

CONTENTS

Pillow Talk: Credibility, Trust and the Sexological Case History Ivan Crozier	375
Galileo, Bruno and the Rhetoric of Dialogue in Seventeenth-century Natural Philosophy Stephen Clucas	405
Liars, Experts and Authorities Graeme Gooday	431
Science, Scientific Careers and Social Exchange in London: The Diary of Herbert McLeod, 1885–1900 Hannah Gay	457
Notices of Books	497
Notes on Contributors	499
Index to Volume 46	500

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PILLOW TALK: CREDIBILITY, TRUST AND THE SEXOLOGICAL CASE HISTORY

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INTRODUCTION

In the last part of the nineteenth century, a new field emerged within psychiatry and psychology that was specifically concerned with human sexuality. This field — sexology — developed theories of sexual behaviour and recorded observations of sexual activity based on case histories of various ‘perversions’ (initially homosexuality, but soon afterwards sadism, masochism, fetishism, etc.). These case histories were enrolled in sexological texts as the basis for scientific speculation. To use Ian Hacking’s formulation, sexology “made up” the “kinds of people” who can be found in these case histories.¹ It is the uses of these cases, and the sociological issues that surround the construction of sexological knowledge based on them, that will be addressed in this paper.

Sexology differs from other types of medicine. First, there was much trouble with defining what is to be counted as sexual: Desire? Lifestyle? Penetration? Other forms of stimulation? These issues are often not as important in, say, oncology or internal medicine, when the patient is either sick or well, has cancer or not. Furthermore, unlike when a sick patient goes to his or her doctor with certain symptoms caused by an illness that the doctor can interpret from the body through intensive testing, the doctor is rarely there when the subject has sex. And even on the odd occasions when the sexologist was there during intercourse — such as with the people who had sex on a clinical bench with William Masters and Virginia Johnston poking about and taking scientific measurements, resulting in the best-selling *Human sexual response* (1966) — the doctor had to trust the patient’s report about what they were thinking and feeling during the act. Sexologists want this information, as many are interested not only in what people do for sex, and how people’s bodies respond physically, but also in the psychological aspects of sexuality. There are very few meaningful physical signs of sexuality that can tell sexologists much about people’s sexual desires and practices, so psychological information is the basis of sexological knowledge.² This information gathered from the patient is converted into a case history, and is then circulated in the field of sexology.

APPROACHES TO MEDICAL CASE HISTORIES

Case histories are central to medical writing. They are the raw material for medical knowledge claims, acting rather like the data appendix in a scientific paper from another field. Cases are such a familiar trope in medical writing that they are

unproblematically presented in medical journals and textbooks without needing any form of justification. But this unproblematic status does not mean that cases are identical. As Harriet Nowell-Smith notes in her survey of cases in Canadian obstetric medicine, “Nineteenth-century case histories transformed individuals’ bodies into something statistically regular and understandable”.³ The practice of making perversions “regular and understandable” was the central aim of sexologists in the late nineteenth century, and more recently whenever something new has appeared (I will use examples of both below). It is the use of case histories in this way that makes sexology a medical discipline, rather than simply musing about sexuality.

In clinical medicine, cases were important to everyday work. For example, Charles Lyman Greene, Professor of Clinical Medicine at the University of Minnesota College of Medicine, emphasized in 1910 that “*case-taking, reading and reporting should be carried into everyman’s practice*. Old casebooks well kept are wells of knowledge, and the science of medicine would be greatly enriched were the workers in the city and hamlet alike to give to it reports of the unusual cases now for the most part allowed to pass without record”.⁴ Likewise, Noble Chamberlain, Lecturer in Clinical Medicine at the University of Liverpool in the 1930s, stressed the centrality of the case to practice, writing:

When the history of the case is complete, the physician should have a mental picture not only of the patient’s present symptoms, but of the manner in which these developed and of the type of background of personal and family life upon which they have been grafted. Too often we have been rightly accused of studying the disease rather than the patient.⁵

This standpoint can be seen in current medical writing when cases are written up, and they take a codified form nowadays known as the S.O.A.P. method: Subjective (history) data; Objective (clinical) data; Assessment; Plans for treatment. Julia Epstein argues that by writing in such a disinterested, objective style, the S.O.A.P. method of case reporting allows for a reading which ignores all personal issues, and instead focuses on the medically ‘relevant’ issues.⁶

This reduction of the patient to their salient medical features is criticized by Katheryn Montgomery Hunter, who argues that medicine should do more to think about the individual in the case, rather than merely construct a reductionist history of a disease process. She protests that “The medical case is not the patient’s story”.⁷ Hunter, like Oliver Sacks, is keen to put the person back into the case study. For Sacks, medical case histories are

a form of natural history — but they tell us nothing about the individual and *his* case history; they convey nothing of the person, and the experience of the person, as he faces, and struggles to survive, his disease. There is no ‘subject’ in a narrow case history; modern case histories refer to the subject in a cursory phrase ... which could as well apply to a rat. To restore the human subject at the center ... we must deepen the case history to a narrative or tale.⁸

Nevertheless, there are reasons that medical case histories do not reflexively address the case history as a literary form; it is assumed that all medical writing will be written in a passive, unbiased voice. This voice-without-compassion was not always a feature of medical writing. As the cultural historian Thomas Laqueur has shown, the modern medical case developed initially as a form of humanitarianism, which was interested in the human details expounded in the narrative.⁹ But as a number of medical historians have demonstrated, such humanism declined in the context of clinical medicine.¹⁰

One of the key ways of writing the history of medicine that addresses individual lives has come from 'historians-from-below', a part of the Marxist and, more recently, cultural and feminist historiographical lineage.¹¹ Medical historians-from-below examine some of the ways that patients have reacted to the differing interpretations of their cases, or focus on the doctor/patient relationship as a power relation. A central way of accessing cases from below is to consider patients' voices. One of the earliest proponents of this (medical) historiography was Barbara Duden, who reconstructed the lives and experiences of women, rather than contemporary medical ideas, from a doctor's casebook.¹² Roy and Dorothy Porter pioneered this approach in England,¹³ following the axiomatic programme set out by Roy Porter in 1985.¹⁴

In the history of sexuality, attention to the patients' voice over and against doctors' writing comes from Harry Oosterhuis, Alice Dreger, and Joy Dixon.¹⁵ This work on the patient in a medical text is important, as often in these texts, patients' 'confessions' were rewritten by the medical practitioner, thus removing the bulk of the patient's voice from the account (usually excepting minor quotations). Some sexologists, however, such as Richard von Krafft-Ebing, used a large amount of autobiographical material, especially in the later editions of his *Psychopathia sexualis*. While the history-from-below makes a similar use of the case study as some of the authors in the literary tradition — by examining the patients' 'side' in a medical engagement — these cultural-histories do not try to elucidate what the case is doing in medicine, nor do they relate it back to central epistemological issues which are raised whenever a doctor writes anything for publication in the field of medicine.

John Forrester argues that psychoanalysis — and by implication other human sciences — is different from experimental psychology and other evidence-based medicine because it does not think in terms of statistical evidence, but in terms of cases.¹⁶ Fundamental to the writing of a psychoanalytic text is an account of a patient. Individual difference — and the related conjectural style of reasoning — is also analysed by Carlo Ginzburg, who argues that "medical semiotics, or symptomatology — the discipline which permits diagnosis ... on the basis of superficial symptoms or signs" — was key to a heuristic tradition which both preceded and survived the development of a mathematico-scientific method in the seventeenth century.¹⁷ In this conjectural method, an 'individualizing' technique was used which concentrated on personal differences as clues to an underlying reality. This is the central method, according to Ginzburg, of the human sciences. It is "indirect, based on signs and scraps of evidence".¹⁸ As it is an interpretative style of reasoning, this semiotic method

relies on abstraction — often subjective abstraction — from particular cases which can be held up as having particular qualities which make it individual.

Michel Foucault addressed this issue when speaking of the development of the human sciences, noting that:

The examination, surrounded by all its documentary techniques, makes each individual a 'case' The case is ... the individual as he may be described, judged, measured, compared with others, in his very individuality.... This turning of real lives into writing is no longer a procedure of Heroization; it functions as a procedure of objectification and subjugation.... The examination as the fixing, at once ritual and 'scientific', of individual differences, as the pinning down of each individual in his own particularity ... clearly indicates the appearance of a new modality of power in which each individual receives as his status his own individuality, and in which he is linked by his status to the features, the measurements, the gaps, the 'marks' that characterize him and make him a 'case'.¹⁹

Foucault was interested in the way that individuals are 'converted' into texts through a number of literary technologies, including medicine. It is the focus on individual difference that is key to Foucault's conception of what constitutes a case. The individual in the human sciences is considered not in terms of their similarities to general characteristics in populations, but to their deviations from the norm that are annotated and analysed. This is how sexological cases were used, although the compilation of their differences created a sense of the 'normal perversion': the type was established with a conception of 'normal sexuality' in mind.²⁰

These issues of case-taking, and the various relationships between the 'individual' and the medical professional, do not go far enough in examining the uses of case histories by doctors, or the social processes that surround their collection and deployment. It would be wrong to think of cases as unprocessed relics of people's sex lives, even when they are written in the first person — as they often were. Rather, we would do well to pick up on some of the hints provided by Foucault.

Even beyond Foucault's (correct) assessment of the uses of cases, we can look at various social processes that were fundamental to the construction of sexological knowledge claims. Of particular worth is attention towards the issues of trust and credibility in the field of sexology. The issue of trusting what the patient reports is especially important during doctor–patient interactions: when patients write to doctors for advice about sexual issues and problems; when doctors interview patients seeking the details of homosexuality, cross-dressing, etc.; when other doctors refer new patients to a sexologist. These were all typical ways for sexologists to gather the material necessary to turn case histories into texts about sexuality in the period. It is not too much to claim that trust is central to sexological practice at a number of levels, from the initial encounter between the doctor and the patient, through to the production of credible scientific facts about human sexuality. Trust is important, because there are even fewer testing mechanisms in sexology than in other sciences — as patients are individuals, so another sexologist cannot replicate a sexological

claim exactly.²¹ Credibility in sexology relies on trust more than in many other fields of scientific research precisely because of the nature of evidence-gathering in sexology, not to mention the social opprobrium in which many types of sexual behaviour in which sexologists were interested were held, which affected patient responses.²² But before addressing these issues in detail, it is profitable to more generally examine how sexologists use cases in their work.

CASE HISTORIES

In this section, the main issues I want to consider briefly are: (1) the pedagogical case history: how sexologists are taught to think in cases; (2) choice of case history: how cases are selected for publication, both theoretically and practically; (3) meaning of case history: how doctors frame case histories, and how they are interpreted; and (4) negotiations about what cases mean: how other doctors reinterpret cases.

1. *The Pedagogical Case History: How Doctors Are Taught to Think in Cases*

The importance of the case history to medical training is two-fold. First, new students need cases that display particular symptoms or signs that are to be interpreted through the theoretical medical system that the students are also taught. This characterization is true of clinical medicine in general, where enculturation into modes of practice is employed.²³ Such a medical framework provides the general guiding principles for appreciating the differences between pathology and normality.²⁴ In sexology, however, training does not operate in the same way as in clinical medicine in the period we are looking at, as it is essentially a field constituted of established (usually psychiatric) practitioners branching out into a new subject area, and as such does not involve the same tacit component — as these sexologists are never ‘trained’ or ‘enculturated’ to see cases of perversions.²⁵ In sexology, cases are distributed through publications in psychiatric journals, relying on the linguistic differentiation of the world into categories that are learned through practice and through accepted ‘correct’ classification. That is, sexological cases are something of a substitute for ‘real’ medical experience; sexologists simply read about perversions and relate them to their own clinical practices. They operate through ostension: “the ingredient which knots terms with the environment itself”, and many of Barry Barnes’s ideas about how students are taught in science are still germane to how sexologists learn about new objects.²⁶ This relationship between theory and practice is important. Case histories are proxies of the experiences with which the theory ‘knots’. These case notes are the interface between what the student knows and what the patient exhibits. These notes become essential to understanding the condition of the patient, and show what the doctor (or neophyte medic) taking the case thought was important. By the time the case history is ready for publication, it contains the relevant and important details necessary to transmit knowledge for an accurate classification, while avoiding superfluous information which is already accepted by the field. The sexologist learns the features of particular ‘perversions’ from a text (once Krafft-Ebing’s *Psychopathia sexualis*, now

DSM IV—TR), which allows them to classify an individual according to the accepted schema, or if they have enough authority in the discipline, to eventually change it.

The aspect of medical pedagogy that is important to this paper is that which takes place at the established professional level: the presentation of case material to doctors who are already qualified, but who may be interested in reading about a new phenomenon. Here the case history has the effect of extending the stock of knowledge in a field, by coming up with a new object of inquiry, or by showing (in a particular case) how a previous interpretation of a phenomenon was incorrect, or by putting forward a new interpretation.²⁷ In all instances, the function of the published medical case history in the journal maintains its pedagogical function. Some examples of this professional pedagogy are discussed later in this paper. In particular, I will address the ‘first’ case of homosexuality described by Karl Westphal.

2. Choice of Case History: How Cases Are Selected for Publication, Both Theoretically and Practically

Case histories are not natural objects, but are very much reliant upon a field of knowledge. Medical knowledge does not develop by theory-explanation alone, but also requires practice at a fundamental level. This practice involves actually interpreting patients exhibiting symptoms.²⁸ As the field changes, and the stock of knowledge of this field changes, then the cases on which medical knowledge is based also change. This is either by being re-interpreted (see Section 4 below) or by having ‘better’ or newer cases selected which exhibit an issue more appropriately for the current state of knowledge in a field.

Selection of cases to be written up as knowledge claims owes everything to the field. This is one reason why cases from different disciplines are very different, even when they address the same phenomena (compare a sexological case history of homosexuality to Freud’s ‘Rat Man’, for instance, where homosexuality is essential to the case, but in drastically different ways). Cases are selected for publication because they embody the traits that are supposed to be the standard for a disease (for example, textbooks), or show significant variations from this accepted standard (new or unusual cases reported in the professional literature).

The process of selecting cases is also worth mentioning. Cases are usually gathered in clinical practice. Patients regularly exhibit symptoms or anxieties, but occasionally something noteworthy turns up — a new perversion or act, in the case of sexology. These cases are described by highlighting their differences to other ‘normal’ cases (either ‘normal’ in general, or ‘normal’ for that particular condition). This is not the only way that cases are gathered, however. For instance, when writing about ‘Eonism’ (cross-dressing), Havelock Ellis — England’s premier sexologist — asked the Australian sexologist Norman Haire if he had come across any good cases:

Just now I am getting my study on transvestism into shape. (I call it Eonism, after Chevalier d’Eon, as I do not agree that cross-dressing is always the most essential feature). I have 7 or 8 good cases. If you happen to have one I would be likely

to find of interest I should be pleased. I do not include homosexual cases, and at all events only when the homosexual element is clearly a secondary feature. There are no women among the primary cases; for in them the homosexuality always seems primary.²⁹

Ellis specified that he did not want homosexual drag-queens, but that he wanted actively heterosexual men who were aroused by wearing women's clothing. This was because he thought that homosexual men who wore women's clothing were predominantly homosexual, with a concomitant Eonistic streak that related to their gender inversion, whereas heterosexual cross-dressers were mainly gender-inverted, but this did not affect their heterosexuality — even though it was often referred to as “feeble”.³⁰ These informal selection processes were not, however, described in the final text, *Eonism* (1928). I use this example to show how sexologists chose cases that suited their theoretical aims (rather than waiting for patients to approach them), although there were of course many other ways to obtain a case history. Another common way of gathering cases was when the patient wrote to the doctor, explaining their condition — something that we see with the early sexologists such as Krafft-Ebing and the homosexuals and other ‘perverts’ who wrote to him.³¹ Again this does not mean that the doctor was constrained by what these patients said, but rather selected the cases to be written up as they were appropriate to the sexologists' aims. Not all of Krafft-Ebing's ‘perverted’ correspondents were suitable case histories for *Psychopathia sexualis*.

Case histories were selected for publication because they did something useful for medical knowledge. There was no single way that sexological cases are selected, because patients were not ‘normal’: they often decided for themselves that they wanted medical assistance to understand their desires, or — significantly — they were sent for psychiatric evaluation because they had committed a sex crime. In both instances, there was an impetus from outside regular medical channels that allowed doctors access to the material that would be turned into sex psychology.

3. *The Meaning of a Case History: How Doctors Frame Case Histories, and How They Are Interpreted*

To repeat myself, the doctor published a case because it had some meaning. It was new, or typical, or demonstrated significant changes in the practice of medicine, or was an excellent teaching device (the context of publication is important here: teaching cases differ from new cases in the literature). Cases embody changes in theory and practice, which is why they cannot be read as simplistic accounts that have not been processed — even though they are often presented as such by the author. Cases are written up in two major ways: they are third-person accounts by the doctor, which are descriptive and impersonal; or, they are first-person narratives presented by the doctor in the patients' words. In the latter cases, there is always an introduction and conclusion written by the doctor. That is why the historian must be careful when using these cases: it is risky to remove these patients from the context of a medical

encounter, even when they are ‘telling’ their own lives.³² Furthermore, although they are presented in first-person, it is often difficult to determine how much of a guiding hand the doctor used, or how much the patient was emulating other medical case histories. Either way, it remains the fact that the case was selected as publishable that allows us to see such ‘*memoires*’ in the first place, so again it is difficult to accept these cases at face value as some historians do.

I will give two brief examples, again from Havelock Ellis. When writing *Sexual inversion* (1897), initially co-authored with the poet and art historian, John Addington Symonds, who died before the project was completed, Ellis and Symonds discussed the questions that they would get patients to address based on existing sexological texts (especially those by Krafft-Ebing and Albert Moll). These cases were then gathered together by Ellis and re-written in a passive third-person voice. But they were not patients merely writing to Ellis off their own bat; they were sought out by exploiting the homosexual and lesbian networks of Ellis’s and Symonds’s friends, and were being directed by the questionnaire to give certain information required by Ellis and Symonds. These respondents were emphasized by Ellis to be different from the neuropaths and the criminals that appeared in other sexological cases.³³

In another example — *Eonism*, again — Ellis included a number of first-person case histories. All of these seem to be letters written to Ellis until one examines them more closely, and notices that they all deal with the same issues in the same order: early sexual experience; first exhibition of cross-dressing; heterosexuality; the way that cross-dressing is incorporated into sexual practices; abhorrence towards homosexuality. These traits are the ones that Ellis also suggested were the key diagnostic features of Eonism (in the above-cited letter to Haire, and in the book). This indicates some kind of guidance by Ellis (although exactly what kind is difficult to establish, as the surviving correspondence does not deal with this issue). Again, the processes of selecting and interpreting case histories are not presented in the final text. While case histories do much work, they belie the writing practices employed by Ellis. There is no reason to assume that these are atypical practices in sexology; Ellis’s cases are very similar to other sexologists like Albert Moll, Magnus Hirschfeld, etc., all of whom found his work very acceptable.

Rarely, there are cases where the patient resists a particular interpretation. For example, Havelock Ellis wrote a long article about a urolagnic masochist whom he called ‘Florrie’ in the *Psychoanalytic review* (1919), which did not involve a psychoanalysis, but rather was a case of Ellis’s aiding Florrie to come to terms with her condition, and thus helping her to lead a fulfilling life. The renegade psychoanalyst Wilhelm Stekel did not agree with Ellis’s interpretation, and suggested that Ellis must have missed out crucial information in his report, because Stekel’s theoretical commitments to psychoanalysis required certain ‘life events’. For instance, Stekel believed that “The case shows very beautifully the struggle between masculine and feminine tendencies and as a result the flight into pronounced infantilism”³⁴ (which was manifest in Florrie’s urolagnia, and — according to Stekel — her interest in zoophilia). Stekel concluded his discussion of the case (which explored aspects of

Florrie's vivid dreams recorded by Ellis) by writing: "It is certain that Florrie wanted to be a man and envied men the penis."³⁵

Ellis did not accept many of Stekel's commentaries on his case. Some of Stekel's claims, such as that Florrie must have seen "animals coupling" as a child, were repudiated: "Florrie assures me she made no such observations of animals."³⁶ He also denied the veracity of Stekel's claim that she must have had some sexual play with her brothers, and that she must have manifested a sadistic impulse towards animals. Nor had Florrie been given a rectal enema, which Stekel suggested would have stimulated Florrie's scatologic and klismaphiliac interests. Other areas of Stekel's analysis criticized by Ellis included his attention to her dreams, his interpretation of her relationships with her father and her mother, and the insistence that the only proper therapy could derive from psychoanalysis. Florrie herself preferred Ellis's analysis of her case over Stekel's: "I think it is perfectly marvelous how you have done this, & I know it was all so mixed up in my letters, & you have managed to produce order out of chaos."³⁷ She did not agree with the analytic approaches to her life, and felt that Ellis had given her the means of living her life to the fullest, while remaining a satisfied masochistic urolagniac.³⁸

4. Negotiations about What Cases Mean: How Other Doctors Reinterpret Cases

Once a case is published, that does not mean that it has to be accepted by the field. Indeed, often there is much interpretation of cases after they have been published. An extreme and atypical example of this is the vast amount of work that has been done on Freud's cases, especially since the 1970s.³⁹ This work has been predominantly to 'debunk' Freud — although nothing like the same amount of work has been expended on the hundreds of other psychoanalytic cases, let alone the huge number of other 'star cases' like Ellen West (Ludwig Binswanger), Hélène Smith (Théodore Flournoy), etc.⁴⁰ But the important point here is that other doctors have often reinterpreted cases.

In order to illustrate how new cases are interpreted, it is worth considering the 'first' sexological article, Karl Westphal's "Die conträre Sexualempfindung".⁴¹ Westphal was quite explicit that he was describing new symptoms that had not been addressed within psychiatry in any detail.⁴² He noted that he would have been unable to treat the problem without the work done by Wilhelm Griesinger,⁴³ and portrayed his work as a study of cases that made further observations than had previously been provided in the psychiatric literature.

Westphal's first case was of a thirty-five-year-old woman, Fraulein N., who had been brought to the Charité Hospital in Berlin in 1864. This woman reported that she had been filled with particular pleasure as a child when she played boys' games and dressed as a boy. From her eighth year, she had a particular inclination towards other girls, although only to one at a time. Once chosen, Fr. N.'s paramour would be systematically sought out, "formally courted", and "repeatedly and with pleasure" kissed, to the point where Fr. N. must attain her sexually. When she was so taken with another girl, Fr. N. would not be troubled with thoughts of other women.

In the period between 18 and 23 years, Frl. N. had frequent opportunities to satisfy her desires, especially for a five-week period when she shared a bed with her cousin. During this time, she touched her sexual parts while sleeping next to her cousin, although she stopped herself from touching her cousin as well.⁴⁴ These times she described as the happiest of her life. For the next twelve years, Frl. N. would not have the opportunity to indulge her passions apart from alone. “Very often she has the desire to masturbate, after which she has unpleasant feelings, including feeling washed-out and ill-tempered, but the desire is so overwhelming that she is unable to resist it, and many times she must [she reported] ‘violently rub [herself] with her hands’.”⁴⁵

Frl. N. felt no attraction to men. Further, when she lay in bed with her eyes closed, she imagined herself lying next to a naked woman. When she had lustful dreams, she herself appeared as a man.⁴⁶ Westphal reported that the patient “ardently wished” that she would be “freed” from this situation.⁴⁷

These details were significant. They had not been presented to the psychiatric profession previously. They are, it will be noted, filled with descriptions of sexual feelings and sexual responses. The acts themselves were not at issue, but the desires that precipitated such actions. This was one of the ways that Westphal moved away from the forensic style of research based upon evidence of criminal sexual activity which had hitherto dominated medical discussions of same-sex activity to a psychiatric mode.⁴⁸ Nevertheless, he was still bound by the professional interests of other doctors to address some of the physical aspects of Frl. N.’s case. This need was a reaction to the to-date predominant emphasis upon forensic aspects of sexual crime, but also may have been a reaction to the growing interest in degeneration theory that was sweeping across psychiatry in France in particular.⁴⁹

Westphal’s treatment of Frl. N.’s physical attributes was a simple catalogue of her body. He noted that she was a “massively big” woman. Her physiognomy and form displayed no degeneration from a “womanly type”. She had a small head, was hirsute, her eyes were not deformed, but her speech was nasal. In other words, there was nothing in Frl. N.’s exterior mien to draw attention to her aberrant desires. These facts were important, and had to be established for the field, which had hitherto not been able to identify homosexuals. More significantly, Westphal drew attention to Frl. N.’s genitalia. Her labia were said to “gape apart, so that the inner lips and vagina are visible, the clitoris is uncommonly long, the hymen completely intact”.⁵⁰ More disturbingly, Westphal noted that “hardly the tip of the little finger can be forced in [*eindringen*].... *Exploratio per vaginum* caused pain, as did the attempt to force in [*einzufragen*] the finger”.⁵¹ Presumably attention to the pain caused by an attempt to insert a finger was further proof that Frl. N. did not engage in heterosexual sex, and also did not penetrate herself in simulation of such when she masturbated.

Other aspects of Frl. N.’s life were examined by Westphal. It was noted that her father had committed suicide after a bout of melancholia. This sign would have rung alarm bells for any psychiatrist familiar with degenerationist theory, as would the existence of cleft palate in her family. In the same line of inquiry, it was found that

her period lasted only 1–2 days. Such signs would be paid much attention in the nineteenth century in support of the idea that sexual perversions were evidence of hereditary predisposition towards degeneracy.

As I have previously suggested, case histories are never free of theoretical issues, even when they are not surrounded by detailed psychiatric discussions. In many cases, deliberate effort is made on behalf of the psychiatrist either to establish a new theoretical point that is illustrated by the case material, or to fit the case in with existing theory. The most significant point for Westphal was that in both cases (Fraulein N. and a case of a cross-dressing man, Aug. Ha.), there existed a feeling that would have been appropriate for the opposite sex. Such gender inversion had been noted by previous forensic experts, including Johann Casper and Ambroise Tardieu. The best category Westphal thought captured these homosexual desires was moral insanity, but specifically a moral insanity deriving from a hereditary taint, as Griesinger had suggested.⁵²

One final point of great interest for Westphal's article is the strategies he employed to rearticulate sexual inversion into the field of psychiatry. He noted that although there was other literature in which a "reversal of the sexual impulse" had been "partially" reported, it was possible to "draw out the provable pathological state" that had been missed in these discussions (by forensic experts, especially).⁵³ Westphal stressed that his cases supported the conclusion that "the contrary sexual feeling is innate as it appears in certain pathological states".⁵⁴

Westphal's article should indeed be considered significant, as he developed the psychological aspects of homosexuality that were alluded to but not spelled out in Casper's work by providing the detailed case observations that Griesinger lacked. Because of its emphasis on case material, with some discussion of general issues, Westphal's article was hailed by later sexologists as the first article in the field. As such, a cataract of other cases of homosexuality soon followed, with more or less theoretical discussion. But it should be emphasized that in this early work, attention was still paid to physical attributes — as forensic experts had done — and to the individual manifestations of the sexual impulse. Although Griesinger had provided general principles about congenital sexual perversion, it was some time before more general work about the sexual impulse was possible, partially due to this dearth of case material upon which to make observations and base arguments.⁵⁵ Nevertheless, objects had begun to be constructed upon which work could be done, problems began to emerge, and positions within the field were soon formed. It is these reactions to Westphal's work that are the fundamental first steps taken in sexology, rather than the field being formed in one article alone, as other scholars have suggested either explicitly, or in their citing of this one piece in isolation. We might look at the "looping effects" of Westphal's article in two directions — on the people he and other sexologists studied, and on the sexologists who reacted to it.⁵⁶

Initially, a number of simple case reports were published in the German psychiatric press, such as Dr Schmincke's presentation of a man who was artistic, who was attracted to beautiful young men, who had no desire to masturbate, and who

sought help for his melancholy feelings.⁵⁷ But to gain some insight into how this field developed, it should be noted that not all reports accepted Westphal's work at face value.

One of the other important new articles after Westphal's was presented by Dr H. Gock, of the Würzburger Psychiatrische Klinik. Gock's paper, like Westphal's, had two cases: a primary female case based upon his own observations, and an incomplete case of a man that had been sent to him. Gock's paper is important for understanding the formation of the field of sexology, as he did not accept uncritically Westphal's assertion that cases of homosexuality were primarily caused by unsound inheritance. Rather, Gock played up the fact that there were insufficient cases hitherto reported on which one could make these assumptions.⁵⁸ Specifically, Gock suggested that Westphal had paid far too much attention to the side of the central nervous system in the production of homosexuality, and that the psychic side had been ignored. Needless to say, the Würzburger school from which Gock hailed was engaged in researching important psychological questions at the time that Gock made his assertions.⁵⁹

Underlying Gock's suggestion that there was possibly a category of acquired cases of homosexuality was the fact that there could exist a perverted sexual impulse without any sign of physical degeneration.⁶⁰ Furthermore, Gock established in both of his cases that there were other psychological aspects that had been ignored by Westphal, such as the swinging between feelings of exaltation and depression that his inverted cases felt. This feeling was linked, in Gock's female case, with the menstrual cycle. Such 'women's problems', it will be recalled, were pointed out as potentially causing psychological conditions by Griesinger and Westphal, although it should be noted that Gock paid more attention to the other psychological problems of his case, such as obsessional behaviours and thoughts. In order to understand these problems, Gock noted that it is important to consider if the sexual perversion was an isolated phenomenon, or if it was concomitant with other physical problems.⁶¹

In these examples, we can see how case histories are the sites for discussions about interpretation in sexology. These interpretations are theoretical, although they rely on case material. The sexologist struggles to present a case history that will be acceptable and credible to the field for which it was written. But there is no guarantee that the case history will be read in the same way that the sexologist proposes. That is why there are occasionally attempts to reinterpret case material. This is not because the case is doubted, but because the interpretation is considered to be problematic.

TRUST AND CREDIBILITY IN CASES

Having outlined some of the issues pertaining to case histories, and having shown how these issues are germane to understanding the earliest cases in sexology, I now explore further the issues of trust and credibility that underlie the presentation of case material. This approach will take us further away from the accounts of classification practices (and their effects), and instead into the practices of sexology, many of which do not appear on the surfaces of discourses. The three issues of trust and credibility are:

- (1) The relationship of trust between the doctor and the patient.
- (2) Other doctors trusting what the doctor says the patient reported about their sexual feelings.
- (3) Knowledge claims based on what the patient says have to be made credible for other doctors to accept them.

These three areas follow the making of sexological knowledge as it unfolds in practice. The first part, trusting patients, is important because it is the initial sexological encounter, where the patient communicates his or her experiences to the sexologist. The second part includes the first attempts by the sexologist to tell the scientific community about the case. The third part is important for understanding the construction of medical knowledge, as this is when the information gathered by the sexologist is turned into fact through its treatment by the sexological community. In the third part, there are other issues of credibility that are derived in part from the first two 'reporting' parts. Things other than truthful patients and verifiable cases are necessary to make a credible claim: other field-specific techniques and theories have to be negotiated for a case history to tell the sexological community anything important. That is, while cases are important, there is more to sexological knowledge than a collection of 'perverse' biographies. Focusing on case histories in this way can tell us more about the field, but just a little about the people who are in these case histories. Such a focus is entirely proper for the history of science and medicine.

1. *The Relationship of Trust Between the Doctor and the Patient*

The relationship between the doctor and the patient is a complex nexus of power. There is a struggle of sorts between the doctor wanting to get as much information as he can on a particular issue, and the patient, who often was compelled to see the doctor because he had been incarcerated, but who also had his own issues at heart — and was thus not passive victims of the medical world view. Much has been written about these kinds of power relationships by feminist scholars and by social historians of medicine.⁶² In this section, I want to consider how the issues of trust and credibility are involved in some of these relationships.

(a) *Doctors trusting patients*

The first issue is the credibility to be afforded to the patient's sexual account. Trusting the patient who is reporting his or her sexual behaviour and feelings to a doctor is not unproblematic: crosschecking this evidence is often impossible, especially when the psychological aspects of the patient's behaviour are concerned, including the patient's discussion of his or her sexual experiences.⁶³ The doctor has to use his better judgement in many cases. An example of this reliance on the patient's report can be taken from the English venereologist, William Acton (1813–75), who discussed the ways that physical signs of sodomy left on the anus were not as plain as they first might appear. Acton discussed syphilitic chancres on the anuses of prostitutes, but warned

the reader that these did not necessarily indicate that sodomy had been committed, and suggested giving prostitutes the benefit of the doubt, as there were cases of syphilitic material dribbling back from an infected vagina to cause anal lesions, even if there was no direct evidence of vaginal syphilis. This example shows that doctors were often bound by what the patient said. Unless the patient admitted to committing sodomy, a syphilitic anus did not signify anything definite about his sex life.⁶⁴

Not all doctors trusted their informants absolutely, however. French psychologist and physician, Charles Féré, noted that the doctor should take other factors into account when relying on homosexual case histories — such as the propensity for the patient to lie. He wrote:

A doctor told Krafft-Ebing that he had had to do with more than 600 uranists [homosexuals], without meeting a single case of malformation of the genital organs among them. It should, however, be remembered *à propos* of this that inverters are exceedingly vain and given to lying....⁶⁵

Similar ideas about lying homosexuals were held by Marc-Andre Raffalovich, a Russian émigré who published on homosexuality. Raffalovich criticized Krafft-Ebing's reliance on the patient, writing: "Krafft-Ebing seems to me to put too much confidence in the protestations of his patients."⁶⁶ Raffalovich justified his scepticism, stating that "the tales and confessions of inverters are ... worthy of little credence. Inverters, as I have said before and shall repeat again, are liars, and in speaking of their childhood they seek to exculpate themselves or to make themselves interesting by virtue of their passion and ignominy."⁶⁷ Havelock Ellis tempered these ideas by suggesting that it was not a congenital propensity to lie that was the issue, but the problem of blackmail.⁶⁸ Sodomy was illegal when Ellis wrote, and thus homosexual men were left open to blackmail, which made them used to having to lie in the course of their day-to-day lives. Changing the law, Ellis argued, would change the social behaviour of homosexual men.⁶⁹ Nevertheless, the issue of trusting the homosexual was not far from the minds of many sexologists. But it was not common for them to deny what the patient said absolutely, and indeed, because all sexologists were somewhat bound by the conventions of using case histories, there seems to have been a general acceptance of using the patient's 'confession' without dwelling on this issue in the majority of cases, even in the 'unofficial' correspondence between doctors about a particular case.

Not all issues of trust pertained to the patient's lying. Arrigo Tamassia, the Italian criminologist, reported some trouble in getting one of his cases to provide detailed information: an issue of concealment rather than fabrication. P. C., a peasant's son, was "indignant" about being examined by the Italian professor, and "Was very reticent about his sexual functions",⁷⁰ thus evidencing that the patient might not be completely passive, and also suggesting that the patient could make judgement about trusting the doctor and his/her motives. This reticence to speak was also noticed by Krafft-Ebing in his first discussion of sexual perversion in 1877.⁷¹ Krafft-Ebing discussed a nobleman, Herr von N., who "has desire toward the male sex, and confessed especial

friendship for certain men; denies having entered into sexual relations with men, but his blushes and former unsuccessful attempts betray him".⁷² A different issue that Kraft-Ebing brought up was the idea that the patient would not talk to the doctor, because he/she was ashamed of his or her feelings. Another case in Krafft-Ebing's 1877 paper was "ashamed to tell the doctor of his condition, as he [Krafft-Ebing] must consider him a horrible creature, and look upon him with disgust".⁷³ A further case that spoke to his doctor about his feelings, even though he was ashamed of them, was reported in England in 1884 by George Savage.⁷⁴ In this case, there was no legal imperative for the man to approach Savage for advice, but rather the patient wanted to be 'cured' of his sexual feelings for other men. He found his own homosexual desires abhorrent, according to Savage.

These examples show that although issues of doubt might be raised in the minds of sexologists concerning some of what the patient reported, or did not report, there was still an overwhelming propensity by sexologists to accept that the patient was not making up stories about their sexual prowess, and thus exaggerating, or that they were not actually enjoying confessing details of their sex lives to the sexologist. Although there exist numerous cases where patients got in touch with doctors because they felt some affinity with the previous cases which were reported in the medical literature, and personally felt that they would be able to add to medical science by recording their history for a doctor, doctors rarely seem to reflect on the motives for why a homosexual man, for example, would get in touch with a sexologist, except to help the cause of homosexual liberation.⁷⁵ In the literature, case histories are treated as completely normal, and do not raise many theoretical issues. They are just the everyday practices of medical writing. It was rare to reflect on issues of trust, as they were shared by both the authors and the readers within the scientific community.

(b) *Patients trusting doctors*

A flip-side of the doctor trusting the patient is the patient trusting the doctor enough to inform them of their erotic life. For patients to get in touch with a doctor, they need to have a good reason: a legal problem (in which case it is often beyond their control, having been apprehended); a need for psychological help; the need to feel 'normal'. These aspects are all important avenues for sexologists to gain patients. Havelock Ellis was slightly different in gathering his cases for *Sexual inversion* (1897), as he used a network of homosexual friends of Symonds and Edward Carpenter, and of his lesbian wife, Edith Lees.⁷⁶ The cases used in *Sexual inversion* were provided by homosexuals in the belief that science will help them socially — which was indeed the aim of Ellis's book. But in all of these instances, the patient can control the extent of the information they provide for the doctor — childhood sexual experiences, sexual desires, dreams, and current sexual experiences — and thus have to trust that the doctor will use this information in a suitable way.

Gathering information from patients through other means than the usual clinical experiences relies very much on trust, as in these instances, the patient needs to know

why the doctor is so keen to talk to them. Take, for a much more recent example, the advertisement used by psychologist Joanne Denko when locating patients in order to study klismaphilia (Denko named the condition, and wrote one of the first scientific papers on this paraphilia in 1973,⁷⁷ although this advertisement was for a longer follow-up study with numerous case histories):

WANTED — for scientific study by a qualified psychiatrist first person accounts of use of enemas with or instead of sex. I am particularly interested in the first such experience and how it developed, whether enemas were administered by parents, and connection of the practice with other non-coital forms of eroticism. If you are willing, please include identifying data so I can contact you to clarify obscure points. If not willing to be identified, please send your account anyway.⁷⁸

This case shows how the doctor needs to establish a credible position that the klismaphiliac would feel comfortable approaching: a “scientific study by a qualified psychiatrist”. Denko tried to extend this relationship of trust further by getting “identifying data” (presumably an address), but was still happy to accept anonymous accounts, thus showing the power that the patient has over the doctor in some aspects of the medical encounter (although they have no final say over the production of the discourse).⁷⁹ Denko noted that the advertisement brought reports from persons who

were much more accepting and open about their klismaphilia and, in some cases, their other sexual deviances.... Their associated sexual deviances were usually quite pervasive in that they affected their entire lives.... Others who accepted the bona fide nature of my advertisement even sent photocopies of other printed material concerning erotic enemas, appearing in slick or underground publications or paperbacks about sex.⁸⁰

Many of these klismaphiliacs wanted to see a wider social discussion of their paraphilia, an issue that we can see in the patient contacting the doctor in the first place. They were willing to submit to psychology in order to attain this publicity.

In both sides of the relationship between the sexologist and the paraphiliac, it is important to establish a bond of trust that will both facilitate the gathering of useful information that can later be transformed into medical knowledge claims about paraphilias, and will have a positive impact on the paraphiliac themselves (in the form of support, treatment, etc.). Obviously, not all cases are going to lead to either publications or acceptance of individual desires, but the point remains that trust is essential to establish the relations necessary for either or both of these outcomes in the first place.

2. Other Doctors Trusting What the Doctor Says the Patient Reported about their Sexual Feelings

It was important that sexologists established their own credibility with other specialists in a number of ways. First, there was the social position of the sexologist, who was almost always a physician.⁸¹ Second, there was the reporting style employed

by the sexologist to impart information. Different tropes were used to establish the credibility of the case history. These aspects of credibility refer to both the external and internal functioning of sexology, and they are both important for establishing a sexological fact.

(a) *The importance of being a doctor*

It is not insignificant that it was standard for sexologists to be medically qualified in the early days of the discipline, in order to speak about sex properly.⁸² The most famous example is Havelock Ellis who became a doctor specifically so he could write about sex.⁸³ It is, however, important to note that non-medical sexologists were not treated the same way as their medical-trained counterparts. While this issue is not often raised, there are a few examples. For instance, it is important to note that John Addington Symonds wrote to Havelock Ellis with the intention of writing a book on homosexuality because Ellis was a doctor. Indeed, Symonds wrote to Edward Carpenter, a mutual friend of his and Ellis's, that he "need[ed] somebody of medical importance to collaborate with", for he realized that "[a]lone, [he] could make but little effect — the effect of an eccentric".⁸⁴ Symonds had already written two books on homosexuality, but neither of them carried his name, and both were published privately. He sought out Ellis because he thought that a medical name on the book would help it to be accepted by a wider community of medical and legal men.⁸⁵ The outcome was a defence of *Sexual inversion* by the medical press after the book was banned as an obscene publication in 1897.⁸⁶

Not publishing with a doctor was a bad idea. Edward Carpenter, who was not medically trained, wrote a number of books on homosexuality which were almost identical in their analysis of the historical and medical literature as Ellis's *Sexual inversion* (although crucially they lacked case histories, and were all but ignored by most sexologists). However, Carpenter's works were ridiculed in the *British medical journal*, a journal that commented that Ellis's work was scientific. The reason for the differences in approach by the medical press was surely that Carpenter was not considered to have the 'right' to speak about homosexuality.⁸⁷ That is, Ellis the doctor enjoyed credibility denied to non-doctors writing about sexual perversions.

It would seem that there was a necessity to be a doctor to speak about sex credibly. This is no longer the case, as many contemporary sexologists are psychologists rather than doctors, although it should be kept in mind that the discipline was not organized in the same way in the nineteenth century. Being a doctor meant that the author of a text could be expected to have the minimum training to talk about an issue like sexuality. This attitude is clearly seen in one of American psychiatrist James Kiernan's first of many writings on sexuality: "The present subject may seem to trench on the prurient, but in medicine the prurient does not exist, since 'science like fire purifies everything'."⁸⁸ Credibility and trust is afforded to trained clinicians but not to quacks or polemicists.⁸⁹

(b) *The presentation of the case*

Proper presentation of the case is important to facilitate the trust of other doctors. The presentation of a case is necessary to frame the knowledge claim that is based on the patients' narratives. While early sexological texts about things like homosexuality or masochism did not refer to other theories — but only to other case histories — it soon became normal to position any new discussions of case material in the literature that had been developed by Casper, Westphal, Krafft-Ebing, Kiernan, Ellis, etc. This practice partially involved positioning the new work in an established scholarly trajectory, which in turn helped the reader understand the paper in the way that the author intended. But it also created the scope for criticism that was essential for the development of a field like sexology.

