Schele de Vere, ‘A Trip’, p. 342.
- 17 ‘A New View’, p. 164.
- 18 ‘Anoki ben Ierah’, ‘The Moon’, p. 379.
- 19 Among the most useful, for example, one would include Richard Yeo, ‘Science and intellectual authority in mid-nineteenth-century Britain: Robert Chambers and *Vestiges of the Natural History of Creation*’ in *Victorian Studies* 28 (1984), pp. 5–31; Bernard Lightman, *The Origins of Agnosticism: Victorian Unbelief and the Limits of Knowledge* (Baltimore, Johns Hopkins University Press, 1987); Donald Mackenzie, *Statistics in Britain, 1865–1930: The Social Construction of Scientific Knowledge* (Edinburgh, Edinburgh University Press, 1981); Donald R. Benson, ‘Facts and constructs: Victorian humanists and scientific theories on scientific knowledge’ in James Paradis & Thomas Postlewait (eds), *Victorian Science and Victorian Values* (New York, Academy of Science Annals, 1981), pp. 299–318.
- 20 Whewell, *Of the Plurality of Worlds*, pp. 172–74.
- 21 George Leigh, ‘Are the Planets Inhabited?’ in *Once a Week* 9 (1863), pp. 80–2. I have been able to trace no background information on Leigh.
- 22 ‘Occultation of Jupiter, Jan 2’ in *Monthly Notices of the Royal Astronomical Society* 17 (1857), pp. 69–83.
- 23 ‘Fog-Seas of the Moon’, *Chambers’s Journal* 7 (1857), pp. 188–90. All quotations are from p. 89.
- 24 Richard A. Proctor, *The Moon: Her motions, aspect, scenery, and physical condition* (3rd edn, London, Longman, Green and Co., 1886), pp. 280–1. Proctor did also offer a counter-argument to the uniformitarian model of the planets.

Chapter 15 'You Should Get Your Head Examined': Freudian Psychoanalysis and the Limits of Nineteenth-Century Science

Peter Naccarato

- 1 Robert C. Bolles, *The Story of Psychoanalysis: A Thematic History* (California, Brooks/Cole Publishing, 1993), p. 100.
- 2 John C. Burnham, *Psychoanalysis in American Civilization before 1918* (Ann Arbor, UMI, 1973), p. 7.
- 3 Burnham, *Psychoanalysis*, p. 7.
- 4 Reuben Fine, *The History of Psychoanalysis*, (New York, Continuum, 1990), p. 10.
- 5 Burnham, *Psychoanalysis*, p. 7.
- 6 Fine, *History*, p. 5.
- 7 Fine, *History*, p. 6.
- 8 James Strachey, 'The Neuro-Pathology of Defence' in James Strachey (ed.), *The Standard Edition of the Complete Psychological Works of Sigmund Freud, Volume 3* (London: The Hogarth Press, 1981 [1962]), p. 64. All subsequent references to Freud's work will be by volume and page numbers from *The Standard Edition*.
- 9 Fine, *History*, p. 22.
- 10 For a detailed discussion of the personal relationship between Breuer and Freud, see the first volume of Ernst Jones, *The Life and Work of Sigmund Freud*, 3 vols. (New York, Basic Books, 1953–57).
- 11 Julie Thompson Klein, *Interdisciplinarity: History, Theory and Practice* (Detroit, MI, Wayne State University Press, 1990), p. 104.
- 12 Morton Hunt, *The Story of Psychology* (London, Doubleday, 1993), p. 177.
- 13 Hunt, *Story*, p. 203.
- 14 See also Seymour Fisher and Roger P. Greenberg, *The Scientific Credibility of Freud's Theories and Therapy* (New York, Basic Books, 1977), p. 4.
- 15 Fisher and Greenberg, *Scientific Credibility*, pp. 6–7.
- 16 Robert R. Sears, *Survey of Objective Studies of Psychoanalytic Concept* (New York, Social Science Research Council, 1943), p. 133.
- 17 Fisher and Greenberg, *Scientific Credibility*, p. 15.
- 18 Fine, *History*, p. 357.
- 19 Sears, *Survey*, p. 133.
- 20 See for example Charles Fisher's work, which uses studies on REM sleep to evaluate Freud's dream theory (specifically, 'A Study of the Preliminary Stages of the Construction of Dreams and Images', *Journal of the American Psychoanalytic Association*, 5 (1957), pp. 5–60; 'Subliminal and Supraliminal Influences on Dreams', *American Journal of Psychiatry*, 116 (1959), pp. 1009–17; 'Psychoanalytic Implications of Recent Research on Sleep and Dreaming', *Journal of the American Psychoanalytic Association*, 13 (1965), pp. 197–303.
- 21 Heinz Hartmann, 'Review of *Survey of Objective Studies of Psychoanalytic Concepts*, by Robert R. Sears', *The Psychoanalytic Quarterly*, 13 (1944), p. 101.
- 22 Hartmann, 'Review', p. 102.