In the first twenty years of reporting sexological cases, and later as well, there was a propensity to re-write what the patient said. Havelock Ellis did this so as to protect the identity of his informants, as they were not legal cases, and in some cases (e.g., Horatio Brown's) held prominent public offices.⁹⁰ Other doctors wrote like this because it was standard in medical writing of this time not to use the first person narrative to record a case.⁹¹ But within the field of sexology, the issue of credibility soon came to rest on the patient's own word (against the trend in clinical medicine). For example, Albert von Schrenck-Notzing, the Munich-based hypnotherapist, noted: "Where possible, for the sake of objectivity, I have allowed patients to speak for themselves, and give their letters *verbatim*."⁹² Likewise, Joanne Denko, of klismaphilia fame, wanted "first person accounts" in order to establish her own credibility in the eyes of her profession. Cases of paraphilia were soon often written in the first person, not least so that other doctors reading the report could interpret the material as it appeared, and so it was unfettered by the author's own literary style; rather it was how the patient 'really was'.⁹³ Havelock Ellis, as discussed above, used first person accounts in his *Eonism*, but they have to be thought of as framed by Ellis's original questions. But the use of the individual voice rather than the omnipotent doctor's rewording became important in establishing the credibility of the report as much as including data at the end of a biochemical report — it gave the reader the opportunity to check the conclusions against the 'raw' data much more, and thus has a useful function in getting a claim accepted. This development must be thought about as an important reaction to the fact that it is impossible to test a doctor's claims, but of course, there is still the issue that the author must be trusted — and that the reader must believe that the author of the case history did not skilfully manipulate if not fabricate the case as it is presented.⁹⁴

There were other tropes that were useful in framing patient accounts as credible. These could involve framing the patient as respectable, intelligent, or a professional. For example, Havelock Ellis described many of his homosexual patients as respectable members of society in *Sexual inversion*. A number of them were upper-class, or were doctors or lawyers or artists. Likewise, Denko described a number of her patients as "intelligent", thus encouraging other doctors to read the patients as capable of giving useful information. Of her first patient she wrote: "Therapy was easy with this

intelligent, articulate man.”⁹⁵ These devices have always been a part of sexological case histories. They have the effect of stopping the reader from conceiving of the patient’s discourse as the ramblings of a madman, and facilitate the reader’s belief that they are entering the mind of a sophisticated sexual pervert instead.

The narrative about the patient could also address stereotypes that other doctors would look for in a patient. Effeminacy was played up in many of the early homosexual case reports.⁹⁶ Many of the male homosexuals in these case histories blushed, or wore dresses, or had feminine bodies. Likewise, many lesbians were masculine in appearance and desire, according to the sexologists who wrote about them. These aspects fitted with preconceived ideas about homosexuality and gender, and thus seemed to fit with the expectations of the reader, another thing that would help support the accuracy of the report.

In all of these cases, the doctor is at pains to produce a credible report that will be acceptable to the scientific community. This is not to suggest that the sexological community was excessively sceptical, but rather to address the ways that doctors reporting cases did as much as possible to make their work fit the discipline, and to make themselves out to be the best person to be addressing these issues responsibly and without a hint of prurience.

3. Knowledge Claims Based on What the Patient Says Have to be Made Credible for Other Doctors to Accept Them

While the above two aspects of trust are essential to the proper operation of the field of sexology, they are somewhat subordinate to this third issue of framing a credible case. Indeed, the most important part of creating medical knowledge is to make it credible for the reader.⁹⁷ There is no point disclosing case histories unless they are made to fit in with the ever-changing stock of knowledge of the field of sexology.

(a) Making a credible claim

An issue related to the use of the patient’s voice pertains to how other sexologists treated the case material that was presented. In the framing of the case, in some instances, other sexologists do not completely credit the case being reported, not because they think the patient did not exist, but because the reporting analyst did not specify information that the reader thought would have been important to an interpretation of the case. By focusing on how credibility is constructed, we are getting further away from the initial interactions between sexologists and informants, and closer to the production of sexological knowledge.

A good place to observe the process of constructing credibility is to look at the emergence of new objects of inquiry. It is essential not to overlook the importance of placing the new object in the field, and sexology offers the historian with many ‘new’ medical objects in a relatively short period. Unlike with disciplines such as anatomy, where there have been important discourses for many preceding centuries, sexology sprang up from the meeting of forensic medicine, psychiatry and hypnotherapy between the 1850s and 1900s. All of the objects of sexological inquiry

— homosexuals, fetishists, masochists, etc. — were new, and could be described as cases relatively unfettered by a long history of description.⁹⁸ But as the number of cases grew, it became normal practice for doctors to cite each other, either positively or negatively, on theoretical interpretations of their material, as seen in the discussion of Westphal, above. This practice developed in order to make observations that will have some meaning for the field, and to openly display the perspective that the doctor is employing, by demonstrating what they had read, what they agreed with, and what they repudiated.⁹⁹

If it is a new paraphilia that is being described, the sexologist has to explain it in terms that the field will recognize. An example of a new case history that illustrates this practice is Denko's aforementioned cases of klismaphilia. The twenty-seven-year-old army officer who first approached Denko was the first case she could find in the literature. She emphasized the novelty of her discovery:

Our librarian's search of Index Medicus and antecedent indexes dating back to 1896 have failed to unearth other cases. A Medlars search for the years from 1966 to 1969 in English, Russian, and German, failed to disclose references. My own skimming of the index and relevant sections of Krafft-Ebing's and Havelock Ellis's classical works on sex did not produce a comparable case. While one can understand that information may be hard to get from individuals suffering from this problem, I am puzzled by lack of reports of this entity, since it is obviously common enough for me to have heard about four other cases within a year.¹⁰⁰

This justification of the novelty of her case allowed Denko to present a new object of sexological inquiry. Two other medical mentions of the condition were cited, so as to orientate the reader,¹⁰¹ but Denko very much had the control of the discussion. Nevertheless, many other tropes were used to discuss the two klismaphiliac cases (she found one other case very shortly after the twenty-seven-year-old soldier who would only correspond anonymously through sealed envelopes), such as giving a typical history that involved childhood exposure to enemas, current sexual experiences, attitudes to sex, etc. Using these narrative devices allowed for a typical unfolding of the case, even though the case material was new. Denko also mobilized the theoretical ideas of the field, such as the relationship between the patient and their parents, which presented an interpretive framework that the reader would appreciate, even if they had never seen a klismaphiliac before.

(b) *Challenging credibility: What the doctor missed*

Cases that are not presented in a way that the reader will be able to understand in terms of the field-specific norms will be re-thought. But this is generally the situation in instances where the case is from a different field, say psychoanalysis rather than sexology. In these instances, the 'useful' information is filtered out, and the 'dogma' is ignored.¹⁰² One of the ways that this special kind of boundary working is achieved is by challenging the interpretation of a case when material is not included. We saw this above in two cases: Havelock Ellis and Wilhelm Stekel discussing Florrie, and

Gock responding to some of the non-acquired ideas of Westphal. Another important example is the way that Freud and Ellis try to out-negotiate each other over an issue such as homosexuality.¹⁰³ About Freud's theories, Ellis wrote:

Freud regards it as a well-known fact that boys and girls at puberty normally show plain signs of the existence of a homosexual tendency. Under favourable circumstances this tendency is overcome, but when a happy heterosexual love is not established it remains liable to reappear under the influence of an appropriate stimulus. In the neurotic these homosexual germs are more highly developed.¹⁰⁴

Ellis was here highlighting that even though psychoanalysts have identified the early existence of homosexuality, it is the existence of "homosexual germs" — a congenital predisposition towards homosexuality — that is fundamental to the development of homosexuality. The importance of Ellis's seemingly banal comment is that it was closer to his own theory, based on many case histories which he incorporates in his own work, where homosexuals describe their own early homosexual desires which Ellis interpreted as showing that they had a congenital predisposition to homosexuality. Ellis accepted psychoanalytic doctrine only when it was interpreted through his own theoretical commitments, based on his own analysis of cases that he gathered.

Rather than critically engage with Freud on all aspects of the psychoanalytic programme, Ellis simply redescribed the processes of the Oedipal complex, castration complex, narcissistic turning of the sexual desire onto the self, and youthful sexual desire for "youthful males resembling themselves, whom they love as their mother loved them", before noting that "Their pursuit of men is thus determined by their flight from women". Ellis drew attention to the fact that "Freud himself ... is careful to state that this process only represents one type of stunted sexual activity, and that the problem of inversion is complex and diversified".¹⁰⁵ He also stressed that Freud's view derived from an understanding of human sexual development based on the notion of universal bisexuality. As such, he attributed to Freud that idea that "homosexuality arises from the suppression, owing to some accident, of the heterosexual component, and the path through an autoerotic process of Narcissism to homosexuality".¹⁰⁶ Of course, this last quotation reveals much about what Ellis was doing, for the concepts of Narcissism and auto-eroticism — as Freud himself noted — were developed by Ellis.¹⁰⁷

Ellis proceeded by noting "Most psychoanalysts are cautious in denying a constitutional or congenital basis to inversion, though they leave it in the background".¹⁰⁸ Ellis, however, also stressed that "The mechanism of the genesis of homosexuality put forward by Freud need not be dismissed off hand".¹⁰⁹ Although not universally accepting the Oedipus complex as an explanatory framework, Ellis defended it against the prejudicial treatment such a concept had received because of its association with incest. He further noted "It is quite easily conceivable that the psychic mechanism of the establishment of homosexuality has in some cases corresponded to the course described by Freud". Ellis even conceded that "the pronounced *horror feminae*

occasionally found in male inverts may plausibly be regarded as the reversal of an early and disappointed female attraction". However, Ellis noted, "It is impossible to regard this mechanism as invariable or even infrequent".¹¹⁰ Thus Ellis rethought Freud's use of cases on the basis of his own original observations.

Unsurprisingly, Ellis believed that a solution to the problems with which he charged the Freudian view of the aetiology of homosexuality could be found in an additional role for congenital aspects of character. This he felt had been demonstrated by a number of his own case histories. He wrote, "This hereditary factor seems indeed to be called for by the Freudian theory itself". Ellis also stressed, "Freud himself recognizes this and asserts congenital psychosexual constitution, which must involve predisposition".¹¹¹

In this case, Freud and Ellis continually challenged each other not on the details of their respective theories — for numerous personal reasons — but on the details of the cases that they used as the data for their theories.¹¹² Ellis claims that the patients were most likely to be congenitally pre-disposed towards homosexuality because of their precocious sexual behaviour as well as their familial heredity, of which Freud did not include details, whereas Freud criticized Ellis for not paying enough attention to the details of his cases' early lives. These differences are not challenges as much as corrections to one another's works based on the cases. It is implied that different facts would have been established had better — or more credible — cases been used, or better questions asked of the cases as they reported their histories. These are debates of interests and commitments, not facts, and certainly not about the 'real lives' of the cases.

CONCLUSION

The main point underlying this article is that the best way to study sexology in history is to conceive of it as a field, and to see how it uses field-specific tropes and practices, such as the case history that embodies acceptable theory, in order to establish a stock of knowledge. These case histories are not neutral reports, but are the basis for sexological thought: selected, manipulated, and framed in order to establish sexological facts which will be taken up by other members of the field. But in order to establish sexological facts, there are cycles of credibility that need to be engaged in order to gather the material necessary for making a sexological claim. These cycles are very similar to those found in other sciences; they rely on citation, on framing, and on extending the stock of knowledge by correcting previous errors, or by uncovering new objects of inquiry.¹¹³ The style of reasoning through which this process operates in sexology and the other human sciences is through the case history, although there are of course many other ways of making more theoretical claims. The more "external" a claim in sexology (that is, the more theoretical a claim is, rather than descriptive), the more the claim is extended beyond the 'natural history' type observation of a paraphilia.¹¹⁴ When the field of sexology is understood in this sociological manner, we are in a better position to appreciate the implications of classification for the human sciences. This point, which has perhaps been best articulated by Ian Hacking

with his ideas of “making up people” and “looping effects”, is ripe for a sociological treatment. While Hacking’s focus is primarily on the effects of naming and on the construction of human kinds (and therefore on the differences in methodology between the human and natural sciences), he does not consider the ways in which these practices are effected in the field. Such attention, as this paper has argued, is crucial if we are to understand how the field produces knowledges about humans; how the very objects emerge in discourse itself.¹¹⁵

To write the history of sexology in the way I am proposing, it is important to step outside such attention to the internal theoretical developments of sexology,¹¹⁶ to avoid excessive political criticism of the temporally-bound material,¹¹⁷ to move away from searching for origins,¹¹⁸ and rather to look at how sexologists think. This approach requires that the historian focus not only on what doctors wrote, but also upon the practices on which sexology relies. Other critical work in the history of psychiatry has proceeded in this way,¹¹⁹ but in the history of sexology there is still much work to be done. Focusing on case histories and what they do in practice is but one area of this critical historiography.

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 5. E. Noble Chamberlain, *Symptoms and signs in medical practice: An introduction to medical diagnosis* (Bristol, 1936), 6.
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 8. Oliver Sacks, *The man who mistook his wife for a hat* (New York, 1985), pp. ix–x. For some sceptical comments on Sacks, see Michael Neve, “It’s got bells on”, *London Review of Books*, 21 June 1984, 3.
 9. Thomas Laqueur, “Bodies, details, and the humanitarian narrative”, in Lynn Hunt (ed.), *The new cultural history* (Berkeley, 1989), 176–204. Laqueur did not, however, address the behind-the-scenes practices which were specific to the different fields of practice which he considered — forensic medicine, pathological anatomy, novel-writing, legal reports, etc. Such an approach would bring us closer to the historiographies propounded by Hacking and especially Michel Foucault.
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 15. Harry Oosterhuis, *Stepchildren of nature* (Chicago, 2000); Oosterhuis, “Richard von Krafft-Ebing’s ‘stepchildren of nature’”, in Vernon Rosario (ed.), *Science and homosexualities* (New York, 1997), 67–87; Alice Dreger, “Hermaphrodites in love: The truth of the gonads”, in Rosario (ed.), *Science and homosexualities*, 46–66; Joy Dixon, “Sexology and the occult: Sexuality and subjectivity in theosophy’s new age”, *Journal of the history of sexuality*, vii (1997), 409–33.
 16. John Forrester, “If *p*, then what? Thinking in cases”, *History of the human sciences*, ix (1996), 1–25.
 17. Carlo Ginzburg, “Morelli, Freud and Sherlock Holmes: Clues and scientific method”, *History workshop journal*, ix (1980), 5–36, p. 12.
 18. Ginzburg, “Morelli, Freud and Sherlock Holmes”, 16.
 19. Michel Foucault, *Discipline and punish* (Harmondsworth, 1991), 191–2.
 20. See Ivan Crozier, “La sexologie et la définition du ‘normal’ entre 1860 et 1900”, *Cahiers du genre*,

- xxxiv (2003), 17–37. Again, Hacking’s insights are important in this context.
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 23. For more on enculturation, see Andrew Warwick, *Masters of theory* (Chicago, 2003). Work by Chris Lawrence on medical cultures is also relevant. See Lawrence, “Incommunicable knowledge: Science, technology and the clinical art in Britain, 1850–1914”, *Journal of contemporary history*, xx (1985), 503–20; Lawrence, “Still incommunicable: Clinical holists and medical knowledge in interwar Britain”, in Chris Lawrence and George Weisz (eds), *Greater than the parts: Holism and biomedicine, 1920–1950* (Oxford, 1998), 94–111; Lawrence, “A tale of two sciences: Bedside and bench in interwar Britain”, *Medical history*, xliii (1999), 421–49.
 24. See Georges Canguilhem, *The normal and the pathological*, transl. by Carolyn R. Fawcett and Robert S. Cohen (New York, 1989), who uses the example of sexual inversion to make the same point.
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 26. Barry Barnes, “On the conventional character of knowledge and cognition”, *Philosophy of the social sciences*, xi (1981), 303–33, p. 306. Also see Barnes, *T. S. Kuhn and social science* (London, 1982), chap. 2. It would be proper to look on sexological cases, such as of masochism for example, in terms of artificial kinds, and therefore to use Martin Kusch’s elaboration of Barnes’s work. See Barnes, “Social life as bootstrapped induction”, *Sociology*, xvii (1983), 524–45, and Martin Kusch, *Psychological knowledge* (London, 1998).
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 28. Again, see Canguilhem, *The normal and pathological* (ref. 24). Also see Michel Foucault, *Birth of the clinic*, transl. by Alan Sheridan (London, 1973).
 29. Ellis to Haire, British Library, Add MS 70540, 3 May 1925.
 30. For more on this selection process, see Ivan Crozier, “Havelock Ellis, Eonism, and the patients’ discourse”, *History of psychiatry*, xi (2000), 125–54.
 31. See Oosterhuis, “Krafft-Ebing’s ‘stepchildren of nature’” (ref. 15). Although looking at the archives at Krafft-Ebing’s correspondents, Oosterhuis does not consider why some cases were chosen for publication in *Psychopathia sexualis* and not others.
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 33. For more on this text, see Crozier, Introduction to the critical edition of Havelock Ellis and John Addington Symonds, *Sexual inversion* (1897) (Basingstoke, 2008). Ellis and Symonds discuss this questionnaire in their correspondence, but I have been unable to locate any copies of it. I do, however, pay further attention to it in my introduction to the reprint of their book.
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 35. Stekel, *Sadism and masochism*, i, 242.
 36. Havelock Ellis, “The history of Florrie”, *Psychoanalytic review*, vi (1919), 207.

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39. See papers by the psychiatrist David Lynn, “Sigmund Freud’s analysis of Albert Hirst”, *Bulletin of the history of medicine*, lxxi (1997), 69–93; David Lynn and George Vaillant, “Anonymity, neutrality, and confidentiality in the actual methods of Sigmund Freud: A review of 43 cases, 1907–1939”, *American journal of psychiatry*, clv (1998), 163–71. A huge list could be produced looking at how Freud (ab)used his cases. Attention should be drawn to Mikkel Borch-Jacobsen, *Remembering Anna O.: A century of mystification* (London, 1996).
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41. Michel Foucault, *The history of sexuality*, i: *An introduction*, transl. by Robert Hurley (London, 1990), 43.
42. For further discussion of this important text, see Arnold Davidson, *The emergence of sexuality* (Cambridge, MA, 2001).
43. Karl Westphal, “Die conträre Sexualempfindung: Symptom eines neuropathischen (psychopathischen) Zustandes”, *Archiv für Psychiatrie und Nervenkrankheiten*, ii (1869–70), 73–108, p. 106n. See also Wilhelm Griesinger, “Vortrag zur Eröffnung der psychiatrischen Klinik”, *Archiv für Psychiatrie und Nervenkrankheiten*, i (1868), 650.
For one of the few detailed discussions of Westphal’s oft-cited paper, see David Halperin, “How to do the history of homosexuality”, *GLQ*, vi (2000), 87–123. The majority of anglophone scholars have contented themselves (explicitly or not) with following Michel Foucault’s (inaccurate) reference to this paper in *History of sexuality*, i (ref. 41), 43.
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45. Westphal, “Die conträre Sexualempfindung”, 76.
46. Westphal, “Die conträre Sexualempfindung”, 76.
47. Westphal, “Die conträre Sexualempfindung”, 76.
48. See Johann Casper, “Ueber Nothzucht und Päderastie und deren ermittelung seitens des Gerichtesarztes”, *Vierteljahrsschrift für gerichtliche öffentliche Medizin*, i (1852), 21–78.
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51. Westphal, “Die conträre Sexualempfindung”, 78.
52. Westphal, “Die conträre Sexualempfindung”, 96–97, incl. 96n.
53. Westphal, “Die conträre Sexualempfindung”, 97.
54. Westphal, “Die conträre Sexualempfindung”, 107.
55. Most important in this respect was Richard von Krafft-Ebing, “Ueber gewisse Anomalies des Geschlechtstriebes und die klinisch-forensich Verenthug derselben als eines wahrscheinlich functionellen Degenerationszeichens des centralen Nervensystems”, *Archiv für Psychiatrie und Nervenkrankheiten*, vii (1877), 291–312.
56. See Hacking, “Looping effects” (ref. 1); Hacking, “Kinds of people” (ref. 1).
57. Dr Schmincke, “Ein Fall von conträrer Sexualempfindung”, *Archiv für Psychiatrie und Nervenkrankheiten*, iii (1872), 225–6. Westphal himself also presented another short case. See K. Westphal, “Zur conträren Sexualempfindung”, *Archiv für Psychiatrie und Nervenkrankheiten*, vi (1876), 620–1.
58. H. Gock, “Beitrag zur Kenntniss der conträren Sexualempfindung”, *Archiv für Psychiatrie und*

- Nervenkrankheiten*, v (1875), 564–74, p. 564.
59. See Kusch, *Psychological knowledge* (ref. 26).
 60. Gock's female case had no signs of physical degeneration, and even her long, gaping labia and sensitive and tight vagina were considered normal. See H. Gock, "Beitrag zur Kenntniss der conträren Sexualempfindung" (ref. 58), 568.
 61. Gock, "Beitrag zur Kenntniss der conträren Sexualempfindung", 574.
 62. These ideas are not new. See, for example, some of the papers in Ruth Hubbard, Mary Sue Henifin, and Barbara Fried (eds), *Women look at biology looking at women: A collection of feminist critiques* (Boston, 1979); for social historians of medicine who consider the doctor/patient relationship, see for example Porter, "The patients' view" (ref. 14).
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 64. For more on what anuses tell doctors looking for signs of sodomy, see Crozier, "All the appearances were perfectly natural" (ref. 2). For more on Acton, see Ivan Crozier, "William Acton and the history of sexuality: The medical and professional contexts", *Journal of Victorian culture*, v (2000), 1–27.
 65. Charles Féré, *The evolution and dissolution of the sexual instinct* (Paris, 1904), 190–1.
 66. Marc Raffalovich, "Uranism, congenital sexual inversion: Observations and recommendation", transl. by C. Judson Herrick, *Journal of comparative neurology*, v (1895), 33–65, p. 38.
 67. Raffalovich, "Uranism, congenital sexual inversion", 46.
 68. Ellis and Symonds, *Sexual inversion* (ref. 33). Congenital propensity to lying was considered atavistic. See David Horn, "Blood will tell", in Forth and Crozier (eds), *Body parts* (ref. 2), 17–43.
 69. For more on this see Ivan Crozier, "The medical construction of homosexuality and its relation to the law in nineteenth-century England", *Medical history*, xlv (2001), 61–82.
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 71. Krafft-Ebing, "Ueber gewisse Anomalies des Geschlechtstriebes" (ref. 55).
 72. Shaw and Feris, "Perverted sexual instinct" (ref. 70), 201, citing Krafft-Ebing, "Anomalies des Geschlechtstriebes".
 73. Shaw and Feris, "Perverted sexual instinct" (ref. 70), 202, citing Krafft-Ebing, "Anomalies des Geschlechtstriebes".
 74. George Savage, "A case of sexual perversion", *Journal of mental science*, xxx (1884), 390–1.
 75. See Oosterhuis, "Krafft-Ebing's 'Stepchildren of nature'", on homosexuals and other paraphiliacs writing to Krafft-Ebing. Some of Havelock Ellis's correspondents, and indeed his co-author, John Addington Symonds, were keen to put sexology on a scientific footing by providing their cases. See their *Sexual inversion* (ref. 33). Earl Lind wrote his 'confession' of homosexual life in the nineteenth century to provide medical and legal readers with an 'inside story' of homosexual desire. See Lind, *Autobiography of an androgyne* (New York, 1918).
 76. I discuss the collection of case histories for *Sexual inversion* in my introduction to the Palgrave edition (ref. 33).
 77. Joanne D. Denko, "Klismaphilia: Enema as a sexual preference", *American journal of psychotherapy*, xxvii (1973), 232–50.
 78. Joanne D. Denko, "Amplification of the erotic enema deviance", *American journal of psychotherapy*, xxx (1976), 236–55.
 79. Power is relational, but not equal. See Michel Foucault, "Truth and power", in Colin Gordon (ed.), *Power/Knowledge* (Brighton, 1980), 109–33; "Intellectuals and power: A conversation between Michel Foucault and Gilles Deleuze", in Michel Foucault, *Language, counter-memory, practice*,

- ed. by D. Bouchard (Ithaca, 1977), 205–17.
80. Denko, “Amplification of the erotic enema deviance” (ref. 78), 250.
 81. Indeed, I maintain that for a discourse to be considered properly sexological, it has to be written by a doctor, and usually contain reference to case material — other discourses about sex do of course exist (such as by feminist or homosexual activists), but they belong to a different “discursive formation” (see Michel Foucault, *Archaeology of knowledge*, transl. by A. Sheridan (New York, 1972)).
 82. Vern Bullough, “American physicians and sex research and expertise, 1900–1990”, *Journal for the history of medicine and allied sciences*, lii (1997), 236–53. See also Denko, who emphasized that she was a psychiatrist when encouraging patients to write to her.
 83. See Havelock Ellis, *My life* (London, 1939); Chris Nottingham, *The pursuit of serenity* (Amsterdam, 1999); Phyllis Grosskurth, *Havelock Ellis: A biography* (New York, 1980).
 84. John Addington Symonds to Edward Carpenter, 29 December 1892, in the *Letters of John Addington Symonds*, ed. by Herbert M. Schueller and Robert L. Peters (Detroit, 1967–69), iii, p. XXX.
 85. See Crozier, Introduction to Ellis and Symonds, *Sexual inversion* (ref. 33).
 86. For more about this event and the responses in the medical press, see Crozier, Introduction to Ellis and Symonds, *Sexual inversion* (ref. 33); “Charge of publishing and selling obscene literature”, *British medical journal*, 5 November 1898, 1466.
 87. See the review of Carpenter in *British medical journal*, 26 June 1909, 1547. This was much more harsh than the earlier reviews of Ellis. See *Lancet*, 1901, i, 108; review of *Sexual inversion, Alienist and neurologist*, xix (1898), 185–6, etc. Not all of these reviews of Ellis were completely positive. See *British medical journal*, 8 February 1902, 339–40.
 88. James Kiernan, “Sexual perversion”, *Detroit lancet*, vii (1884), 481–4, p. 481.
 89. Clearly issues of boundary working are important here. See Thomas Gieryn, “Boundary-work and the demarcation of science from non-science: Strains and interests in professional ideologies of scientists”, *American sociological review*, xlvi (1983), 781–95; Andrew Abbott, “Boundaries of social work or social work of boundaries?”, *Social service review*, lxix (1995), 545–62.
 90. These letters do not survive in Ellis’s correspondence, although some of the cases are discussed in other letters between Symonds, Edward Carpenter, Horatio Brown, etc.
 91. To see how certain changes in hospital record keeping came about, see Risse and Warner, “Reconstructing clinical activities” (ref. 10); Suzuki, “Framing psychiatric subjectivity” (ref. 10); Fissell, “The disappearance of the patient’s narrative” (ref. 10).
 92. Albert von Schrenck-Notzing, *Therapeutic suggestion in psychopathia sexualis (pathological manifestations of the sexual instinct) with especial reference to contrary sexual instinct*, transl. by C. G. Chaddock (Philadelphia, 1895), p. viii.
 93. See for one of many examples, Anon., “The gentleman degenerate”, *Alienist and neurologist*, xxv (1904), 62–70.
 94. For more on the controversy of Freud manufacturing his cases, see John Forrester, *Truth games* (Cambridge, MA, 1997), or Mikkel Borch-Jacobsen, “How fabrication differs from a lie”, review of Han Israëls, *Der Fall Freud: Die Geburt der Psychoanalyse aus der Lüge*, *London Review of Books*, 13 April 2000, which can be read at: <http://www.lrb.co.uk/v22/n08/borc01.html>. Also consider Michael Neve’s sceptical review of Oliver Sacks, *A leg to stand on*, entitled “It’s got bells on”, *London Review of Books*, 21 June 1984, 3.
 95. Denko, “Klismaphilia” (ref. 77), 238.
 96. E.g., Westphal, “Die conträre Sexualempfindung” (ref. 43).
 97. See Shapin, “Cordelia’s love” (ref. 22).
 98. See for example, Dr H—, “Gynomania”, *Medial record*, ix (1881), 336, a discussion of homosexuality and cross-dressing that did not cite any of previous literature by Westphal, Krafft-Ebing, or Casper,

which further framed the case as ‘masturbation’ rather than same-sex desire. This point extends Hacking’s claims about homosexuality in “making up people” to all perversions — although unlike Hacking, the emphasis here is on the field rather than the object.

99. An example of this type of writing is Meyer Solomon, “Two dreams”, *Alienist and neurologist*, xxxvi (1915), 12–35. Solomon here repudiates Freud’s libido theory and the Oedipus complex, and uses two dream cases as evidence. For more on citation and the gaining of authority, see Pierre Bourdieu. “The specificity of the scientific field and the social conditions of the progress of reason”, *Social science information*, xiv (1975), 19–47.
100. Denko, “Klismaphilia” (ref. 77), 246.
101. I. Bieber, “Enemas and sex”, *Medical aspects of human sexuality*, iv (1970), 2; and a close-call by Wilhelm Stekel, *Patterns of psychosexual infantilism* (New York, 1966).
102. See how Havelock Ellis treated other sexological and psychoanalytic discourses in his work *Eonism and other supplementary studies* (Philadelphia, 1928). For more, see Crozier, “Havelock Ellis, Eonism, and the patients’ discourse” (ref. 30).
103. See Ivan Crozier, “Taking prisoners: Havelock Ellis, Sigmund Freud, and the construction of homosexuality, 1897–1951”, *Social history of medicine*, xiii (2001), 61–82.
104. Ellis, *Sexual inversion*, 3rd edn (Philadelphia, 1915), 80–81. Ellis continually stressed that there were more schools of psychoanalysis than Freud’s, thus permeating the image that he was up against one solid body of psychoanalytic opposition.
105. Ellis, *Sexual inversion*, 3rd edn, 304. For a further description of the processes of sexual development see Freud, “The dissolution of the Oedipal complex”, in Freud, *On sexuality*, ed. by Angela Richards (London, 1991), 313–22.
106. Ellis, *Sexual inversion*, 3rd edn, 304. Ellis also criticized the psychoanalytic views of Abraham, “Ueber hysterische Traumzustände”, *Jahrbuch für psychoanalytische Forshung*, 1910, and Sadger, *Die Lehre von Geschlechtsverirungen* (1921), based on universal bisexuality in *Eonism*, 16–17.
107. Sigmund Freud, “Three essays on the theory of sexuality”, *On sexuality* (Penguin Freud Library, vii; Harmondsworth, 1977), 33–169.
108. Ellis, *Sexual inversion*, 3rd edn, 304.
109. Ellis, *Sexual inversion*, 3rd edn, 306.
110. Ellis, *Sexual inversion*, 3rd edn, 307. Ellis himself had certain ‘troubles’ with his sex life with women — he preferred to watch women urinate, and did not have intercourse until he was in his fifties. See Grosskurth, *Havelock Ellis* (ref. 83).
111. Ellis, *Sexual inversion*, 3rd edn, 308.
112. See Crozier, “Taking prisoners” (ref. 103).
113. For an account of cycles of credibility that owes much to Pierre Bourdieu’s writing, see Bruno Latour and Steve Woolgar, *Laboratory life* (Beverly Hills, 1979), chap. 5. To learn more of the Bourdieuan roots of Actor-Network Theory, see Bourdieu, *Outline of a theory of practice*, transl. by Richard Nice (Cambridge, 1977); and applied specifically to science see Bourdieu, “Specificity of the scientific field” (ref. 99).
114. Cf. Trevor Pinch, “Towards an analysis of scientific observation: the externality and evidence significance of observational reports in physics”, *Social studies of science*, xv (1985), 3–36.
115. Although not solely through these objects, but also through modes of enunciation, formations of concepts, and deployments of strategies that are also field-specific. See Foucault’s *Archaeology of knowledge* (ref. 81).
116. See Vern Bullough, *Science in the bedroom* (New York, 1994).
117. For example, Jennifer Terry, *An American obsession* (Chicago, 2000); Carol Groneman, *Nymphomania* (New York, 2000).
118. See Michel Foucault, “Nietzsche, genealogy, history”, in Paul Rabinow (ed.), *The Foucault reader*

(London, 1984), 76–100. A useful application of Foucault’s historiographical standpoint to medical history is Anne Kveim Lie, “Origin stories and the Norwegian radesyge”, *Social history of medicine*, xx (2007), 563–79.

119. For example, see W. F. Bynum and Michael Neve, “Hamlet on the couch”, in Roy Porter and W. F. Bynum (eds), *The anatomy of madness*, i (London, 1985), 189–304.

GALILEO, BRUNO AND THE RHETORIC OF DIALOGUE IN SEVENTEENTH-CENTURY NATURAL PHILOSOPHY

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When I was with some distinguished men recently, I asserted that the Socratic method of discussion, as expressed in the Platonic dialogues, seemed to me outstanding. For not only are minds imbued with truth through familiar conversation, but one can even see the order of meditation itself, which proceeds from the known to the unknown, provided each person replies for himself when asked an appropriate question, with no one suggesting the right answers. When I had made this claim, they asked me to try to revive so very useful a thing by producing a specimen, which, by that very experiment, would show minds to be endowed with the seeds of all knowledge. I excused myself at length, confessing that this matter was more difficult than might be believed. For it is easy to write dialogues, just as it is easy to speak rashly and in no particular order; but to compose a speech in such a way that truth itself might gradually shine out of the darkness, and knowledge might grow spontaneously in the mind, this is really only possible for someone who has himself gone into the reasons very carefully on his own, before taking it on himself to teach others.

These extremely self-conscious observations on the heuristic value of the dialogue form come from the opening of Gottfried Wilhelm Leibniz's dialogue, *Prima de motu philosophia*, written in 1676.¹ Although Leibniz's suggestion that the Platonic dialogue was a form which needed reviving in the 1670s is questionable in a century which had seen so many attempts to use the dialogue to promote natural philosophy, including Galileo's *Dialogue on the two world systems* (1632)² and *Discourses ... concerning two new sciences* (1638),³ Thomas Hobbes's *Dialogus physicus* (1661), and Robert Boyle's *Sceptical chymist* (also 1661), Leibniz's comments offer us a useful way of thinking about the reason why seventeenth-century natural philosophers chose this particular form to elaborate their ideas.

In this paper I shall be comparing two of Galileo's dialogues with a late sixteenth-century dialogue by Giordano Bruno, in order to suggest some of the ways in which the dialogue form establishes what Leibniz refers to as the "order of meditation [*ordo meditandi*]", and the benefits which it gains from allowing truth to be revealed "gradually [*paulatim*]". I shall also look at some of the rhetorical advantages of the dialogue form over the systematic treatise, and the way in which it facilitates methodological and procedural reflections in natural philosophy, by embodying and enacting particular styles of philosophical argumentation, and establishing certain kinds of epistemological claims.

GALILEO'S *DIALOGUE ON THE TWO WORLD SYSTEMS*

The role of rhetoric in science has attracted a lot of interest from historians of science in recent years,⁴ and the rhetoric of Galileo has received particular attention from both English and Italian scholars.⁵ Despite a considerable number of studies devoted to the dialogue form from the fifteenth to the seventeenth century,⁶ however, very little attention has been paid by Galileo scholars to his rhetorical deployment of this form.⁷

The scholar who has undertaken the most thorough investigation of the rhetorical and literary aspects of Galileo's *Dialogo* is Maurice A. Finocchiaro, who in his 1980 study *Galileo and the art of reasoning* undertook a meticulous (albeit at times overly schematic) analysis of the work's logical and rhetorical structure.⁸ Strangely, Finocchiaro does not have much to say about the work's dialogic form, even though he spends a few pages discussing the socratic method which emerges as a topic of discussion during the second dialogue.⁹ This is a curious omission for a study which purports to deal with the rhetorical structure of the work, and is all the more curious because Galileo himself not only talks about his choice of the dialogue form, but emphasizes its freedom, versatility and aptness for digression (a fact which is self-consciously pointed out in the dialogues themselves by the interlocutors).

In the preface to the *Dialogo*, Galileo tells his readers why he has chosen to write his work as a dialogue. "I thought it would be very appropriate to explain these ideas in dialogue form", he says, "for it is not restricted to the rigorous observation of mathematical laws, and so allows digressions which are sometimes no less interesting than the main topic".¹⁰ It would have been very easy for Galileo to have set out his defence of Copernicanism in a systematic treatise with mathematical demonstrations, just as Copernicus had done in the *De revolutionibus*, but clearly Galileo saw this as a constraint. The dialogue is "not restricted [*non esser ristretto*]" to the "rigorous" rhetoric of the mathematical demonstration. More significantly it "allows digressions", and Galileo clearly saw these digressions as departures from the main thrust of the argument, but departures which were valuable in themselves.

In a letter to Elia Diodati written in October 1629, Galileo describes the dialogue he is preparing to write on the Copernican hypothesis:

[B]esides the material on the tides, there will be inserted many other problems and a most ample confirmation of the Copernican system by showing the nullity of all that had been brought by Tycho and others to the contrary. The work will be quite large and full of many novelties, which by reason of the freedom of dialogue I shall have scope to introduce without drudgery or affectation.¹¹

Again Galileo emphasizes the freedom of the dialogue form where diverse subject matter ("many other problems") can be accommodated "without ... affectation" within the naturalizing rhetoric of familiar conversation (changes of conversational topic being more easily accepted by readers of a dialogue, obviating the "drudgery" of discursive preamble and introduction which inclusion of a diversity of topics in a treatise would entail).

Galileo also brought up his decision to write his defence of the Copernican system as a dialogue at his trial. In his second deposition, he sought to partially “excuse” himself by pointing out the necessity of fairly representing both sides of an argument in this kind of work:

[W]hen one presents arguments for the opposite side with the intention of confuting them, they must be explained in the fairest way, and not be made out of straw to the disadvantage of the opponent, especially when one is writing in dialogue form.¹²

This insistence is part of what Finocchiaro calls Galileo’s “rhetoric of indecision” in the *Dialogo*, which is announced even on the title page, where Galileo claims that he is “proposing indeterminately philosophical and natural reasons as much for the one side [i.e. the Ptolemaic] as for the other [i.e. the Copernican]”.¹³ This masquerade of probabilism is continued throughout the dialogue, where Salviati (who frequently “lapses” into the rhetoric of categorical statement and necessary demonstration) stresses the probability rather than the truthfulness of his statements, and even suggests (as Galileo’s deposition statement attests) that he is not even advancing a case for the probability of the Copernican hypothesis, but rather *acting the part* of someone who is arguing in favour of the Copernican hypothesis:

[I]n these discussions, I act as a Copernican and play his part with a mask, as it were.... I do not want to be judged by what I say while we are involved in the enactment of the play, but by what I say after I have put away the costume; for perhaps you will find me different from what you see when I am on stage.¹⁴

While he publicly maintained this pretence of arguing both sides of a rhetorical question, it is clear that Galileo sought other rhetorical advantages from dramatizing a debate between a Copernican and a defender of Aristotelians, and the digressions which he singles out as one of the principal advantages of the dialogue form, as I will show, play an important part in this rhetorical agenda.

Galileo presents his dialogue as a kind of posthumous homage to the conversations and “refined speculations” which he indulged in with two of his friends, Giovanfrancesco Sagredo and Filippo Salviati, in the company of an unnamed Peripatetic philosopher, who “seemed to have no greater obstacle to the understanding of the truth than the fame he had acquired in Aristotelian interpretation”. Galileo preserves the anonymity of this unnamed philosopher, giving him the name ‘Simplicio’ ostensibly named for “his excessive fondness for Simplicius’s commentaries” — but more probably for the pun on the Italian word for simpleton — whilst preserving the names of his deceased friends in the dialogue. In this way, he says, he can “prolong their existence, as much as my meagre abilities allow, by reviving them in these pages of mine and using them as interlocutors in the present controversy”.¹⁵ What it also did, of course, was to put his own controversial opinions concerning the Copernican hypothesis into the mouths of two conveniently deceased spokesmen. The presentation of the dialogue as a “public monument” of “undying friendship”, notwithstanding its presumed sincerity, was one of many distancing effects — what Annabel Patterson in

her book *Censorship and interpretation* has called “strategies of indirection” — used by Galileo in an attempt to cushion the controversial ideas he was advancing from ecclesiastical censorship.¹⁶

Although Galileo pointedly contrasts the logical procedures of natural philosophy, “whose conclusions are true, necessary, and independent of the human will”, with the rhetorical procedures of “human studies [*studi umani*]” such as law, where “there is neither truth nor falsehood” and so “intellectual subtlety, verbal fluency, and superior writing ability” can be used by the scholar to “make his reasoning appear and be judged better”, the *Dialogo* in actual fact makes full use of such rhetorical tools.¹⁷ Galileo is quite explicit about this in his preface where he expresses the hope that, by imitating the conversational style of his learned friends, they may help him, “through the memory of their eloquence, to explain to posterity the aforementioned speculations”.¹⁸

Finocchiaro’s work, while ostensibly accepting that rhetoric “is sometimes crucial in science; and hence, rhetoric has an important role to play in scientific rationality and the rhetorical aspects of science should not be neglected”,¹⁹ ultimately seeks (in spite of various qualifications) to separate the logical and scientific aspects of argumentation, from what he calls the “rhetorical” and “literary-aesthetic” aspects of argumentation.²⁰ He explicitly states that the purpose of his analysis of the rhetoric of the *Dialogo* is to “extract its rhetorical force, structure and content” so that he can deal with “its scientific content, scientific in the sense of natural science”.²¹ Having argued that rhetoric may play a crucial role in scientific reasoning, he goes on, in fact, to say that the historian of science and the historian of rhetoric are analysing completely different things in completely different ways. “[T]he only responsible way of defining [... the *Dialogue*’s] scientific content in terms of the rhetoric of the earth’s motion”, he argues, “is in the context of the science of rhetoric; for after all the study of the art of [rhetoric ...] is the subject of a discipline which is at least as old as Aristotle’s rhetoric. So the only business that a historian of *science* has meddling into the rhetorical analysis of the *Dialogue* is if he is writing the history of the science of rhetoric, not if he is concerned with the history of natural science”.²² So while on the one hand Finocchiaro seems to be arguing that rhetoric is “crucial”, on another he seems to suggest that the study of science’s rhetorical element is not a valid part of the history of science. This view, I would argue, is part of Finocchiaro’s debate with what he calls the “irrationalism” of Paul Feyerabend’s *Against method*, which emphasizes the importance of non-logical and non-rational elements in scientific paradigm shifts.²³ In Finocchiaro’s view, “Galileo is *first and foremost* a logician”,²⁴ and this thesis shapes and structures his view of the rhetorical structure of the *Dialogo*. Thus, while he acknowledges Giorgio de Santillana’s view that the *Dialogo* appears to be “unfinished, unpolished, at times inconsistent”, a work which “meanders at ease across the whole cultural landscape of the time, carrying in its broad sweep strange material of various origin”, his own belief is that, in spite of its “*apparent* lack of structure, or lack of explicit structure”, the *Dialogo* is a profoundly logical work.²⁵

While I would not argue with Finocchiaro’s view that there is an implicit logical

structure in the *Dialogue* (after all, Galileo refers to the discussions as “reflecting more systematically” on the questions which his interlocutors had initiated when “casually engaged in sporadic discussions”), I would argue that the work has a *deliberately and self-consciously* digressive structure, and as this is emphasized by Galileo himself both in the text and in other documents, an account of the reasons for this digressive strategy needs to be addressed in any analysis of the work’s rhetorical structure.²⁶

In presenting Galileo’s rhetorical structure as something that is detachable from its logical structure, I would argue, Finocchiaro denatures Galileo’s work, and ignores the essentially rhetorical character of argumentation in seventeenth-century natural philosophy. As Jean Dietz Moss has argued in her 1993 study of the rhetoric of the Copernican controversy *Novelties in the heavens*, Finocchiaro’s view of rhetoric is “ahistorical and evinces no knowledge of the discipline [of rhetoric]”.²⁷ Not only does he neglect what Moss calls the “powerful alliance” of rhetoric and dialectic in the seventeenth century (referring to rhetoric as “alogical but not illogical”²⁸), but he also makes an artificial distinction between what he insists on calling the “literary-aesthetic” and “merely cosmetic verbal expressions of desires and intentions” on the one hand, and “rhetoric”, seen as non-logical but intellectually substantial (“a type of intellectual content”) on the other.²⁹ Finocchiaro’s definition of rhetoric, I would suggest, is one which would be equally unrecognizable to the seventeenth-century orator, literary author and natural philosopher, and says more about Finocchiaro’s anxieties about the perceived threat of Feyerabend’s “rhetoric of irrationalism” to the history of science than it does about the actual rhetorical procedures of Galileo’s text.

In his 1989 abridged edition of *The two world systems*, Finocchiaro returns to the subject of the literary and rhetorical aspects of Galileo’s work. Singling out a witty exchange between Salgreto and Simplicio, he notes that it is

one of many examples giving the *Dialogue* considerable rhetorical and aesthetic value. Indeed the book can be read from the viewpoint of literature, though such an appreciation depends on understanding its scientific and philosophical content. Like all great literature, the book’s aesthetic dimension is hard to translate since such an accomplishment would involve essentially the creation of a new work of art. I do not claim that my translation does justice to the literary power of the original, for I have focussed on intellectual and conceptual accuracy. We should neither be oblivious to such passages nor let ourselves be distracted from the main thread of the discussion.³⁰

Finocchiaro’s point seems to be that an appreciation of the literary effects of Galileo’s text and “intellectual and conceptual accuracy” are potentially at odds, a “distraction” from the “main thread” of the book’s arguments. This is quite ironic as Galileo, in choosing the dialogue form for this work over that of a straightforwardly analytical treatise written in a unified authorial voice, makes great play (as I have already indicated) out of its essentially digressive nature, and draws attention to this in his prefatory epistle. What I would like to consider first of all, is the role of digression in Galileo’s text, and additionally, why this literary strategy is a vital part of its

intellectual agenda, rather than something which distracts from a presumed “main thread” of argumentation.

It is something of a truism in studies of Classical or Renaissance dialogue that there is a tension between the dialogic and monologic nature of the literary dialogue. Whilst preserving the outward form of free discussion and open argument, dialogues are, after all, produced by authors, who ventriloquize various kinds of opposition and disagreement, but often in such a way that the author’s view of a particular question (often placed in the mouth of a privileged interlocutor, such as Socrates in the dialogues of Plato, or Salviati in Galileo’s dialogues) is allowed to predominate and finally to quash the counter-arguments proffered by other interlocutors. The dialogue is thus a monologue masquerading as a dialogue; but it is, perhaps, fair to say that the reverse is also true. Ernst Cassirer, in one of the essays of *The logic of the cultural sciences*, has drawn attention to the essential interconnectedness of monologue and dialogue. “Plato has said”, Cassirer writes,

that there is no other entrance into the world of ideas than through “questioning and answering each other in speech”. In question and answer the ‘I’ and the ‘you’ must be distinguished, not only to understand each other but to understand themselves. Both aspects constantly intervene in one another.... This “dialectical” relation can be displayed not only in real dialogue but also in monologue. For even thinking to oneself is, as Plato remarked, “a conversation of the soul with itself”. Paradoxical as it may sound, we can say that in the monologue it is the function of *splitting up* that prevails, whereas in the dialogue it is the function of *reunification*. For the “conversation of the soul with itself” is possible only if the soul, as it were, splits itself. It must take over the task of speaking and hearing, of questioning and answering.³¹

The question that this splitting-up and reunification of authorial voice in dialogue raises for any particular instance of the genre, is: What ethos is served by this splitting, and what is sought in the coercive reunification of its dissonant voices? It is this question which I will pose in relation to the dialogues of Galileo and Bruno, and the answer, I think, will show that Finocchiaro’s insistence on a monological “thread” of argumentation in dialogue misses the rhetoric or ethos of these kinds of natural philosophical texts.

In order for new philosophical discourses to assert themselves in the late sixteenth and early seventeenth centuries, they were obliged to figure in advance the objections of an immensely powerful Aristotelian consensus. This consensus had considerable institutional and social investments, which new modes of philosophizing threatened to undermine. The objections of Aristotelian philosophers often made use of counter-arguments which were not always philosophical or logical in character. Both Bruno and Galileo, while purportedly transmitting new modes of philosophical discourse, also created texts which ventriloquized their real (or imagined) Aristotelian opponents, not simply to anticipate possible objections to their theories, but to characterize the philosophical problems of the Aristotelian consensus as they saw it, and especially

the question of authority (as opposed to reasoned argument) in philosophical discourse. The dialogue is a form which lends itself to this task, because it *dramatizes* the Aristotelian resistance to new modes of thinking and at the same time shows the new modes of reasoning *in action*. That is to say, the dialogue is a performative text which, in a lively manner, confirms the superiority of new forms of reason and reveals the irrational basis of much Aristotelian conservatism. Galileo's *Dialogue on the two world systems* is performative in this way, and to that extent it is not simply a work of natural philosophy, but a self-reflexive work on natural philosophy (a meta-discourse). This is what Alexandre Koyré meant when he said:

The *Dialogue on the Two Chief World Systems* pretends to be an exposition of two rival astronomical systems. But, in fact, it is not a book of astronomy, nor even one of physics. It is above all a book of criticism, a work of polemic and struggle; it is at the same time a pedagogical work, and a philosophical work....³²

Like Bruno's dialogues, of course, Galileo's work is *both*: that is to say, it is a book which seeks to present an exposition of rival astronomical systems, but it is also at the same time a work of criticism and polemic, and this double agenda is reflected in the dramatic structure of the dialogue itself. It is a work which is simultaneously a work of scientific discourse and scientific meta-discourse (or, to use Finocchiaro's terms, a work of scientific "methodology" — although in so far as it purports to examine the *motivations* for the Aristotelian consensus, it steps beyond the bounds of methodology *per se* into a kind of social-psychological commentary). The digressive form of the dialogue is what allows him to unify these two interrelated projects without (in Galileo's words) "drudgery or affectation". The dialogue form allows dramatic *caricatures* of Peripatetic criticisms of the Copernican hypothesis, and in addition to showing their arguments failing ("showing the nullity" of their objections, as Galileo puts it) and ridiculing (in the figure of Simplicio) their haplessness in the face of close reasoning, there is a serious aim behind the caricature and satire: to reveal the obstinacy of Aristotelian beliefs, and their irrational appeal to extra-philosophical legitimacy.³³

First, however, I want to focus on the ways in which the dialogue enables Galileo to develop the logical arguments in favour of the Copernican hypothesis. As Leibniz suggests in his *Prima de motu philosophia* (which I quoted earlier), the challenge posed to the author (or perhaps one should say *orator*) is "to compose a speech in such a way that truth itself might gradually shine out of the darkness, and knowledge might grow spontaneously in the mind". This is only possible, he says, if the orator "has himself gone into the reasons very carefully on his own, before taking it on himself to teach others". Galileo certainly saw his *Dialogo* as serving such a pedagogical function. This is clear from his allusions in the text to the Socratic method of the Platonic dialogues. In Plato, Socrates famously claimed not to teach, but to practice a kind of "mid-wifery [*maieusis*]" to knowledge — his task was not to inform his interlocutors of new opinions, but simply to remind them of what they already knew, but had forgotten (*anamnesis*). That Galileo had something like this

in mind can be seen from the following exchange with Simplicio:

Salv. The answer to [this question ...] depends upon some facts which you know and believe no less than I do; but, because you do not remember them, you do not see the answer. So without my teaching them to you (because you already know them), but by simply reminding you of them, I will make you answer the objection yourself.

Simp. I have thought several times about your manner of reasoning; it makes me think you are inclined to accept Plato's doctrine that "our knowledge is a form of recollection". So, please resolve my doubt by telling me what you mean.

Salv. I can explain by words and deeds how I feel about Plato's doctrine. In the arguments discussed so far, I have already explained myself more than once by deeds; I will follow the same style in the particular case at hand.³⁴

While Galileo steers clear of explicitly stating that he is an adherent of the Platonic doctrine of recollection, it is clear that he sees the argument of the *Dialogo* as functioning *more socratico*, and keeps playing with the idea of *maieusis* and *anamnesis* throughout this dialogue. "You will learn," Salviati tells Simplicio, "or rather you already know, the rest [of the argument] in the same manner you learned so far; by thinking about it, you will remember it on your own. However, to speed up the process, I will help you remember it".³⁵ Galileo also talks about expounding his ideas "by interrogations [*per interrogazioni*]" in his *Discorsi e dimostrazioni*,³⁶ and continues his playful identification with the Socrates of Plato, by referring to the "demon [*demonio*]" of his interlocutors.³⁷ Galileo clearly saw the pedagogical value of the Platonic dialogue — which dramatizes the reasoning process, making it explicit, rather than implicit. One learns how to practice logic, Salviati tells Simplicio at one point, "by reading books full of demonstrations, which are exclusively the books of mathematicians and not those of the logicians".³⁸ By having Simplicio advance Aristotelian ideas, using Aristotelian reasoning, and then showing Salviati demolishing them with his mathematical reasoning, Galileo teaches his form of logical reasoning, and shows the "nullity" of the "books of ... logicians". In the *Dialogo*, Galileo has Salviati use a device which we know that Galileo himself employed in private disputations. In early 1616, well before the composition of the *Dialogo*, the poet and diplomat Antonio Querengo described Galileo's debating technique. "[A]lthough the novelty of his opinion leaves people unpersuaded," Querengo wrote,

yet he convicts of vanity the greater part of the arguments with which his opponents try to overthrow him [... and] before answering the opposing reasons he amplified them and fortified them with new grounds which appeared invincible, so that, in demolishing them subsequently, he made his opponents look all the more ridiculous.³⁹

If the function of Simplicio is generally to play the Aristotelian 'stooge' to Salviati's mathematical philosopher, the role of Sagredo is to advance more sophisticated objections and "difficulties [*difficoltà*]" — often of a technical, mathematically informed nature, so that Salviati might have the opportunity to enhance, ventilate or develop

his arguments, or provide proleptic solutions to anticipated objections. “The questions are good,” Salviati tells Sagredo, “and I have often thought about them. I shall tell you my reasoning, and what I have ultimately deduced therefrom”.⁴⁰ Sagredo and Salviati, then, are in effect aspects of Galileo’s own reasoning processes, and he uses Sagredo in order to show the *process* of the reasoning, rather than simply the end-result. That Galileo deliberately embraced this dynamic mode of representing arguments can be seen from the following exchange in the *Discorsi*. Salviati is apologizing for the digressive nature of dialogic argument, while Sagredo supplies us with its defence:

Salv. ... perhaps it will seem to you inopportune to digress [*diuertir*] at length from the road that we started on, and hence will be distasteful.

Sagr. Please let us enjoy the benefit and privilege that comes from speaking with the living and among friends, about things of our own choice and not by necessity, which is very different from dealing with dead books that excite a thousand doubts and resolve none of them. So make us partners in whatever reflections suggest themselves to you in the course of your discussions.⁴¹

The dialogue is living speech (*parlar con i viui*) which makes the interlocutors (and the reader) ‘partners’ (or “participants” — *partecipi*) in Galileo’s reasonings. Significantly, Sagredo’s evocation of the discussion of “dead books [*libri morti*]” which “excite a thousand doubts and resolve none of them”, recalls one of the dominant literary modes of Aristotelian debate, the *quaestio disputata*, understood as a stale or ‘dead’ form.⁴² This attack on the literary practices of contemporary Aristotelianism, can also be seen in Salviati’s mocking of Simplicio’s argument that solutions to problems can often be found by juxtaposing fragments from different parts of the Aristotelian *corpus*. Salviati compares this appeal to things “scattered here and there [*disseminate in qua, e in là*]” to the *centonismo* and piecemeal quotation of humanist argumentation, which were increasingly scorned by the literati of the *seicento*:

But then, what you and other learned philosophers do with Aristotle’s texts, I will do with the verses of Virgil or Ovid, by making patchworks of passages [*centoni*] and explaining with them all the affairs of men and secrets of nature.⁴³

By contrast to these stale and bankrupt literary modes of argumentation, Galileo presents the dialogue as a vital and dynamic mode of logical investigation. After mocking the patchwork nature of Aristotelian discourse, Salviati exhorts Simplicio to “come freely with reasons and demonstrations (yours or Aristotle’s) and not with textual passages or mere authorities, because our discussions are about the sensible world and not about a world on paper”.⁴⁴ While Galileo does not quarrel with Aristotelianism as an academic discipline (he approves of Aristotle’s works “being examined and diligently studied”), he does quarrel with what he sees as a refusal to engage rationally with other theories.⁴⁵ He abhors the idea of “submitting to him in such a way that one blindly subscribes to all his assertions and accepts them as unquestionable dictates, without searching for other reasons for them”. This results in a philological rather than a philosophical use of Aristotle’s works. It is “shameful” he

says, when dealing with “demonstrable conclusions”, to use texts (rather than demonstrations) to “shut the mouth of an opponent”. These appeals to texts as inviolable authorities makes such scholars “historians or memory experts [*ò Istorici, ò Dottori di memoria*]” rather than philosophers.⁴⁶

What Galileo sought in his dialogues was the ability to present his reasonings outside of the more restricted (*ristretto*) literary modes which characterized Aristotelian philosophical controversy: the *quaestiones*, *commentaria* and *animadversiones* inherited from the scholastics. In the *Discorsi*, Salviati — in a prelude to a potentially controversial discussion of indivisibles — invokes the dialogue form as a space of caprice and provisionality outside of the normative certainty of theological doctrines:

So with our customary freedom [*la solita libertà*], let it be agreed that we bring in our human caprices, as we may well call them in contrast with those theological [*sopranaturali*] doctrines that are the only true and sure judges of our controversies and the unerring guides through our obscure and dubious, or rather labyrinthine, opinions.⁴⁷

Try as he might, Galileo was hard pressed to invent this “customary freedom” in an atmosphere which was far from tolerant in matters of philosophy. His only recourse was to establish the dialogue as a space for the rhetorical elaboration of opinions *in utramque partem*, to be judged by higher authorities.⁴⁸ While Sagredo plays (rather too well) the part of someone convinced by Copernican arguments and even Simplicio is made to voice grudging admiration for the “beautiful new and forceful considerations”⁴⁹ advanced by Salviati, Salviati himself insists upon a strict, probabilistic neutrality:

For I have not concluded this, just as I am not about to conclude any other controversial proposition; instead I have meant to produce, for one side as well as for the other, those reasons and answers, questions and solutions which others have found so far, together with some that have come to my mind after long reflection, leaving the decision to the judgement of others.⁵⁰

Both Finocchiaro and Moss have commented on the failure of this rhetorical strategy. For Finocchiaro the failure is due to the fact that, in spite of Galileo’s claims for oratorical neutrality, the *Dialogo* “[leaves no doubt] as to which side is more plausible or probable” and so “determines a decision” by default.⁵¹ Moss sees the problem arising as a consequence of two very different conceptions of the dialogue which were current in the seventeenth century. On the one hand there is the Platonic dialogue, which functions by “exposing contrary ways of looking at issues in order to arrive cooperatively at the truth”. On the other hand there is the Ciceronian dialogue, “a more managed rhetorical examination of a subject with a presiding speaker and an airing of opinions on all sides of an issue, but without one side clearly winning”.⁵² Moss argues that Galileo puts himself in an impossible position: if he pursues the Ciceronian ethos implied by his title, he will compromise his belief in the Copernican hypothesis. If he pursues a Platonic ethos, and shows his interlocutors accepting the

truth of Copernicanism, he will be in violation of the 1616 edict. The “indeterminate dialectics” implied by his title is, Moss argues, a “contradiction in terms” — in so far as the function of dialectics is to determine a question under investigation.⁵³ However, Moss’s point doesn’t take into account the intermittent probabilistic rhetoric of the dialogue. The arguments are posited as “indeterminate” because, as Galileo continually emphasizes, he is not making absolute truth-claims but adopts a *probabilistic* viewpoint which argues for the greater probability and likelihood (rather than the truth) of the Copernican position. In this way Galileo’s dialogue, in spite of its constant flaunting of its Socratic method, is masquerading in the probabilistic weeds of the Ciceronian dialogue.⁵⁴ In a carefully-managed rhetorical balancing act Galileo leads his readers to believe that he is writing a Ciceronian style of dialogue, but is in actual fact writing a Platonic style of dialogue in which the Copernican hypothesis is tacitly being posited as the truth.

While the preceding comments do not really do justice to the various ways in which Galileo uses the dialogue form to positively advance his arguments (whilst appearing not to), I want to turn my attention now to the way in which the *Dialogo* addresses itself — in the “critical” mode identified by Koyré — to the problematic of the Aristotelian consensus, and how this becomes an integral *formal* feature of Galileo’s works.

The digressions to which Salviati repeatedly (and self-consciously) draws the reader’s attention, are in fact precisely organized to permit the introduction of this critical material.⁵⁵ While both Salviati and Sagredo invoke these digressions as detours and diversions from the main argument, they are (as Galileo stresses in his preface) “not less interesting [*non meno curiose*]” than the positive arguments. At the end of the first day, for example, Sagredo asks that they avoid “another sort of ceremonial digression” because, he says, “right now I am a philosopher and have come to school, and not to city hall”. That is to say, he dismisses the digressions as rhetorical rather than substantial. Salviati’s response marks off this change of discourse by announcing the ensuing discussion as “the *beginning* of our reflections [*il principio della nostra contemplazione*]”.⁵⁶ To take these statements at face value, however, would be misleading, as “ceremonial” or not, the digressions contribute to the task of gaining acceptance for the Copernican hypothesis. In these digressions, what Galileo introduces are the extra-philosophical obstacles to the acceptance of non-Aristotelian modes of philosophizing. Take, for example, the following exchange between Salviati and Simplicio (whose feathers have been ruffled by Salviati’s remarks about a “paralogism” in one of Aristotle’s arguments):

Simp. Please Salviati, speak of Aristotle with more respect. How can you ever convince anyone that he could have committed a serious error like assuming as known what is in question, given that he was the first, only and admirable explainer of syllogistic forms, demonstrations [and] fallacies ... and in short the whole of logic? Gentlemen, one must first understand him perfectly, then try to impugn him.

Salv. Simplicio, we are here talking to each other in a friendly manner [*familiarmente*] to inquire into the truth. I will never hold it against you that you show me my errors; when I do not penetrate Aristotle's mind you should freely rebuke me, and I will be grateful to you for it. In the meantime, allow me to state my difficulties and also to respond to your last remarks.⁵⁷

Galileo here contrasts two styles of natural philosophical argumentation: one which insists on the infallibility of Aristotle and prior mastery of his works as a *sine qua non* for meaningful philosophical discussion, and on the other what Finocchiaro describes as the "epistemological modesty" of Salviati's position which stresses amicable and open discussion of philosophical questions. What Galileo attempts to do through these kinds of interventions is to show that the resistance of Aristotelians to Copernicanism (or *any* non-Aristotelian argument in natural philosophy) has no philosophical foundations, but is rather social and institutional in character. Thus Simplicio argues (or at least expresses the opinion) that Copernicanism is a threat to philosophical, social and even cosmic order:

This manner of philosophizing tends to subvert all natural philosophy and to throw into disorder and to upset the heavens, the earth, the whole universe. But I believe the foundations of the Peripatetics are such that one need not fear that upon their ruins one can erect new sciences.⁵⁸

In Salviati's response, Galileo counters this unreasoning response with an argument in favour of open debate:

Philosophy itself cannot but benefit from our disputes because if our thoughts are true then we will make new gains, and if they are false the their refutation will confirm further the earlier doctrines.⁵⁹

Salviati advances the argument (which became something of a truism amongst proponents of the "new philosophy") that Aristotle himself would have been more open to modern ideas like the Copernican hypothesis because he gave "priority to sensible experience over natural theorizing".⁶⁰ His Aristotelian contemporaries on the other hand, are seen as unworthy philosophical successors, "submissive and slavish servants of Aristotle" who "deny all experience and all observation in the world and even refuse to use their senses in order not to have to make the confession; they would say the world is as Aristotle said and not as nature wants".⁶¹ Simplicio, however, invokes the same community of philosophers as guardians of philosophical order who do not want to be left "without a guide, without protection, and without a head in philosophy".⁶² Through the mediating figure of Sagredo, Galileo simultaneously empathizes with and criticizes such slavish clinging to philosophical authority. In a rhetorical passage, which is deliberately signalled in a marginal gloss as "Declamazione di Simplicio", Sagredo says,

I sympathize with Simplicio, and I see he is much moved by the strength of these very conclusive reasons; on the other hand, he is much confused and frightened by the fact that Aristotle has universally acquired great authority, that so many

famous interpreters have laboured to explain his meaning, and that other generally useful and needed sciences have based a large part of their reputation on Aristotle's influence. It is as if I hear him say: "On whom shall we rely to resolve our controversies if Aristotle is removed from his seat? Which other author shall we follow in the schools, academies and universities? Which philosopher has written on all parts of natural philosophy, so systematically, and without leaving behind even one particular conclusion? Must we then leave the building in which are sheltered so many travellers? Must we destroy that sanctuary, that Prytaneum, where so many scholars have taken refuge so comfortably...? Must we tear down that fortress where we can live safe from all enemy assaults?"⁶³

This apostrophic evocation, whilst ostensibly compassionate, also functions to point out the unworthy motivations behind Aristotelian resistance to new modes of reasoning, much as *exclamatio* is later used to support Galileo's arguments and to undermine those of Copernicus's detractors.⁶⁴

Sagredo is also used by Galileo to inveigh against the irrationality of arguments from authority. His anecdote about the Aristotelian physician who is presented with anatomical evidence and refuses to accept it because it contradicts Aristotle's texts, ends with Sagredo railing at the "absurdity" of the peripatetics who fail to "produce other experiences or reasons of Aristotle, but [resort to] mere authority and the simple *ipse dixit*".⁶⁵ The absurdity of this obstinacy is dramatized in the figure of Simplicio, who although he is often forced into positions where he has to concede the forcefulness of Salviati's arguments, insists on clinging to his Aristotelian faith, if only for aesthetic reasons:

I would like to keep my old opinion without having to hear anything else, because it seems to me that even if it were false, the fact that it is supported by such likely reasons would render it excusable. If these are fallacies what true demonstrations were ever so beautiful?⁶⁶

By contrast, Galileo uses the figures of Salviati and Sagredo to dramatize the new philosophical *modus operandi*, which is open to new arguments, and especially to new observations and experiments. In the *Discorsi*, this difference of philosophical styles is self-consciously articulated by Sagredo, who ironically contrasts the freedom of their own "private" discussions with the problematic realm of public philosophical discourse which is subject to unphilosophical motivations:

Not only this proposition, but many others of yours are so far from the opinions and teachings commonly accepted, that to broadcast them publicly will excite against them a great number of contradictors; for the innate condition of men is to look askance on others working in their field whose studies reveal truth or falsity which they themselves fail to perceive. By calling such men [as you] "innovators of doctrines [*innouatori di dottrine*]", a title most unpleasant to the ears of the multitude, they strain to cut those knots they cannot untie, and to demolish with underground mines those edifices which have been built by patient artificers, working with ordinary instruments. But to us, who are far

from any such motives, the experiments and reasons adduced up to this point are quite satisfactory.⁶⁷

The ignoble “motives” (or “pretensions [*pretensioni*]”) of the Aristotelians are ascribed to their viciousness (their “innate condition”) rather than to a consideration of the reasons and experiments. Later, Sagredo suggests that many of his readers will prefer the “long and inexplicable altercations [*lunghe, & inesplicabili altercazioni*]” of Aristotelian philosophy to the “evidence [*evidenza*]” and “easiness [*agevolezza*]” of his experimental proofs concerning accelerated motion. The refusal to accept such “short and easy reasoning” is attributed by Salviati to “envy” against those who “reveal fallacies” and “a desire to maintain inveterate errors rather than to permit newly discovered truths to be accepted”. Such envious scholars, he suggests, contradict arguments which “in their own hearts” they know to be true, merely to “to keep down the reputations of other men in the estimation of the common herd of little understanding”.⁶⁸ It is because of this perceived irrationalism of the Aristotelian consensus that Galileo, both in the *Dialogo* and in the *Discorsi*, feels obliged to present his positive arguments, alongside such critiques of Aristotelian prejudice. By the time he wrote the *Dialogo* he had already attracted some fairly vituperative criticisms of his natural philosophy. In the *Discorsi*, for example, he recalls the critique of Orazio Grassi, who (he says) criticized his *Il saggiatore* “very inappropriately [*assai poco a proposito*]” by linking his arguments about indivisibles with the atheism and impiety of the Epicureans.⁶⁹ Significantly he puts the criticism of Grassi into the mouth of the Aristotelian Simplicio, who contrasts the poor taste of this “malicious opponent” with the religious orthodoxy and “temperate and orderly mind” of Salviati.⁷⁰ Galileo’s dialogues then are as much a critique of the indecorous and irrational philosophical discourse of his Aristotelian opponents as they are a positive treatment of his new mathematical philosophy, and the continuously digressive structures of his works reflect this double task.

GIORDANO BRUNO’S ASH WEDNESDAY SUPPER

Over forty years before the publication of the *Dialogo* and the *Discorsi*, Giordano Bruno had published a series of dialogues on natural philosophy written in the vernacular, including two works — *De l’infinito, universo e mondi* and *Cena de le ceneri*,⁷¹ both published in 1584 — with an astronomical-cosmological focus. In the *Cena* Bruno, like Galileo, had a “new philosophy” to advance, and wished to promote the Copernican hypothesis (or at least a Nolan version of a heliocentric cosmology).⁷² Also like Galileo, he used the dialogue form both to expound his philosophy and to reflect upon the problematic nature of the Aristotelian consensus. In his important study of the reception of Giordano Bruno, Saverio Ricci — despite the fact that there is no positive evidence that Galileo was familiar with the Italian dialogues of Bruno — found sufficient “affinities” between Galileo’s *Dialogo* and Bruno’s astronomical and cosmological themes, to dedicate to them a lengthy section of his chapter on Bruno and the “new science [*nuova scienza*]”,⁷³ and he suggests that at least one

of Galileo's contemporaries, Tommaso Campanella, made connections between the cosmological ideas of Bruno and Galileo.⁷⁴ In 1997 Hilary Gatti suggested that "the time seems ripe to re-propose a comparison between Bruno and Galileo",⁷⁵ and I intend to follow Gatti's suggestion here, although the focus of my own comparison will not be on the cosmologies of the two Italian natural philosophers, but rather on the very different rhetorical uses which they make of the dialogue. While I do not think that there is much likelihood of a direct influence of Bruno's *Cena* on Galileo's *Dialogo*, I do think that a comparison between their respective uses of the dialogue form to defend the Copernican hypothesis is instructive for an understanding of early modern scientific rhetoric.

In the *Cena*, Bruno emphasized the logical (or dialectical) order of his presentation. As the subtitle of this work suggests, Bruno was keenly aware of the dialogue as a means by which to present an ordered argument; the supper, he says, is "described in five dialogues, through four interlocutors, with three considerations on two subjects [*Descritta in cinque dialogi, per quattro interlocutori, Con tre considerazioni, Circa doi suggestij*]"⁷⁶ Whilst Bruno emphasizes the orderly presentation of propositions in his preface to the dialogue, he also takes pains to emphasize the "historical" (or narrative) nature of the work, which purports to describe a supper held at the London residence of Sir Fulke Greville, where Bruno disputed the Copernican hypothesis with two Oxford scholars, represented by the figures Nundinio and Torquato. These two characters, who are distant relations of Galileo's Simplicio, are described as "two ghastly harridans, two dreams, two ghosts, two quartan agues".⁷⁷ Bruno distinguishes carefully between the "historical" (or narrative) aspect of the dialogue, and its underlying argumentative structure:

while the historical meaning of all this is being sifted and then tasted and chewed, we shall draw appropriate topographies of a geographical, ratiocinative and moral order, and then make some speculations of a metaphysical, mathematical and natural order.⁷⁸

Bruno compares his historical narrative to the "Silene statues" described in Plato's *Symposium* 215b: statues of satyrs which contained images of the gods. In his work there are, he says, "many diverse subjects" which must be

put together [so] that they do not appear to constitute a single topic, but appear here like a dialogue, here a comedy, here a tragedy, here poetry, and here rhetoric, here praise, here vituperation; here demonstration and teaching; here we have now natural philosophy, now mathematics, now morals, now logic; in conclusion, there is no sort of knowledge of which there is not here some fragment. Consider, sir, that the dialogue is historical and, while occasions, movements, passages, meetings, gestures, affectations, discourses, propositions, answers, subjects and blunders are reported, all of them subjected to the rigours of judgement of four men, there is nothing that will not be set forth for some reason. Consider also that not one word will be superfluous, for everywhere there will be things of no little importance to reap and unearth; and perhaps [there will be] more where less is apparent.⁷⁹

Just as Galileo's dialogue parades its digressive nature, so Bruno draws attention to the generic complexity of his dialogue, whilst insisting upon its underlying philosophical coherence. While Bruno's self-conscious literariness is more burlesque and baroque, and less urbane and measured than Galileo's, they both exploit the tension between the linear flow of logical argumentation and the apparent discontinuities of their dialogic narratives. While the second of the five dialogues is presented by Bruno as being "more poetic and perhaps allegorical than historical",⁸⁰ his presentation of the remaining four dialogues emphasizes the orderly and logical treatment of particular propositions. The purpose of the whole, he says, is "to amplify grave and valuable propositions" in opposition to the "trifles and ... inadequate grounds" propounded by his academic opponents.⁸¹ Teofilo, who plays the Salviati role in Bruno's dialogue, insists that his colleagues take account of the fact that his "discourse [is] a dialogue since although we are four persons, we will be two in the matter of proposing and answering, discoursing and listening".⁸² In actual fact, the *Cena* is a dialogue *within* a dialogue, as Teofilo in the narrative relates the discussions which took place between "the Nolan" (Bruno) and Greville's guests to his friends Prudenziio and Smitho. Bruno is thus an indirect presence in the dialogues (much as Galileo appears only as the shadowy "Academician" in the debates of Salviati, Sagredo and Simplicio). Nonetheless the structure of proposition, reply and reasoning is common to both. Like Galileo, Bruno uses the dramatic potential of the dialogue form to contrast two philosophical styles: those "happy and talented minds [*ben nati ingegni*]" who "have free intellect and clear sight and are children of heaven", represented in the dialogue by Teofilo (and the Nolan), and those who "because of some credulous folly, stubbornly wish to remain in the darkness of what they have once learned badly", represented by the Oxford doctors and Prudenziio.⁸³ In Bruno's dialogue the role of Smitho (like that of Dicsono in *De la causa*) is comparable to that of Sagredo: an intelligent, but ultimately sympathetic foil to help advance the author's ideas.

Bruno, who had something of a gift for satire and burlesque, also makes more use of comical devices in undermining his academic opponents. Taking advantage of the fact that his dialogue is written largely in reported speech, he indulges in comical caricatures of academic pomposity. Thus Torquato is described preparing to answer a point made by the Nolan:

[he] rose, withdrew his arms from the table, shook his back a little, puffed and sprayed somewhat with his mouth, arranged the velvet *beretta* on his head, twisted his moustache, put his perfumed face in order, arched his brows, expanded his nostrils, settled himself with an oblique look, put his left hand to his left side in order to start the duel, pointed the first three fingers of his right hand and began to wag his hand back and forth, saying: *Tune ille philosophorum protoplastes?*⁸⁴

If Galileo's Simplicio is relatively successful at adumbrating Aristotle's ideas, Bruno makes his Oxford doctors far less effectual. The Nolan asks Torquato "to advance propositions through which he [Torquato] could argue positively or probably in favour of the other protoplasts against this new protoplast", and accuses him of

using “words and jokes [*paroli e scommi*]” rather than “reasons [*raggioni*]”.⁸⁵ Bruno’s technique differs from Galileo’s in this key respect: while Galileo takes pains to give his imagined Aristotelian opponents a reasonably fair representation of their arguments (even using Salviati to strengthen some of their arguments before demolishing them), Bruno resorts to cruder caricatures. Bruno’s opponents are shown superciliously delivering Latin tags from Erasmus, or paraphrases of Osiander’s prefatory epistle to the *De revolutionibus*, in place of substantial arguments.⁸⁶ While Bruno espouses the same argumentative ethos as Galileo, i.e. that one should not “search and ask according to one’s own principles, but according to the ones admitted by the opponent”, he makes Torquato and Nundinio into stooges who are unable to marshal any “solid arguments and persuasions” against Copernicus’s hypothesis, and are thus declared to be “ignorant ... of the art of disputation”.⁸⁷ This is made clear from the beginning, when Teofilo says of Torquato that he had only “paged through” Copernicus’s book and “remembered only the names of the author, the book, and the printer, the place where it was printed, the year, the number of quires and pages...”⁸⁸ The irrationalism of Aristotelian opposition to Copernicanism is also painted more luridly in Bruno, whose interlocutors laugh, sneer, shout over their opponents’ arguments, and even threaten to resort to physical violence.⁸⁹ Despite the disparity of their respective treatments of their Aristotelian opposition, there is a common attention to the un-philosophical nature of anti-Copernicanism. Thus, when criticizing Nundinio’s insistence that the universe is finite, Teofilo says that he is

[o]ne of those who say what they say through faith or habit; and deny what they deny because of unpopularity and novelty (as is common among those who think little and are not masters of their own rational as well as their natural actions).⁹⁰

This blind, habitual acceptance of authority — as in Galileo’s complaint against the unfair treatment of refutations — is evoked as an almost insurmountable obstacle to genuine philosophical debate. Just as Galileo has Salviati ask Simplicio to abandon the usual methods of Aristotelian controversy (i.e. the marshalling of Aristotelian texts, rather than Aristotelian arguments), so Bruno inveighs against the Aristotelian tendency to cavil and quibble in debate. “I will not give them leave”, says Teofilo,

to take the role of interrogators or disputants before they have heard the whole course of philosophy; because, if the teaching is perfect in itself and has been completely understood by them, it will purge all doubts and clear away all contradictions.... For it is impossible to know how to doubt and inquire purposefully, and with profitable system, about any art or field of knowledge, if one has not first listened.⁹¹

Bruno’s ideal of philosophical debate is thus rather monologic. His opponents should sit in silence until they have heard all the principles of his philosophy elaborated, unlike the Aristotelian academics “who want to discuss even the clearest things, wasting as much time as can be imagined”.⁹² Smitho here objects that Teofilo’s desire

runs contrary to the conservative inertia of contemporary institutions. In the universities, he says, “there is an innumerable multitude of those who presume to learning and esteem themselves worthy of being constantly listened to”. They are full of critical quibblers (*Aristarchi*). Smitho argues that Teofilo is underestimating people’s attachment to the tradition in which they have been brought up (“the discipline and habits of our home”). “Do you not know how powerful is the habit of believing and of being brought up on certain opinions...?”, he asks rhetorically. “Now tell me, with what art will you, more quickly than someone else, win the ear of a person in whose mind there is perhaps less tendency to listen to your propositions than to those of a thousand others?”⁹³ Bruno’s answer is to rely on *synderesis*, the divine spark in the human soul which recognizes truth, a “true guide” which “illuminate[s] your inner spirit”. Smitho, however, persists in his analysis of the socially conditioned nature of consensus. “[O]ne usually follows the common opinion,” he retorts, “so that in case of error, he will not be without general approval and companionship”. Bruno retreats into an élitist position: “wise and sublime men are ... rare”, he says, and thus it is “safer to seek the true and the proper outside the mob”.⁹⁴

Galileo, too, was sceptical about the ability of the general populace to appreciate the arguments in favour of the Copernican hypothesis. He refers to the “stupidities” which “make the common people stubbornly unwilling to listen (let alone accept) these novelties”, and despairs of the idea that logical argumentation will prevail with them: “what gain would you think you could ever make with all the demonstrations in the world when dealing with brains so dull that they are incapable of recognizing their extreme follies?”, Salviati asks.⁹⁵ Despite the polemical tenor of the *Dialogo*, Galileo has serious doubts about whether one could prevail over the old Aristotelian cosmology simply by refuting them logically. “It is inane to think of introducing a new philosophy by refuting this or that author,” says Salviati,

one must first learn to remake human brains and render them fit for distinguishing truth from falsehood, something that only God can do. But, where have we come, moving from one argument to another? I would not know how to get back to the main road without the guidance of your memory.⁹⁶

Although he rejects the task as “inane”, it is clear that the digressive departures from the “road” of the main argument are devoted precisely to the refutation of Aristotelian cosmology in Galileo’s work. If he appears to defer the task of “remaking” human brains to God, it is equally clear that he hoped the main road of his argument — the maieutic dialogue in which Simplicio is helped to remember what he already knows — is Galileo’s attempt to remake the minds of his contemporaries by showing the force of his arguments in action. It is the dialogue form itself — which allows both truth and the refutation of error to emerge *paulatim* — that allows Galileo to perform these two functions simultaneously and with the naturalness of familiar conversation. In the *Discorsi*, the printer’s letter to the reader acknowledges this twofold purpose of Galileo’s dialogues.

[T]hose who through the acuity of their wits, have reformed things which have been found previously, discovering the fallacies and errors of many ages, are worthy of great praise and admiration: such discovery is considered praiseworthy, even if they have only removed the falsity without introducing the truth *per se*, it being so difficult to attain; in accordance with the saying of the prince of orators [*i.e. Cicero*]: *Vtinam tam facîle possem vera repirire, quam falsa convincere* [“I only wish I could discover the truth as easily as I can expose falsehood”].⁹⁷

Galileo’s work, it goes on to say, does both: it shows the fallacies and falsity of the arguments of the Aristotelian defenders of the Ptolemaic cosmology, and puts forward convincing mathematical arguments for the Copernican hypothesis. If he was not completely convinced that logical argumentation was in itself enough to enable Copernicanism to prevail (and he was, after all, labouring under the handicap of his pretence of probabilistic neutrality), it seems clear that he saw the dialogue would be an effective means of leading people towards the truth. One learns logic, as he says, “by reading books full of demonstrations, which are exclusively the books of mathematicians...”.⁹⁸ By giving his readers a vivid and blow-by-blow representation of such demonstrations in action he clearly hoped he would render his reader’s minds “fit for distinguishing truth from falsehood”. Bruno, I think, while he pays lip service to the Socratic method, in that he declares that the Nolan “desire[s] to prove the imbecility of contrary ideas by using the very principles which seem to confirm them...”,⁹⁹ is ultimately less willing to fully engage with the principles of his opponents, offering instead a much less convincing representation of the voice of Aristotelian opinion. This is particularly marked in the final dialogue of the *Cena*, where Teofilo is allowed a virtually uninterrupted opportunity to expound Bruno’s doctrines about the motions of the earth (with strong corroborative support from Smitho). Galileo is truer, in this way, to Leibniz’s ideal of the dialogue, in that by fairly representing both sides of the cosmological controversy (and by using Sagredo to reveal some of the problems which had to be overcome in order to attain his final formulations), he succeeds in “composing a speech” which reveals him to be “someone who has himself gone into the reasons very carefully on his own, before taking it on himself to teach others”. In this, perhaps, he is more philosophically (and rhetorically) astute than Bruno who was not one of the natural diplomats of the sixteenth-century Republic of Letters.

In spite of their differences in oratorical and philosophical style, however, the dialogues of both Bruno and Galileo are works of ‘critique’ in Koyré’s sense, in that they make the subject of their dialogues not just the positive doctrines, but the nature of entrenched and traditional beliefs. Their dialogues are as much about the nature of authority and styles of philosophical argumentation as they are texts of natural philosophy. While it would have been possible to combine these two agendas in other kinds of prose narrative, the agonistic form of the dialogue was uniquely suited to this task. By *performing* (rather than simply reflecting upon) the conflict between two antagonistic views of cosmology, two opposed epistemologies, and two very different philosophical styles, the dialogue encompassed more in its digressions than a straightforward expository ‘line’ could ever have hoped to achieve.