- 23 Franz Alexander, *The Medical Value of Psychoanalysis* (New York, W.W. Norton & Co., 1932), p. 35.
- 24 Alexander, *Medical Value*, p. 162.

Chapter 16 Scholars, Scientists and Sexual Inverts: Authority and Sexology in Nineteenth-Century Britain

Heike Bauer

- 1 I want to express my thanks to the British Federation of Women Graduates for generously awarding me a Kathleen Tillotson Bursary and thus enabling this research. I am grateful to the Wellcome Institute, and especially Lesley A. Hall, for granting me access to the Krafft-Ebing papers.
- 2 See Gert Hekma, ‘“A Female Soul in a Male Body”: Sexual Inversion as Gender Inversion in Nineteenth-Century Sexology’ in Gilbert Herdt (ed.), *Third Sex, Third Gender: Beyond Sexual Dimorphism in Culture and History* (New York, Zone Books, 1994).
- 3 See Reinhard Wittmann, *Geschichte des deutschen Buchhandels: ein Überblick* (München, Verlag C.H. Beck, 1991), p. 245.
- 4 The only account of Chaddock’s life survives in form of an article written by two of his students: James O’Leary and Walter L. Moore, ‘Charles Gilbert Chaddock: His Life and Contributions’, *Journal of the History of Medicine and Allied Sciences*, 8, (1953), pp. 301–17. During his lifetime, Chaddock was established as a distinguished, well-connected psychiatrist with a special interest in neurology.
- 5 During Krafft-Ebing’s lifetime, *Psychopathia Sexualis* was also translated into Russian, Japanese, French, Italian, Hungarian and Dutch. See Harry Oosterhuis, *Stepchildren of Nature: Krafft-Ebing, Psychiatry and the Making of Sexual Identity* (Chicago, University of Chicago Press, 2000), p. 275.
- 6 The financial transactions reinforce the popularity of the work. Enke paid 2000 Mark for the rights of *Psychopathia Sexualis* (letter from Ferdinand Enke to Friedrich von Krafft-Ebing, 26.09.1907); Fuchs, who was chosen as the editor, received 1000 Mark for editing the 13th edition, plus 1500 Mark for each subsequent edition (letter from Alfred Enke to Friedrich von Krafft-Ebing, 16. 02. 1907). Both letters are held in the Krafft-Ebing papers at the Wellcome Institute, Ms. PP/KE Box 13, File 60.
- 7 Including Honorary Fellowships at the Chicago Academy of Medicine and the Neurological-Psychiatric Society of Moscow, as well as national Austrian accolades; Krafft-Ebing papers, Wellcome Institute, Ms. PP/KE Box 13, File 58.
- 8 Anon., ‘Obituary’, *British Medical Journal*, 1 (1903), p. 53.
- 9 Havelock Ellis, *My Life* (Toronto & London, William Heinemann, 1940), p. xii.
- 10 Havelock Ellis, ‘Thomas Hardy’s Novels’, *The Westminster Review*, 119 (April 1883), p. 334; Phylliss Grosskurth, *Havelock Ellis: A Biography* (New York, Knopf, 1980), pp. 58–59. See also R.L. Purdy and Michael Millgate (eds), *The Collected Letters of Thomas Hardy* (Oxford, Oxford University Press, 1978), p. 117.
- 11 Ellis, ‘Make Room for the Pioneer!’ in Havelock Ellis, *My Confessional: Questions of Our Day* (London, John Lane /The Bodley Head, 1934), pp. 53–4.