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6. See, for example, Jon R. Snyder, *Writing the scene of speaking: Theories of dialogue in the late Italian Renaissance* (Stanford, CA, 1989); Bodo Guthmüller and Wolfgang G. Müller (eds), *Dialog und Gesprächskultur in der Renaissance* (Wolfenbütteler Abhandlungen zur Renaissanceforschung, 22; Wiesbaden, 2004); Eva Kushner, *Le dialogue à la Renaissance: Histoire et poétique* (Cahiers d’Humanisme et Renaissance, 67; Geneva, 2004); Anne Godard, *Le dialogue à la Renaissance* (Paris, 2001); K. J. Wilson, *Incomplete fictions: The formation of English Renaissance dialogue* (Washington, D.C., 1985); David Marsh, *The quattrocento dialogue* (Cambridge, MA, 1980); and C. J. R. Armstrong, “The dialectical road to the truth: The dialogue”, *French Renaissance studies* (Edinburgh, 1976), 36–51.
7. Despite the promise of their titles, neither Virginia Cox, *The Renaissance dialogue: Literary dialogue*

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8. Maurice A. Finocchiaro, *Galileo and the art of reasoning: Rhetorical foundations of logic and scientific method* (Boston Studies in the Philosophy of Science, 61; Dordrecht, 1980).
9. Finocchiaro, *Art of reasoning* (ref. 8), 116–17. He makes some brief remarks about the nature of “Socratic cross-examination” on p. 172 (“Socratic method and unconscious knowledge”), but he doesn’t develop this into a consideration of Galileo’s adoption of this form of argument, where “the person involved is being forced to reason about the rationale underlying his beliefs”.
10. Galileo Galilei, *Galileo On the Two World Systems: A new abridged translation and guide*, transl. and ed. by Maurice A. Finocchiaro (Berkeley, Los Angeles and London, 1997), 81. Galileo, *Dialogo*, “Al Discreto Lettore” (unsigned sheets inserted before sig. A): “Ho poi pensato tornare molto a proposito lo spiegare questi concetti in forma di Dialogo, che per non esser ristretto alla rigorosa osservanza delle leggi Matematiche, porge campo ancora a digressioni tal hora non meno curiose del principio argomento.”
11. Letter of Galileo to Elia Diodati, October 1629, in Stillman Drake, *Galileo at work: His scientific biography* (Chicago and London, 1978), 310.
12. Galileo, *World Systems* (ref. 10), 203, fn. 165. See also Maurice A. Finocchiaro, *The Galileo Affair: A documentary history* (Berkeley, 1989), 278.
13. See Finocchiaro, *Art of reasoning* (ref. 8), “The title, or the rhetoric of indecision”, pp. 12–18.
14. Galileo, *World Systems* (ref. 10), 153. *Dialogo*, 124–5: “[I]n questi discorsi di da Copernichista, e lo imito quasi sua maschera; ma quello che internamente abbiano in me operato le ragioni, che par ch’io produca in suo fauore non voglio che voi lo giudichiate dal mio parlare mentre siamo nel feruor della rappresentazione della fauola, ma dopo che aur deposto l’abito, che forse mi trouerete diuerso da quello, che mi vedete in scena.”
15. Galileo, *World Systems* (ref. 10), 82.
16. Annabel Patterson, *Censorship and interpretation: The conditions of writing and reading in early modern England* (Madison, WI, 1984), 45. Patterson sees these strategies as part of a “hermeneutics of censorship” which leads authors to develop “a highly sophisticated system of oblique communication, of unwritten rules whereby writers could communicate with readers or audiences (among whom were the very same authorities who were responsible for state censorship) without producing a direct confrontation” (*ibid.*). Although Patterson’s remarks are aimed at state censorship in early modern England, they are extraordinarily pertinent to the ecclesiastical censorship practised in Galileo’s Italy.
17. Galileo, *World Systems* (ref. 10), 101; *Dialogo*, 45–46.
18. Galileo, *World Systems* (ref. 10), 82; *Dialogo*, “Al Discreto Lettore”.
19. Finocchiaro, *Art of reasoning* (ref. 8), 5.
20. For an example of Finocchiaro’s qualification of his approach see *Art of reasoning* (ref. 8), 66: “nonlogical rhetorical devices have their own standards of value; hence, though the pure logician, may act as if they did not exist, the concrete logician or theorist of reasoning, cannot do so.”
21. Finocchiaro, *Art of reasoning* (ref. 8), 70–71.
22. Finocchiaro, *Art of reasoning* (ref. 8), 70.
23. See Finocchiaro, *Art of reasoning* (ref. 8), 4–5, and chap. 8, 180–201.
24. Finocchiaro, *Art of reasoning* (ref. 8), 44. See chap. 7, “The primacy of reasoning: The logical character of Galileo’s methodology”, pp. 167–79, and chap. 16, “Galileo as a logician: A model and a data basis”, pp. 343–412.
25. Giorgio de Santillana, *The crime of Galileo* (Chicago, 1955), 174, cited by Finocchiaro, *Art of reasoning* (ref. 8), 27.

26. Finocchiaro has a brief paragraph on digression on p. 118 (“Fanciful vs. relevant digressions”) which he includes as part of an inventory of “methodological topics”. He merely notes that “There are two kinds of digressions: some are justified by their relevance to the main argument and by the logic of the discussion.... Others are justified by the whim of the writer or persons involved or by the intrinsic beauty rather than by the reasoned tenability of the ideas involved.... The digressions up to this point have been typically of the first type”. He makes no attempt to analysis the digression as a rhetorical strategy, or to explain the contrast which Galileo makes between the digression and the “rigorous observation of mathematical laws”.
27. Jean Dietz Moss, *Novelties in the heavens: Rhetoric and science in the Copernican controversy* (Chicago, 1993), 268.
28. Finocchiaro, *Art of reasoning* (ref. 8), 23–24.
29. See Finocchiaro, *Art of reasoning* (ref. 8), 46, 56.
30. Galileo, *World Systems* (ref. 10), fn 71, p. 147. For a fuller treatment of the rhetorical dimension of Galileo’s natural philosophy see Finocchiaro, *Art of reasoning* (ref. 8).
31. Ernst Cassirer, *Zur Logik der Kulturwissenschaften: Fünf Studien* (1942), transl. by S. G. Lofts as *The logic of the cultural sciences: Five studies* (New Haven and London, 2000), pp. 53–54.
32. Alexandre Koyré, *Études Galiléennes* (Paris, 1966, repr. 2001), 212: “Le *Dialogue sur les deux plus grands systèmes du monde* prétend exposer deux systèmes astronomiques rivaux. Mais, en fait, ce n’est pas un livre d’astronomie, ni même de physique. C’est avant tout un livre de critique; une oeuvre de polémique et de combat; c’est en même temps une oeuvre pédagogique [et] une oeuvre philosophique....”
33. Quintilian had identified the various advantages of introducing “fictional personages” into an oration in the *Institutio oratoria* (IX.1): “by the introduction of fictitious personages we may bring into play the most forcible form of examination. We may describe the results likely to follow some action, introduce topics to lead our hearers astray, move them to mirth or anticipate the arguments of our opponent.” See *The Institutio Oratoria of Quintilian*, transl. by H. E. Butler (4 vols, London, 1921–22), iii, 365.
34. Galileo, *World Systems* (ref. 10), 174; *Dialogo*, 185.
35. Galileo, *World Systems* (ref. 10), 178; *Dialogo*, 188.
36. Galileo, *Discorsi*, 32; *Galileo Galilei. Two new sciences. Including Centers of gravity and Force of percussion*, transl. by Stillman Drake (Madison, 1974), 40.
37. See Galileo, *Discorsi*, 14, 19; *Two new sciences* (ref. 36), 22, 26.
38. Galileo, *World Systems* (ref. 10), 88; *Dialogo*, 27.
39. Moss, *Novelties* (ref. 27), 298.
40. Galileo, *World Systems* (ref. 10), 72; *Discorsi*, 69.
41. Galileo, *Two new sciences* (ref. 36), 35; *Discorsi*, 27: “*Salu. ...* mà forse il diuertir tanto lungamente dal cominciato cammino potrebbe parerui importuno, e però poco grato. *Sagr.* Di grazia godiamo del beneficio, e priuilegio, che s’hà dal parlar con i viui, e tra gli amici, e più di cose arbitrarie, e non necessarie, differente dal trattar co’i libri morti, li quali ti eccitano mille dubbii, e nissuno te ne risoluono. Fateci dunque partecipi di quille considerazioni, che il corso de i nostri ragionamenti vi soggreisce....”
42. On *quaestiones disputatae*, and other mediaeval genres of Aristotelian philosophical writing inherited by Renaissance authors see Charles B. Schmitt, “Aristotelian literature”, in his *Aristotle in the Renaissance* (Cambridge, MA, 1983), 34–63. See also Brian Lawn, *The rise and decline of the scholastic Quaestio disputata: with special emphasis on its use in the teaching of medicine and science* (Education and Society in the Middle Ages and Renaissance, 2; Leiden, 1993).
43. Galileo, *World Systems* (ref. 10), 121; *Dialogo*, 101. On *centonismo* see Christoph Hoch, *Apollo Centonarius: Studien und Texte zur Centodichtung der italienische Renaissance* (Tübingen,

- 1997).
44. Galileo, *World Systems* (ref. 10), 127–8; *Dialogo*, 106.
 45. William A. Wallace, in *Galileo's logic of discovery: The background, content, and use of his appropriated treatises on Aristotle's Posterior analytics* (Boston Studies in the Philosophy of Science, 137; Dordrecht, 1992), has argued that Galileo was profoundly indebted to the Aristotelian tradition, and William E. Carroll has recently argued that Galileo's "challenging" of Aristotelian orthodoxy is a "myth" masking an essential indebtedness to Aristotelian methodology. See William E. Carroll, "Galileo Galilei and the myth of orthodoxy", in John Brooke and Ian Maclean (eds), *Heterodoxy in early modern science and religion* (Oxford, 2005), 115–44.
 46. Galileo, *World Systems* (ref. 10), 126–7; *Dialogo*, 105–6.
 47. Galileo, *Two new sciences* (ref. 36), 38–39; *Discorsi*, 31: "[E] però con la solita libertà sia lecito produrre in mezzo i nostri umani capricci, che tali meritamente possiamo nominargli in comparazione delle dottrine soprannaturali, sole vere, e sicure determinatrici delle nostre controuersie, e scorte inerranti ne i nostri oscuri, e dubbii sentieri, ò più tosto Labirinti."
 48. On the prevalence of argument in *utramque partem* in early modern rhetoric see Quentin Skinner, *Reason and rhetoric in the philosophy of Hobbes* (Cambridge, 1996), 9–10.
 49. Galileo, *World Systems* (ref. 10), 119; *Dialogo*, 100.
 50. Galileo, *World Systems* (ref. 10), 118; *Dialogo*, 100.
 51. Finocchiaro, *Art of reasoning* (ref. 8), 15–16.
 52. Moss, *Novelties* (ref. 27), 266, fn 21.
 53. Moss, *Novelties* (ref. 27), 267.
 54. For Cicero's probabilism see, for example, *Tusculan Orations*, II, ii, 5: "we, however, whose guide is probability and who are unable to advance further than the point at which the likelihood of truth has presented itself, are prepared both to refute without obstinacy and be refuted without anger [*nos, qui sequimur probabilia nec ultra quam ad id, quod veri simile occurrit, progredi possumus, et refellere sine pertinacia et refelli sine iracundia parati sumus*]. Cicero, *Tusculan disputations*, transl. by J. E. King, (Cambridge, MA, and London, 1996), 150–3.
 55. For Salviati's explicit references to the digressive nature of the discussion see, for example, Galileo, *World Systems* (ref. 10), 106, 117, 127–8, 202; *Dialogo*, 49, 99, 106, 205.
 56. Galileo, *World Systems* (ref. 10), 128; *Dialogo*, 106. My emphasis.
 57. Galileo, *World Systems* (ref. 10), 87; *Dialogo*, 26–7.
 58. Galileo, *World Systems* (ref. 10), 90; *Dialogo*, 29.
 59. *Ibid.*
 60. Galileo, *World Systems* (ref. 10), 96; *Dialogo*, 42.
 61. Galileo, *World Systems* (ref. 10), 224; *Dialogo*, 317.
 62. Galileo, *World Systems* (ref. 10), 98; *Dialogo*, 43–44.
 63. Galileo, *World Systems* (ref. 10), 105; *Dialogo*, 48.
 64. See, for example, Galileo, *World Systems* (ref. 10), 242; *Dialogo*, 331: "*Sagr*: Oh, Nicolaus Copernicus, how pleased you would have been to see this part of your system confirmed by such clear observations!" and *World Systems*, 245; *Dialogo*, 349: "*Salv.* ... Look at the reckless arrogance of certain people! They undertake the confutation of someone else's doctrine but misunderstand its primary foundations"
 65. Galileo, *World Systems* (ref. 10), 120; *Dialogo*, 101.
 66. Galileo, *World Systems* (ref. 10), 155; *Dialogo*, 126.
 67. *Two new sciences* (ref. 36), 86; *Discorsi*, 83–84: "*Sagr*: Non questa sola, mà molte altre insieme dalle vostre proposizioni son così remote dalle opinioni, e dottrine comunemente riceute, che spargendosi in publico vi conciterebber numero grande di contraddittori: essendo che l'innata

condizione de gli huomini non vede con buon'occhio, che altri nel loro esercizio scuopra verità, ò falsità non scoperte da loro; e col dar titolo di in- l nouatori di dottrine poco grato à gli orecchi di molti, s'ingegnano di tagliar quei nodi, che non possono sciorre, e con mine sutterranee dissipar quelli edifizij, che sono stati con gli strumenti consueti da pazienti artefici costrutti: mà con esso noi lontani da simili pretensioni l'esperienze vostre, e le ragioni bastano à quietarci: tuttauia quando habbiate altre più palpabili esperienze, e ragioni più efficaci le sentieremo molto volentieri."

68. *Two new sciences* (ref. 36), 161 (slightly modified); *Discorsi*, 165.
69. See Orazio Grassi, *Ratio Ponderum librae et simbellae: in qua quid e L. Sarsii libra astronomica, quidque e Galilei simbellatore, de cometis statuendum sit, collatis utriusque rationum momentis, Philosophorum proponitur* (Paris, 1628), See Examen XLI, 156: "id vnum tamen scio scholam illam, quam bonam, praeclaro nomine appellat Galileus, Epicuri scholam fuisse, hominis eo omnia dirigentis, vt aut Deum tolleret, aut illum mundi cura leuaret." See also Examen XLVIII, 172–182, esp. p. 179: "Sed quoniam in hac noua philosophandi ratione, plus aliquid audendum videtur, quam in veteri illa, ac religiosa nimis", and p. 173: "Nunc me illa voca de calore digressio, in qua se è schola Democriti, atque Epicuri vnum profitetur Galilaeus" [marg. gloss "Sag. f. 196, l.26"]. On p. 174 he argues that Galileo's views contradict Catholic doctrine on transubstantiation.
70. *Two new sciences* (ref. 36), 34; *Discorsi*, 26.
71. Giordano Bruno, *La Cena de le Ceneri. Descritta in cinque dialogi, per quattro interlocutori, Con tre considerationi, Circa doi suggestj* (London: 1584). All translations here are from *La Cena de le Ceneri. The Ash Wednesday Supper*; Giordano Bruno, ed. and transl. by Edward A. Gosselin and Lawrence S. Lerner (Hamden, CT, 1977; repr. Toronto, 1995; hereafter: *Ash Wednesday*). All cross-references to the Italian text refer to the Belles Lettres edition: *Le Souper des Cendres*, ed. by Giovanni Aquilecchia, in Giordano Bruno, *Oeuvres complètes*, ii (Paris, 1994; hereafter: *Souper*).
72. See Bruno, *Ash Wednesday*, 85, where Teofilo says of "the Nolan" (i.e. Bruno) that "he saw through neither the eyes of Copernicus nor those of Ptolemy" (cf. *Souper*, 37); or *Ash Wednesday*, 192: "'I care little about Copernicus,' said the Nolan, 'and little care whether you or others understand him.'" (cf. *Souper*, 227). On Bruno's problematic relationship to Copernicanism, see Hélène Védrine, *La conception de la nature chez Giordano Bruno* (Paris, 1967), 216–36; Robert S. Westman, "Magical reform and astronomical reform: The Yates thesis reconsidered", in *Hermeticism and the Scientific Revolution. Papers read at a Clark Library Seminar, March 9 1974 by Robert S. Westman and J. E. McGuire* (Los Angeles, 1977), 5–91; and Miguel A. Granada, "Giordano Bruno, Thomas Digges e il copernicanismo in Inghilterra", in *Giordano Bruno 1583–1585: The English experience / L'esperienza inglese*, ed. by Michele Ciliberto and Nicolas Mann (Florence, 1997), 125–55. See also, more recently, Dilwyn Knox, "Ficino, Copernicus and Bruno on the motion of the earth", *Bruniana et Campanelliana*, v (1999), 333–66; Dilwyn Knox, "Bruno's doctrine of gravity, levity and natural circular motion", *Physis*, xxxviii (2001), 172–209 (esp. pp. 200–8); and Dario Tessicini, *I dintorni dell'infinito: Giordano Bruno e l'astronomia del cinquecento* (Bruniana et Campanelliana Supplementi, 20, Studi, 9; Pisa and Rome, 2007).
73. Saverio Ricci, *La fortuna del pensiero di Giordano Bruno 1600–1750* (Naples, 1990), 96–110. On the 'affinity' (*affinità*) between some of Galileo's arguments in the *Dialogo* and Bruno's *De l'infinito*, see *ibid.*, 98–99.
74. Ricci, *Fortuna* (ref. 73), 106–10.
75. Hilary Gatti, "Giordano Bruno's *Ash Wednesday Supper* and Galileo's *Dialogue of the two Major World Systems*", *Bruniana e Campanelliana*, iii (1997), 283–300, p. 284. For another comparison of Bruno and Galileo see Arcangelo Rossi, "Bruno, Copernico e Galilei", *Physis*, xxxviii (2001), 283–303.
76. For a facsimile reproduction of the title page see Bruno, *Souper*, 1.

77. Bruno, *Ash Wednesday*, 68; *Souper*, 9, 11.
78. Bruno, *Ash Wednesday*, 68; *Souper*, 19, 21.
79. Bruno, *Ash Wednesday*, 72; *Souper*, 11.
80. Bruno, *Ash Wednesday*, 69; *Souper*, 11, 13. Bruno describes this dialogue as a “moral topography [una topografia morale]”, which his reader must discern “with Lynceus’s eyes”.
81. Bruno, *Ash Wednesday*, 73; *Souper*, 21.
82. Bruno, *Ash Wednesday*, 84; *Souper*, 35.
83. Bruno, *Ash Wednesday*, 93; *Souper*, 57. Cf. Gatti, *op. cit.* (ref. 75), 290: “Galileo’s Simplicio is in many ways a direct development of Bruno’s fiercely caricatured neo-Aristotelians, Torquato and Nundinius and even more of his cautious Prudentius.”
84. Bruno, *Ash Wednesday*, 182–3 (Latin slightly amended); *Souper*, 205.
85. Bruno, *Ash Wednesday*, 183; *Souper*, 205.
86. For the use of Erasman adages see Bruno, *Ash Wednesday*, 186–7; *Souper*, 213–15, and for Osiander see *Ash Wednesday*, 137–9; *Souper*, 127–131.
87. Bruno, *Ash Wednesday*, 189; *Souper*, 223.
88. Bruno, *Ash Wednesday*, 137; *Souper*, 127.
89. Bruno, *Ash Wednesday*, 92–93, 153, 157; *Souper*, 55, 161–3, 173.
90. Bruno, *Ash Wednesday*, 153; *Souper*, 161.
91. Bruno, *Ash Wednesday*, 97; *Souper*, 65: “[N]on voglio ch’abbino facultà di esercitar atti de interrogatore o disputante, prima ch’abbino udito tutto il corso de la filosofia: per che all’ora se la dottrina è perfetta in sé, e da quelli è stata perfettamente intesa, purga tutti i dubii, e toglie via tutte le contradizioni ... non è possibile saper, circa una arte o scienza, dubitar et interrogar a proposito, e co gli ordini che si convengono, se non ha udito prima.”
92. Bruno, *Ash Wednesday*, 97; *Souper*, 67.
93. Bruno, *Ash Wednesday*, 98–99; *Souper*, 67–71.
94. Bruno, *Ash Wednesday*, 99; *Souper*, 71–73.
95. Galileo, *World Systems* (ref. 10), 233–4; *Dialogo*, 324.
96. Galileo, *World Systems* (ref. 10), 106; *Dialogo*, 49.
97. Galileo, *Discorsi*, “Lo stampatore ai lettori”, sig. *3 verso: “Parimente quelli, i quali con l’acutezza dei loro ingegni, hanno riformato le cose già trouate, scoprendo le fallacie, & gli errori, di molte età, sono degni di gran lode, & ammiratione: atteso medesimamente, che tale scoprimento, è laudabile, se bene i medesimi scopritori, hauesseno solamente rimossa la falsità, senza introduce la verità, per se, tanto difficile à conseguirsi; conforme al detto del principe de gl’oratori. *Vtinam tam facilè possem vera repirire, quam falsa convincere.*” The Latin quotation is from Cicero, *De natura deorum*, I, 91.
98. Galileo, *World Systems* (ref. 10), 88; *Dialogo*, 27.

LIARS, EXPERTS AND AUTHORITIES

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No doubt there is everywhere a growing disposition to put more and more trust in tribunals of experts and to place little confidence in the view that any man gifted with common sense can form a sound opinion about anything — say, the merits of the latest invention in electrical science.... [Yet the] evidence of experts is apt to be listened to with incredulity, and the saying of a well-known judge that liars might be divided into three classes — liars, great liars, and scientific witnesses — recommends itself to many.

Editorial, *The Times*, 4 April 1882¹

What is the significance of the ‘expert’ in the history of modern science? Have experts been central figures throughout modern technoscience, offering essential authoritative advice in periods of change and uncertainty? If so, when and how did experts come to take up such a key role? If not — or at least, if the role of experts is not quite so straightforward — what approach should historians take to this subject? Are we simply engaging in anachronistic projection from the early twenty-first century, presuming all too complacently that the experts of the past were too essentially like the experts of the present? Are we inclined too glibly to treat the terms ‘expert’ and ‘authority’ as if they are interchangeable and have always been so? Putting it that way, we clearly need to ask some historical and historiographical questions about the past identity and status of the expert and of the authority to resolve some of the uncertainties here. That is what this paper sets out to do, taking some clues to the problematic status of the expert from the contemporary literature on the sociology of technical experts.

Anthony Giddens’s celebrated critique of modernization theory offers a telling example of our shifting understanding of the expert. In *The consequences of modernity*, Giddens analyses the notion that ordinary citizens have allegedly needed experts to help them adapt to the ever more perplexing demands of modern technocracy.² Such a presumption has featured in two major historical accounts of how industrializing cultures assimilated new electrical technologies from the late nineteenth century.³ Yet Giddens’s account undercuts such a simple analysis by showing that since experts cannot be omnipresent, the laity has selectively developed its own distinctive forms of expertise to cope with the everyday technical challenges of modern cultures.⁴ Consequently, we should thus be wary of over-stating the epistemic contrast between knowledgeable experts and the so-called ‘laity’. Indeed, in his analysis of the longer environmental term effects of fallout in Cumbria from the 1986 Chernobyl disaster, Brian Wynne shows how the prognosis of élite professional scientific experts was cogently challenged by ‘lay experts’ in the Cumbrian sheep farming community. Wynne’s provocative dissolution of the traditional expert–lay dichotomy has spawned

a sociological literature that maps out a spectrum of different kinds of expertise, recognizing thereby both the fallibility of formal ‘scientific’ experts and the critical expertise of these ‘laypersons’ outside the scientific community.⁵

It is my contention, however, that these problems of expert culture were not first discovered in the late twentieth century. As this paper will show, the notoriety of the fallible ‘expert’ was a recurrent feature of legal and cultural debates on electricity (and in other subjects too) in the later decades of the nineteenth century. From the 1882 editorial in *The Times* quoted above we see it was a popular joke to identify the expert, not as an impartial authority, but as one prepared to lie to serve the interests of a paying patron. This was especially the case in the 1880s and 1890s, when the prospects of electrification opened up large technical and ethical questions that were not amenable to disinterested responses from those who had invested heavily in sufficient technical knowledge to serve as experts. Among late Victorians there was, as I show, an alternative social figure to which the laity could presumptively turn in its quest for unbiased impartial wisdom on difficult matters, namely the ‘authority’. I show that for the Victorians the categories of expert and authority were — in principle at least — somewhat distinct. This notional distinction rested partly on grounds of their differentiated social roles, and premised on different financial underpinnings: unlike experts, authorities were neither paid for their pronouncements nor (presumptively) had they any direct financial interests in matters on which they pronounced. Even so, we shall see that the prerogatives of the authority role could also be contested and shown publicly to have limitations, if not to the same degree of notoriety as the venal ‘expert’. To add further complexity to the picture we shall see that the expert/authority boundary was permeable since an authority figure could take up the role of an expert more readily than vice versa.

The first section builds upon Tal Golan’s recent study of science in the courtroom⁶ to show how in the nineteenth century the term ‘expert’ emerged as a journalistic — and often disparaging — shorthand for the ‘expert witness’ so regularly embroiled in adversarial courtroom litigation. The next part looks at how this notion of the expert was extended to the extra-judicial domain, specifically in the USA in relation to fraught debates over the alleged public dangers of electricity in the context of judicial electrocution. These debates over the risks of electricity hinged to some extent on the elusive nature of electricity. So I shall then move on to consider how that matter was debated without resolution among textbooks and public addresses by rival aspirants to authority in electrical matters such as Oliver Lodge and William Preece. Finally I consider how the role of ‘authority’ in extrapolating from the mysteries of electricity to technocratic futures could easily be overstepped by those to whom this position was imputed. Focusing on the case of chemist-electrician William Crookes, I examine how the British press denied him the prerogative of beguiling the public with speculations beyond the remit of established knowledge. In my conclusion, I consider the wider implications of these episodes for the historiography of the ‘expert’ in modern science.

1. THE 'EXPERT': SCIENTIFIC WITNESS AND LIAR?

The position of the expert witness is one which has always puzzled laymen. It seems curious to them that an honourable man should be prepared to testify for either side according to his retainer. Argument as to law they can understand, but they feel that a fact is a fact and that there cannot be an opening for two views of it.

H. Fletcher Moulton, *The life of Lord Moulton* (1922)⁷

Whence came the public culture of 'experts' in techno-science and why has it been so constitutively marked by conflicting judgement? Carol Jones argues that experts have long featured in two kinds of specialist activity. The first is evaluative and performative, providing putatively authoritative opinion in such apparently diverse roles as scientific witnessing in the courtroom, membership of investigative committees for governments, and punditry for published media. Their less visible activity involves specialist skill in the practical or theoretical accomplishments of esoteric technical disciplines as evidenced in the refereed publication culture of academic life. Jones argues plausibly that these forms of expertise have been closely related since at least the early nineteenth century, and arguably much earlier, since the body of academics and scholars has characteristically furnished law courts, tribunals and the press with expert witnesses called in for the public resolution of contested claims.⁸

Jones suggests somewhat broadly that 'experts' performed in this role to stake a claim for power in social decision-making. By contrast Tal Golan and Christopher Hamlin see a more mundane motivation: paid witnessing work was a crucial source of income for the impecunious Victorian man of science.⁹ These two explanations for the rise of an 'expert' culture in the nineteenth century are not incompatible; yet neither explains how scientific witnesses came to acquire the title 'expert', nor how this role was extended beyond the courtroom or tribunal.¹⁰ In what follows I build upon Golan's analysis to examine how reporting on the courtroom role of "expert witnesses" reveals both the origins of the embattled nature of expert culture and the journalistic coining of the term 'expert' as a term of derogation.

Golan observes that the advent of paid specialist testimony in the nineteenth-century law court brought many chemists, engineers and physicians into controversially partisan roles that clashed rather uncomfortably with their concurrent attempts to fashion themselves as unworldly seekers of disinterested truth.¹¹ The courtroom experience of being hired to support a partisan case could exact a heavy toll on the dignity and reputation of scientific witnesses. Hostile cross-examination by the opposing team's lawyers often led to a humiliating deconstruction of their expert competence and trustworthiness, as the young Michael Faraday discovered in an 1819 fire insurance hearing, discussed further below.¹² This problem was specific to Anglo-American culture since inquisitorial courts on the European continent typically required a panel of experts to arrive at a *collective* judgement. By contrast, Anglo-American cases antagonistically pitted the testimony of prosecution expert witnesses against those of the defence. The operation of this constitutively adversarial system persistently produced conflicting interpretations of material facts from two (sets of) supposedly impartial specialists: each expert produced a judgement that seemed remarkably

congenial to the interests of the paying client. This perverse dissonance in the reputedly impartial domain of scientific judgements disturbed many nineteenth-century observers, arousing suspicion that expert pronouncements were merely the product of client-driven expediency.¹³

Following the failure of a Society of Arts conference to resolve the matter in 1860, it was — as Golan notes — scientific practitioners rather than lawyers or the court system that generally bore the burden of the blame for producing conflicting scientific testimony.¹⁴ In January 1862 an editorial in the *Saturday Review* appealed to this account to explain the “remarkable discrepancies” involving evidence from expert witnesses. This was most notable in patent cases in which figures of the “highest scientific eminence” flatly contradicted each other on the newness of an invention, with similar discord among medical witnesses in criminal cases concerning allegations of poisoning. Apparently judges and lawyers now inferred that the skilled scientific testimony which ought to have been decisive was in fact the “most suspicious and unsatisfactory”.¹⁵ It was in this context of critical suspicion that the *Saturday Review* dubbed the perpetrators of this problem ‘experts’, as if to ironize the contrast between their high level of specialist wisdom and their low level of trustworthiness. The term ‘expert’ was typically used thereafter in the press with clear overtones of ambivalence; it is significant then that it was not commonplace until at least the early twentieth century for scientific witnesses to describe themselves publicly as ‘experts’.¹⁶

Faced with such attacks on ‘experts’, the rising journalist and chemist William Crookes defended the integrity of his scientific brethren in his journal *Chemical news* of 1862:

The evidence of experts is now the subject of general derision. Smart newspaper writers, wishing to indite a telling sarcastic article, select the discrepancies in scientific evidence for a theme; ... and barristers, ready to advocate any opinion ... when addressing a jury, dilate with a well-simulated indignation that eminent scientific men are to be found in the witness box on opposite sides....¹⁷

Crookes argues that the problem arose not from moral corruption but from the unavoidable ambiguity of evidence. Men of science were regularly called upon to express opinions on matters “which do not admit of demonstration”, and about which they could thus conscientiously come to different conclusions. He thus preferred to concede the uncertainty of science in order to preserve the reputation of its practitioners. It does not seem, however, that Crookes won over his fellow journalists or editors to this ameliorative position — let alone the courts in which expert witnesses were presumed competent to provide testimony of a high degree of *certainty*. Outside the community of expert witnesses — in the press and among the legal professions — the problem was thus still readily identified as the problematic integrity of science and its ‘experts’.

The impartiality of scientific witnesses in court cases remained in doubt in ensuing decades: many suspected that their expert testimony was more driven by the expediency of serving the interests of the client that paid them than of upholding

the canons of empirical objectivity. It was in reference to this that the famous joke began circulating among the late nineteenth-century Anglo-American legal fraternity about the three principal species of mendacity: “There are liars, damned liars, and scientific experts.”¹⁸ While this is popularly attributed to George W. W. Bramwell (1808–92), from the evidence available it seems likely that Justice Bramwell only ever told a particular variant of this joke that extended the denomination of liars to a fourth category to include “my brother Fred”, the engineer and expert witness Frederick Bramwell.¹⁹ Whatever its provenance, we know that the original version of the joke was certainly in circulation by 1882 — as the above epigraph from *The Times* demonstrates. And it was known to the scientific community by 1885 when T. H. Huxley noted of the X-club’s deliberations on 5 December that year: “Talked politics, scandal, and the three classes of witnesses ... liars, d–d liars, and experts.”²⁰

By this time the problem of expert untrustworthiness had become particularly serious since telephone patents became the subject of vigorous litigation by companies that stood to gain or lose vast profits depending on which expert judgement was upheld in the adjudication of alleged patent infringements. One such case reported in *The Times* law reports in late June 1886 concerned the United Telephone Company’s successful protection of its Edison transmitter patent against a small provincial firm of telegraph engineers. A subsequent attempt to insinuate that Edison’s well-paid lawyers had simply used their client’s wealth to purchase compliant expert witnesses ended up backfiring publicly, however, on the complainant, the forthright Quaker Silvanus P. Thompson. Writing to *The Times* about the outcome of this case on 29 June, this electrical physicist-engineer and Principal of Finsbury Technical College complained at the increasingly prevalent practice of lawyers’ straining the reasonable interpretation of patent specifications to bring the greatest benefit to their “wealthy supporters”:

This evil tendency, which is deliberately fostered by eminent leading counsel and by a few professional experts, who lend themselves to this mode of securing a monopoly for their patentee, is rapidly bringing into discredit the administration of the patent laws.²¹

Thompson was, however, immediately rebuked for his remarks in *The Times* correspondence pages by the eminent patent agent, John Imray. Regarding Thompson’s complaints about the expert system, Imray archly observed that the “learned Professor” had curiously failed to mention that he himself had performed as an expert witness in telephone patent cases — but indeed on the losing side. The clear implication was that Thompson’s allegation was grounded in undignified sour grapes and as such merited no considered reply. Hence members of the legal fraternity could once again divert attention away from possible corruption in the purchasing of expert testimony and focus instead on the apparent disingenuousness of the morally compromised ‘expert’ scientific witness.²²

Not all lawyers were inclined, however, to attribute the blame in such matters to the moral inconsistencies of scientific experts. Yet their resolution of the matter was

not unanimous, as we can see by looking at the views of two senior counsel specializing in electrical patent cases — and both of them Wranglers in the Cambridge Mathematics Tripos. One view of the resolution can be found in the autobiographical testimony of one eminent judge, Viscount Alverstone, a former electrical patent lawyer who — as Richard Webster — served as Attorney-General under the Conservative Prime Minister, Lord Salisbury. In many scientific and patent cases, Alverstone had found “honest experts” of the greatest assistance to both judges and juries. This was because — *pace* Sylvanus Thompson — many of them had “declined to adopt views in the interest of those who retained them”. In particular, Alverstone cited the late Frederick Bramwell, John Imray and John Hopkinson as expert witnesses on whose evidence courts or juries could place such “thorough reliance”. Indeed, Alverstone suggested that “No-one had a more extensive experience of experts” than he, and he had certainly found no difficulty, on the one hand, in confirming the “best expert testimony” as reliable enough to warrant consideration by the court, and on the other, exposing the partisan views of the “mere advocate”.²³ Alverstone silently passed over the point that while eminent experts could afford to turn down all but the best cases, others could ill-afford such selectivity. Hopkinson, for example, held university and consultancy posts in electrical engineering that gave him a degree of financial autonomy in choosing his cases that would not have been feasible for court witnesses four decades earlier or for many, more impecunious expert witnesses.²⁴ Such was the personalized socio-economic route for the élite scientific witness out of the experts’ courtroom dilemma as outlined by William Crookes in 1862.

A rather different explanation of the paradox of expert conflict can be found in the biography that H. F. Moulton wrote of his father, the great patent lawyer, Fletcher Moulton — who like Hopkinson stood eminent as a Senior Wrangler. Moulton often had to deal with the conflict between paid partisan expert witnesses that seemed so readily to confound laymen’s sense of the “honourable” character of those called upon to perform this role. Yet contrary to the layperson’s view, according to Moulton, it was not the court’s job to decide which of the opposing witnesses’ testimony was the more truthful. Rather it was the role of the court to form “the proper legal deduction” from the wide range of factual evidence presented to it.²⁵ By casting such “proper” legal deduction from conflicting testimony as a somewhat transcendental process comprehensible only to the judiciary, the integrity of the expert witnessing culture could thus be protected from critique by the under-informed. Thus even though this culture often led to “a series of duels” between counsel and opposing witnesses, Moulton himself was reputedly on very “amicable” terms even with those whom he regularly cross-examined with the “greatest severity”.²⁶

From the above we can thus see how the role of expert achieved some degree of respectability, at least among judges, by the turn of the twentieth century. Yet the same could not so easily be said of the emergent role of the ‘expert’ in the contemporary extra-judicial domain. In the next section I track how this new and broader expert role developed in the USA, and was imported to the British public forums of tribunals and press — again with a lingering notoriety for being partial or at least ineffectual.

2. THE EXTRA-JUDICIAL EXPERT IN ELECTRICAL TECHNOLOGY

“In a multitude of counsellors there is wisdom” is an ancient proverb, but those who embark in the electric lighting enterprise will, after an exhaustive inquiry into the merits and demerits of the various systems and applications, come to the conclusion that in a multitude of counsellors there is naught but confusion and vexation of spirit, and, considering the wide divergence of opinion amongst experts on nearly all matters connected with electric lighting, it can scarcely be wondered at that some local authorities have declined to enter the business.

Albert Gay and Charles H. Yeaman, 1899²⁷

In the final decades of the nineteenth century, the role of the expert in adjudicating socio-technical questions was by no means limited to the adversarial dramas of the courtroom. Indeed we need to explain how the meaning of the term ‘expert’ extended beyond the performance of courtroom witnesses to other domains of cultural life. Contemporary newspapers and periodicals offered an extra-judicial tribunal for paid specialists to offer their views on novel electrical technologies of power, light and communication — whether experienced in the expert witness role or not. This was especially apparent in the USA in the context of new electrical technologies during the 1880s; the new types of extra-judicial ‘expert’ were soon picked up by the British press in discussing the same topics.

The multi-faceted and somewhat partisan role of the expert placed patent-chasing and publicity-hungry inventors such as Thomas Alva Edison in a somewhat ambivalent position. When a *Times* reporter visited Edison in January 1880, the celebrated inventor was initially dismissive of these self-serving and increasingly freelance critics:

He is partially deaf, and very modest. Yet when he finds that his visitor really sympathizes with him, and is not a “professional expert” whose object is only to criticize, he warms up into one of the most entertaining men I ever met. He hates electrical “experts” and “interviewing reporters” and has a standing reward of \$50 to start the Menlo-park graveyard with its first corpse, which he hopes may be either the one or the other...

In the half-hour domestic interview conducted by this journalist, Edison enjoyed reading out newspaper cuttings on the claims of an “ambitious ‘expert’” who had offered to forfeit \$100 for every lamp that Edison could keep burning over 20 minutes.²⁸ As the interview was conducted under the illumination of prototype lamps that had apparently glowed continuously for at least three days, the discomfiture of that over-critical expert seemed quite inevitable to the *Times* reporter.

Ironically, however, in both his recurrent patent litigation and his wider publicity campaigns, Edison typically was forced to rely on the testimony of experts, sometimes in his own pay. This reliance was necessitated by continuing disappointment in late 1880 that, after two years, he still had not produced a working *public* demonstration of his lighting system. Writing for the *North American review* in October 1880, he eagerly cited the testimony of those few ‘experts’ who agreed that his enterprise was not hopelessly impracticable.²⁹ A year later, at the Paris Electrical Exhibition,

Edison finally produced an international demonstration of his filament lamp and almost immediately entered into patent litigation against Joseph Swan. Indeed it was in part the diverse views of the parade of specialist juries and panels commenting on electrical technology displays at the Paris Exhibition that prompted a *Times* editorial in April 1882 to lament a growing disposition to put “more and more trust in tribunals of experts” rather than common sense. Given that the conflicting evidence of experts was “apt to be listened to with incredulity”, *The Times* noted how this context bore out the well-known judicial joke about the putative mendacity of experts.³⁰

Dishonesty, or at least bad faith, was not the only troubling theme for contemporaries in articulating the nature of and scope of the expert’s developing role. In the electrical engineering doldrums of the mid-1880s journalists identified other explanations for the proliferation of ‘expert’ views. As underemployed electrical engineers rushed into the popular and technical press to offer advice on achieving the fantasy of a fully electrified future, the cacophony of their testimony owed more to the multiplicity of solutions available than to any sponsored duplicity. As I have shown elsewhere, the impotence of electrical engineers to resolve apparently rudimentary matters in the design of alternate current technology caused much vexation in the late 1880s. The problem was the persistent lack of closure on the debate about how to make alternators that could usefully work in tandem (for they typically could not be made so to work). Stark disagreements among commentators prompted one leading technical journal, the *Electrician*, to lament the impotence of expert testimony that generalized unduly from highly particularized experiences. As it archly observed, the practical dynamo-builder did not derive any effective guidance from “the differences of experts” on how much self-induction was required in the armature of alternators. By contrast, the dynamo designer needed a stable consensus on the relevant conditions of design — and that was evidently not forthcoming from advising experts.³¹

Even when the design of electrical lighting and power systems became conventionalized a decade later, in the febrile domain of technological publishing the “differences of experts” were still often more visible than their agreements. The problematic role of the electrical ‘expert’ was most troubling in matters of life and death.³² This was most obviously so in New York State when the death penalty by electric chair was controversially first scheduled for William Kemmler, convicted in 1889 of murdering his common-law wife Tillie Ziegler. Much concern was raised over the brutality of using electrocution to implement the death penalty. The first proposals for electrocution focused on the allegedly deadly qualities of alternating current. The merits of using this as the appropriate vehicle of death elicited rival testimony from the two leading factions in the fraught contemporaneous battle over the disputed merits of the direct (d.c.) v. alternate (a.c.) current systems. As Thomas Hughes and Mark Essig have shown, affiliates of Edison’s d.c. enterprise worked hard to meet his partisan goal of showing that the alternate current was perfectly suited to easy and immediate death, and thus by implication too dangerous to be allowed in the home. The a.c. manufacturer George Westinghouse strongly denied this allegation and hired amenable electrical experts to support his counter claims.³³

However, the prospect that death by the electric chair might constitute a ‘cruel and unusual punishment’ prompted the New York State legislature to call a quasi-judicial review of the new technology as a matter of habeas corpus proceedings. Such was the transatlantic interest in the case that the *Electrical review* invited its readers to glimpse this drama of opposed expert testimony reported by the *New York herald* on 12 July. It reported that the first three days of the hearings had been taken up with the judgements of “expert Harold P. Brown” that the electric chair would cause an instantaneous and painless death. Following that testimony in favour of Edison, the opposing Westinghouse perspective was put by F. L. Pope, ex-President of the American Institute of Electrical Engineers and an employee of Westinghouse. His opinions “differed radically” from Brown’s, for he claimed that electric appliances recently purchased by New York State for executing condemned murderers were by no means certain to bring so straightforward an execution.³⁴

In the heated conflict that unfolded, Brown repeatedly argued that the condemned criminal would be despatched instantaneously and painlessly when executed by the electric chair. He was even able to persuade Referee Becker that his testimony was free of any vested interest in the matter, just as Edison plausibly testified that Brown was a mere acquaintance of his. Becker thus allowed Kemmler’s execution to proceed by alternate current — or “Westinghousing” as Edison preferred to call it. Yet the execution of Kemmler proved — contrary to Brown’s assurances — to be an exceptionally grisly and protracted business. As historians of this notorious episode have observed, Brown’s credibility as an impartial ‘expert’ further deteriorated when his claims to have been entirely independent of Edison’s financial interests were rebutted by the popular press’s reporting of stolen letters which proved that Edison had been Brown’s paymaster all along. Even though the electric chair lived on in US culture, neither Edison’s integrity nor Brown’s expert status survived well thereafter.³⁵

While the effective demise of the direct current supply system in the USA was the most obvious technological casualty, the status of the putative ‘expert’ was yet further lowered in social esteem. As the New York inventor C. F. Heinrichs commented on the role of various figures that had embarrassed the proceedings:

It is to be regretted that some of our electrical experts of so-called standing, not only assist in keeping the facts from the public, but tell when under oath only half the truth.... One of these experts had to admit in the Kemmler investigations that all of his knowledge as to the harmless nature of the Westinghouse current was obtained by him from observations made upon himself and friends receiving alternating currents from an electro-medical apparatus.³⁶

The notoriety of the freelance expert was not merely a problem for those concerned with execution by the electric chair. As Essig has emphasized, much of the excitement in New York about this episode had been fuelled by the spate of recent accidental electrocutions across the USA, many of workmen accidentally touching overhead high voltage a.c. wires. Since twenty fatalities were from that city alone the *American architect and building news* liberally quoted from the *New York commercial advertiser*

that the death rate was higher in the USA than in any other country. Correlatively the problem of untrustworthy experts was equally great, and these publications passed especially harsh judgement on those hired commercial experts who had claimed the high death rate to be a mere geographically localized quirk. Notwithstanding what had been “erroneously stated by experts in the employ of the companies interested in the deadly high-voltage currents”, other countries had “not been without such ‘accidents’”. It then quoted at length the litany of electrical ‘accidents’ across Europe documented by Heinrichs during the 1880s to show that whatever else could be said about electricity, no experienced practitioner could claim it was absolutely safe.³⁷

Such was the widespread concern among the electrical cognoscenti about the fear of electricity cultivated by the press and feuding practitioners that the *North American review* staged a debate exposing the factional interests of Edison and Westinghouse for all to see. In January 1890 it published the testimony of the Glaswegian Professor Sir William Thomson, as if serving as the impartial authority on the subject. He suggested that adherence to safety regulations recently issued by the UK’s Board of Trade should resolve all the problems of electrical risk faced in the USA — although evidently his expectation was not fully borne out.³⁸ And as I have shown elsewhere, the transatlantic debate about electrical dangers did not only affect citizens in the USA. As James Gordon reported in 1891, stories reprinted from the US press left ordinary UK consumers somewhat perplexed about the source of risk in electrical power and light. If electricity itself was not intrinsically dangerous, did the problem lie in the kind of current used — direct or alternate? Or was it the workmanship of construction and insulation that rendered electrical devices safe or otherwise? Rival experts gave rival views on these matters, leading to the despairing comment of two London power station engineers, Gay and Yeaman, in 1899 to novices that great “confusion and vexation of spirit” was prompted by the “wide divergence of opinion amongst experts” on nearly every aspect of electrical illumination.³⁹

Given the range of expert opinions on electrical technology among which ordinary lay observers had to choose, some members of the public developed their own folk understandings to deal with the situation, especially as it related to the perils of personal contact with electricity. The Scots curtain manufacturer and popular science writer Charles Gibson gleefully relayed to readers of the *Romance of electricity* (1906) an amusing conversation he had overheard between two gentlemen on a Glaswegian tram:

One asked the other how it was that a person might walk along the rails of an electric tramway and yet not receive a shock from the dynamo to which they are connected. His friend’s reply was that the rails only carried negative electricity, which was quite harmless, and that it was the positive electricity, carried by the trolley wire, that killed.⁴⁰

As we shall see in the next section, there was as much debate on the character of electricity — and indeed of the number of varieties it came in — as there was over its safety, and the two matters were clearly inter-related. Yet as we shall see, this debate

brought out much more concern about the role of the presumptively disinterested ‘authority’ in electricity than that of the often partisan ‘expert’. Even the apparently robust ‘authority’ of high status professional figure proved little better at resolving the epistemic problems of electricity than the ‘expert’ statements of the embattled courtroom witness.

3. “WHAT IS ELECTRICITY?”: AUTHORITY IN UNCERTAINTY

We know little as yet concerning the mighty agency we call electricity. “Substantialists” tell us it is a kind of matter. Others again view it, not as matter, but as a form of energy. Others, again, reject both these views. Professor Lodge considers it “a form, or rather a mode of manifestation of the ether.” Professor Nikola Tesla demurs to the view of Oliver Lodge, but thinks that “nothing would seem to stand in the way of calling electricity ether associated with matter, or bound ether.” High authorities cannot even agree whether we have one electricity or two opposite electricities.

William Crookes, 1892⁴¹

One of the less well documented minor scandals in the history of electricity is that throughout the nineteenth and early twentieth centuries, hardly anybody seemed to know what electricity actually *was*. No reader of the massive literature on the subject could easily have found any single authoritative view. We know from those who set themselves up as authorities on electricity that members of the public expected an answer from them. This was especially the case from the 1890s when the prospect of ubiquitous electricity supply sharpened general interest in the question: “What is electricity?”⁴² As John Ambrose Fleming observed in a *Popular science monthly* paper on the subject in 1901: the “intelligent but non-scientific inquirer is often disappointed when he finds no simple, and as he thinks essential, answer forthcoming”.⁴³ Moreover, Fleming noted such critical ‘lay’ enquirers had the temerity to ask *why* an answer could not be “furnished”. Surely, from their point of view, it was the role of authorities in science to answer such questions on public demand?⁴⁴

Even before the arrival of the electrical power station brought an urban ubiquity to electricity — concurrently with the arrival the deadly electric chair — so making the question more pressing, readers of textbooks on this subject encountered a bewildering degree of disagreement as to the nature of this mysterious agent. The divergence of views concerned more than just whether electricity consisted of two electrical fluids or just one such fluid,⁴⁵ or was a phenomenon of the ether, or special form of energy — or indeed something hitherto altogether unknown. Some argued in response to the diverse answers to the question “What is electricity?” that it should be treated only hypothetically; others still that it was much more helpful to ask questions about the electromagnetic ether instead, and still others argued that focusing on electricity’s *utility* would effectively provide a pragmatic solution to the problem. Such disparity left readers to judge for themselves which ‘authority’ they should believe; importantly, though, since no matter of jurisprudence or life or death hung directly on this issue, the role of ‘authority’ here was never as viciously impugned as the status of the ‘expert’ had been.⁴⁶

Historians have noted the question “What is electricity?” as having long featured in electrical discourse, but have not examined its persistence for over a century, nor sought to explain why the debate on the nature of electricity really seemed to matter to at least some constituencies.⁴⁷ One social group for whom the question was significant was the emerging cohort of telegraph trainees who needed to understand how to treat electricity in their daily working lives. In his *Elementary treatise on electrical measurement for the use of telegraph inspectors and operators* (1868), Latimer Clark advised them that even though “philosophers are not in accord as to its nature and the theory of its action” they should probably treat electricity as a substance — like water or gas — that could be pumped by a telegraph or battery.⁴⁸ Less burdened by technological concerns, James Clerk Maxwell advised readers of his *Treatise on electricity and magnetism* (1873) that — apart from dismissing the two-fluid theory — they should not “too hastily” make any assumption about the nature of electricity.⁴⁹ Completely ignoring Maxwell’s Cantabrigian authority, however, many British textbooks openly promoted the two-fluid theory of electricity during the 1870s.⁵⁰ One prime example of this was *Magnetism and electricity* (1876) by Frederick Guthrie, a work that canonized the two-fluid theory as the curricular standard for the UK’s national physics examinations for the following decade.⁵¹

Faced with this challenge, followers of Maxwell promoted the ether theory of electricity. Prominent among aspirant Maxwellians was Oliver Lodge, who in 1880 was an assistant at University College London and also a popular public lecturer. In December that year he lectured at the London Institution on the late Maxwell’s theory that light was a form of electrical propagation. Noting his audience’s inevitable perplexity at the diversity of opinion, Lodge acknowledged it was only natural and “proper” for them to demand “What do you mean by electricity? What do you mean by light?” Although he now had an answer to the latter question, he admitted that when asked what electricity was, the simple answer was “We don’t know” — and certainly *not* a form of energy. He thus moved swiftly to demonstrate the identity of electricity to his audience by demonstrating its “known behaviour” in electromagnetism and spark production.⁵² Returning to the same venue two years later as Professor of Experimental Physics at University College Liverpool to lecture on “the ether and its functions”, Lodge nevertheless avoided any temptation to assert that ‘electricity’ was directly identifiable with the electromagnetic ether.⁵³

As electrical engineers developed new technologies of lighting and power, many of them did — *pace* Lodge — come to view electricity as a kind of energy that could be bought and sold like any other purchasable energy-bearing commodity, notably coal or gas. This view was noisily championed by William Preece, the prominent and pugnacious Chief Electrician of the Post Office, taking his public role well beyond his remit of telegraphy and telephony. On 28 December 1881 and 4 January 1882 Preece lectured on “Recent wonders of electricity” to a juvenile audience at London’s Society of Arts. Presenting engineers as masters of the transformations of the “mysterious” agency of energy, he assured his young listeners that electricity was a mode of energy just like magnetism, light, heat, chemistry and motion.⁵⁴ Lodge retaliated

in his lectures “Modern views of electricity” to packed audiences at the London Institution on 1 January 1885 and Birmingham’s Midland Institute on 15 November 1886, lectures soon published as *Modern views on electricity*.⁵⁵ Citing Maxwell’s authority, Lodge characterized the energy of an electrical current as moving through the “medium” *outside* the wire; the energy could thus not be directly associated with whatever electricity might flowing inside the wire itself.

Preece retaliated by attacking Lodge’s “modern” view in his address to Section G, Mechanical Science, at the British Association’s meeting at Bath in summer 1888. As a self-styled “practical man” who had acquired his technical dexterity among the “stern realities of daily experience”, rather than in Lodge’s domain of the laboratory, Preece was repelled by this “wild hypothesis” of the ether as a “peculiar form of matter pervading all space”. He thus enjoined his audience to dismiss the ethereal view of electricity as a mere physicist’s fantasy.⁵⁶ Other critics found much to object to in Lodge’s *Modern views of electricity* — sympathetic readers concerned to learn the identity of electricity were evidently confused by the variety of accounts he employed.⁵⁷ Whilst the opening “advertisement” indicated his book would explain “what is known of the nature of electricity” in terms of the “etherial theory”, Lodge promiscuously appealed both to single- and two-fluid theories of electricity to explain how electrical circuits worked. Although electricity disappeared in later chapters as he developed a mechanical model of the ether, Lodge later reassured his readers that the ether somehow comprised both positive and negative electricity.⁵⁸ Reviewers who praised Lodge’s book as a treatise on the ether were thus unimpressed on the key issue of electricity’s nature: the *Athenaeum* saw him as offering no more than a “very vague analogy” for electricity while *Nature* criticized him for his atavistic appeal to the antique two-fluid theory.⁵⁹

In the face of such conflicting and indecisive attempts to present an authoritative view of electricity, the bafflement of contemporaries continued. Many declined to believe either Preece or Lodge, taking their disagreement to signify that the nature of electricity was unknown. As the electrical engineer Sydney F. Walker wrote in 1889 in the opening glossary of his prospectus for domestic electrification, *Electricity in our homes and workshops*: “What is electricity? We do not know.” For practical purposes, Walker suggested it was necessary only to know enough to make electricity “obedient to our will”.⁶⁰ Similarly agnostic was the journal *Lightning* that commenced publication a year later, targeted at a readership prospectively interested in installing domestic lighting. Its regular “glossary” column characteristically commenced with the question that was presumptively of paramount importance to readers: “Electricity. A definition of this has yet to be given; it may be popularly understood as a natural force recognized by some of its effects or manifestations.”⁶¹ Determined to have at least some specific understanding of the commodity that was increasingly present in the home and urban environment, some lay observers adopted the folk view of electricity as a fluid manufactured — like coal-gas — in civic power stations.⁶²

In the context of such lingering unresolved issues of authority on a key public question, we can understand why William Crookes addressed this problem directly

in the very opening sentence of “Some possibilities of electricity” published by the *Fortnightly review* in February 1892. At the outset of his technocratic survey of future prospects of electrification, he readily admitted that “little as yet” was known about even the basic character: “High authorities cannot even agree whether we have one electricity or two opposite electricities.” With no little irony he contrasted the views of a group of un-named American “Substantialists” convinced it was a material substance, whereas unnamed “Others” — presumably practical electrical engineers — viewed it as an especially manipulable form of energy. Not a complete convert to the Maxwellian canon, Crookes noted that even those committed to the Maxwellian electromagnetic ether such as Lodge and the Serbo-American engineer Nikola Tesla could not quite agree among themselves as to how they should interpret the etherial nature of electricity. Was it a mode of the ether or was it ether bound to matter?⁶³

In the face of such divergence between “high authorities”, Crookes modestly proposed his own ambivalent answer. Methodologically, the only way to tackle “the difficulty” was to persevere with experiment to learn the useful attributes and functions of electricity. Yet despite his stoical concession that authorities in the subject might “never learn what electricity is”, he then moved to offer his own account anyway. In conformity with Crookes’s own long-standing claims for identifying a fourth state of radiant matter in the cathode ray tube, research by Hermann von Helmholtz and Lodge indicated that electricity came in discrete forms.⁶⁴ He thus advised readers to adopt the heterogeneous strategy of embracing the hypothesis of the “atomic character of electricity” while deferring to Maxwellian ether theory for general explanations of electrical phenomena.⁶⁵ Just like Lodge and fellow authorities on electricity, Crookes was obliged to adopt a pragmatic pluralism to meet the presumptive public expectation that at least some kind of answer would be forthcoming on the great question.

To understand Crookes’s modesty in this point, it is important to note that his prognostications on electrical matters had received scathing criticism from the Tory periodical *The spectator* in the previous November. From this encounter, we can glean some further insight into the circumscribed status of the ‘authority’ in the late nineteenth century, taking us back to the agonistic forum of the press in which the status of authority was also debated. We shall see indeed that the contemporary press critiqued the role of authority figures perhaps as much as they did the role of ‘experts’, but for different reasons and with different consequences.

4. BREACHING THE BOUNDARIES OF ‘SCIENTIFIC AUTHORITY’

The scientific authorities of to-day have fallen into a rather provoking and tantalizing habit of taking the public into their confidence, making known to it discoveries that are as yet only half-known to themselves, and building upon the basis of those discoveries a bewildering fabric of conjectural possibilities.... No doubt the really scientific man should have no difficulty in sifting what is actually true from what is only possible; but the unscientific host, to which we confess that we belong, is apt to get rather bewildered by such revelations, and confusing science with pure conjecture, to believe in all manner of impossibilities and absurdities.

The spectator, 21 November 1891⁶⁶

William Crookes was among a number of characters in electrical science and technology treated at certain key junctures in the 1890s as ‘authorities’. Their authoritative status was bolstered by aristocratic patronage: it was during Lord Salisbury’s office as Prime Minister that this noble gentleman-electrician raised both Sir William Thomson to the peerage as Lord Kelvin in 1892 and Crookes to a knighthood in 1897.⁶⁷ Thomson and Crookes both had broad ranging interests, and the limits of their specialist knowledge to certain specific areas did not inhibit them from seeking to exercise their authority on matters some way beyond their areas of specialization.⁶⁸ By 1891 William Crookes had achieved eminence not only as a chemist, editor and adept experimenter with cathode-ray tubes, but also as the President of the Institution of Electrical Engineers in London — a position which Thomson had held two years earlier. Both men had been elected to this position in virtue of their contributions to the electrical industry, and both had close connection with its entrepreneurial side. Thomson assiduously managed lucrative rights in his patents for marine compasses and electrical meters⁶⁹ while Crookes vigorously pursued patents on electrical lighting.⁷⁰

Both managed this financial aspect of their electrical practice with considerable circumspection, their public speeches bearing at first sight little evidence of their financial as opposed to professional interests — although one exception to this will be indicated below. At the same time, however, their authority was far from absolute. For example, many could and did easily ignore Kelvin’s oft-expressed doubts about Maxwell’s electromagnetic theory of light.⁷¹ And when William Crookes was invited by the *Jewish chronicle* in early 1892 to judge whether using electrical lamps on the Sabbath broke Talmudic prohibitions on lighting of fires, his negative answer failed to convince informed conservative sceptics in the Anglo-Jewish community. The debate thus continued unresolved with Crookes’s authority insufficient to be decisive in influencing the opinions of his audience.⁷² And as we shall see shortly, Crookes’s authority was not only insufficient to meet the demands of his publics, but his exercise of it was criticized in November 1891 by a variety of periodicals including the Tory *Spectator*, for over-reaching his status as a “scientific authority” in his pronouncements *qua* President of the IEE.

To appreciate the Press’s ethical critique of Crookes, it is important first to appreciate how contemporary audiences for science saw such authority figures as constrained by a form of social contract. For the case of electricity, I suggest that the public looked to such practitioners for trustworthy wisdom on how to deal with the risky phenomenon of electrification. In her account of this matter Carolyn Marvin appeals to the twentieth-century notion of the impartial ‘expert’. She argues that the late nineteenth-century laity turned to an élite body of electrical ‘experts’ to explain what they could expect of electricity and its novel technologies.⁷³ Appealing to an account of the moral reciprocity of a “currency of promises” in which Crookes was embedded, experts upheld the laity’s right to share in “electrical prosperity” in return for public recognition and indulgence in their expert ingenuity. Marvin suggests that this “vague” but binding bargain was monitored by both the popular

and the technical press for breaches in reciprocity — such as broken promises or inappropriate mistrust.⁷⁴

By substituting the term ‘authority’ for ‘expert’ in her text we can see a plausible account of how Crookes engaged with the public in what was not quite a dialogue but at least a form of symbiotic relationship. In what follows I extend Marvin’s account of a moralized social contract to allow for reciprocity in *epistemic* matters too: part of the social contract binding the ‘authority’ was the limits to which speculative pronouncements could be made without actively misleading or confusing the public. As hinted earlier, however, the distinction between an expert and an authority was more a matter of Victorian idealization than of rigid social taxonomy. But professors and presidents whose utterances were not (closely) connected with their income were certainly candidates for ‘authority’ in a way that specialist technical witnesses were not; the testimony of the former was not solicited by a litigious party, purchased by partisan interest, or guided by financial interests.

While the testimony of an authority was thus presumptively more *trustworthy* than that of an expert, those who performed these roles were drawn from a common community of practitioners. Since a given group of practitioners could in different times and places serve in either role, the practical demarcation between expert and authority was not so clear. Tal Golan has noted, for example, how this parity of roles led to the embarrassment the young Michael Faraday, unworldly chemical researcher and Superintendent of London’s Royal Institution, experienced in 1820. Notwithstanding the extensive laboratory experiments he had carried out to support the claims of the Phoenix Insurance Company, his courtroom testimony on the mechanism of whale oil combustion was attacked under hostile cross-examination — an episode published in *The Times*.⁷⁵ His partisanship and courtroom defeat in this case were not, however, sufficient to thwart his later development as one of Britain’s leading authorities on non-commercial aspects of electromagnetism.⁷⁶

As Iwan Morus has rightly emphasized, *context* was a major issue in nineteenth-century experimentalists’ claims to authority. A performance outside the practitioner’s accustomed domain was risky, especially since audience expectations might differ radically from one venue to another, e.g. the courtroom or lecture theatre. A performance judged as authoritatively persuasive in one context might leave audiences sceptical and unimpressed in another.⁷⁷ In other words, authority was a *performative* attribute, not simply guaranteed by institutional or educational credentials, but to some extent judged by audiences on the management of its performance. Those commonly accorded the status of authority could have their credentials in this regard challenged by audiences — especially critics in the press.

Such challenges circumscribing the prerogatives of authority spoke not just to geographical-political considerations. There were also moral-epistemic questions about the license that authority figures had to offer their audiences deliberations beyond the domain of well-attested evidence. The trustworthy status of experts was circumscribed by expectations of what was and what was not appropriate to utter in the voice of ‘authority’ — prudence and self-restraint being important forms of self-discipline. Ironically

indeed, the identification of authority was at its most explicit just in those instances when such expectations were breached — although debates around the prerogatives of putative ‘authorities’ never quite attained the notoriety of the shamelessly partial ‘expert’. Such were the tacit protocols concerning Crookes’s status as an authority that press questions about his role indicate that he was not licensed to trade on this status to indulge in wild conjectures.

In an after-dinner speech at the Institution of Electrical Engineers held in a hotel in London’s Piccadilly district in November 1891, engineers and journalists alike heard Crookes’s claim that a single cube of the all-pervasive ether contained 10,000 foot-tons of energy. He drew this hint from the spectacular efforts of Nikola Tesla to show how the new high frequency alternate current technology of fluorescent lighting could be communicated without wires. Crookes averred that the “electrician of the future” should seek to unlock this boundless store of energy and “subdue” it to the service of mankind.⁷⁸ As a conservative watchdog concerned with exposing the *parvenu* excesses of scientists, the *Spectator* magazine immediately responded that Crookes had gone too far. An editorial comment complained that the “scientific authorities” of the day had fallen into the rather provocative habit of taking the public into their confidence and then announcing a “bewildering” fabric of conjectural possibilities that were liable to lead the laity astray:

What, for instance, may we believe from Prof. Crookes’ speech before the Institution of Electrical Engineers on Friday last. Here is an undoubted scientific authority; and yet — and yet, it is really difficult to know whether we should understand him literally, and take all his statements as the latest scientific truths....⁷⁹

The brain of the “unscientific man” reeled before claims that the ordinary room contained thousands of foot-tons of energy per cubic foot: he could only wait until the day arrived to “summon up sufficient energy to believe it”. Archly the *Spectator* invited Crookes to consider the case of the young Mrs Abbott who — the very day after his lecture — had entranced audiences at London’s Alhambra Theatre with uncanny displays of physical strength. As no strongmen had been able to wrest a billiard cue from her hand all evening, the *Spectator* threw down the gauntlet to Crookes:

... Is it not the plain duty of Professor Crookes to discover whether or not that lady has been poaching on the preserves of electricity, and has filched the aetheral energy which he had reserved for the future electrician? In the mean time, the mind of the sober and unscientific person swings uncomfortably between credulity and incredulity, and finds no rest in either.⁸⁰

On the *Spectator*’s view Crookes was not licensed to offer such conjectural claims to the public nor should he expect the public to accept them merely on his word. Most revealingly figured here is the wryly styled “sober and unscientific person” whose prerogative it was to question the judgement of “scientific authority”: the putatively “unscientific” mind was in fact quite “scientific” enough to suspect a flagrant implausibility when it saw one.

Following this scathing assault by the *Spectator*, Crookes was somewhat more cautious in his public utterances of a futurist technocracy in his piece “Some possibilities of electricity” published by the *Fortnightly review* in February 1892 and widely read thereafter.⁸¹ As I have shown elsewhere, the scope of Crookes’s conjectures ranged from the advent of ubiquitous electrical power supply and wireless telegraphy all the way to fantasies of controlling the weather by electricity. He was criticized by other sectors of the press, notably the *Review of reviews*, edited by William Stead, and the semi-technical weekly periodical the *Electrician*. The latter gently taunted Crookes for writing a “Jules Verne-like romance”, treating it as one instance of a trend for such pieces that “harmlessly” amused the public with the “half-baked notions with which some thinkers are busying themselves”.⁸² Although I have no scope to pursue the matter further here, we can note that with the rising popularity of H. G. Wells from the mid-1890s, leading figures in the scientific community could increasingly expect to be spared such chiding from the press for indulging in speculative futurism.⁸³

Finally we should note that in his *Fortnightly* pronouncements on electrical futures, Crookes was not writing in quite the disinterested mode that his seemingly authoritative stance might have led readers to imagine. Contemporary critics did not explicitly comment on his passing observation in “Some possibilities of electricity” that alternating current had “at best a somewhat doubtful reputation” in regard to safety. Yet this is evidence that he was using this journalistic forum to promote the rival direct current technology. Having taken out unremunerative patents for electric lighting and participated as an expert witness in related litigation (Edison and Swan v. The Brush Company) in 1888, Crookes moved the following year to become a director of the Notting Hill Electric Light Company that offered low-tension (low voltage) direct current supply to his home district. This initially unprofitable company was struggling against the more economical yet reputedly more hazardous technology of high-tension alternate currents linked to many recent deaths in the USA.⁸⁴ In Crookes’s writing of early 1892, it is easy to see his allusion to the fate of an irresponsible mythical Roman king as an attack on the a.c. competition:

Whilst we are seeking for cheaper sources of electricity, no endeavour must be spared to tame the fierceness of those powerful alternating currents now so largely used. Too many clever electricians have shared the fate of Tullus Hostilius, who, according to the Roman myth, incurred the wrath of Jove for practising magical arts, and was struck dead with a thunderbolt. In modern language, he was simply working with a high tension current, and, inadvertently touching a live wire, got a fatal shock.⁸⁵

In this subtle and erudite denigration of alternate current, we can see how easy it was for the canons of ‘authority’ to be subverted by those institutionally located in its ambit. Indeed once he was free of the trappings of office associated with the presidency of the IEE in 1891, Crookes served more overtly in public to fight battles for his supply business, ending up as Chairman of a very profitable company. Significantly, however, when Crookes became President of the British Association in

1898, friends had to guide him against dedicating the address primarily to his somewhat controversial psychical researches. Crookes also attained the presidency of the Royal Society (1913–15) but only after playing down determined opposition inspired by his son's exploitation of contested rights on a chemical patent.⁸⁶ The acquisition and maintenance of scientific authority in Victorian Britain was thus something of a contingent matter, not so readily demarcated after all from the partisan world of either expert or entrepreneurial cultures. Further research on the public careers of such leading scientific figures might thus usefully shed more light on the ways in which their attainment and maintenance of 'authority' was not necessarily just a function of professional-institutional status but had to be managed to avoid the controversies that bedevilled the role of the 'expert'.

CONCLUSION

This paper has shown that the terms 'expert' and 'authority' have complex and multilayered histories that need further attention from historians. We can no longer unreflectively presume that Victorians' uses of such terminology — especially in the case of the 'expert' — can unproblematically map onto our own. Indeed such terms are just too slippery to be slotted in unproblematically in the explanans of any historical narratives.

More specifically, three sorts of conclusion can be drawn from the foregoing. First, the Victorian idealized distinction between an expert and an authority has disappeared as the term 'expert' has shed its awkward courtroom opprobrium and has been upgraded to serve effectively as a synonym for 'authority' (even though the role of the expert witness is still troubling to many, and we are still getting used to the seemingly oxymoronic role of the 'lay expert'). Then again, the Victorian notion of authority has been somewhat deflated by the controversies of the mid- to late twentieth century. The notion that scientists can offer trustworthily impartial knowledge has been sufficiently thrown into doubt that authority is nowadays more typically vested in specialist extra-judicial experts largely in view of their esoteric knowledge on key matters. Specialization has perhaps become the key to knowledge management, rather than social status. Historians might thus fruitfully reflect on how such power-knowledge categories as expert and authority were understood in other periods and contexts, and indeed how deployment of these notions has changed over time. There is no good scholarly excuse for invoking the expert and authority to interpret science's past without striving to use these terms with the minimum of anachronism.

Such an approach could add fertile themes to the agenda of the historiography of knowledge. Historians could analyse the interpretations of unstable 'expert' status to examine how knowledge communities in the past coped both with partisanship and also with a pervasive lack of certainty or agreement about key technical matters.⁸⁷ For the particular case studied here: how else could the enterprise of electrification have proceeded with only fallible experts and authorities to guide them? We need a finer analysis of how contemporaries viewed and discussed the prerogatives of such

participants in the political economy of technoscience. And we need to understand how the performance of practitioners was judged, and in which tribunals of culture and media, to see how addressing some technical questions rather than others enabled certain individuals to become ‘authorities’ or ‘experts’ — on particular terms and with particular limits, judged so by an ever more expert ‘laity’.

Finally, we can see a new way of looking at the role of the late nineteenth-century periodical press. This not only derives from the important fact that it was the press that first introduced the notion of the ‘expert’ to describe the problematically compromised role of the scientific witness paid to testify in court. The press also played a broader role in critiquing the limits of what could legitimately be said and done by ‘experts’ and scientific authorities. This prompts us to consider a loose analogy with courtroom cross-examination of hired experts that I discussed earlier. Both periodical and courtroom were public tribunals in which special testimony was solicited or appropriated to comment on major questions, and the bearers of that special knowledge were evaluated for their performance in such tribunals. There are obviously disanalogies: authority figures could choose their own topics on which to expatiate, and would only suffer the informal censure of particular periodicals if they breached presumptive norms of conduct. But the overall point should be made that to understand the categories of expert and authority and how they changed over time and altered in moving from one context to another, we can perhaps fruitfully look beyond traditional institutional sites of science to the courtroom and the press to see where the key transformations occurred. After all, that is where the best jokes about the increasing denominations of liars were circulated.

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2. Anthony Giddens, *The consequences of modernity* (Cambridge, 1990).
3. Carolyn Marvin, *When old technologies were new: Thinking about communications in the late nineteenth-century* (Oxford, 1988); Peter Broks, *Media science before the Great War* (London 1996). For a contrasting approach, see Iwan Morus, *Frankenstein’s children: Electricity, exhibition, and experiment in early-nineteenth-century London* (Princeton, NJ, 1998).
4. “Technical expertise is continuously re-appropriated by lay agents as part of their routine dealings”.

- Giddens, *op. cit.* (ref. 2), 144.
5. Harry Collins and Robert Evans, *Rethinking expertise* (Chicago and London, 2007); Alan Irwin, *Citizen science: A study of people, expertise, and sustainable development* (London, 1995); Harry Collins and Robert Evans, “The third wave of science studies: Studies of expertise and experience”, *Social studies of science*, xxxii (2002), 235–96; Brian Wynne, “Misunderstood misunderstanding: Social identities and public uptake of science”, *Public understanding of science*, i (1992), 281–304; and Brian Wynne, “May the sheep safely graze? A reflexive view of the expert–lay knowledge divide”, in Scott Lash, Bronislaw Szerszynski and Brian Wynne (eds), *Risk, environment and modernity: Towards a new ecology* (London, 1996), 44–83.
 6. Tal Golan, *Laws of men and laws of nature: The history of scientific expert testimony in England and America* (Cambridge, MA, and London, 2004).
 7. Hugh Fletcher Moulton, *The life of Lord Moulton* (London, 1922), 45.
 8. Carol A. G. Jones, *Expert witnesses: Science, medicine, and the practice of law* (Oxford, 1994). See also Roger Smith and Brian Wynne, *Expert evidence: Interpreting science in the law* (London, 1989).
 9. Golan, *op. cit.* (ref. 6), Christopher Hamlin, “Scientific method and expert witnessing: Victorian perspectives on a modern problem”, *Social studies of science*, xvi (1986), 485–513, and Christopher Hamlin, *A science of impurity: Water analysis in nineteenth-century Britain* (Berkeley, Los Angeles and Oxford, 1990).
 10. The term ‘expert’ itself seems to have been coined by the periodical press in the 1850s, as a somewhat sardonic shorthand for ‘expert witness’, e.g. “Experts in insanity”, *Saturday review*, vi (1858), 645. See below for further discussion of the *Saturday review*’s advocacy of this term.
 11. Sometime this led to whole new fields of expertise being opened up, e.g. water purity and food analysis in the mid-nineteenth century, Hamlin, *op. cit.* (ref. 9); or as Hans-Georg Hofer has shown, medical expertise in electrical injuries was developed in *fin de siècle* Vienna by Hans Jellinek to deal with the growing phenomenon of compensation claims following accidents in the electrical industry. “Dem Strom auf der Spur: Stefan Jellinek und die Elektropathologie”, *Blätter für Technikgeschichte*, lxxv–lxxvi (2004/5), 165–98.
 12. Golan, *op. cit.* (ref. 6), 67. See further discussion below.
 13. Golan, *op. cit.* (ref. 6).
 14. The conference papers included Robert Angus Smith, “Science in our courts of law”, *Journal of the Society of Arts*, vii (1860), 135–47, and William Odling, “Science in our courts of law”, *Journal of the Society of Arts*, vii (1860), 167–8. See discussion in Golan, *op. cit.* (ref. 6), 111–20.
 15. “Expert witnesses”, *Saturday review*, xiii (1862), 32–33; Golan, *op. cit.* (ref. 6), 104–5.
 16. Golan’s account does not address the question of which nineteenth-century commentators used the term ‘expert’ and which preferred not to do so.
 17. [Editorial], *Chemical news*, v (1862), 183; Golan, *op. cit.* (ref. 6), 121.
 18. Attributed by *The Times* in 1882 to a “well-known judge”, it is unclear whether this quip either preceded or reworked the more famous comment concerning “lies, damned lies and statistics” commonly attributed to Benjamin Disraeli. See Mark Twain, “Chapters from my autobiography”, *North American review*, clxxxiii–clxxxvi (1906/7), 5 July 1907; in fact no reliable source has yet documented Disraeli’s utterance of such a phrase (he died in 1881). As the first published source of this phrase is probably the first Earl of Balfour quoted in the *Manchester Guardian*, 29 June 1892, 5, it is arguable that the jocular dismissal of scientific experts as liars predates the joke about lies and statistics.
 19. For gently censored recollections of the Bramwell augmentation of this joke, see Alan A. Campbell Swinton, *Autobiographical and other writings* (London, 1930), 61, and Moulton, *op. cit.* (ref. 7), 47. For discussion of the popular but unresolved attribution of the *original* version of the joke to

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20. Leonard Huxley, *The life and letters of Thomas Henry Huxley* (2 vols, London, 1900), i, 255, 257–8.
 21. “Judicial warping of patents”, S. P. Thompson to the editor of *The Times* (29 June 1886), *The Times*, 6 July 1886, 7, col. E.
 22. “Judges and patents”, John Imray to the editor of *The Times* (6 July 1886), *The Times*, 7 July 1886, 6, col C. For the bankruptcy of the New Telephone Company that had purchased Thompson’s telephone patents in 1884 see Jane Smeal Thompson and Helen G. Thompson, *Silvanus Phillips Thompson: His life and letters* (London, 1920), 116–18.
 23. Right Hon. Viscount Alverstone [Richard E. Webster], *Recollections of bar and bench* (London, 1914), 282–3.
 24. For Hopkinson see Efstathios Arapostathis, “Consulting engineers in the British electric light and power industry, c. 1880–1914”, unpublished D.Phil. thesis, University of Oxford, 2006.
 25. Moulton, *op. cit.* (ref. 7), 44–45.
 26. Such was Moulton’s characteristic tendency to use his specialized knowledge of science to critique expert witnesses’ testimony in cross examination that one judge remarked sarcastically: “Moulton generally gave scientific evidence while the witnesses argued the law.” Moulton, *op. cit.* (ref. 7), 46–47.
 27. “Expert opinions”, Albert Gay and Charles H. Yeaman, *Central station electricity supply* (London, 1899), 13–14 (also 2nd edn, 1906, 11). Gay was the Chief Electrical Engineer of the Metropolitan of Islington and Yeaman was Chief Electrical Engineer of the County Borough of Hanley, see Gay and Yeaman, *op. cit.*, 2nd edn, p. iii.
 28. [*The Times*’s Philadelphia Correspondent], “Edison’s electric light”, *The Times*, 14 January 1880, 8, col. A.
 29. Thomas Edison, “The success of the electric light”, *North American review*, cxxxi (1880), 295. For further discussion see Graeme Gooday “Illuminating the expert–consumer relationship in domestic electricity”, in Aileen Fyfe and Bernard Lightman (eds), *Science in the marketplace: Nineteenth-century sites and experiences* (Chicago and London, 2007), 231–68.
 30. [Editorial], *The Times*, 4 April 1882, 9.
 31. [Editorial note], “Alternate current working”, *Electrician*, xxiv (1889), 325. For further discussion see Graeme Gooday, *The morals of measurement: Accuracy, irony and trust in late Victorian electrical practice* (Cambridge, 2004), chap. 5.
 32. See Gooday, *op. cit.* (ref. 29).
 33. “Execution by electricity”, *The electrical review*, xxv (1889), 108; Thomas P. Hughes, “Harold P. Brown and the executioner’s current: An incident in the AC–DC controversy”, *Business history review*, xxxii (1958), 143–65; and Mark Essig, *Edison and the electric chair: A story of light and death* (New York, 2003).
 34. “Execution by electricity” (ref. 33).
 35. Essig, *op. cit.* (ref. 33), 174–89.
 36. “Electricity’s victims in Europe”, *The American architect and building news*, xxv (1890), 27 — quoting from *The New York commercial advertiser*. For Heinrichs’s dependence on Westinghouse income and subsequent suicide, see “An inventor kills himself; driven to suicide for want of means to continue work”, *New York Times*, 15 October 1891, 5.
 37. “Electricity’s victims in Europe” (ref. 36).
 38. Thomas A. Edison, “The dangers of electric lighting”, *The North American review*, cxlix (1889),

- 625–35; George Westinghouse, “A reply to Mr Edison”, *The North American review*, cxlix (1889), 653–64; William Thomson, “Electric lighting and public safety”, *The North American review*, cl (1890), 189–96; and George Westinghouse, “Sir Wm. Thomson and electric lighting”, *The North American review*, cl (1890), 321–9.
39. Gay and Yeaman, *op. cit.* (ref. 27).
40. Charles R. Gibson, *The romance of modern electricity* (London, 1906), 315. Gibson was one of the most prolific and internationally successful popular science writers and lecturers of the early twentieth century (approximately half of the forty-five books he published between 1906 and 1930 were on electrical topics), and all the while he was running Gibson Bros, the family’s curtain manufacturing business in Glasgow. See Peter Bowler, “Experts and publishers: Writing popular science in early twentieth-century Britain, writing popular history of science now”, *The British journal for the history of science*, xxix (2006), 159–87, p. 171, and James Muir, “Memoir of the late Charles R Gibson, LL.D, FRSE”, *Proceedings of the Glasgow Philosophical Society*, lix (1931), 59–62.
41. William Crookes, “Some possibilities of electricity”, *Fortnightly review*, n.s., li (1892), 173. For a survey of Crooke’s work in electricity see William H. Brock, *William Crookes 1832–1919 and the commercialization of science* (Aldershot, 2008), 243–66. For Crookes’s position in the physics–spiritualism nexus see Richard Noakes, “Ethers, religion and politics in late-Victorian physics: Beyond the Wynne Thesis”, *History of science*, xl (2005), 415–55, and “Cromwell Varley FRS, electrical discharge and Victorian spiritualism”, *Notes and records of the Royal Society of London*, lxi (2007), 5–22.
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44. Fleming, *op. cit.* (ref. 43), 8. See for example the writings of the Secretary of the London Institution, Frederick Hovenden, *What is heat? A peep into nature’s most hidden secrets* (London, 1894); *What is life? or, where are we? what are we?* (London, 1897); and *What is heat, what is electricity?* (London, 1899).
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46. See Graeme Gooday, *Domesticating electricity: Technology, gender, and uncertainty, 1880–1914* (forthcoming, London, 2008), chap. 2, and later in the present article.
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61. "Glossary", *Lightning*, i (1891), 9.
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63. Gibson reported a common presumption that electrical power stations worked by analogy with coal-gas works, manufacturing a “mysterious material fluid” to pump through wires to the home. Gibson, *op. cit.* (ref. 40), 13–14, from chap. 1, “What is Electricity?”.
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75. Marvin, *op. cit.* (ref. 3), 16, 55.
76. Golan, *op. cit.* (ref. 6), 63–68. *The Times*, 18 December 1820, 3, where Faraday’s name is mis-spelled as ‘Ferriday’.
77. Geoffrey Cantor, David Gooding and Frank A. J. L. James, *Faraday* (Basingstoke, 1991). For examples of how conflicting expert testimony influenced the trajectory of nineteenth-century scientific careers, see Hamlin, *op. cit.* (ref. 9).
78. See Iwan Morus, “‘More the aspect of magic than anything natural’: The philosophy of demonstration”, in Fyfe and Lightman (eds), *op. cit.* (ref. 29), 336–70, p. 365.
79. See “Dinner of the Institution of Electrical Engineers”, *Electrician*, xxviii (1891), 70, subsequently reproduced in *Popular science monthly* for February 1892.
80. “Science and conjecture”, *Spectator*, lxxvii (1891), 723. For further discussion of Crookes as a challenged authority, see Graeme Gooday, “Profit and prophecy: Electricity in the late Victorian periodical”, in Geoffrey Cantor, Sally Shuttleworth *et al.* (eds), *Reading the magazine of nature: Science in the nineteenth-century periodical* (Cambridge, 2004), 238–47.
81. “Science and conjecture”, *op. cit.* (ref. 80).
82. Crookes, *op. cit.* (ref. 41). As a journalist for *The Times* noted, “Considerable attention was attracted last February by an article on the future of electricity, from the pen of Professor Crookes”. See “Wire-to-wire electric communication”, *The Times*, 22 November 1892, 7, col. A.
83. “Notes”, *Electrician*, xxviii (1892), 341–2. Lacking the archness of the *Spectator* piece, the *Electrician*

did not refer to him ironically as an ‘authority’.

84. See the discussion of H. G. Wells in Broks, *op. cit.* (ref. 3), 98–100.
85. Fournier D’Albe, *op. cit.* (ref. 68), 291–310. For an example of the troubled early history of alternating current, see discussion of the tribulations of Sebastian de Ferranti’s controversial Deptford generating station in Britain, see Thomas P. Hughes, *Networks of power: Electrification in Western society* (Baltimore, 1983), 239–47, and John F. Wilson, *Ferranti and the British electrical industry, 1864–1930* (Manchester, 1988).
86. Crookes, *op. cit.* (ref. 41), 179.
87. Fournier D’Albe, *op. cit.* (ref. 68), pp. v–vii, 309–10, and Brock, *op. cit.* (ref. 41), 457–77.
88. This paper thus complements the discussions of ‘technological expertise’ in Ben Marsden and Crosbie Smith, *Engineering empires: A cultural history of technology in nineteenth-century Britain* (Basingstoke, 2005). Since ‘expertise’ was not an actors’ category for forms of technical knowledge in the nineteenth century, I have restricted discussion of it in this paper to authors writing in the twentieth or twenty-first centuries.