- 12 Ellis, *My Life*, p. 41.
- 13 John Addington Symonds, *The Memoirs of John Addington Symonds*, ed. Phyllis Grosskurth (London, Hutchinson, 1984), p. 239.
- 14 For a full list of Symonds's publications see Percy L. Babington, *Bibliography of the Writings of John Addington Symonds* (London, John Castle, 1925).
- 15 John Addington Symonds, *A Problem in Greek Ethics being an Inquiry into the Phenomenon of Sexual Inversion addressed especially to medical psychologists and jurists* (London, privately printed, 1901–1920), p. 1. The series of publication details indicates that Symonds's treatise was continually privately reprinted and circulated. It was written in 1873 and first published in 1883, when Symonds printed ten private copies. Here I consulted the copy held by the Wellcome Library, London. It is a first generation photographic reprint of the surreptitious edition of 1901. The range of possible dates of publication is due to the presence of another copy of the edition, which is thought to be a photographic reprint of the above edition. The second reprint bears the following inscription: 'Jan 5, 1920'. This suggests that the above cited first generation reprint could have been reprinted any time between the 1901 edition's publication and the date of the second generation reprint.
- 16 Symonds, *Memoirs*, p. 240.
- 17 John Addington Symonds, *A Problem in Modern Ethics being an Inquiry into the Phenomenon of Sexual Inversion addressed especially to medical psychiatrists and jurists* (London, privately printed, 1896), p. 3.
- 18 Symonds, *Memoirs*, p. 232.
- 19 Symonds, *A Problem in Modern Ethics*, n.p.
- 20 Karl Heinrich Ulrichs, *Prometheus* (Leipzig, Gerbe'sche Verlagsbuchhandlung, 1870), p. 19, translation is mine.
- 21 Symonds, *Memoirs*, pp. 271–9.
- 22 John Addington Symonds, 'Letter to Arthur Symons, June 1892' in Herbert M. Schueller and Roland L. Peters (eds), *The Letters of John Addington Symonds: Volume 3* (Detroit MI, Wayne State University, 1969), p. 691.
- 23 Ellis, *My Life*, p. 294.
- 24 Ellis, *My Life*, p. 295.
- 25 Ellis, *My Life*, p. 296.
- 26 Havelock Ellis and J.A. Symonds, *Das Konträre Geschlechtsgefühl*, trans. Hans Kurella (Leipzig, Georg H. Wigand, 1896).
- 27 Albert Moll, *Das Konträre Geschlechtsgefühl* (Berlin, n.p., 1891).
- 28 Ellis, *My Life*, p. 294. Like Krafft-Ebing, Moll regarded homosexuality as a naturally occurring variation.
- 29 Havelock Ellis and J.A. Symonds, *Sexual Inversion: Studies in the Psychology of Sex, Volume 1* (London, Wilson and Macmillan, 1897).
- 30 Symonds's daughter Margaret in her family biography makes no mention of her father's involvement in the work, nor does she at any point refer to his homosexuality. See Margaret Symonds, *Out of the Past* (London, John Murray, 1925).
- 31 Havelock Ellis, *Sexual Inversion: Studies in the Psychology of Sex, Volume 2* (London, University of Watford Press, 1897). Note that Ellis here renumbered *Sexual Inversion* as volume two of his *Studies*.
- 32 Ellis, *My Life*, pp. 297–8.

- 33 For the links between sexology and *The Well of Loneliness* see for example Heike Bauer, 'Richard von Krafft-Ebing's *Psychopathia Sexualis* as Sexual Sourcebook for Radclyffe Hall's *The Well of Loneliness*', *Critical Survey*, 15:3 (2003), pp. 23–38.
- 34 See Havelock Ellis, *A Note on the Bedborough Trial* (London, Watford University Press, 1898).
- 35 See for example Lucy Bland and Laura Doan (eds), *Sexology Uncensored: The Documents of Sexual Science and Sexology in Culture* (Cambridge, Polity Press, 1998); Franz X. Eder, Lesley Hall and Gert Hekma (eds), *Sexual Cultures in Europe*, 2 vols. (Manchester, Manchester University Press, 1999); Sander L. Gilman, 'Sigmund Freud and the Sexologists: A Second Reading' in Roy Porter and Mikuláš Teich (eds), *Sexual Knowledge, Sexual Science: The History of Attitudes to Sexuality* (Cambridge, Cambridge University Press, 1994); Chris Nottingham, *The Pursuit of Serenity: Havelock Ellis and the New Politics* (Amsterdam, Amsterdam University Press, 1999); Harry Oosterhuis, *Step-Children of Nature: Krafft-Ebing, Psychiatry and the Making of Sexual Identity* (Chicago, University of Chicago Press, 2000); John Pemble, (ed), *John Addington Symonds: Culture and the Demon Desire* (Basingstoke, Macmillan, 2000).

Chapter 17 Unmasking Immorality: Popular Opposition to Laboratory Science in Late Victorian Britain

Martin Willis

- 1 I am indebted to The Wellcome Trust and The Wellcome Library for the History and Understanding of Medicine for a research grant that enabled me to complete the majority of the research for this paper.
- 2 D.S.L. Cardwell, *The Organisation of Science in England* (London, Heinemann, 1972).
- 3 L. Huxley (ed.), *Life and Letters of Thomas Henry Huxley*, 2 vols (London, Macmillan, 1900) vol. 1, p. 100.
- 4 R.M. McLeod, 'The Support of Victorian Science' in Peter Mathias (ed.), *Science and Society* (Cambridge, Cambridge University Press, 1972), p. 171.
- 5 Peter Alter, *The Reluctant Patron: Science and the State in Britain 1850–1920* (Oxford, Berg, 1987), p. 52.
- 6 McLeod, 'Support of Victorian Science', p. 156.
- 7 Anon., (ed.), *A History of the Cavendish Laboratory 1871–1910* (London, Longman, Green and Co., 1910); Egon Larsen, *The Cavendish Laboratory: Nursery of Genius* (London, Edmund Ward, 1962).
- 8 Archive of the Lister Institute of Preventive Medicine, Special Collections, Wellcome Library for the History and Understanding of Medicine [SA/LIS/A-Z]. The notes following this refer only to the particular element of the archive under which the documents may be found. In this first instance the letter from 'Lanckester to Whitehead, 12 June 1889', would be found under C.1.
- 9 'Huxley to Whitehead, 25 June 1889', C.1.
- 10 'Lanckester to Whitehead, 28 June 1889', C.1.

- 11 'Lankester to Whitehead, 28 June 1889', C.1.
- 12 'Huxley to Whitehead, 25 June 1889', C.1.
- 13 '*The Lancet*, 28 July 1894', E.8.
- 14 '*The Gloucester Chronicle*, 13 October 1894', E.8.
- 15 '*The Echo*, 1 August 1894', E.8.
- 16 '*The Star*, 9 November 1894', E.8.
- 17 E.8.
- 18 '*The Bristol Mercury*, 1 June 1894', E.8.
- 19 '*The Bristol Mercury*, 1 June 1894', E.8.
- 20 '*Westminster News*, 22 January 1898', E.8.
- 21 '*The Star*, 3 March 1896', E.8.
- 22 A.W. Whalley, 'Survey of the Residents of Chelsea Gardens, 11 April 1894', E.1.
- 23 'Report of Parade and Mass Meeting at Pimlico', E.5.
- 24 'Report of Parade', pp. 2–6, E.5.
- 25 'Report of Parade', pp. 2–6, E.5.
- 26 'Minutes of the Board of Trade, 7 June 1894', E.1.
- 27 'Minutes', E.1.