SCIENCE, SCIENTIFIC CAREERS AND SOCIAL EXCHANGE IN LONDON: THE DIARY OF HERBERT McLEOD, 1885–1900

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INTRODUCTION

1. *Methodological Approach*

Herbert McLeod's diary, with daily entries from 1860 to 1923, reveals much about scientific life in London.¹ It has been a major source for a number of papers.² This paper, following one written about McLeod and some of his contemporaries at the start of their careers, focuses on a period when they were well established in the scientific community. While McLeod is again the principal player, the paper has a large cast of characters. Its purpose is not biographical; rather it is to illustrate aspects of the everyday lives of some of the more established scientists working in or near London during the period 1885–1900, to show some of the ways in which their scientific and social lives intersected, and some of the ways in which ideas were exchanged.

Herbert McLeod, FRS (1841–1923) was not a major research scientist but his work was sufficiently well regarded for him to have been elected to the fellowship of the Royal Society at the age of forty. He was professor of chemistry at the Royal Indian Engineering College and engaged fully in the scientific life of the capital.³ He attended meetings at learned societies and served on the councils of the Chemical Society, and of the Physical Society of which he was a founder member. He served also on the British Association council and on various of its committees, and regularly attended the annual meetings. He was active, too, in the council and committees of the Royal Society. He had many friends in the scientific community — in academic circles, in industry, and in the trades.⁴ His laboratory skills were widely admired and his advice on technical matters was sought by other scientists. As will be shown, McLeod was an 'invisible' presence in many laboratories. His skills and knowledge were widely dispersed within the London scientific community, and sometimes beyond. In his diary McLeod mentions many people and the work they were doing. He also recorded social events at which scientists exchanged information. It is for these various reasons that the diary is such a good historical source. McLeod rarely passed judgement. His diary is a simple chronicle of daily events and, for that reason, is probably more reliable, if less easily readable, than an opinionated diary would be.

In the paper covering the start of McLeod's career, some methodological problems related to the historical recovery of the everyday world of London scientists were outlined.⁵ Many of the same problems arise in writing about this later period and will be restated only briefly here. However, James Secord's keynote address

“Knowledge in transit” at the 2004 meeting of the Canadian, American and British history of science societies in Halifax has helped me to reflect further on the many activities McLeod described.⁶ Secord remarked that history of science is becoming an increasingly fractured discipline and expressed concern that work in the field not become overly antiquarian. Indeed, for nineteenth-century London alone, historians have uncovered so much new scientific activity, and at all levels of society, that it is difficult to think about it systematically. How to place people and their local activities within larger stories and provide some analysis is a challenge. In a small way this paper is concerned with the “movement of local knowledge”, something that Secord said “we need to think more explicitly about”. Perhaps McLeod’s wide range of scientific and technical exchanges will one day illustrate a larger story of scientific communication — but we are not yet in possession of the plot. For that reason this paper is not a case study, it is a microhistory; though, as will be shown, one with a few elements of generality.

In a recent paper on the historiography of nineteenth-century science, Iwan Morus, like Secord, points to the limitations of older generalizing categories such as ‘professionalization’ and ‘discipline formation’ in accounting for the growth and development of science.⁷ New detail on people and activities that earlier were not included within the scientific fold has complicated our picture of who counts as scientist and what counts as science. We now understand that what becomes institutionalized is the result of many disputes in a wide open marketplace, is highly political, and not entirely foreseeable. But our more nuanced picture of scientific activity should not blind us to the existence of hierarchy. Science is too important a cultural phenomenon to be anything other than hierarchical, however much some might wish for it to be otherwise. Morus is sensibly critical of older diffusionist accounts of the spread of knowledge. That there were/are many centres of knowledge production, and that the flow of information was/is in many directions, is surely true. But his claim that “there was no trickle-down effect in nineteenth-century culture” is probably false. Centres of knowledge production are rarely fully *sui generis* and there is hierarchy among them. The very fact that the “gentlemanly elite” found that their scientific ideas were, as Morus states, “strongly contested on all sides” implies that those ideas were indeed trickling down.⁸ Scientific ideas continue to trickle down today, albeit from different elite sources and, as earlier, they are debated in many arenas.

Other scholars working on popular and consumer science have shown a similar tendency to de-emphasize hierarchy.⁹ While historically interesting in its own right, the wider marketplace for scientific ideas is something distinct from scientific production (not that this is denied by scholars of popular science). It takes a great deal of hard work and good fortune to become a successful scientist. Already in the nineteenth century hopeful research scientists had to have access to apprenticeship or advanced education, laboratories, research groups, industrial support, granting bodies or independent wealth, and publication, to make major careers. Further, the criteria for carrying out good science, then as now, go beyond the cultural and political, however important they may be. Scientific ideas have to fulfil several consistency

criteria for acceptance; but what distinguishes science from other activities is its need for at least fair consistency with the natural world. Much skill is needed to seek such consistency, and to recognize when and how it has been met. Together with the aforementioned requirements, this makes for hierarchical distinctions between élite, everyday and popular practitioners, and between producers, teachers and consumers. Élite science is not some scandal that needs rectifying. It needs to be fully described with all its dependencies and, where possible, its role in society explained. This does not imply that sectors lower in the hierarchy are not worth studying, on the contrary. Nor does it imply that individuals can easily be placed. McLeod, for example, lived his life at many sites. Some were élite social and scientific spaces, others were not. He, himself, was not at the top of the scientific hierarchy, but his diary entries reveal much about dependency at the upper levels, about patterns of socialization and of scientific exchange. As will be shown McLeod's exchanges were, by and large, conducted either orally or through practical demonstration. Secord has shown elsewhere that oral communication, whether in conversation or in lectures, was especially important in McLeod's period.¹⁰ And, more generally, scientific practice is now widely recognized as a form of knowledge.

While science is not, and never has been, the monopoly of any particular class, some people will always have the means to go first along its many pathways. Élitism in science should be distinguished from other forms of élitism with which it overlaps. As my work on McLeod's diary illustrates, the privileged spaces of monied, liberal Anglican, or aristocratic élites were never identical with those of science, even though many nineteenth-century practitioners came from privileged social backgrounds. Older élites typically attempt to take hold of the new, but in the case of scientific novelty with no longer-term success than other groups. Today's world is more meritocratic and many formerly élite spaces have shrunk away. Nonetheless science remains hierarchical with some scientists sitting atop important intellectual, academic, technological, medical, industrial and governmental trees. Their views tend to dominate.

Can a paper on McLeod's diary be of any help in addressing Secord's concerns? Perhaps, in a limited way. Some aspects of communication and of the ways in which knowledge travels before becoming institutionalized can be seen in the many activities described by McLeod. However, a truly satisfying explanation of how and why some, but not all, knowledge becomes firmly institutionalized and then circulates around the globe is not available at present. In facing some of our discipline's limitations Secord was led to the view that historians of science have suffered a "loss of direction" in recent years.¹¹ Perhaps the humanities and social sciences, more generally, need a new paradigm. Modernism has been cannibalized for so long that there are few ideas left for scholars to feed on.¹² It is unclear whether, when, or where a new paradigm will emerge — though possibly from the frontiers of anthropology and biology. In the meantime the theoretically inclined will continue trying, with some difficulty, to say something new. We often forget that historical methodology is fundamentally utopian. In seeking better methods we believe that we can unlock the

past and gain a deeper understanding of human behaviour. Each new paradigm does allow understanding of ourselves, and of human history, to open out. But unlike for scientific knowledge where near universal consensus on many issues is reached for long periods, historical and socio-cultural understanding is more local, and never completely satisfying.

As in the earlier paper many mundane stories are brought together within a loose narrative framework. The period covered is again one of roughly fifteen years and, as before, the stories move back and forth in time. Many scientific and technological sites are mentioned and are defined not only geographically but also in ideological, disciplinary, social or institutional terms. While the terms ‘circle’ and ‘network’ are used less in this paper than earlier, they are used idiosyncratically. The circle is scenic, something envisaged by an individual as the social group to which he or she belongs. In envisaging their circles people learn how to act and respond to events. Circles change over time but, while overlapping in multiple ways, each is specific to an individual. The network is a distinct cultural metaphor, used here in the context of the transmission of ideas and practices. People can become well known within their circles; but only if the circle’s values, its knowledges and representations, move beyond its boundaries and interact with others within a larger network will it have any longer-term historical significance.

The circle is interesting also as a locus of consumption. Consumerism allows people to construct their own narratives and become centres of attention in their own right. In an expanding culture more and more narratives are possible but that does not entail that cultures more generally, or scientific cultures in particular, are non-hierarchical. Resistance within the culture to its seemingly repressive or faulty aspects is simply a reminder that the system needs to rebalance in order to survive. Further, within the larger culture a balance between production and consumption is needed for a vibrant economy and for social order to be maintained. Consumerism helps to diffuse resentment and maintain social stability.¹³ Just as with today’s electronic consumerism, popular science in the expanding marketplace of the nineteenth century allowed large numbers of people to become part of the new, to be included among the modern and not be left behind. One other point worth noting is that in Western cultures, and increasingly elsewhere, science is recognized not simply in terms of its products. It is seen as teaching us something about who we are and what we can achieve. In this connection, while science is highly valued it is also, to varying degrees, feared. Nonetheless there appears to be faith in the fundamental goodness of nature — McLeod certainly had such faith. The fact that scientists are allowed to be curious and to investigate even seemingly dangerous aspects of the natural world is something that would repay further investigation.

On a personal level McLeod and his many acquaintances appear to have shown balance between consumption and production, though not identically so. Serious production, whether of new ideas, novel artifacts or processes, needs more withdrawal from society than McLeod ever showed. Such withdrawal is risky since people can never be sure that it will be rewarded in the longer term. McLeod was a major con-

sumer of ideas and practices but he managed to produce enough of what was deemed interesting to be accepted within circles of influence. He read books and papers, and attended many talks, dinners, exhibitions, and other events. His type of consumption made for continual meeting and chatting. This allowed him to impress others with his own productivity — a reciprocal process.

Regardless of hierarchy, stories about scientists cannot be told simply in terms of archetypes, they require also the particulars of everyday life. As has already been noted, such particulars often bring out complexities that challenge the categories of more systematic narratives. But writing interestingly about everyday activity is difficult and requires the weaving together of isolated and mundane stories from different sites into meaningful wholes. For this paper, simply reducing the many diary entries has been difficult. On their own the entries are often trivial, but collectively they allow some insight into the ways in which people conducted their working lives, and show how social exchanges important to science occur in many different ways, and at many different locations. They also show how individual knowledge and practical expertise become widely dispersed within scientific communities.

As noted in the earlier paper, it was both McLeod's technical ability and his inclusion in conservative religious circles that helped to launch his career. His particular mix of religious and scientific interests attracted the patronage of Lord Salisbury and members of the Cecil and Balfour families; it served him well throughout his life. In this connection it is worth restating that confrontational models are insufficient in explaining how new generations of people achieve professional and intellectual authority. While conflicts in science over ideas, resources and direction are perennial, as are conflicts between the generations, it is important to be discriminating in associating them with the acquisition of cultural authority more generally. Both radical and conservative behaviours can lead to the new and to the modern.¹⁴ Indeed authority in science would appear to be best achieved through the judicious abstraction of older cultural forms and their blending with the new.

McLeod, conservative and religious, was opposed to naturalist views of the kind held by T. H. Huxley and others (see ref. 14). Nonetheless he was part of the modern world and saw much of what we might term secularism as being compatible with his religious views. Only those with more gnostic temperaments, whether religious or secular, are likely to embody the kind of existential anxieties that lead to conflict between religion and science. I will return to this abstract point in the coda.

2. Scientific Background

While this paper is about the everyday lives of scientists, the late nineteenth century had its fashions and excitements which influenced the ways in which people thought and behaved. McLeod paid little attention to theoretical work but was attentive to experimental work in physics and chemistry, especially to work that crossed the border between the two disciplines. Among the big stories that interested him in this period were Lord Rayleigh's work on the densities of pure gases, and the subsequent discovery of the noble gases by William Ramsay and his co-workers. Rayleigh was

interested in finding out whether Prout's Hypothesis, namely the claim that the atoms of all the other elements were integral multiples of hydrogen atoms, was true.¹⁵ This led him to the production of very pure hydrogen and oxygen, and to the construction of a highly sensitive balance for weighing the gases. Rayleigh wanted to see whether an atomic weight ratio of exactly 1:16 could be demonstrated.¹⁶ As will be shown McLeod played a small role in this work. He was able to give Rayleigh some chemical advice, and to help in the refining of various pieces of apparatus for the handling of gases. Having constructed suitable apparatus, Rayleigh decided to determine also the density of pure nitrogen and found that nitrogen produced chemically appeared to have a lower density than nitrogen isolated from the air. It was this anomaly that led Ramsay to the noble gases.¹⁷ At the same time the liquefaction of air, and the separation of gases by fractional distillation at very low temperatures, was attracting enormous interest. In Britain much of this work was carried out by James Dewar at the Royal Institution, and while McLeod played no role in it he did attend many of Dewar's lectures and demonstrations, and made a number of comments in his diary on what was going on.¹⁸ As it happens Ramsay did not use Dewar's liquid air for his work in isolating the noble gases. Rather he was supplied by William Hampson who had invented a good air liquefier which he developed at Brin's Oxygen Company from 1895 on.¹⁹ Perhaps the most accessible account of Rayleigh's and Ramsay's work is given in their Nobel Prize lectures of 1904. As Ramsay noted, it was the convergence of gas density work, spectroscopy, and air liquefaction that set the stage for the isolation of the noble gases. Earlier, in 1885, Brin's process for the extraction of oxygen from air had caused a sensation at the Inventions Exhibition.²⁰ McLeod was asked to prepare a report on the process for the British Association which he presented at the meeting that year. This relates to a further point, namely that McLeod and his contemporaries spent much time at various exhibitions which were important sites for the exchange of scientific information.

It is interesting to see how scientists hoping to ride a promising new wave behave. In the case of the work mentioned above, many people wanted to be part of the scene. For example, intrigued by Prout's hypothesis others, too, were still attempting to produce very pure samples of various elements in the late nineteenth century.²¹ Further, once helium had been identified on the sun people began to look for it on earth. There was much interest in gases emitted from hot springs. Helium was eventually detected, first among volcanic gases from Mount Vesuvius by Luigi Palmieri in 1881. Ramsay, too, was looking at gases from hot springs, and at gases occluded in a wide range of minerals. He and others identified helium (later recognized as a product of radioactivity) among the gases occluded in cleveite, a uranium mineral. Ramsay isolated the helium in 1895.²² But that was not the end of it. Scientists continued to examine minerals in the search for further interesting gases, hoping to make similar discoveries. Unlike Ramsay not everyone was strictly disciplined by Mendeleev's table.²³

Meteorology was another of McLeod's interests since he had been asked by the Meteorological Office to supervise daily measurements taken at Cooper's Hill. Relatedly, he was interested in calculating machines about which he was very

knowledgeable, and in the construction of sunshine recorders, a field to which he made an important contribution.²⁴ Of interest are the many exchanges that McLeod had with people working at the Kew Observatory and at the Meteorological Office, and with those building and using calculating machines. Some of these exchanges will be mentioned below.

McLeod's career was historically specific in yet another sense. He lived at a time when it was still possible to know a sizeable fraction of one's scientific peers. Close to one-half of the authors of papers published in the *Proceedings of the Royal Society* in the period 1885–1900 are mentioned in his diary. McLeod had a scientific exchange, however brief, with each. Since he had many exchanges with scientists, technicians and tradespeople who never published in the *Proceedings*, this is far from being a representative measure of his scientific acquaintances. But it is suggestive of a relatively compact and interactive scientific community. While London was the largest centre of scientific and technological activity in Britain, home to a wide range of activity across all the sciences, the scientific community was much smaller than it was to become after the First World War. Further, while scientists were slowly becoming specialized, they were far less so than is the case today. As a consequence the social and intellectual lives of scientists in different fields were closely interconnected in ways that were to disappear in the early twentieth century. Today we often hear the need expressed for interdisciplinary research, an idea that would not have occurred to many at a time when exchanges across disciplines were an everyday occurrence. In order to demonstrate this, and to show something of the routines of some London scientists, the historical material is presented in three interconnected narratives. The narratives focus on McLeod's activities within learned societies, his work as an examiner and reviewer, his scientific and technical interests, and his social and cultural life. Several stories are confined to endnotes since their inclusion in the main narratives would disturb the flow. However while discursive they, too, are important to the historical recovery being attempted in this paper. Given the many ways in which the various work and social activities overlap, the compartmentalization below is somewhat artificial. It is used nonetheless for the convenience of both author and reader.

LEARNED SOCIETIES, PROFESSIONAL COMMITTEES, EXHIBITIONS AND CONFERENCES

During a typical month in the academic season McLeod attended two of the ordinary meetings at each of the Royal Society, Chemical Society, and Physical Society. Occasionally he would attend meetings elsewhere such as at the Geological or Linnaean Societies, at the Institute of Chemistry, or at the Society for Telegraph Engineers.²⁵ As a consequence he heard between fifteen to twenty papers a month, delivered on a variety of subjects. In August or September, when he attended the British Association meetings, he would listen to yet more lectures on a wide range of topics.²⁶ In addition he attended many committee meetings and, wherever he went, made brief notes on what he heard, whom he met, and the kind of exchanges he had. It is clear from the diary that he was not alone in following this kind of routine. It

was expected of scientists who were well established in their careers that they keep abreast a wide range of activity, do their share of committee work, and be seen at important events. McLeod often chatted with people at tea, which was served before meetings at the Royal Society, or at dinners where the papers just heard would be discussed. Typically, on every second Thursday, senior chemists would attend the meeting at the Royal Society, then dine together before listening to further papers at the Chemical Society in the evening. For example, on 18 November 1885, McLeod listened to four papers at the Royal Society, including one by J. W. Judd, professor of geology at the Normal School of Science, on “deposits from the Nile Delta”, and another by William Ramsay on “evaporation and dissociation”. McLeod then dined at the Chemical Club, seated between Ramsay and his old friend Charles Groves.²⁷ All three then went to hear John Gladstone and others deliver papers at the Chemical Society.²⁸ At the time Ramsay was still professor of chemistry and Principal of University College, Bristol. Over dinner he asked McLeod to support his entry into the Savile Club, something McLeod agreed to do.²⁹ Later that month McLeod was back at the Royal Society for the anniversary meeting and recorded sitting between W. C. Unwin and W. J. Russell at the dinner that followed.³⁰ A few years later, at another anniversary dinner, McLeod sat between Russell’s son-in-law, Alexander Scott, and Ludwig Mond. Scott was one of the chemists, alluded to above. Like Rayleigh, he had an interest in Prout’s hypothesis and was engaged in the preparation of pure elements and atomic weight determinations. It was McLeod’s other dinner companion, Ludwig Mond, who was to finance construction of the Davy-Faraday Laboratory where Scott was to work in the late 1890s.³¹

In 1888 McLeod noted that Lockyer gave a paper at the Royal Society on the “spectrum of the aurora”, and that “Dewar made some spiteful remarks to which Lockyer replied”.³² In the late 1860s McLeod had assisted Lockyer in some of his earlier solar spectroscopic work and he remained loyal. He often visited Lockyer in South Kensington and accurately described the observatory there as “a lot of instruments in rough sheds”.³³ McLeod heard many other papers in 1888, and on a wide range of topics. At a meeting where Rayleigh spoke on the relative densities of oxygen and hydrogen, McLeod also heard Edward Poulton speaking on “true teeth in the young *Ornithorhynchus paradoxus*”. This was of sufficient interest to McLeod that he corresponded with Poulton and others on the subject of “birds teeth”, as he put it.³⁴ He regularly attended papers given by the botanist Harry Marshall Ward, a good friend and a colleague at Cooper’s Hill. They stayed in close touch after Ward moved to a chair at Cambridge in 1895. McLeod helped Ward with some of his experiments; for example, in the setting up of apparatus for the cultivation of plants under reduced pressure and at various temperatures; and in the analysis of the gases given off. He also helped in the construction of apparatus for studying the effect of light on bacteria. McLeod attended several important lectures given by Ward and they often dined together, both in London and at their homes. At a dinner party given by McLeod, at which the chemist Henry Armstrong was also present, Ward entertained the company by singing nursery rhymes set to music of his own composition. In

1894 McLeod dined with Ward at the Savile Club before Ward gave a lecture on his bacteria work at the Royal Institution.³⁵

Before entering the lecture theatre to hear Arthur Schuster give the Bakerian Lecture, McLeod had a long chat over tea with Ramsay and Wyndham Dunstan about the proposed union between the Institute of Chemistry and the Society of Public Analysts. Dunstan was very much against this since he thought it would “degrade” the Institute.³⁶ A meteorological paper on how to figure out the height and motion of clouds from photographs, given at the Royal Society by Richard Strachey and G. M. Whipple, clearly fascinated McLeod.³⁷ He also heard many medical papers at the Royal Society. Two Croonian lectures which he much admired were given by visitors: one, delivered in French by Émile Roux, on Pasteur’s work, “Les inoculations préventives”, and another by Rudolf Virchow on the current state of pathology.³⁸ In 1900 A. W. Tilden gave the Bakerian lecture on the relation of atomic weight to specific heat, but his expected audience was largely absent having had a difficult time making it to the Royal Society. There were large crowds in the street celebrating the turn of events in the South African War; many people were waiting to see the Queen. After the lecture McLeod was Tilden’s guest at dinner with the Philosophical Club of the Royal Society.³⁹

McLeod knew about Rayleigh and Ramsay’s work on argon before it was announced at the British Association meeting at Oxford in 1894, and he read their paper in advance of its presentation to the Royal Society in 1895. He noted that there was “much anticipation”, and that a “great crowd” showed up to hear what would be claimed. During the discussion several people questioned the evidence but Rayleigh stated that it “was quite satisfactory”.⁴⁰ Three months later Ramsay gave an account of the discovery of helium. As earlier he relied on William Crookes for the spectroscopic data.⁴¹ Despite his many successes Ramsay did not have the unmitigated approval of his fellow chemists. McLeod noted a heated meeting of the research fund committee of the Chemical Society at which the members decided to award Ramsay the Longstaff Medal in 1897, but only after “a long struggle” with the supporters of William Perkin Jr. After this tense meeting Vernon Harcourt took McLeod to dine at the Royal Society Club.⁴²

McLeod engaged only marginally in Royal Society politics. Among chemists it would appear that Henry Armstrong was a major political force.⁴³ Armstrong would organize his fellow chemists to discuss strategy on a range of matters including who should be supported in Royal Society elections. For example, in 1889 he organized a lunch meeting to discuss the fellowship candidates before those present went to hear Arthur Rücker deliver the Bakerian Lecture.⁴⁴ Later there was disappointment when Horace Brown was the only chemist elected that year.⁴⁵ McLeod worked on behalf of some of his friends in their attempt to become fellows. He was especially pleased when G. S. Clarke was elected in 1896 since he had lobbied on Clarke’s behalf for a number of years. He lobbied hard also on behalf of his friends Oliver Lodge and William Cawthorne Unwin. Both had to wait a few years before being elected fellows, Unwin in 1886 and Lodge in 1887. The case of Lodge is a little

puzzling in that McLeod and George Carey Foster were advised by the Secretary, Michael Foster, not to put Lodge's name forward earlier. However, when his name was put forward in 1887 he was immediately elected.⁴⁶ McLeod was also at the Royal Society for meetings of the library and catalogue committees, and for social events such as soirées and conversaciones. Lord Rayleigh sometimes held dinner parties before the soirées. For example, in 1891 McLeod was invited to dinner at 4 Carlton Gardens along with R. B. Clifton, O. J. Lodge, J. J. Thomson, W. H. M. Christie and A. Schuster. And, before a soirée in 1896, the other guests were Rayleigh's son R. J. Strutt, H. E. Armstrong, A. Schuster, and W. G. Adams.⁴⁷ In addition to social exchange the soirées were occasions for showing off the new. On one occasion those attending were treated to a performance from the Paris Opera transmitted by telephone, demonstrations by Crookes of some nitrogen flames, some of Nikola Tesla's experiments, and a show of Lockyer's celestial photographs.⁴⁸

In 1874 McLeod had helped to found the Physical Society and he regularly attended the fortnightly Saturday meetings. Typically he would arrive in South Kensington in the morning, look around some of the laboratories, and then have either lunch or "beer and buns" at the South Kensington Museum (later Victoria and Albert Museum) before attending the meeting.⁴⁹ Council meetings were sometimes held before the ordinary meetings, and discussion of the papers would continue into the evening and over dinner. For three months in 1885 McLeod combined all of this with visits to the Inventions Exhibition being held nearby. For example, on one occasion he began his Saturday with a visit to Frederick Guthrie's laboratory where he chatted with Guthrie and Shelford Bidwell.⁵⁰ He also chatted with one of his former laboratory assistants, J. W. Clark, then working with Oliver Lodge, who was setting up a demonstration for a paper he was delivering that day on the electrolysis of mercuric iodide at high temperatures. After observing what was going on, McLeod had lunch at the museum with Guthrie, Bidwell, Clark, Unwin, Reinold, G. F. Rodwell, and W. Abney.⁵¹ After lunch Unwin showed him some things in his laboratory and in the evening the two had dinner together before going on to the exhibition. There they looked at some American displays: watches made by the Waltham company, and an electrical breaker made by Westinghouse.⁵² Two weeks later McLeod was back at the society and, after listening to four papers, he returned to the exhibition with some of his colleagues to look around and listen to the Strauss Band performing under electrical illuminations. McLeod then walked across Kensington Gardens to have dinner with J. H. Gladstone and his family. On 28 November 1885 McLeod was nominated for the vice-presidency of the Physical Society at the council meeting (he was later elected) and then attended papers at the ordinary meeting by the instrument maker Adam Hilger on a new spectroscope, and by William Ayrton on a new method for calibrating galvanometers.⁵³ C. V. Boys also showed his new calculating machine. These practically oriented sessions were the backbone of the Physical Society and McLeod enjoyed them. Later that evening he went to stay with Rayleigh for a scientific weekend at Rayleigh's country home, Terling Place, in Essex.

Occasionally the Physical Society held meetings away from South Kensington.

McLeod noted one that was held at Eton College when T. C. Porter, the science master, gave a paper on geysers.⁵⁴ Another was held in Bristol and several people including Ayrton and his new wife Hertha travelled down to hear papers given at Clifton College, and look around the laboratories at the Merchant Venturer's College, and at University College where Ramsay showed them his apparatus for measuring vapour pressure and H. S. Hele-Shaw showed some of his automatic recording instruments.⁵⁵ At Clifton College, where his friend William Shenstone taught chemistry, McLeod was as interested in the chapel improvements as he was in the laboratories. One of the papers read was by G. F. Fitzgerald critical of S. P. Thompson's way of illustrating the electromagnetic nature of light.⁵⁶

McLeod mentions attending at least one technical or scientific exhibition a year and was on several exhibition juries. The Inventions Exhibition of 1885, held in South Kensington, was perhaps the most important of those held during the period covered by this paper. It was an international exhibition that attracted many scientists, engineers, musical instrument makers and inventors. It also presented an opportunity for visitors to address learned societies in the capital. McLeod was on two juries, one for chemical inventions and the other for slide rules and calculating machines. On 28 March he took some of his sunshine recorders to South Kensington for display at the exhibition, then had lunch at the museum with Guthrie, G. Forbes,⁵⁷ Ayrton, Bidwell and Reinold. After lunch they listened to Joseph Edmondson give a talk at the Physical Society on the history of calculating machines. Several of Edmondson's machines were on display at the exhibition.⁵⁸ At the same meeting General Babbage spoke further on calculators, and G. F. Fitzgerald gave a talk on the model that he was exhibiting to show the properties of the ether.⁵⁹ After the meeting McLeod had a long chat with Rayleigh about his apparatus for preparing pure oxygen, and then was a guest, along with A. G. Greenhill and W. G. Gregory, at a dinner party given by Unwin.⁶⁰ On one visit to the exhibition McLeod was shown an electric tramway and some large dynamos, and "met Fred Smith" who had an "ingenious ergometer" on display.⁶¹ McLeod had to attend weekly meetings of both juries. The chemical jury chaired by W. Odling included also W. Perkin, H. Roscoe and W. Weldon.⁶² Before and after jury meetings, and over a ten week period, the jurists would look carefully at the relevant exhibits and then debate their merits. According to McLeod, the chemists were especially taken with exhibits in the German court which included a "fine collection of chemicals", and with some of the chemical engineering processes on display in the machinery gallery.⁶³

Many of McLeod's closest friends were chemists whom he met regularly at Chemical Society, Society of Chemical Industry and Institute of Chemistry meetings. Among them were old friends from his student days, Alec Gillman, Charles Groves and David Howard.⁶⁴ The scientific bodies held regular dinners and other social events. For example, after attending a meeting of the Institute council, McLeod went to a dinner held at the Café Royale at which Sir Charles Cameron, Dublin's Medical Officer of Health, gave the main after-dinner speech. McLeod recorded sitting with Odling, Groves, F. Japp, and P. F. Frankland.⁶⁵ In 1887 McLeod and William Crookes

organized some decorations at the Chemical Society for the Queen's jubilee, and on 21 June many of the fellows and their wives sat on the roof of Burlington House watching the jubilee procession on Piccadilly below.⁶⁶ In 1889 McLeod dined at the Savile Club with Crookes, Armstrong, Japp, Ramsay, T. Stevenson⁶⁷ and P. F. Frankland before attending the anniversary meeting of the Chemical Society when Crookes spoke about his spectroscopic work on the rare-earth metals. Two years later the jubilee dinner, open to all members, was a large affair. Held at the Hotel Metropole, some 230 people were present and twelve speeches were made; "good" ones by Lyon Playfair and Lord Salisbury according to McLeod.⁶⁸ The Chemical Society organized a major memorial for A. W. Hofmann in 1893 at which Lord Rayleigh gave a "capital speech" on the condition of chemistry before Hofmann's arrival in England, Sir Frederick Abel spoke on the formation of the Royal College of Chemistry, and William Perkin on the coal-tar dye discoveries.⁶⁹ On another occasion, in 1896, P. P. Bedson, professor of chemistry at Durham, gave the Lothar Meyer memorial lecture, after which William Roberts-Austen held a reception at the Mint at which Roentgen rays "were shown on a screen" (shadows of metal objects).⁷⁰ Roentgen (X-) rays were new in 1896 and there was much excitement when W. C. Roentgen came to address the British Association meeting that year. In the same period the work of Dewar and Ramsay was keenly followed; attendance at meetings at which they spoke was especially high. For example, McLeod recorded that a large crowd came to hear Dewar talk about some of his liquid air experiments at the Chemical Society in 1895.⁷¹

The British Association meetings were excellent occasions for the exchange of information in a relaxed atmosphere. McLeod attended regularly and would often combine the meetings with walking holidays with friends. For example, before the Aberdeen meeting in 1885 McLeod and Unwin visited some of McLeod's family members in the border country, went to look at the progress being made on the Forth Bridge in Edinburgh, and then for a short walking holiday in the Highlands. Others must have done the same since McLeod records meeting several friends on their walks; for example John Gladstone and family when they stopped to watch the Atholl Gathering, and Harcourt and H. B. Dixon at Braemar. Harcourt told McLeod that they had been carrying out some experiments at the Ben Nevis observatory and asked him to lobby Lord Salisbury for improvements to the telegraphic service to and from the observatory, which he did.⁷² On arriving in Aberdeen McLeod had a long informal chat with Lord Rayleigh about some of his (Rayleigh's) experiments, and then went to visit Gladstone and his family where they were staying. Gladstone showed him around the cathedral. McLeod listened to many papers and gave two of his own, one being an assessment of the Brin Frères method of making oxygen and nitrogen; it gave rise to "lively discussion". He was shown around the astronomical observatory and admired many of the instruments. Of especial interest to him were a new electrical mechanism for moving the telescope in the equatorial, and a chronograph with four drums that was regulated electrically once a second by a sidereal clock. Before leaving Aberdeen McLeod attended the annual Red Lion

dinner, a convivial gathering at which Dixon made fun of some of the lectures in his after-dinner speech.⁷³

In 1886 McLeod took another Highland walking holiday with friends before attending the British Association meeting in Birmingham. There he stayed with the Albright family.⁷⁴ According to McLeod, at the first Section B meeting there was as much talk about the South Carolina earthquake as about the chemistry papers.⁷⁵ That year Crookes drew much attention with his spectroscopic work on the lanthanide elements. Rutherford gave an evening address “on the sense of hearing”. McLeod noted his “deep voice” and that he spoke for far too long on elementary matters. Birmingham manufacturers held a *soirée* with many interesting displays, and “a great crowd” was present. McLeod enjoyed visits to local industries and to other places of interest such as Chatsworth where he was taken by the Albrights.⁷⁶ In the following year the meeting was in Manchester where Henry Roscoe presided overall and Edward Schunck was president of Section B. McLeod attended a garden party hosted by Schunck and noted the splendour of both home and entertainment. Schunck had a well equipped private laboratory, “very luxurious with library and billiard room above”.⁷⁷ At the Newcastle meeting in 1889 McLeod crossed the high bridge to Gateshead, “a very dirty place”, to visit the Abbot works and, by way of contrast, attended a dinner later that day hosted by Lord Armstrong at Jesmond Dene with “splendid entertainment”.⁷⁸ At Leeds in 1890 McLeod heard T. E. Thorpe give a spirited presidential address to Section B, defending Priestley as the discoverer of oxygen, in answer to P. E. M. Berthelot who had been championing Lavoisier.⁷⁹ Also interesting was a trip to a bottle factory at Castlefors which had an “ingenious” machine for making beer and pickle bottles.⁸⁰ At Edinburgh in 1892 Archibald Geikie presided overall and McLeod was president of Section B. The McLeods and the Crookes’s were guests of the Kempe family. The visiting chemists were well entertained including by Alexander Crum Brown, the professor of chemistry at Edinburgh, who hosted a big dinner party. McLeod took Mrs Kipping, one of three sisters who were married to chemists, in to dinner and, after the women had left the table, discussed catalysis, on which he was then working, with Wilhelm Ostwald.⁸¹ At Dover in 1899 T. E. Thorpe moored his yacht in the harbour and invited McLeod and others on board. McLeod listened, as usual, to the chemists in Section B, heard several other papers including Francis Galton on fingerprints, watched a failed attempt by balloonists to cross the channel, and witnessed Marconi’s more successful cross-channel communication with Boulogne by wireless telegraphy.⁸²

Aside from the announcement of a new gaseous element at Oxford in 1894, and hearing Roentgen and others talk about the new rays at Liverpool in 1896, perhaps the most exciting of the BA meetings in the 1890s was the one held in Toronto in 1897. This brought people together for a longer period since many of the British and other European delegates shared also the Atlantic crossing. McLeod had to think twice about whether he could afford the trip which cost about £35. Others could afford to bring family members but McLeod could not.⁸³ He travelled to Liverpool with Ramsay and Mrs Ramsay and spent much time with the Ramsays during his month

away. It could be said that Ramsay, who gave several papers on his work with the new gases, was the star of the meeting; though Becquerel and radioactivity, too, drew much attention. Once on board ship McLeod met Silvanus Thompson who introduced him to Professor Brauner from Prague, another person with whom he spent much time while in Canada.⁸⁴ On board McLeod noted a general fear of icebergs; many were seen and the scientists tried to figure out their size. They also watched spouting whales and observed the *aurora borealis* through spectrosopes. McLeod noted that most of his fellow passengers were emigrants, that a Canadian bishop conducted religious services, and that Canadian scientists gave talks to the delegates on what they should see while in Canada. On arriving in Montreal, McLeod had dinner with Thompson, Meldola and Ewing.⁸⁵ The following day there was a reception at McGill University and tours of the laboratories. The chemical laboratories were still under construction but McLeod and Unwin were both impressed with the facilities for engineering, far better than those they were to see in Toronto and better than most in Britain. McLeod and Lord Lister were taken on a tour of Mount Royal by the professor of mathematics at McGill, and in the evening they had dinner with Sir John and Lady Evans.⁸⁶ McLeod and the Ramsays then took the night train to Toronto where they were guests in the same house. McLeod was impressed with the speed of the tram cars in Toronto and especially enjoyed the outing to Niagara (by steamboat as far as Niagara-on-the-Lake). He noted that it was mostly the chemists and engineers who went on the trip. His close companions on that occasion were Unwin, Perry and Meldola.⁸⁷ During the 1880s Charles Brush began using his arc lights to illuminate the falls at night, something witnessed by the visitors. Water turbines powered his electricity generator and were used also for non-electric power from the falls. This was transmitted by belts and was used to run a number of small factories, some of which McLeod and others were shown around. Unwin was a major contributor to the design of the hydroelectric power station at Niagara which opened in the early twentieth century.⁸⁸ After a brief trip to Ottawa where McLeod viewed a pulp mill and the parliament buildings, he and many others returned home.

WORK AS AN EXTERNAL EXAMINER

McLeod married relatively late in life, in 1888. Five children soon followed and he sought ways to supplement his income. For several years he was an external examiner in chemistry and worked consecutively at four universities. His first appointment was at the University of Oxford in 1889–90. These two years were, perhaps, his most enjoyable as an examiner. He was a little tentative about taking on the position and consulted Armstrong before doing so; but having made the decision he took the work seriously. It entailed being in Oxford for much time during the months of June and November. In addition to expenses, he was paid between £25 and £30 for each visit, depending on the number of students being examined. He shared responsibility for both the preliminary and final examinations and began by studying earlier examination papers set by other chemists. At first he worked closely with V. H. Veley who came to visit at Cooper's Hill where they composed the exams for 1889. For much

of June McLeod stayed in Oxford with Veley. While there he read the examination papers which he thought “not good”, gave oral (*viva voce*) exams which helped improve several of the students’ positions, and supervised some of the practical exams held in the museum.⁸⁹ But there was time left over and McLeod was able to lead a sociable life.

McLeod had made several visits to Oxford before going there as an examiner — principally to stay with his friends Edward and Lavinia Talbot. Edward Talbot, pious and a brilliant scholar, was appointed the first Warden of Keble College in 1870.⁹⁰ During those visits McLeod met some Oxford scientists and visited their laboratories. For example, in 1885 he, the Talbots and Bob Cecil, examined the new electrical lighting system at the Oxford Union (gas engines, dynamo machines and Pilsen lamps), looked around the new physiology laboratory, and over dinner had a discussion about vivisection which McLeod defended “when properly carried out”. This was a hot topic since the physiology laboratory and its director, John Scott Burdon-Sanderson (1828–1905), Waynflete professor of physiology, were under serious attack by anti-vivisectionists. McLeod had known Burdon-Sanderson for many years and had earlier carried out some research with him on gases dissolved in blood.⁹¹ As an examiner McLeod was invited to dine at various colleges, and with both the Scientific and Ashmolean Clubs. He visited people in their homes, noting some interesting details; for example, the disarray at Odling’s when electricity was being installed.⁹² In 1890 McLeod stayed with Walter Fisher on his June visit, and with the Odlings in November.⁹³ Odling held a number of dinner parties and McLeod records chatting with Burdon-Sanderson who was a guest on a couple of occasions.

McLeod recorded a few details of what was being done in the various laboratories. He noted that R. B. Clifton was very proud of the new dynamo room at the Clarendon, and that various pieces of electrical apparatus were being constructed at the Millard Laboratory where he spent time with Smith. He noted also the optical and chemical work being carried out at Balliol where he visited Conroy and Nagel.⁹⁴ McLeod’s technical skills were much in demand at Oxford, especially by Harcourt who needed his help in the design and repair of various pieces of glass apparatus.⁹⁵ McLeod gave a lecture on catalysis at the museum, and included some demonstrations using spongy platinum; he had a good audience.⁹⁶ He attended parties at Somerville and Lady Margaret Hall and much admired the new principal of Somerville, Agnes Maitland. Maitland was a specialist in domestic science and a great modernizer. She introduced electrical lighting at the college and tried to have women admitted for Oxford degrees. Unlike at Cambridge, women science students worked alongside men in the laboratories of the men’s colleges. The women chemists mentioned in the diary appear to have been students of Harcourt and were examined separately from the men.⁹⁷

In 1891 McLeod took on some examining for the Board of Education in South Kensington. There he worked on large numbers of papers together with T. E. Thorpe and A. W. Tilden. In the following year he succeeded Tilden as an examiner for the University of London where he earned approximately £225 a year. H. E. Armstrong,

the chief chemistry examiner, was succeeded by Wyndham Dunstan in 1894.⁹⁸ The work was far more arduous than at Oxford since there were many more students and a wide range of examinations but, being well paid, it was highly sought. The examiners and their assistants were responsible not only for the matriculation, undergraduate and DSc examinations, but for the chemistry exams taken by medical students and some others.⁹⁹ The composition of exams was a group activity and took much time; about four or five people were involved with each. McLeod's close friend Charles Groves worked with him on the practical exams. Overnight marking sessions were often held at Armstrong's home in Lewisham. On one occasion McLeod noted marking exams on Armstrong's balcony while watching a fireworks show taking place at the nearby Crystal Palace.¹⁰⁰

In 1892 Ramsay complained in a letter to McLeod that some of his students had not done as well in the BSc examinations as he expected. This was just the first of many such complaints. For example in 1894 Ramsay, annoyed at not having been elected an examiner that year, publicly criticized the University of London chemistry examination questions at the Chemical Society. Some of his complaints were made also in a letter to *Chemical news* in which he noted that the "examiners are not at present subject to any court of censure save that of the candidates", that there was too much ambiguity in the questions, and that outdated chemical names such as "red chromate of potassium" were being thoughtlessly perpetuated. McLeod wrote to Ramsay about his behaviour and received a reply in which "he half apologizes". In 1896 Dunstan was very angry when Ramsay complained yet again. Once more McLeod had to calm things down.¹⁰¹ In 1897 McLeod helped Ramsay to join the examining board but recorded that Ramsay held himself a little aloof from the other examiners; for example, by refusing to join them for lunch after collective marking sessions.¹⁰² McLeod records receiving many letters asking for help from people who wished to be appointed assistant examiners. F. S. Kipping was lucky to be appointed an assistant in 1894 when he faced stiff competition.¹⁰³ In the same year McLeod was given an additional appointment as examiner at the Pharmaceutical Society. But he found the joint work too burdensome and resigned from the pharmacy examinership in 1898.¹⁰⁴ His term as examiner at London University came to an end in 1900 and one year later he was offered work at Cambridge which he turned down at the time but accepted in 1903. In 1901 he accepted an examinership at Birmingham. He enjoyed his time there, staying with Percy Frankland in what he described as a beautiful home. He also wrote that Frankland's exams were "very stiff" compared to those at other universities, and that "many of the questions are beyond me".¹⁰⁵ While in Birmingham he spent some time in Oliver Lodge's laboratory and had frequent chats with J. H. Poynting and J. J. Thomson.¹⁰⁶ In addition to examining students, McLeod refereed many papers in this period and reviewed books, mainly for *Nature*. He also wrote many reference letters for friends and former students seeking work.