Select Bibliography

- Alter, Peter, *The Reluctant Patron: Science and the State in Britain, 1850–1920* (Oxford: St Martin's Press, 1987).
- Anon. (ed.), *A History of the Cavendish Laboratory 1871–1910* (London: Longman, Green and Co., 1910).
- Bakhtin, M.M., *Speech Genres and Other Late Essays*, trans. by Vern W. McGee, ed. by Caryl Emerson and Michael Holquist (Austin: University of Texas Press, 1986).
- _____, *The Dialogic Imagination*, ed. by Michael Holquist, trans. by Caryl Emerson and Michael Holquist (1975; Austin: University of Texas Press, 1987).
- Batachelder, Eugene, (ed.), *A Romance of the Sea Serpent or the Ichthyosaurus and a Collection of Ancient and Modern Authorities* (Cambridge: John Bartlett, 1849).
- Becker, Lydia Ernestine, 'On the Study of Science by Women', *Contemporary Review*, 10 (1869), pp. 338–404.
- Bowler, Peter J., *Reconciling Science and Religion* (Chicago: University of Chicago Press, 2001).
- Bynum, W.F., and Roy Porter, *Medical Fringe & Medical Orthodoxy, 1750–1850* (London: Croom Helm, 1987).
- Cannadine, David, *The Decline and Fall of the British Aristocracy*, rev. edn (New York: Vintage, 1999).
- Cardwell, D.S.L., *The Organisation of Science in England* (London: Heinemann, 1972).
- Carpenter, Dr W.B., F.R.S., 'The Radiometer and its Lessons', *Nineteenth Century*, (April 1877), pp. 242–56.
- _____, W.B., C.B., M.D., LL.D., F.R.S., 'Psychological Curiosities of Spiritualism', *Fraser's Magazine*, (November 1877), pp. 541–64.
- Cayleff, Susan, *Wash and Be Healed: The Water-Cure Movement and Women's Health* (Philadelphia: Temple University Press, 1987).
- Collini, Stefan, *Public Moralists: Political Thought and Intellectual Life in Britain, 1850–1930* (Oxford: Clarendon Press, 1993).
- Cooter, Roger, *The Cultural Meaning of Popular Science: Phrenology and the Organisation of Consent in Nineteenth-Century Britain* (Cambridge: Cambridge University Press, 1985).
- _____, and Steven Pumfrey, 'Separate Spheres and Public Places: Reflections on the History of Science Popularization and Science in Popular Culture', *History of Science*, 32 (1994), pp. 237–267.

- Cowling, Mary, *The Artist as Anthropologist: The Representation of Type and Character in Victorian Art* (Cambridge: Cambridge University Press, 1989).
- Crabtree, Adam, *Animal Magnetism, Early Hypnotism, and Psychical Research, 1766–1925: An Annotated Bibliography* (New York: Kraus International, 1988).
- Crookes, William, 'Another Lesson from the Radiometer', *Nineteenth Century*, (July 1877), pp. 879–87.
- Crowe, Michael J., *The Extraterrestrial Life Debate, 1750–1900* (Cambridge: Cambridge University Press, 1986).
- Dick, Steven J., *Plurality of Worlds: The Origins of Extraterrestrial Life Debate from Democritus to Kant* (Cambridge: Cambridge University Press, 1982).
- Elkana, Yehuda, *The Discovery of the Conservation of Energy* (London: Hutchinson, 1974).
- Evans, Elizabeth C., 'Physiognomics in the Ancient World', *Transactions of the American Philosophical Society*, 59 (1969), pp. 5–97.
- Faivre, Antoine, and Jacob Needleman (eds), *Modern Esoteric Spirituality* (New York: Crossroad, 1992); vol. 21 of *World Spirituality: An Encyclopedic History of the Religious Quest*.
- Gates, Barbara T., (ed.), *In Nature's Name: An Anthology of Women's Writing and Illustrations, 1780–1930* (Chicago: University of Chicago Press, 2002).
- Gillispie, C.C., *Genesis and Geology* (Cambridge, MA: Harvard University Press, 1951, repr. 1996).
- Gooding, David, 'Metaphysics versus measurement: the conversion and conservation of force in Faraday's physics', *Annals of Science*, 37 (1980), pp. 1–29.
- Graham, John, *Lavater's 'Essays on Physiognomy': A Study in the History of Ideas* (Berne: Peter Lang, 1979).
- Gully, James Manby, *The Water Cure in Chronic Disease* (London: J. Churchill, 1847).
- Haynes, Renée, *The Society for Psychical Research 1882–1982: A History* (London: Macdonald, 1982).
- Heuvelmans, Bernard, *In The Wake of Sea Serpents*, trans. by Richard Garnet (New York: Hill and Wang, 1968).
- Hunter, Archibald, *Health, Happiness and Longevity* (Glasgow: J. Menzies & Co., 1885).
- Kelley, Audrey, *Lydia Becker and the Cause* (Lancaster: Centre for North-West Regional Studies, University of Lancaster, 1992).
- Kingsford, Anna, and Edward Maitland, *The Perfect Way, or, The Finding of Christ* (London: 1882; revised edition 1887; repr. Montana: Kessinger Publishing, 1997).
- Klancher, Jon P., *The Making of English Reading Audiences 1790–1832* (Wisconsin: University of Wisconsin Press, 1987).
- Kuhn, Thomas, 'Energy conservation as an example of simultaneous discovery' in *Critical Problems in the History of Science* (1959; repr. Madison: University of Wisconsin Press, 1969).
- Larsen, Egon, *The Cavendish Laboratory: Nursery of Genius* (London: Edmund Ward, 1962).
- Laurence, Jean-Roch and Campbell Perry, *Hypnosis, Will, and Memory: A Psycho-Legal History* (New York: Guilford Press, 1988).

- Lightman, Bernard, *The Origins of Agnosticism: Victorian Unbelief and the Limits of Knowledge* (Baltimore: Johns Hopkins University Press, 1987).
- _____, (ed.), *Victorian Science in Context* (Chicago: University of Chicago Press, 1997).
- Livingstone, David N., D.G. Hart and Mark A. Noll (eds), *Evangelicals and Science in Historical Perspective* (Oxford: Oxford University Press, 1999).
- Lynch, J.M., *Vestiges and the Debate Before Darwin*, 7 vols (Bristol: Thoemmes Press, 2001).
- Lyell, Charles, *A Second Visit to the United States of North America*, 2 vols (London: John Murray, 1847).
- Mackenzie, Donald, *Statistics in Britain, 1865–1930: The Social Construction of Scientific Knowledge* (Edinburgh: Edinburgh University Press, 1981).
- MacLeod, Roy, and Peter Collins (eds), *The Parliament of Science: The British Association for the Advancement of Science, 1831–1981* (Middlesex: Science Reviews Ltd, 1981).
- Mathias, Peter, (ed.), *Science and Society* (Cambridge: Cambridge University Press, 1972).
- Morrell, Jack, and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (London: Royal Historical Society, 1984).
- Neeley, Kathryn A., *Mary Somerville: Science, Illumination, and the Female Mind* (Cambridge: Cambridge University Press, 2001).
- Noakes, Richard John, ‘“Crank and Visionaries”: Science, Spiritualism and Transgression in Victorian Britain’ (doctoral thesis, University of Cambridge, 1998).
- Oppenheim, Janet, *The Other World: Spiritualism and Psychical Research in England, 1850–1914* (Cambridge: Cambridge University Press, 1985).
- Owen, Alex, *The Darkened Room: Women, Power and Spiritualism in Late Victorian England* (London: Virago Press, 1989).
- Palfreman, John, ‘Between Scepticism and Credulity: A Study of Victorian Scientific Attitudes to Modern Spiritualism’ in *On the Margins of Science: The Social Construction of Rejected Knowledge*, ed. by Roy Wallis, Sociological Review Monograph, 27 (Staffordshire: University of Keele Press, 1979), pp. 201–36.
- Phillips, Patricia, *The Scientific Lady: A Social History of Women’s Scientific Interests, 1520–1918* (London: Palgrave Macmillan, 1991).
- Pick, Daniel, *Faces of Degeneration: A European Disorder, c.1848–c.1918* (Cambridge: Cambridge University Press, 1989).
- Prothero, I.J., *Artisans and Politics in Early Nineteenth-Century London: John Gast and his Times* (London: Methuen, 1979).
- Rabinbach, Anson, *The Human Motor: Energy, Fatigue, and the Origins of Modernity* (New York: Basic Books, 1990).
- Rudwick, Martin, ‘Uniformity and Progression: Reflections in the Structure of Geological Theory in the Age of Lyell’ in *Perspectives in History of Science and Technology*, ed. by D. Roller (Norman: University of Oklahoma Press, 1971).
- _____, M.J.S., *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985).