THE THAMES VALLEY AND BEYOND: SCIENCE AND SOCIAL LIFE

McLeod lived close to his laboratory in a house belonging to the Royal Indian Engineering College. He had many connections in the neighbourhood.¹⁰⁷ One major addition to the area was Royal Holloway College, opened by Queen Victoria on 30 June 1886. Matilda Bishop was the first principal.¹⁰⁸ The professors at Cooper's Hill and their wives helped the new college in a number of ways. Mrs McLeod helped Miss Bishop set up some continuing education classes at the college and McLeod helped to design the chemistry laboratory, together with Vernon Harcourt and the first chemistry lecturer, Margaret Seward. Later he was to help Seward's successor, Eleanor Field, with some of her work.¹⁰⁹ The new college added greatly to social life in the area. Each summer there was a garden party and throughout the year there were many cultural events such as concerts, special lectures, and soirées in the picture gallery. Many in the neighbourhood were present when a statue of the Queen was unveiled and the new electrical lighting system was put on display.¹¹⁰ McLeod knew much about electrical lighting and, together with his students, designed and installed electrical lighting for a number of special events, including for a naval exhibition held at the Royal Hospital in Chelsea in 1891.¹¹¹

McLeod had a collegial relationship with other professors at the Royal Indian Engineering College. For example, he worked well not only with Ward but also with W. P. D. Schlich, the professor of forestry with whom he spent a walking holiday in the Ardennes in 1893. He was friendly with R. Warrington and A. H. Church, both of whom came on a part-time basis to teach the forestry students some organic chemistry. He appears to have been of great help to Alfred Lodge who used calculating machines in his work and relied on McLeod's assistance when they broke down.¹¹² McLeod was interested also in G. Minchin's selenium photo-electrical cells and, in 1891, noted that Minchin had "a very sensitive one". Three years later Minchin telegraphed from an observatory in Ireland that he had "results with his cells with Sirius, Jupiter and Venus". Minchin used McLeod's calculating machine for his work and sought his help when occasionally using vacuum tubes.¹¹³ P. M. Duncan, the visiting professor of geology, was a close friend until his death in 1891.¹¹⁴ McLeod regularly distilled mercury for Unwin in London and also helped the non-academic staff. For example, he tested wallpapers for arsenic, and after testing some hair dye for the cook suggested she not use it.¹¹⁵ In the late 1880s he developed an interest in beekeeping and, with help from the outdoor staff, kept the college well supplied with honey.

McLeod continued to help his friend Lord Sackville Cecil with technical work on the District Railway until Sackville Cecil's death in 1898. His advice was sought on a range of problems from managing the gas works at Mansion House and Lillie Bridge, to problems with train signalling, sparking at contacts, and with a new telephone exchange.¹¹⁶ The two men visited each other in their homes and had a common interest also in electrical clocks. When Sackville Cecil died he left McLeod his scientific instruments.¹¹⁷ Other friends, too, were helped in various ways and many came to stay for social and scientific weekends.¹¹⁸

McLeod had no systematic line of research. His laboratory at Cooper's Hill was

used for all kinds of technical tinkering in addition to various small research projects and the instruction of students. He spent much time working with meteorological instruments, calculating machines, and electrical clocks. He and his students ran a small meteorological observatory at the college and collaborated with people at Kew and at the Meteorological Office.¹¹⁹ In that connection McLeod worked with George Stokes on a solar radiation report for the British Association, helped also by Charles Chree. He also paid attention to water treatment procedures since he was responsible for the safe delivery of water at the college.¹²⁰ Further, he helped to set up an analytical laboratory at the college which was used by the India Office for the routine analysis of materials destined for, and sent from, India.¹²¹

McLeod also acted as something of an unofficial long-distance research associate to a number of other scientists, some of them prominent. He was able to help by conducting small research projects that were related to more systematic research programmes elsewhere. This led to some interesting scientific exchanges. For example when J. J. Thomson was working on the production of ozone, McLeod was doing much the same, but trying to produce it electrolytically rather than in a discharge tube. McLeod showed some of his ozone experiments at the Chemical Society in 1886 and corresponded with Thomson on this work.¹²² When Thomson began work on the dissociation of iodine by electric sparks, he met McLeod a couple of times at the Savile Club to discuss his research and seek help. At this stage McLeod probably knew more about the practical end of things than did Thomson who had many problems, especially with respect to his glass apparatus. Discharge experiments were new to Cambridge, and somewhat frowned upon by the more dominant theoreticians. But Thomson, a top wrangler and serious theoretician in his own right, had their respect. He built up a good practical school at the Cavendish, and after X-rays and radioactivity appeared on the scene was in an excellent position to bring his experimental and theoretical knowledge together. However, when Thomson, at the start of his experimental work at Cambridge, gave the Bakerian Lecture on the dissociation of the halogens McLeod was not entirely approving. He recorded pulling the lecture apart with Perkin and Armstrong. Later, after having been asked to referee the paper for publication, he noted that he liked the written version much better.¹²³

Of the better known scientists it was Rayleigh who received the most help from McLeod in this period. The two had known each other since their early thirties, and over the years had many discussions on a wide range of topics including tuning forks, colour vision, electrical cells, and spectroscopy.¹²⁴ In the 1880s and early '90s it was mainly Rayleigh's work in preparing and weighing pure oxygen and hydrogen, and then nitrogen, that occupied them. Discussion of the associated problems took place in London, Cooper's Hill, at British Association meetings and at Terling Place where Rayleigh's laboratory was situated. On a visit to Terling in November 1885 McLeod was the sole guest.¹²⁵ He spent the weekend helping Rayleigh with his gas preparation, and looked also at the balance that Rayleigh had recently constructed for the weighings. It was situated in a separate underground laboratory so as to be free from disturbance. The globes of gas were suspended in a closed space but their

weights fluctuated in a way that Rayleigh did not understand. McLeod was soon advising Rayleigh on his glass apparatus in very practical ways, including the suggestion that Sprengel stopcocks made by Becker, an instrument maker on St Martin's Lane, be used. I suspect that it was Rayleigh who encouraged McLeod to prepare pure ozone since no sooner had he left Terling on that occasion than he began reading about the preparation of the gas and, as noted above, learned how to prepare it by electrolysis.¹²⁶

Sometimes McLeod was invited to Terling together with other scientists. For example, in May 1886 he travelled there with Lockyer. Already present were Sir William and Lady Thomson, and Lord Rosse arrived later. By then Rayleigh's balance was much improved. The scientists were shown all kinds of things and appear to have spent time discussing the finer details of various pieces of apparatus such as mirrors, gratings and electrical equipment. At Terling time was also spent in the chapel at morning prayers and at other services. After Sunday matins Rayleigh took the four men to see the monastic house at Toppinghoe. On their return they had a long talk about Lockyer's work.¹²⁷ In the weeks following this visit McLeod corresponded with William Thomson about various pieces of apparatus that Thomson wished to improve. By the end of 1886 Rayleigh believed that he had finally made pure hydrogen but was having some difficulty weighing it. Later he decided that his hydrogen was not fully dry. He used both P_2O_5 (helped by McLeod), and liquid air in drying the hydrogen and had comparable results using both methods. By 1888 the two men were having many chats about the preparation of pure oxygen and, later, of pure nitrogen.¹²⁸ Rayleigh also visited Cooper's Hill on a couple of occasions learning how to make chlorine and encouraging McLeod to prepare pure H_2O_2 , which he then tried to do. McLeod also attempted to prepare samples of some pure elements, including silver.¹²⁹ Rayleigh will have sought advice also from others; but the kind of activity just described tells us something of the collective nature of science and of 'invisible' assistance behind major discoveries.¹³⁰

In his diary McLeod mentions a number of scientific expeditions, not just those organized by the Physical Society mentioned above. For example, together with chemists Armstrong, Japp and Groves, he joined the Telegraph Engineers on an outing to Swindon where they were shown around the railway works and watched casting, turning and hydraulic rivetting. Two weeks later the Society of Chemical Industry organized a boat tour of industrial sites along the Thames. The principal stop was at the Beckton gas works where they disembarked at one of the huge piers for landing coal. They looked around the "enormous" retort houses, at the condensers, scrubbers, purifiers, and around the chemical works where about 18,000 tons of ammonium sulphate were made each year. Anthracene, naphtha for lamps, and other chemicals were also manufactured. At another stop at Crossness they looked around a plant for the manufacture of potassium permanganate used for deodorizing the nearby sewage and pumping station. They also looked at the new docks at Tilbury and McLeod was amazed at the efficiency of a steam navy that could fill a railway truck with clay with just two huge scoops. By contrast the group watched a barge race, "a very pretty

sight, the coloured sails looking very well in the low sunlight".¹³¹

Summer was a time for such expeditions and for numerous garden parties. Lady Hooker hosted an annual party at Kew, there was the annual 'visitation' at the Royal Observatory at Greenwich, and many parties were given by private individuals. McLeod rarely missed going to Greenwich and enjoyed looking at the astronomical instruments and chatting with people while there. Ludwig Mond held an annual garden party at his home in St John's Wood with extravagant entertainment (army bands, circus entertainers, classical musicians). Other regular party givers included W. E. Ayrton, W. Preece, H. Roscoe, J. Lubbock, A. H. Church, W. H. Perkin and J. N. Lockyer. Some, such as Ayrton, usually offered musical entertainment and others, such as Lockyer, scientific entertainment.

Holidays were often spent with other scientists and not always in connection with British Association meetings. Hiking in Wales and Scotland was popular. For example, in 1885 McLeod joined Unwin and Greenhill on a combined walking and engineering holiday in Snowdonia. Much time was spent looking over the construction of the Vyrnwy reservoir and going inside the pipes that were to carry the water down. Sometimes a day visit would be made to a scientist living nearby where he was staying. For example in 1898 the McLeod family took a holiday on the Isle of Wight and McLeod spent a day with geologist and mining engineer, John Milne. Earlier, when working in Japan, Milne together with J. A. Ewing developed the first good seismograph for the detection of earthquakes. Milne, known as 'earthquake Milne', built an observatory at his home near Newport which became a major centre for earthquake science. McLeod looked at the seismographs, at other instruments, and at the seismic record for the previous week. He noted that a small 'quake had occurred on 15 April. He also enjoyed being shown some "Japanese curiosities" by the Milnes.¹³²

CONCLUSION

The above narratives were constructed by piecing together stories from McLeod's diary, not always chronologically. Many more stories could have been told but those chosen, when combined with material from some other sources, show something of the everyday lives of some well established scientists working in or near London in the late nineteenth century. As was noted in the earlier paper, the world of science has many circulatory currents. For McLeod and his circle, manipulation, apparatus, technical devices — indeed all things material — were of especial interest. People with manipulative ability embodied the new as much as those in possession of mathematical and philosophical skills. However, in the borderlands of physics and chemistry those who possessed both theoretical and practical skills, people such as Rayleigh and William Thomson, were able to achieve much. J. J. Thomson, too, worked in the borderland and, while not very adept in the laboratory, arrived at some important ideas by paying close attention to experimental work.

While this paper has not addressed scientific fashion, it is clear that many people were working in the same fields as Rayleigh and Ramsay, though with relatively

less success.¹³³ McLeod is interesting in part because of his proximity to leading figures such as Rayleigh and Ramsay. Comments in his diary make clear not only their considerable ability but also some of their dependencies. The diary also throws light on some of the ways in which their ideas were discussed and then began to circulate. But McLeod's connections were not simply to the scientific élite. His diary entries provide evidence of a lively scientific culture that lasted until the First World War. Before the huge expansion of science, technology and industry in the twentieth century, it was expected that scientists keep up with much of what was new, that their interests be broad, and that they take an interest also in innovations in industry and engineering.

Several of McLeod's acquaintances were scientific entrepreneurs, something he resisted becoming even though he was invited to join a number of different ventures. Nonetheless he was probably unusual in the degree to which he engaged with the overall scientific culture and used it to his advantage. He roamed the world of technology and was a major consumer of ideas and practices. This behaviour contributed to his circle of acquaintances being as large as it was, and it allowed his skills to be widely recognized. His social skills matched his manipulative ones and led him in many different directions — too many for any serious scientific legacy. But he helped others in a range of fields: for example, the botanist H. M. Ward, the physiologist J. D. Burdon-Sanderson, the mathematician A. Lodge, and the engineers W. C. Unwin and Sackville Cecil, as well as numerous chemists and physicists — notably in this period, Lord Rayleigh. McLeod's scientific knowledge and practical skills were widely dispersed and he had a small hand in many endeavours. His career is a reminder of an aspect of the collective nature of science not always recognized. What has been described was not team work, but collective nonetheless. This is not to deny the superior skills or greater importance of people like Rayleigh, J. J. Thomson or Ramsay, but simply to suggest that historians of science make more explicit a point widely recognized, namely that successful scientists are embedded in complex systems of exchange in ideas and practice. While such scientists are well aware of their place in the overall hierarchy, they are not always conscious of their dependencies.

We have seen how social activity contributed both to the working environment of scientists and to science itself. In the days before the telephone and the internet it was necessary for scientists often to meet face-to-face. Oral exchange was very important in McLeod's period and, for that reason, there was a need for all the different scenes described: the frequent learned society meetings, the British Association meetings, clubs, dinners, scientific holidays, expeditions, visits to each other's laboratories and homes, and so on. There was a need to network in a personal way so as to share ideas of what was important. The fact that McLeod married relatively late in life gave him many years to build connections before family life demanded more of his time. It was only when he was in his mid-fifties, and with five young children, that he began to retire somewhat from the social and scientific life that he had built for himself. He reluctantly turned down many weekend invitations such as some to Terling Place, missed some of the British Association meetings, and attended fewer

scientific and social events than earlier. But he was still able to do much and, because of connections made earlier, was not sidelined from discussions that interested him. Perhaps part of his success can be attributed to an extended youth — a reminder of the importance of expanding one's circle when young.

In McLeod's period, narrowly defined disciplinary work was mostly still in the future. Outsiders could still enter debates and ask questions. In turn, scientists had to be able to give replies that were intelligible to general audiences. This cultural climate allowed someone like McLeod to lead the kind of scientific life he did and to be seen as successful. Further, on the basis of a longer reading of his diary, I believe his religious outlook can in part explain why he did not appear to care where he landed up intellectually. As will be discussed briefly in the coda this outlook can partially explain his lack of focus and more conventional ambition. By retaining an interest in all sorts of things his outlook remained forever young. He was not overly bothered by the weight of the careerist world. In his period it was still possible to behave in that way and to have access to some of science's more privileged spaces.

McLeod's diary shows something of the hidden resources that scientists draw upon. But it also supports the idea that social life is built around what people believe they need to know. That is why the social lives of scientists reveal so much about their intellectual and working lives. At one important level scientific knowledge is distributed through publication and lectures. But before any such public sharing can take place there has to be much private sharing. This entails discussion of ideas and practices, learning from others how to manipulate apparatus or chemicals, and some reliance on others for laboratory or other help. In the late nineteenth century much scientific exchange, both private and public, was organized in a manner that was socially enjoyable. The varied social technologies that have been described enabled the advance of science in ways both large and small.

CODA: SCIENCE AND RELIGION

McLeod was a seriously religious man who spent three to four hours a week — sometimes more — in religious observances. His religious views, central to his more general behaviour, have been discussed elsewhere.¹³⁴ This paper has not been given a religious context since that would have made it too long. However, McLeod's religious life should be recognized. For that reason I will end with a few abstract comments on science and religion in the hope they will throw some light on a life such as his and on the way he used his time in science. Using the terms in a very general way, individuals can be said to be either 'orthodox' or 'gnostic' in both their religious and scientific behaviours. The early gnostics, unsatisfied with earthly existence, believed that they held the key to the true God and to a more perfect world. More generally, one can label as 'gnostic' the inability to accept much uncertainty in life and the associated search for existential comfort in esoteric knowledge.¹³⁵ In religion (not just Christianity) gnosticism can lead to the adoption of a variety of firmly held fundamental beliefs. In science it can lead to the cult of the expert, to various ideas of heroic science, to modernist utopian visions — and, occasionally, to

important scientific breakthroughs. Further, the fear of science is somehow tied to the erroneous belief that all scientists are gnostics. Given my own exposure to scientists, and to the history of science, I would claim that most scientists are orthodox. This is the case whether or not they are religious. By orthodox, I mean they are able to set aside existential questions, are humble in the face of the unknown, and are largely accepting of traditional scientific authority. McLeod was orthodox in this sense.

An earlier paper on McLeod is included in an edited volume entitled *Religion and the challenges of science* (2007). The illustration on the book's dust jacket includes an image of the well known 1857 painting by Cristiano Banti that shows Galileo appearing before the Inquisition in 1633. Galileo is clearly an iconic figure, portrayed as a hero of science standing *against* an entrenched orthodoxy and *for* reason and a brighter future. But Galileo is also symbolic of what is, perhaps, a universal fear; namely that science, in its desire and ability to change the world, is dangerous. This danger was something recognized in Galileo's time though the inquisitioners can have had no idea of the iconic status Galileo was to acquire.¹³⁶ One can only speculate what McLeod would have thought of being written about in a book with such a title, and with such a cover illustration. While a 'high-church' Anglican, a lover of ritual in religious services and a ritualistic diary keeper, his science was deritualized. By this I mean that he did not feel any need for supernatural explanation, was not especially interested in the mathematics and metaphysics used to express scientific theory and saw no need to think of them in theological or ontological terms. He had a basic trust in what he saw as God's creation and believed that the job of the scientist was simply to find out how it works. God wanted human beings to be free and that society can, and should, be changed by new scientific discoveries. This view validated his practical approach. This is not to say that McLeod believed that science can lead only to progress, on the contrary. He believed that freedom means also the ability to do wrong and he accepted uncertainty. He would probably have sympathized with both sides in Banti's picture and seen the conflict as wrongheaded. In this he would have been only partially right since the picture, regardless of Galileo's own beliefs, symbolizes well the ongoing struggle between gnostics of secular and religious persuasions.

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1. Imperial College London Archives, Herbert McLeod Diary (1860–1923). The first eleven years of the diary have been transcribed; see Frank A. J. L. James (ed.), *Chemistry and theology in mid-Victorian London: The diary of Herbert McLeod, 1860–70* (London and New York: Mansell microfiche, 1987).
2. See Hannah Gay, "'Pillars of the College': Assistants at the Royal College of Chemistry 1846–71",

Ambix, xlvii (2000), 135–69; “Science and opportunity in London, 1871–85: The diary of Herbert McLeod”, *History of science*, xli (2003), 427–58; “‘The Declaration of Students of the Natural and Physical Sciences’, revisited: Youth, science, and religion, in mid-Victorian Britain”, in William Sweet and Richard Feist (eds), *Religion and the challenges of science* (Aldershot, 2007), 19–37; and “Technical assistance in the world of London science, 1850–1900”, *Notes and records of the Royal Society*, lxii (2008), 51–75.

3. The college was situated at Cooper’s Hill near Egham and was within easy reach of London by train. The site is now occupied by a campus of Brunel University. McLeod was first appointed in 1871 as professor of experimental science, but when W. N. Stocker was appointed professor of physics in 1883 McLeod became professor of chemistry. In 1901 McLeod and several other of the academic staff lost their jobs when the college cut back its offerings. The college closed permanently in 1906. McLeod was appointed Director of the Royal Society Catalogue of Scientific Papers in 1901.
4. Trades shops were a major site for scientific exchange. This is discussed in Gay, *op. cit.* (ref. 2, 2008). McLeod’s connections to people in the scientific trades will not be discussed further here.
5. Gay, *op. cit.* (ref. 2, 2003).
6. James A. Secord, “Knowledge in transit”, *Isis*, xcvi (2004), 654–72; quotation below, p. 660.
7. Iwan Morus, “Replacing Victoria’s scientific culture”, *Interdisciplinary studies in the long nineteenth century*, ii (2006); www.19.bbk.ac.uk.
8. Morus, *op. cit.* (ref. 7), 8.
9. See, for example, Aileen Fyfe and Bernard Lightman (eds), *Science in the marketplace: Nineteenth-century sites and experiences* (Chicago, 2007). While it is not necessary to share the views held by historical actors, T. H. Huxley’s notions on hierarchy have been too easily dismissed in some of these essays (see, for example, Fyfe and Lightman, introduction, p. 3).
10. See James A. Secord, “How scientific conversation became shop talk”, in Fyfe and Lightman, *op. cit.* (ref. 9), 23–53. Secord describes how much of what once was deemed polite scientific conversation changed its colours and came to be seen as shop talk, as new power brokers entered society late in the nineteenth century. However, scientific topics remained commonplace in day-to-day conversation. In McLeod’s case he engaged both in polite scientific conversation and in much private technical discussion.
11. Secord, *op. cit.* (ref. 6), 671.
12. By modernism, I am thinking of a range of post-Romantic ideas on human identity and sensibility beginning roughly with Nietzsche and ending with postmodernism.
13. In some quarters today we hear complaints that young people spend too much time consuming the products of science (video games, downloading and listening to/watching iPods, talking on mobile phones, etc.) and not enough time consuming/studying mathematics, science or engineering. There is concern over where the producers of tomorrow will come from. Governments, in turn, struggle to restore equilibrium by finding ways of encouraging future producers into technological areas of higher education. I use the verb ‘diffuse’ not ‘defuse’ since resentment never vanishes, it recycles in new forms.
14. Much existing literature on the type of scientific naturalism promoted by some of McLeod’s contemporaries portrays it as radical, and focuses on ways in which it was used successfully to contest older ways of thinking. See, for example, Frank M. Turner, *Between science and religion: The reaction to scientific naturalism in late Victorian England* (New Haven, 1974) and *Contesting cultural authority: Essays in Victorian intellectual life* (Cambridge, 1993). A good overview of how some of the science/naturalism/theology/religion debates are viewed today can be found in Bernard Lightman, “Victorian sciences and religions: Discordant harmonies”, in John Hedley Brooke, Margaret J. Osler and Jitse M. van der Meer (eds), *Science in theistic contexts: Cognitive dimensions* (*Osiris*, xvi (2003)), 343–66. McLeod, while seriously religious, played only a

marginal role in these debates. He sided with his friend Arthur Balfour who, as Lightman points out, opposed any two-sphere account in which science was to reside together with naturalism (at least with how T. H. Huxley construed it) in one sphere, and religion in the other. For McLeod, science and religion could not easily be separated. The élite cultural spaces challenged by people such as Huxley have certainly shrunk, but they have done so for a multitude of reasons.

15. See W. Prout, "On the relation between the specific gravities of bodies in their gaseous state and the weight of their atoms", *Annals of philosophy*, vi (1815), 321, and vii (1816), 111. For more on the outcome of Prout's Hypothesis see David Knight, *Atoms and elements: A study of theories of matter in England in the nineteenth century* (London, 1967), chap. 7; and W. H. Brock, *From protyle to proton: William Prout and the nature of matter, 1785–1985* (Bristol, 1985). For Rayleigh see Kostas Gavroglu, "Strutt, John William, third Baron Rayleigh (1842–1919)", *Oxford dictionary of national biography* (Oxford, 2004).
16. Atomic weights are experimentally determined weights of atoms relative to some arbitrary standard. If hydrogen is assumed to have an atomic weight of one then, if Prout's Hypothesis were true, oxygen should have one of sixteen. Prout's Hypothesis was finally abandoned after isotopes were discovered.
17. Argon was the first to be isolated: Lord Rayleigh and William Ramsay, "Argon, a new constituent of the atmosphere", *Proceedings of the Royal Society*, lvii (1895), 265–87. The various means of producing pure nitrogen are discussed in some detail in this paper. William Crookes provided spectroscopic data for Rayleigh and Ramsay. Ramsay had connected Rayleigh's work on nitrogen with the 1783 experiment of Henry Cavendish in which 'dephlogisticated air' (largely nitrogen) and oxygen were combined using sparks from frictional electricity. Cavendish found there was a gaseous residue in the nitrogen that would not react (with oxygen). Ramsay isolated argon from atmospheric nitrogen by low-temperature distillation and fractionation; further fractionation and spectroscopic analysis led to the separation of krypton, neon and xenon. For Ramsay see K. D. Watson, "Ramsay, Sir William (1852–1916)", *Oxford dictionary of national biography* (Oxford, 2004) and for an account of the discovery of the noble gases by one of Ramsay's co-workers see M. W. Travers, *The discovery of the rare gases* (London, 1928).
18. McLeod also noted the politicking that went on at the Government Grants Committee at the Royal Society before a grant of £400 was awarded to Dewar in 1894 (diary entries on grant, April and May 1894). For Dewar at the Royal Institution see William H. Brock, "Exploring the hyperarctic: James Dewar at the Royal Institution", in Frank A. J. L. James (ed.), *"The common purposes of life": Science and society at the Royal Institution of Great Britain* (Aldershot, 2002), 169–90.
19. In 1894 when Rayleigh and Ramsay announced the discovery of argon, Dewar ridiculed the claim, stating that the new gas was simply an allotrope of nitrogen. Ramsay retaliated for this and other slights by challenging Dewar for the presidency of the Chemical Society in 1897, even after Dewar had been nominated by the council. The competitive tension between these two men will have contributed to Dewar's refusal to supply Ramsay with liquid air. See Brock, *op. cit.* (ref. 18), 183–5. See also Mansell Davies, "William Hampson (1854–1926): A note", *The British journal for the history of science*, xxii (1989), 63–73. Hampson was highly gifted. He had studied classics, was called to the Bar, and later qualified as a physician. Clearly he also had considerable engineering skills. Like Robert Lennox, the designer of Dewar's apparatus, he made use of the Joule-Thomson effect. But Hampson's apparatus was simpler in design and possibly more efficient. He took out a patent in 1895 and, by the late 1890s, was able to produce a few litres of liquid air per hour.
20. For Ramsay's Nobel Prize lecture see Nobelprize.org. Arthur and Leo Brin (Brin Frères et Cie.) developed their process in France. The Brin Oxygen Company produced oxygen in Britain under patent from 1886. Later the company became the British Oxygen Company. Barium monoxide was made to react with oxygen from the air. The product, when heated to about 870°C, releases the oxygen taken up at lower temperatures. The system was sometimes metaphorically referred

to as the ‘barium oxide lung’. McLeod was on the chemical jury for the Inventions Exhibition (see below).

21. The preparation of very pure samples of the elements and the determination of atomic weights was an obsession before the discovery of isotopes (see Knight and Brock, *op. cit.* (ref. 15)). The first determination of the atomic weight of oxygen was by J. B. Dumas in 1842 but others followed. See, for example, Hannah Gay, “The chemical philosophy of Theodore W. Richards”, *Ambix*, xlv (1997), 19–38. Richards, while a student of Josiah Cooke at Harvard, determined the atomic weight of oxygen relative to hydrogen as one. His ratio was 15.869 (J. P. Cooke and T. W. Richards, “Atomic weights of oxygen and hydrogen”, *Journal of the American Chemical Society*, x (1888), 81 and 191). Rayleigh published a series of results; for one published in the same year as Cooke and Richards see Lord Rayleigh, “On the relative densities of hydrogen and oxygen”, *Proceedings of the Royal Society*, xliii (1887–88), 356–63. That year his ratio was 15.912; later measurements gave a slightly lower ratio. Richards won the 1914 Nobel Prize for chemistry for his work on atomic weight.
22. Helium was first detected extraterrestrially in the sun’s chromosphere by P. J. C. Janssen who was a member of an eclipse expedition to India in 1868. Later J. N. Lockyer and E. Frankland interpreted the spectroscopic data, confirmed the existence of a new element and named it helium. In 1895 Ramsay, prompted by the American geochemist W. F. Hillebrand who had noted some new lines in the spectra of gases occluded in the mineral cleveite, isolated helium from the mix. In 1900 he separated helium from some atmospheric neon using low temperature liquefaction and distillation. When Ramsay presented this latter work to the Royal Society, mentioning the earlier identification of helium, McLeod did something typical of him. He went to the literature and found that Ramsay had not cited the earliest mention of terrestrial helium, as he had claimed. Ramsay received the 1904 Nobel Prize for chemistry for his work on the noble gases. In the same year Lord Rayleigh was awarded the Nobel Prize for physics.
23. On 4 June 1896 Lockyer gave a paper at the Royal Society in which he discussed work on gases occluded in minerals, and the search for new elements. At the same meeting Dewar and J. A. Fleming spoke on a different topic, namely on the properties of bismuth and mercury at very low temperatures. This was part of a larger study of the electrical and magnetic behaviour of metals at low temperatures. Relatedly, and during the same period, Edward Matthey, of the metal refiners Johnson & Matthey, presented several papers on the purification of bismuth (removal of arsenic and antimony). McLeod mentions attending at all these papers.
24. For a brief description see Herbert McLeod, “Note on a sunshine recorder”, *Nature*, 5 February 1885, 319–20.
25. At the Geological Society McLeod was often the guest of the paleontologist H. G. Seeley (Harry Govier Seeley, FRS (1839–1909) was professor of geography and geology at King’s College London). Occasionally Seeley invited him also to Geological Club dinners (for example, 24 May 1899 when he sat between Seeley and Archibald Geikie) and to his home where McLeod enjoyed looking at his fossil collection (for example, 18 February 1892). Sometimes McLeod complained about the regular meetings at the Royal Society; for example, “too many papers were read too rapidly” (diary, 18 June 1885). Papers at the Royal Society were very varied. For example, on 21 January 1886, McLeod listened to F. Galton on “Family likeness in stature”, to W. Crookes “On radiant matter spectroscopy”, and to Lord Rayleigh “On the Clark Cell as a standard of EMF”. Like many others, McLeod had been to Galton’s anthropometric laboratory in South Kensington to be ‘measured’.
26. For more on the role of the British Association and its influence on the lives of practising scientists, see 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* (Northwood, Middlesex, 1981), chap. 3.

27. Diary, 18 November 1885. Taking borings in the Nile Delta was a major geological project of the period which received much funding. Judd reported trying, but not yet succeeding, to reach the rocky floor of the delta. Ramsay was not yet a Fellow of the Royal Society. He and his assistant were beginning work on critical phenomena in liquids and their paper was communicated by G. G. Stokes. See William Ramsay and Sydney Young, "On evaporation and dissociation: Part one", *Proceedings of the Royal Society*, xxxix (1885), 228–9. Charles Edward Groves, FRS (1841–1920) was a friend of McLeod's from his student days at the Royal College of Chemistry. He was a lecturer at Guy's Hospital Medical School and editor of the Chemical Society journal. There are many mentions of both Groves and Ramsay in McLeod's diary including their sitting together and chatting at various dinners. The Chemical Club was a dining club for chemists. For this and other clubs see Hannah Gay and John W. Gay, "Brothers in science: Science and fraternal culture in nineteenth-century Britain", *History of science*, xxxv (1997), 425–53.
28. John Hall Gladstone, FRS (1827–1902) had been something of a mentor to the younger McLeod. Both men were very religious though differing in their approach. Gladstone was an evangelical 'low-church' Anglican while McLeod was 'high-church'. Gladstone was a promoter of the Young Men's Christian Association and of the Christian Evidence Society. He was also active in the Liberal Party. McLeod was not politically active and voted Tory. Gladstone had been Fullerian Professor of Chemistry at the Royal Institution but retired early. Independently wealthy, he conducted some physical chemistry research privately. He is mentioned many times in McLeod's diary, as are his many daughters. (Gladstone had six daughters and one son. One of his daughters married the politician Ramsay Macdonald.)
29. Diary, 13 December 1885. Ramsay moved to the chair of chemistry at University College London on the retirement of A. W. Williamson in 1887. The Savile Club was a favourite meeting place for privileged scientists in this period. Membership was exclusive and expensive. In 1899 when his children's education bills were mounting McLeod could no longer afford membership and resigned.
30. William Cawthorne Unwin, FRS (1838–1933) was a close friend. Unwin had been a professor at the Royal Indian Engineering College before moving to the chair of engineering at the City and Guilds' Central Technical College in South Kensington in 1884. William James Russell, FRS (1830–1909) studied chemistry under Thomas Graham at University College and, in the late 1880s, was lecturer in chemistry at St. Mary's Hospital Medical School. Like McLeod he was active in many societies. He was a founder of the Institute of Chemistry, and was elected President of the Chemical Society in 1889.
31. Diary, 30 November 1893. For the final paper in a series by Scott on the atomic weights of hydrogen and oxygen see A. Scott, "On the composition of water by volume", *Philosophical transactions of the Royal Society*, clxxxiv (1893), 543–68. Alexander Scott had studied chemistry with Alexander Crum Brown and James Dewar in Edinburgh. Later he was Dewar's demonstrator in Cambridge and the two of them carried out some atomic weight measurements there. Scott was an active member of the Chemical Society and became President in 1915. Another of his interests was archaeology. After losing his job as superintendent at the Davy-Faraday laboratory in 1910 he worked privately, including as a consultant to archaeologists, until being appointed director of the British Museum laboratory in 1919. Scott is mentioned often in McLeod's diary and, because of Scott, McLeod was to pay close attention to reports of Howard Carter's archaeological work in Egypt. Mond, a major chemical manufacturer, funded the Davy-Faraday Laboratory with the intention that scientists working there be provided with up-to-date facilities and technical assistance, but no salary. As superintendent, Scott was an exception and was paid £400 a year but his relationship with Dewar, the laboratory's director, deteriorated and he was to blame Dewar for the termination of the superintendent position. The row and subsequent legal tussle over the termination are mentioned in later entries in McLeod's diary. For details of Scott's atomic weight

work see Robert Robertson, “Alexander Scott (1853–1947)”, *Obituary notices of Fellows of the Royal Society*, vi (1948), 251–62. For Scott’s work at the Royal Institution and his dismissal, see Katherine D. Watson, ““Temporary hotel accommodation”? The early history of the Davy-Faraday Research Laboratory, 1894–1923”, in James, *op. cit.* (ref. 18), chap. 9. For Ludwig Mond (1839–1909) see Frank Greenaway, “Mond family (*per.* 1867–1973), chemical manufacturers and industrialists”, *Oxford dictionary of national biography* (Oxford, 2004).

It is difficult to sort out the many people named Scott mentioned in McLeod’s diary, but four in particular stand out. In addition to A. Scott, McLeod was on close terms with the eminent palaeobotanist Dunkinfield Henry Scott, FRS (1854–1934), Jodrell Professor at University College, and his wife, Henderina Scott, who had interests in geology and cinematography. McLeod met Scott, son of the architect Sir George Gilbert Scott, when Scott was assistant professor of botany at the Royal School of Mines (where Henderina was a student). Many years later the families were near neighbours in Richmond. Active in the British Association, Scott and McLeod were the Association’s official auditors in the 1880s and ’90s. Robert Henry Scott, FRS (1833–1916), Director of the Meteorological Office, is another often mentioned. McLeod was responsible to him for measurements taken at Cooper’s Hill and took on some extra tasks such as, in 1898, testing a series of thermometers in the snow.

32. Diary, 19 January 1888. Later, when William Huggins gave an address at the British Association meeting in Cardiff on stellar and solar spectroscopy without mentioning Lockyer’s name, McLeod was similarly annoyed (diary, 19 August 1891).
33. Diary, 1 April 1897.
34. Diary, 15 March 1888. There was much interest in the evolutionary history of mammals and how to place the monotremes. Their embryological/developmental stages were widely studied at this time. Edward Bagnall Poulton (1856–1943) was appointed Hope Professor of Zoology at Oxford in 1893. In the 1880s he was a tutor at Keble College and McLeod met him there. There are several references to Poulton in McLeod’s diary. For more on the Keble connection see below.
35. Harry Marshall Ward, FRS (1854–1906) was an eminent mycologist who had been educated at the Normal School of Science under T. H. Huxley. He worked on fungal disease in coffee plantations in Ceylon, and was briefly a lecturer at Owens College before his professorial appointment at the Royal Indian Engineering College. McLeod recorded attending Ward’s Croonian Lecture, “The relation between host and parasite in certain epidemic diseases of plants” (diary, 27 February 1890), later published in *Proceedings of the Royal Society*, xlvii (1889–1890), 213–16. For work on plants under reduced pressure and different temperatures see diary, 20 March – 1 April 1891, and for the dinner party with nursery rhymes see diary, 27 May 1892. For work on the action of light on bacteria and on fungal spores there are several diary entries in the early 1890s and several papers in *Proceedings of the Royal Society* in the same period; lecture at Royal Institution (diary, 27 April 1894). In 1897 McLeod visited Ward in Cambridge and looked around his new laboratory. The Wards gave a dinner party at which McLeod met Ida Freund, lecturer in chemistry at Newnham (diary, 24–25 April 1897). Henry Edward Armstrong, FRS was professor of chemistry at the City and Guilds Central Technical College. See E. H. Rodd, rev. W. H. Brock, “Armstrong, Henry Edward (1848–1937), chemist and educational reformer”, *Oxford dictionary of national biography* (Oxford, 2004).
36. Diary, 20 March 1890. Arthur Schuster, FRS (1851–1934) was professor of applied mathematics at Owens College, Manchester. Bakerian Lecture: “The discharge of electricity through gases; (preliminary communication)”, *Proceedings of the Royal Society*, xlvii (1889–90), 526–61. Wyndham Rowland Dunstan, FRS (1861–1949) was a lecturer in chemistry at St. Thomas’s Hospital Medical School. He became director of the scientific and technical department of the Imperial Institute in 1896.
37. Diary, 29 April 1891. Lieutenant General Richard Strachey, FRS (1817–1908) joined the

- Meteorological Office after a distinguished military/scientific career in India where, among other things, he founded a meteorological service. George Mathews Whipple (1842–1893) was superintendent at the Kew Observatory and was a specialist in wind pressure and velocity. McLeod mentions both men often and had many dealings with them in relation to calculating machines and meteorological instruments.
38. Diary, 23 May 1889, 17 February 1893. After Virchow's talk, McLeod, together with H. E. Armstrong and Frank Clowes (1848–1923), professor of chemistry at University College, Nottingham, went to a Chemical Club dinner and then to hear T. K. Rose, assistant assayer at the Royal Mint, give a paper at the Chemical Society on gold assaying. McLeod had known Clowes when he was a student at the Royal School of Mines.
 39. Ladysmith had been relieved on 28 February 1900 after a long siege, and the British army was expected to relieve Mafeking (now Mafikeng) at any moment (they did, on 17 March). London's streets were very crowded during much of March. On 8 March, the day of the Bakerian, Queen Victoria was to make a public appearance. After dinner Tilden and McLeod went to the Chemical Society to hear a number of papers. Sir Augustus William Tilden (1842–1926) was then professor of chemistry at the Royal College of Science. He became president of the Chemical Society in 1903.
 40. Diary, 31 January, 23 February, 25 April 1895. For the Royal Society paper see ref. 17. Rayleigh and Ramsay made a preliminary announcement of their discovery at the Oxford British Association meeting in 1894. At that meeting McLeod noted that Rayleigh told him that Ramsay had found yet "another gas from the air" (diary, 13 August 1894; I think he meant helium). (McLeod and Rayleigh also had a private discussion on the new gases at the British Association meeting in Ipswich in the following year.) There was criticism of the new findings, both at Oxford and at the Royal Society. Like Dewar some others believed that argon was simply an allotropic form of nitrogen (see also ref. 122 below for work on nitrogen allotropes by J. J. Thomson). Interestingly McLeod was working on the allotropes of arsenic (another group 5a element) in the same period. He delivered a paper on the topic at the same Oxford British Association meeting and published a summary. See Herbert McLeod, "On Schuller's yellow modification of arsenic", *Chemical news*, 21 September 1894, 139. For more on the counter arguments to argon see William H. Brock, *The Fontana history of chemistry* (London, 1992), 331–7. Rayleigh's weighing apparatus was later displayed at the Royal Institution. See below for McLeod's giving Rayleigh assistance.
 41. William Ramsay, "On a gas showing the spectrum of helium, the reputed cause of D_3 , one of the lines of the coronal spectrum", *Proceedings of the Royal Society*, lviii (1894–95), 65–67. William Crookes, FRS had been working on the spectroscopic identification of lanthanide (rare earth) and group 3b metals since the 1870s and was an obvious person to ask for help in the spectroscopic identification of argon and helium. W. H. Brock, "Crookes, Sir William (1832–1919)", *Oxford dictionary of national biography* (Oxford, 2004).
 42. Diary, 4 February 1897. McLeod was a supporter of Ramsay; Perkin was to win the Longstaff medal three years later. Augustus George Vernon Harcourt, FRS (1834–1919) was reader in chemistry, and fellow of Christ Church, Oxford.
 43. In 1888 McLeod and Edward Frankland corresponded on who should be nominated for the Royal Society Council. They agreed on Armstrong but McLeod wanted Vernon Harcourt and Frankland wanted James Bell. (Bell headed the laboratory at the Society of Public Analysts and was a specialist in the adulteration of food products. The laboratory was later taken over by the government and upgraded under the direction of T. E. Thorpe.) As it happened, the second chemist to be nominated for the Royal Society Council in 1888 was Henry Roscoe.
 44. Diary, 4 April 1889. Armstrong's politicking activities can be seen also in his correspondence held in the Imperial College London Archives. Arthur Nevil Rücker (1848–1915) was then professor of physics at the Royal College of Science. He did much work on electromagnetism

in collaboration with A. W. Reinold. Arnold William Reinold, FRS (1843–1921), educated at Oxford under R. B. Clifton, was professor of physics at the Royal Naval College at Greenwich. Both men are mentioned often in McLeod's diary. Bakerian Lecture: A. W. Rucker and T. E. Thorpe, "A magnetic survey of the British Isles". This work followed earlier studies by Edward Sabine and others.