- Rupke, Nicolaas, *Richard Owen, Victorian Naturalist* (New Haven: Yale University Press, 1994).
- Secord, Anne, 'Science in the Pub: artisan botanists in early Nineteenth-Century Lancashire', *History of Science*, 32 (1994), pp. 269–315.
- Secord, J., *Victorian Sensation* (Chicago: Chicago University Press, 2000).
- Shortland, M., *Hugh Miller and the Controversies of Victorian Science* (Oxford: Clarendon Press, 1996).
- Shteir, Ann, "Elegant Recreations?: Figuring Science Writing for Women", in *Victorian Science in Context*, ed. Bernard Lightman (Chicago: University of Chicago Press, 1997).
- Smith, Crosbie, and Jon Agar (eds), *Making Space for Science: Territorial Themes in the Shaping of Knowledge* (Basingstoke: Macmillan, 1997).
- Smith, Crosbie, *The Science of Energy: A Cultural History of Energy Physics in Victorian Britain* (Chicago: University of Chicago Press, 1998).
- Smith, Roger, *Inhibition: History and Meaning in the Sciences of Mind and Brain* (Berkeley: University of California Press, 1992).
- Turner, Frank Miller, *Between Science and Religion* (New Haven and London: Yale University Press, 1974).
- Wallace, Alfred R., F.R.S., 'Psychological Curiosities of Scepticism: A Reply to Dr. Carpenter', *Fraser's Magazine*, (December 1877), pp. 694–706.
- Westrum, Ron, 'Knowledge About Sea Serpents', in *Sociological Review Monograph*, 27, ed. by Roy Wallis (Staffordshire: University of Keele Press, 1979), pp. 298–300.
- Wharton, Francis, 'Spiritualism and Jurisprudence', *Lippincott's Magazine*, (October 1875), pp. 423–33.
- Wiener, Martin J., *English Culture and the Decline of the Industrial Spirit, 1850–1980*, 2nd edn (Cambridge: Cambridge University Press, 2004).
- Winter, Alison, *Mesmerized: Powers of Mind in Victorian Britain* (Chicago: University of Chicago Press, 1998).
- Wise, M. Norton, and Crosbie Smith, 'Work and waste: political economy and natural philosophy in nineteenth-century Britain', *History of Science*, 27 (1989), pp. 263–301, pp. 391–449 and 28 (1990), pp. 221–61.
- Wolpert, Lewis, *The Unnatural Nature of Science* (London: Faber, 1992).
- Yeo, Richard, *Defining Science: William Whewell, Natural Knowledge, and Public Debate in Early Victorian Britain* (Cambridge: Cambridge University Press, 1993).