45. Horace Tabberer Brown, FRS (1848–1925), a former student of Frankland's at the Royal College of Chemistry, worked in the brewing industry until the 1890s after which he carried out research on fermentation at the Royal Botanical Gardens at Kew.
46. George Sydenham Clarke (1848–1933), later Lord Sydenham, was an army engineer who taught geometry and engineering drawing at Cooper's Hill before being sent overseas as a colonial administrator. He returned in the 1890s to become head of the gun-carriage department at Woolwich where McLeod visited him and, on occasion, gave advice — for example, on the training of a gun by electric motor (diary, 23 May 1895). After further government service related to Imperial defence, Clarke was appointed Governor of Victoria (Australia) in 1901 and Governor of Bombay in 1907. His wife and daughter both died in Bombay and McLeod recorded receiving grieving letters. At Cooper's Hill the two men had collaborated on various technical projects and published some joint papers. In later years they grew apart as Clarke became increasingly reactionary. After the First World War he supported the eugenics movement of which McLeod disapproved, and blamed the 'decline' of empire on, among others, socialists, pacifists, suffragettes, and Jews. He was to become a prominent anti-Semite. He also headed the British Science Guild, 1917–20. For Lodge and Unwin see, for example, diary, 13 and 15 December 1885. McLeod also helped to collect signatures for the chemists Victor Veley and William Perkin Jr (see, for example, diary entries in February 1890). Aside from in the year after his own election when he described the balloting at the Royal Society in detail, McLeod makes only brief mention of it in his diary. He noted some debate over procedure during the election of foreign members in 1888, the year in which Henri Becquerel was admitted.
47. Diary, 6 May 1891 and 28 April 1896. At that time Robert Bellamy Clifton (1836–1921) was head of the Clarendon Laboratory at Oxford; Oliver Joseph Lodge (1851–1940) was professor of physics and electrotechnics at Liverpool; Joseph John Thomson (1856–1940) was Cavendish Professor at Cambridge; William Henry Mahoney Christie (1845–1922) was Astronomer Royal; and William Grylls Adams (1836–1915), brother of John Couch Adams, was professor of natural philosophy at King's College London.
48. Diary, 15 June 1892. Crookes demonstrated a flame arising from a high voltage a.c. arc in air and claimed that it was due to the oxidation of nitrogen. At an earlier soirée, 9 June 1886, McLeod heard a performance of the Mikado over the phone, "very metallic". At a soirée, 8 June 1898, McLeod reported that Ramsay showed off the spectrum of krypton. On the following day Ramsay read a paper on the new gas, but it was not accepted for publication by the Royal Society since it had been read earlier at the French Academy. A soirée held on 19 June 1889 attracted a large crowd to see E. J. Muybridge's photographs of animals in motion.
49. Going for "beer and buns" with other scientists is something McLeod often recorded. One interesting example was after a law case in which a chemical analyst, William Johnstone, sued the president of the Chemical Society, A. W. Tilden, for wrongfully being thrown out from both the Society and the Institute of Chemistry. It was the latter that Johnstone cared about since it was the body that accredited professional chemists and analysts. The case was settled when Johnstone agreed not to use the letters FIC after his name, but both sides had to cover their costs. After the court settlement McLeod, who had been called as a witness, joined Tilden and other Society members, D. Howard, C. Groves, T. E. Thorpe, F. W. Page, W. Ramsay and B. S. Dyer (an eminent professional analyst and also a witness) for "beer and buns". See diary 31 May 1892.
50. Diary, 23 May 1885. Frederick Guthrie, FRS (1833–86) was educated as a chemist but then turned

to practical physics. He was professor of physics at the Normal School of Science and founder of the Physical Society. One year after this meeting, the day after Guthrie's death from complications during surgery, McLeod received a letter from Mrs Guthrie complaining that the doctors would not tell her what had been wrong with her husband. The Physical Society raised money for her since she was left poorly off. Shelford Bidwell, FRS (1848–1909) was a barrister who taught himself physics and carried out work in South Kensington, mainly on the photoelectric properties of selenium. For many physicists the South Kensington laboratories and the Physical Society provided a popular alternative locus to Cambridge, home of the mathematical physicists. It was also a more comfortable place than the Royal Society for showing new apparatus and presenting sketchy hypotheses. Schoolteachers and women scientists were admitted as members.

51. G. F. Rodwell was a science teacher at Marlborough School where C. V. Boys had been one of his students. Charles Vernon Boys, FRS (1855–1944) studied physics under Guthrie and was assistant professor of physics at the Royal College of Science. His technical skills were widely admired and are discussed in the fine obituary by Lord Rayleigh (R. J. Strutt); see *Obituary notices of Fellows of the Royal Society*, iv (1944), 771–88. One of the things Rayleigh mentions was that Boys, who was on the Royal Society soirée committee for many years, was largely responsible for there always being so many interesting things on display. For Abney, see ref. 124 below.
52. Many novel electrical inventions were on display. The Waltham company was one of several American companies (Singer Sewing Machines was another) which impressed people with their machine tooling and their ability to mass produce.
53. William Edward Ayrton, FRS (1847–1908) was professor of physics at the City and Guilds Central Technical College. For more on Ayrton and electrical measuring devices see Graeme J. N. Gooday, "The morals of energy metering: Constructing and deconstructing the precision of the Victorian electrical engineer's ammeter and voltmeter", in M. Norton Wise (ed.), *The values of precision* (Princeton, 1995), chap. 10.
54. Diary, 26 February 1898.
55. Diary, 8–10 May 1885. Hertha Ayrton (1854–1923) was a scientist in her own right and was then working on the electric arc. Henry Selby Hele-Shaw, FRS (1854–1941) had been an outstanding student at Bristol. He was appointed lecturer on graduating and, in 1881, professor of engineering. In 1885 he moved to become professor of engineering at University College Liverpool. In Bristol he was working on the measurement of wind velocity and direction, on automatic anemometers, bicycle speedometers, and other such devices. He was interested more generally in mechanical integration and shared an interest in calculating machines with McLeod.
56. Silvanus Phillips Thompson, FRS (1851–1916), who had earlier taught at University College Bristol, was professor of physics and Principal of Finsbury Technical College. McLeod had known him for many years. George Francis Fitzgerald, FRS (1851–1901) was professor of physics at Trinity College Dublin and was known to McLeod as a frequent visitor to the Royal Indian Engineering College (see ref. 113) and also through his work as an examiner (see below). Fitzgerald displayed his own model of the ether at the Inventions Exhibition.
57. George Forbes (1849–1936), son of the physicist J. D. Forbes, was professor of natural philosophy at Anderson's Institute before coming to London in 1880 where he worked on electricity generation.
58. Joseph Edmondson, a major manufacturer of calculating machines whose main workshop and factory was in Halifax, was someone with whom McLeod often corresponded. See "Summary of lecture on calculating machines" (full paper delivered 28 March 1885), *Proceedings of the Physical Society of London*, vii (1885), 81–85. In the late nineteenth century the Physical Society held several sessions devoted to calculating machines of various kinds. For example on 13 April 1894 machines based on some ideas of Olaus Henrici, professor of mathematics at the City and Guilds Central Technical College in South Kensington, were discussed.

59. Major General Henry Prevost Babbage (1824–1918) was the youngest son of Charles Babbage and inheritor of the calculating engine dream. He defended the idea of digital computation well before any such machines existed.
60. Diary, 27–28 March 1885. All four dinner companions had been colleagues at Cooper’s Hill where Gregory was a demonstrator in physics. The mathematician Sir Alfred George Greenhill, FRS (1847–1927) returned to Cambridge, and to a chair, after just a few years at the college. In 1876 he was appointed professor of mathematics at the Royal Artillery College in Woolwich. He was a second wrangler and a much honoured mathematician.
61. Diary, 14 May 1885. Frederick John Jervis-Smith (1848–1911) read classics at Oxford, was a self-taught physicist and an excellent inventor of electrical instruments. The Millard Laboratory at Oxford, where he became lecturer in 1888, was largely equipped at his expense. See Tony Simcock, “Mechanical physicists, the Millard Laboratory, and the transition from physics to engineering”, in Robert Fox and Graeme Gooday (eds), *Physics in Oxford 1839–1939: Laboratories, learning and college life* (Oxford, 2005), chap. 5. For an illustration of a later version of Smith’s dynamometer (ergometer) see p. 202. Smith is mentioned often in McLeod’s diary (see also below).
62. William Odling, FRS (1824–1921) was Waynflete professor of chemistry at Oxford but had strong ties to London where he grew up. He was educated at the Royal College of Chemistry and had been lecturer in chemistry at Guy’s Hospital before moving to Oxford. Sir William Henry Perkin, FRS (1838–1907), another former student of A. W. Hofmann at the Royal College of Chemistry, discovered the first aniline dye (mauve), was a chemical manufacturer and carried out private research. Sir Henry Enfield Roscoe, FRS (1833–1915) was then professor of chemistry at Owens College, Manchester. Walter Weldon, FRS (1832–85) was the inventor of a number of chemical processes, especially in relation to bleaching (at Weldon Chlorine Processes, Lincoln’s Inn Fields) and was famed also for his ballads performed at dinners of the Society for Chemical Industry. McLeod had known them all for many years. On the other jury one of McLeod’s colleagues was the physicist C. V. Boys.
63. For example, see diary, 28 May 1885. The Brin process for making oxygen was especially admired, as were the dye chemicals displayed by Badische Anilin und Soda Fabrik (BASF).
64. These three remained lifelong friends and each was asked to be godfather to one of McLeod’s children. In the late 1880s David Howard (1839–1916) was managing his family’s chemical company, Howards & Sons in Essex. He was a co-founder of both the Institute of Chemistry and the Society for Chemical Industry and served terms as president of both institutions. Alexander Gillman (1843–1903) was a brewery chemist who, in 1886, started the successful brewing consultancy firm of Gillman & Spencer with another Royal College of Chemistry graduate, Ernest Spencer. The firm (maltings experts) is still in existence. As young men the four friends shared deeply felt religious views, especially as they related to science. They were collaborators on the Scientists’ Declaration. For Groves see ref. 27. For the Scientists’ Declaration, see W. H. Brock and R. M. Macleod, “The Scientists’ Declaration: Reflexions on science and belief in the wake of *Essays and reviews*, 1864–5”, *The British journal for the history of science*, ix (1976), 39–66; also Gay, *op. cit.* (ref. 2, 2007).
65. Diary, 26 March 1886. Cameron was also the public analyst for large areas of Ireland and professor of chemistry at the Royal College of Surgeons of Ireland. Francis Robert Japp, FRS (1848–1926) was a lecturer at the Normal School of Science, but moved to a chair in Aberdeen in 1890. Percy Faraday Frankland, FRS (1858–1946) was another old friend whom McLeod had known since working for his father, Edward Frankland. P. F. Frankland was professor of chemistry at Mason College, Birmingham (see also below).
66. The Chemical Society then had the rooms now occupied by the Geological Society, in Burlington House, which fronted Piccadilly. In the evening McLeod, his family, and some of his Cooper’s Hill colleagues watched the celebrations in Windsor. The castle was illuminated and the bridge

to Eton had a ceremonial arch at each end (the one at the Eton end was decorated with guns, pistols, ramrods and swords). There were fireworks for several nights in a row. Ten years later on the Queen's sixtieth anniversary five scientists were given jubilee knighthoods: W. Crookes, J. N. Lockyer, W. Huggins, E. Frankland and R. Strachey. Frankland died in the following year (1898) and McLeod attended the funeral in Reigate. The service was conducted by T. G. Bonney, professor of geology at University College, assistant general secretary of the British Association, and an ordained Anglican priest. Lord Lister and Michael Foster represented the Royal Society and many chemists were present.

67. Sir Thomas Stevenson, MD (1838–1908) was a forensic specialist and Senior Analyst at the Home Office. For his earlier career see ref. 120.
68. Diary, 28 March 1889 and 26 February 1891. Lord (Lyon) Playfair (1818–98) was by then long retired from the chair of chemistry in Edinburgh (where both Dewar and Guthrie had been his assistants) and had retired also from his political career. He was president of the British Association in the 1880s. Lord Salisbury was Prime Minister in 1891. Another major Chemical Society dinner was held in honour of six past presidents who had been members of the society for fifty years (Gilbert, Frankland, Odling, Abel, Williamson and Gladstone); Whitehall Rooms, 11 November 1898.
69. Diary, 5 May 1893. Hofmann died in April 1892 aged 74. Sir Frederick Abel, FRS (1827–1902), an expert on explosives, was Chemist for the War Department, working at Woolwich Arsenal until his retirement in 1888. Like Perkin, Abel was one of Hofmann's first students in London.
70. Diary, 28 May 1896. William Chandler Roberts-Austen (1843–1902) was a student and later professor of metallurgy at the Royal School of Mines, before becoming Master of the Mint. It would appear that Roberts-Austen hosted at least one large reception at the Mint each year, usually with technical things on display. For example, on 4 May 1899 a reception was held for the Iron and Steel Institute when a novel electrical furnace was on show.
71. Diary, 19 December 1895.
72. See diary, 13–18 August 1885 for correspondence with Lord Salisbury on improving the connection, and lowering the cost of telegraphy to and from the Ben Nevis Observatory. McLeod was helping Salisbury with the electrification of Hatfield House at the time. In December 1885, after looking over the generating equipment, he asked his friend Unwin for help with the water turbines (diary, 19–22 December 1885; see also Gay, *op. cit.* (ref. 2, 2003)). For meeting in Aberdeen see diary, 25 August – 7 September 1885. Harold Baily Dixon, FRS (1852–1930) had been a student of Harcourt at Oxford where he continued to work until becoming professor of chemistry at Owens College, Manchester, in 1886. As to holidays, after the 1888 British Association meeting in Bath, McLeod and his wife took a more gentle holiday in Devon where they met the Ramsays. Diary, 11 September 1888.
73. McLeod had a long interest in electrical timekeeping. For the British Association meeting, see diary, 8–16 September. Red Lion dinners were a regular feature at British Association meetings but a more select Red Lion Club existed in London. McLeod sometimes dined there as a guest. For the Red Lions see Gay and Gay, *op. cit.* (ref. 27).
74. Arthur Albright (1811–1900) co-founded the chemical manufacturing firm of Albright & Wilson in 1856 with J. W. Wilson (1834–1907). At first the company produced mainly phosphorus and chemicals for the match industry but later diversified. McLeod met the Wilson family while in Birmingham. McLeod noted that Henry Armstrong was also staying with the Albrights and that Armstrong enjoyed playing tennis in the garden with Albright's granddaughters. For the British Association meeting see diary entries, 1–10 September 1886.
75. Retroactively the earthquake is said to have measured 7.3 on the Richter scale and is the largest ever recorded in the southeastern USA; about 60 people were killed.
76. Diary, 1–6 September 1886.

77. Diary, 1 September 1887. In this period Edward (Henry) Schunck (1820–1903) read many papers at the Royal Society on the chemistry of chlorophyll and its role in plants. He was a Manchester industrialist and specialist in dye chemistry.
78. Diary, 12 September 1899. John Abbot & Co. was a large engineering and manufacturing company that made a range of products including railway locomotives. Lord (William George) Armstrong (1810–1900), who profited from all that ‘dirt’, was a major industrialist and armaments manufacturer. Much of Jesmond Dene, a steep sided valley of the Ouse near Newcastle, was owned by Armstrong. He built a house and banqueting hall there and commissioned the design of parkland, now public. By the late nineteenth century Armstrong was no longer living at Jesmond Dene but in a yet grander residence, Craggside, in Rothbury, Northumberland, also surrounded by magnificent gardens; today it belongs to the National Trust.
79. Diary, 2 September 1890. Today the discovery is credited to both Priestley and C. W. Scheele working independently in 1773–4. Lavoisier is credited with recognizing that oxygen was an element about two years later.
80. Diary, 8 September 1890.
81. The Kempes were relatives of Sir Alfred Kempe, later Treasurer of the Royal Society. For party see diary, 5 August 1892. For Kipping see ref. 103 below. Mrs Kipping’s two sisters were married to Arthur Lapworth (see ref. 99) and W. H. Perkin Jr. Wilhelm Ostwald (1853–1932) was professor of chemistry at Leipzig, a founder of classical physical chemistry, and winner of the Nobel Prize for chemistry in 1909 for his work on catalysis and chemical kinetics.
82. Diary, 13–21 September 1899.
83. For Toronto meeting and voyage see diary, 5 August – 8 September 1897.
84. Bohuslav Brauner (1855–1935) was a friend of D. I. Mendeleev and, like Crookes, carried out work on the chemistry of the actinides and lanthanides. He gave a paper in Toronto on the atomic weight of thorium. He had long been a proponent of atomic weight determinations being based on oxygen (rather than hydrogen) and carried out several in that way. His view won out and oxygen became the standard until replaced by carbon 12. In the 1880s Brauner predicted the existence of what were later known as isotopes. His speculation was not unlike that of Richards (see Gay, *op. cit.* (ref. 21)). McLeod enjoyed Brauner’s company and noted that he was a very amusing man.
85. Raphael Meldola, FRS (1849–1915) and Silvanus Phillips Thompson were friends and colleagues at Finsbury Technical College where Meldola was professor of chemistry. Sir (James) Alfred Ewing, FRS (1855–1936) was professor of engineering at Dundee until 1890 when he moved to Cambridge.
86. Lord (Joseph) Lister (1827–1912), renowned for his contributions to antiseptic surgery, was President of the Royal Society. Sir John Evans (1828–1908), Treasurer of the Royal Society, was a paper manufacturer with interests in archaeology and numismatics. He was the father of the archaeologist Sir Arthur Evans. McLeod had many dealings with him in connection with the Royal Society Catalogue of Scientific Papers. Lister, Evans and Kelvin were awarded honorary degrees by the University of Toronto during this visit. It was usual for universities in towns hosting the British Association to award honorary degrees to senior office holders in the Royal Society and British Association.
87. Interestingly not everyone wanted to see the falls, a ‘must-see’ for many of today’s tourists. John Perry, FRS (1850–1920) was professor of mathematics and mechanical engineering at Finsbury Technical College from 1882 until 1896 when he moved to the Royal College of Science. His main interests were in electrical science and he worked together with William Ayrton on a number of projects. In 1885 McLeod wrote about a party at Perry’s (Perry was then a neighbour of Lockyer on Pennywern Road, Earls Court) where a model of Fleeming Jenkin’s telpherage line was on display. Perry and Eustace Balfour tried to persuade McLeod to join in a related business venture

- but he declined. For party at Perry's see diary, 13 February 1885.
88. In the 1880s some direct current was generated and delivered within a radius of about one mile, but only for electrical lighting. It was Thomas Evershed, and then George Westinghouse, who made the first plans for major electricity generation at Niagara. Unwin had joined the Niagara Commission in 1890 and helped to design the flumes. For more on Unwin and hydro-electric power, see E. G. Walker, *William Cawthorne Unwin* (London, 1938).
89. For Veley's visit to Cooper's Hill, see diary entries for April 1889. Quotation, 7 June 1889. Victor Herbert Veley, FRS (1856–1933) was an Oxford educated chemist who made his career at Oxford before moving to London in 1908 to engage in various business ventures.
90. Edward Stuart Talbot (1844–1934) and Lavinia Talbot (1849–1939) were also promoters of women's education at Oxford and active in the foundation of Lady Margaret Hall. Both followed in the footsteps of their fathers who had been keen supporters of the Oxford Movement. The university was divided on the new (Keble) college. Many people viewed it with contempt, perhaps fearing that it would spur further defections to the Catholic church (Talbot's uncle had become a Roman Catholic priest). It helped that a young aristocratic couple was placed in charge at Keble and that the University Test Act had been passed in 1871. Lavinia was the daughter of the fourth Baron Lyttelton and was related to both the Spencer and Cavendish families. Catherine Gladstone was her aunt. W. E. Gladstone and various aristocrats, including Lord Salisbury and members of the Cecil and Balfour families, were frequent visitors at Keble. Such visits helped the college to become more widely accepted. McLeod began a correspondence with Edward Talbot in the 1870s, was first invited to visit Keble College in 1877 and was a regular visitor thereafter. He deeply admired the Talbots, was guided by them in religious matters and remained on friendly terms all his life. The Talbots left Oxford when Edward became Vicar of Leeds in 1888. McLeod records visiting them during there during the British Association meeting in 1890. He also noted that his friend W. A. Shenstone (chemistry teacher at Clifton College) was staying with the Talbots. McLeod continued to visit the Talbots after Edward became successively Bishop of Rochester, Southwark and Winchester. He attended their children's weddings and was present at a number of other family occasions. The Talbots also visited the Royal Indian Engineering College and Edward gave sermons in the chapel when there. McLeod also received invitations to the various homes of the Gladstones. W. E. Gladstone was a Tory when young; however, when it came to extending friendship, religious outlook meant more to the Talbot, Lyttelton, Gladstone clan, and to the Cecil, Balfour, Rayleigh clan, than did political affiliation. For more on the former family group see Sheila Fletcher, *Victorian girls: Lord Lyttelton's daughters* (London, 2001). The two groups were socially connected in many ways. For example, Lavinia Talbot's brother, Spencer Lyttelton, was Gladstone's private secretary during his first premiership. Among Spencer's closest friends were John Strutt (later third Baron Rayleigh) and Arthur Balfour. McLeod, close in age, met these men socially already during the 1870s. Balfour and his sister, who later married Strutt, were also devotees of the Talbots; Balfour and McLeod met in several places including occasionally at Keble. For more on some of the scientific and religious ideas current at Keble see E. S. Talbot, *Memories of early life* (London, 1924), chap. 3; Richard England, "Natural selection, teleology and the logos: From Darwin to the Oxford Neo-Darwinists, 1859–1909", in Brooke, Osler and van der Meer (eds), *op. cit.* (ref. 14), 270–87. For a discussion of anti-naturalism and the views of Arthur Balfour see Lightman, *op. cit.* (ref. 14), 343–66.
91. Diary, March 7 1885. E. A. Robert (Bob) Cecil, then a student at Oxford, was the third son of Lord Salisbury. Another dinner guest was Henry Liddon, canon of St Paul's. Liddon, an eminent clergyman and author of the *Life of Edward Bouverie Pusey* (4 vols, London, 1894) had earlier declined to become the first warden of Keble. He was famous for his sermons and people queued to hear him. McLeod was among his many admirers and noted how "very amusing" he was. For the earlier work with Burdon-Sanderson at Cooper's Hill see Gay, *op. cit.* (ref. 2, 2003).
92. Diary, 22 June 1889.

93. Walter William Fisher (1842–1920) was a tutor at Balliol and chemistry demonstrator for the university. McLeod enjoyed being shown the collection of falcons and owls kept by Odling's son.
94. McLeod first met Clifton in 1870. For Smith see ref. 61. Sir John Conroy, FRS (1845–1900) worked also in the Millard Laboratory, on optics. McLeod was interested in him as much for his connection to Keble College as for his scientific research. Both were admirers of *Lux mundi*, an 1889 collection of essays by Oxford theologians (including Talbot) defending both an Anglo-Catholic moral philosophy and the need for the Church to accept aspects of modern thought and new Biblical criticism. On his death Conroy left Keble a substantial sum for science. Chemist, David Henry Nagel, was a demonstrator under Conroy, later a university demonstrator in physical chemistry.
95. For example, see diary, 23 November 1889 and 7 June 1890. Harcourt was using some gas analysis apparatus built by McLeod. Glassblowing skills were still rare outside London and a few other glassmaking centres.
96. Diary, 26 November 1889.
97. Twenty-seven men took the practical exam in 1889 and thirty-four in 1890. McLeod recorded examining just two women from Somerville in 1889, and one or two (the diary is unclear) in 1890. The women passed their exams as did most of the men.
98. The principal physics examiners in this period were Oliver Lodge and G. F. Fitzgerald. McLeod records having lunch with them on several occasions during examination periods.
99. McLeod almost had a falling out with Armstrong over the DSc examination of Armstrong's former student, Arthur Lapworth, FRS (1872–1941). McLeod and the other examiners were at first reluctant to pass him. McLeod wrote "he is a sharp fellow but careless" (diary, 18 June 1895). Lapworth, a future eminent professor at Manchester, was awarded the DSc in 1895.
100. Diary, 3 August 1893. McLeod often stayed with the Armstrongs, and not just for examination work. He made some interesting comments on the ways in which Armstrong taught his own children, not only chemistry but science more generally. There was a home laboratory and the children kept a large menagerie of guinea pigs, pigeons and other small animals in cages in the garden. Imperial College London Archives (Henry Armstrong papers) has a notebook containing essays by three of Armstrong's children, Harry, Edward and Edith, with interesting details of historical and natural history outings they made. The essays are illustrated with photographs and newspaper cuttings. For Armstrong's pedagogical approach more generally, see W. H. Brock, *H. E. Armstrong and the teaching of science* (Cambridge, 1973) which includes mention of the children's education and essays. McLeod also records staying with Dunstan at Camden Hill in North London for marking sessions and that he, too, had "very nice" children (diary, 2 August 1894).
101. Diary, 21 December 1892, 23 August 1894, 20 February 1896. See also W. Ramsay, letter to the editor, *Chemical news*, 10 August 1894, 72. This was not the end of it since even after joining the examining board, Ramsay publicly criticized Dunstan's questions. Despite all of this McLeod and Ramsay appear to have had a fairly close friendship. Like many friendships made when young, theirs picked up again as they grew older. After Ramsay was diagnosed with cancer of the jaw, in November 1915, McLeod stayed in touch with Lady Ramsay when Ramsay was too ill to receive visitors. Ramsay underwent two operations, radium and X-ray treatment; but there was little the doctors could do and he died in July 1916.
102. Diary entries in 1897–8, especially July 1897.
103. Frederick Stanley Kipping (1863–1949) was a lecturer at the City and Guilds Central Technical College, later professor in Nottingham. Kipping wrote to McLeod at least three times asking for help in getting the examiner's job. McLeod also had letters from four other people that year asking the same. In 1898 Kipping was appointed to succeed P. Frankland as assistant examiner at the Pharmaceutical Society.
104. McLeod noted that in 1898 only 104 out of 340 passed the final exams at the Pharmaceutical Society.

- Diary, 21 July 1898. In the October examination of that year only 66 out of 260 passed.
105. Diary, 29 May 1901.
 106. Poynting was professor of physics at the University of Birmingham. He and Thomson were close friends from their days as demonstrators at Owens College and continued working together for much of their lives. McLeod had earlier helped Thomson with some of his experiments (see below).
 107. In the 1890s it seems that many people, including McLeod, began riding bicycles. This resulted in new forms of socializing, allowing for visits slightly farther afield for those who earlier could not easily afford horse-drawn transport. T. E. Thorpe broke his leg by falling off his bicycle in April 1896.
 108. Matilda Ellen Bishop (1842–1913) was headmistress of Oxford High School for Girls before her appointment at Royal Holloway. She resigned in 1897 when the Council decided to allow non-Anglican denominations to use the chapel for worship. Later she was appointed head of St. Gabriel's College, an Anglican college for training women teachers.
 109. Harry Marshall Ward took an active interest in botanical instruction at Royal Holloway and helped Miss Corry, the first botany lecturer. Amelia (Min) McLeod acted as secretary for the continuing education classes which drew about 50–60 people each term (diary, 25 January 1889). For the planning of the chemistry laboratory see, for example, diary, 18 June 1887. For an example of McLeod's helping Miss Seward, and teaching her students how to blow glass, see 13 December 1888. Margaret Seward had been a student at Somerville and was the first woman to enter the honours mathematics school at Oxford (she obtained second-class standing in the degree examinations) and the first woman to receive first-class standing in the natural sciences (chemistry). She studied with Harcourt and was one of the women who took advantage of the short-term offer by Trinity College Dublin to receive its MA (degrees were denied to women by Oxford). She left Royal Holloway after her marriage and lived for several years in Singapore but was later appointed to a chemistry lectureship at King's College London. Eleanor Field was educated at Newnham College, Cambridge. For Seward see Mark Pottle, "McKillop (née Seward), Margaret (1864–1929)", *Oxford dictionary of national biography* (Oxford, 2004).
 110. This occurred on 16 December 1887 (the Queen's jubilee year) after which the picture gallery could be viewed at night. The pictures were a generous gift from Thomas Holloway, founder of the college, who had made a fortune from the sale of patent medicines. Another Thames Valley institution, Beaumont College, a Jesuit school in Old Windsor (known as the 'Catholic Eton'), appears to have had good science laboratories and McLeod visited there too. This school closed in 1967 and the pupils moved to Stoneyhurst College in Lancashire.
 111. Diary, 5 September 1891. McLeod had many conversations with Eustace Balfour who was chairman of a company that installed electrical lighting in the area around St. James's Square in London. See, for example, diary, 28 February 1889.
 112. Sir William Philipp Daniel Schlich (1840–1925) worked for many years in the Indian Forest Service before being seconded to Cooper's Hill. McLeod also mentions helping the forestry specialist Percy Groom, later a professor at Imperial College, when he came to work at the college after a period in China. On the closure of the Royal Indian Engineering College much of the forestry work moved to Oxford. Robert Warington, FRS (1838–1907), son of a founder of the Chemical Society with the same name, was an eminent agricultural chemist who had worked with John Lawes at Rothamsted. He was briefly (1894–97) Sibthorpean professor of rural economy at Oxford. Warington and Sir Arthur Herbert Church, FRS (1834–1915) were both former students of the Royal College of Chemistry but Church went on to Oxford for a degree. Church, a close friend of McLeod, had wide interests and, aside from organic chemistry, worked in the areas of mineralogy and paint pigments. The latter led to his appointment as professor of chemistry at the Royal Academy of Arts. Church had some problems with the forestry students who claimed

that he expected too much of them (diary, 4 December 1888). For more on Church see Frederick Kurzer, “Arthur Herbert Church, FRS and the Palace of Westminster frescoes”, *Notes and records of the Royal Society*, ix (2006), 139–59. Alfred Lodge (1854–1937), brother of Oliver Lodge, was professor of pure mathematics at the Royal Indian Engineering College until 1904. He then became a mathematics teacher at Charterhouse School. As a mathematician Lodge is best known for his many contributions to the construction of mathematical tables, hence his use of calculating machines. Lodge was a long-time member of the British Association mathematical tables committee, and its secretary 1888–96.

113. George M. Minchin, FRS (1845–1914) was professor of applied mechanics and taught both mathematics and some physics at the college. He was a friend of Alfred Lodge and of fellow Trinity College Dublin graduate, George F. Fitzgerald with whom he carried out some scientific work. Fitzgerald was a frequent visitor to the Royal Indian Engineering College. Minchin appears to have been very congenial, a good tennis player, and a good teacher. He moved to Oxford when the college closed. Minchin used his cells to measure the relative brightness of various stars and planets and gave a paper on this at the Royal Society on 30 January 1896 which McLeod attended. George Minchin, “The electrical measurement of starlight: Observations made at the observatory of Daramona House, Co. Westmeath in January, 1896”, *Proceedings of the Royal Society*, ix (1896–97), 42–52. First quotation, diary, 12 September 1891; second quotation, 9 January 1894. For McLeod’s helping with some of Minchin’s experiments see, for example, 15 September 1897.
114. Peter Martin Duncan, FRS (1824–91) was an apprentice surgeon who took the London MB before turning to natural history and geology. He was professor of geology at King’s College London, a former president of the Geological Society and Wollaston medallist. He was ill for many months before his death. His funeral at Chiswick Church was well attended including by two other geologist friends of McLeod, J. W. Judd and H. G. Seeley. Mrs Duncan was left in poor economic circumstances and McLeod led a successful effort for some financial support, albeit small, from the Royal Society Relief Fund (£25 in each of 1894 and 1895).
115. Diary, 13 June 1890.
116. See Gay, *op. cit.* (ref. 2, 2003). Sackville Cecil was Manager of the District Railway. The telephone exchange was at Mansion House. There are many entries on helping with the railway in the diary.
117. McLeod kept some good chronometers, a good binocular microscope, some drawing instruments and, for sentimental reasons, some submarine cable that Sackville Cecil had laid. The other instruments were sold. See, for example, diary, 28 April 1898. McLeod wrote Sackville Cecil’s obituary; see *Journal of the Physical Society of London*, xvii (1899), 6–8. For more on electrical timekeeping, including brief mention of McLeod’s and Sackville Cecil’s interests in that area, see Hannah Gay, “Clock synchrony, time distribution and electrical timekeeping in Britain, 1880–1925”, *Past and present*, clxxxix (2003), 107–40.
118. Guests included H. E. Armstrong, W. Thiselton Dyer, Alexander Herschel, C. Groves, D. Howard and T. E. Thorpe. Thorpe and McLeod became close friends late in their lives.
119. As mentioned above, McLeod’s own sunshine recorder was a success but he also helped Whipple in a number of ways, for example in testing aneroid barometers and other meteorological instruments. He was to work also with Charles Chree, Whipple’s successor as superintendent at Kew. McLeod records visiting several exhibitions of meteorological instruments held at the Institution of Civil Engineers. At one of these, in 1890, he met George James Symons, FRS (1834–1900) and chatted about black bulb thermometers. Symons worked at the Meteorological Office and was a specialist in rainfall distribution. McLeod visited Symons at his home at 62 Camden Square where a fine collection of instruments was in use in the garden, see diary, 18 March 1886, 20 March 1890 and 1 January 1891.

120. Water treatment had been a principal interest of E. Frankland under whom McLeod had worked earlier. In 1887 McLeod had to deal with some problems to do with sewage from the college's infirmary. He talked to many people about this including Sir Thomas Stevenson (1838–1908), a physician at Guy's Hospital and a specialist in water analysis and public health; and Francis De Chaumont, FRS, professor of hygiene at the Army Medical School. See, for example, diary, 19 May 1887.
121. The person at the India Office responsible for this was Sir Alexander Rendel (1829–1918), a civil engineer who had worked for many years in India. The laboratory was run consecutively by two analytical chemists: the first, Arnold Philip, was later Admiralty Chemist at Portsmouth; the second, Frank William Harbord (1860–1943), a former student at the Royal School of Mines, had worked with W. Roberts Austen at the Royal Mint before coming to Cooper's Hill. He became a private chemical consultant when the college closed, though McLeod tried hard to help him to another institutional position.
122. Diary, 7 May, 13–17 June 1886. Typically of McLeod he read much of the earlier literature on ozone, including J. Tyndall's account of its formation in the atmosphere. Ozone can be produced electrolytically from sulphuric or perchloric acid by using a small area anode and high area cathode so as to have a high anode current density. See Herbert McLeod, "On the electrolysis of aqueous sulphuric acid, with special reference to the forms of oxygen obtained", *Transactions of the Chemical Society*, xlix (1886), 591–608. See also J. J. Thomson and R. Threlfall, "Some experiments in the production of ozone", *Proceedings of the Royal Society*, xl (1886), 340–2. Thomson and Threlfall succeeded in making ozone by subjecting oxygen to a strong electrical field. Also of interest in light of future objections to Rayleigh and Ramsay's work is a paper by Thomson and Threlfall claiming that an allotrope of nitrogen was formed when an electrical discharge was passed through pure nitrogen. J. J. Thomson and R. Threlfall, "On an effect produced by passage of an electric discharge through pure nitrogen", *ibid.*, 329–40. McLeod carried out a number of other electrolysis experiments in this period and corresponded and chatted with several people, notably Armstrong, about his results. See, for example, diary entries, October 1886. Armstrong and McLeod were both on the electrolysis committee of the British Association.
123. See diary, 26 May 1887 for the second chat with Thomson at the Savile Club. For comments on lecture and paper see diary, 28 May and 18 July 1887. J. J. Thomson, "On the dissociation of some gases by the electrical discharge", *Proceedings of the Royal Society*, xlii (1887), 343–5.
124. When K. R. König gave a demonstration of his tuning forks (with English commentary by S. P. Thompson) at the Physical Society on 16 May 1890, both Rayleigh and McLeod were present. König, a German, had been apprenticed with the violin maker J. B. Vuillaume in Paris and his tuning forks were said to be the best available. Rayleigh and McLeod also exchanged ideas with the colour vision specialist, Sir William De Wivesley Abney, FRS (1843–1920), a senior civil servant at the Board of Education. Abney carried out research also in spectroscopy (he produced one of the first infra-red spectra) and photography in a large laboratory in the South Kensington Museum. McLeod visited him often and took some of his students to be tested for colour vision (Abney's laboratory was the centre for testing the vision of sailors for the merchant marine).
125. Lady Rayleigh was away canvassing for her brother-in-law, Charles Strutt, Tory candidate for Saffron Walden in the upcoming general election (he lost).
126. For this visit see diary, 28–30 November 1885. McLeod and Rayleigh returned to London to attend the anniversary meeting of the Royal Society and McLeod noted sitting with Unwin, Tilden, Russell and Thorpe at the anniversary dinner.
127. Diary, 29–31 May 1886. McLeod records other visits at which he met Thomson and that William (later Sir William) Huggins and his wife were Rayleigh's guests on more than one occasion. McLeod mentions that psychologist and psychical researcher Edmund Gurney and his wife

were guests at Terling on 4 June 1887 (Rayleigh was interested in the para-normal). McLeod had been a dinner guest at the Gurneys, in London in 1885. However, McLeod was a sceptic when it came to psychical matters.

128. In 1888–89 McLeod spent time in the Chemical Society library looking up various preparations of oxygen and made especial note of Döbereiner's 1832 method using KClO_3 and MnO_2 . While the presence of MnO_2 allows for a lower decomposition temperature it contaminates the product. Dewar used this method when preparing his oxygen in 1896, but his liquid oxygen was turbid because of the presence of chlorine. Dewar discussed the problem with McLeod on 17 December 1896, the day on which he and Fleming gave a paper on the dielectric constant of liquid oxygen at the Royal Society. In the case of nitrogen, there were many exchanges between McLeod and Rayleigh in 1891–2. Rayleigh had not yet succeeded in preparing the pure gas chemically.
129. See diary entries, January–March 1890.
130. Rayleigh also had the help of instrument maker George Gordon who had worked with him earlier at the Cavendish Laboratory, and the help of family members including his son, R. J. Strutt. Despite continuing invitations, McLeod's visits to Terling declined in number during the 1890s because of family obligations.
131. Diary, 30 June and 9 July 1885. Simon Adams Beck opened the gas works in 1870 just west of Barking Creek and near the new docks. It soon became the largest gas and chemical works in Europe. The site was larger in area than the City of London. During the reclamation of the docklands in the late twentieth century the industrial waste gathered from the gas works was formed into an artificial hill, now parkland. Documents relating to the history of Beckton can be found in the archives of the London Borough of Newham.
132. For Snowdonia, diary, 13–20 April 1885. For Milnes, diary, 21 April 1898. Mrs Milne was Japanese. John Milne (1850–1913) was a former student at the Royal School of Mines.
133. The common existence of parallel research work and the emergence of singular discovery is a topic that was of much interest to the sociologist of science Robert K. Merton. See his *The sociology of science: Theoretical and empirical investigations* (Chicago, 1973), chaps. 15–17.
134. For more explicit treatment of McLeod's religious behaviour see Gay, *opera cit.* (ref. 2, 2000, 2003 and 2007).
135. My use of the term 'gnostic' is somewhat derivative of the way it was used by the political philosopher Eric Voegelin (1901–85).
136. The anticlericalism of the Risorgimento of Banti's period and of the later republican movement in Italy contributed greatly to Galileo's iconic status. He was used politically in both these causes. In the early seventeenth century Galileo was a relatively minor irritant to a Church which, while it saw danger in science, was willing to accept change — but not too quickly, and on its own terms. It is also the case that modern science is the product of a Christian culture, but that is a topic for other scholars.

NOTICES OF BOOKS

Medical Aphorisms: Treatises 6–9. Maimonides, edited and translated by Gerrit Bos (The University of Chicago Press, Chicago, 2007). Pp. 250. \$39.95/£22. ISBN 978-0-8425-2664-7.

Moses Maimonides was born in Cordoba in 1138 and died in Egypt in 1204. His *Medical aphorisms* consists of some fifteen hundred aphorisms culled mainly from the writings of Galen, and arranged in twenty-five treatises. The present volume is the second of five which will present the complete work, with Hebrew and English on facing pages. Treatises 6–9 deal with prognosis, aetiology, therapy, and pathology.

Indian Ink: Script and Print in the Making of the East India Company. Miles Ogborn (The University of Chicago Press, Chicago, 2007). Pp. xxiv + 318. \$40/£24. ISBN 978-0-226-62041-1.

The East India Company was chartered in 1600, and became the centrepiece of late seventeenth- and early eighteenth-century English overseas trade. This study of the importance of the written and printed word in all this activity gives the blurb-writers the chance to point out that “the pen is mightier than the sword” and that “Empire was made of the write stuff”.

A Natural History of Time. Pascal Richet, translated by John Venerella (The University of Chicago Press, Chicago, 2007). Pp. xiv + 471. \$29/£17. ISBN 978-0-226-71287-1.

This important work was originally published in 1999 as *L'âge du monde: À la découverte de l'immensité du temps*. In a wide-ranging account, Richet tells the story of man's attempt to understand the timescale of the Earth and of the universe as a whole, beginning with the cyclic models of Antiquity and the limited linear scales derived from Jewish tradition, through the realization that fossils and geological strata demonstrate that the Earth has existed for millions of years, and that light from remote galaxies has taken comparable tracts of time to reach our eyes.

Bearing the Heavens: Tycho Brahe and the Astronomical Community of the Late Sixteenth Century. Adam Mosley (Cambridge University Press, Cambridge, 2007). Pp. xiv + 354. £55/\$99. ISBN 978-0-521-83866-5.

Mosley explores the astronomical community of the later sixteenth century, in which Tycho Brahe was the dominant figure. He investigates how this community shared information, attracted patronage, and settled disputes, ending the volume with a list of early owners of Tycho's publications and a bibliography of over thirty pages.

Leprosy in Premodern Medicine: A Malady of the Whole Body. Luke Demaitre (The Johns Hopkins University Press, Baltimore, 2007). Pp. xviii + 323. \$45. ISBN 978-0-8018-8613-3.

Leprosy was seen by some as a deserved affliction, by others as “a cancer of the whole body”. The verdict often dictated whether or not the sufferer was spared social ostracization, and permitted to beg. Demaitre uses manuscripts as well as printed sources to illuminate attitudes to the disease, from the first to the eighteenth century.

Science, Time and Space in the Late Nineteenth-century Periodical Press: Movable Types. James Mussell (Ashgate, Aldershot, 2007). Pp. xii + 237. £50. ISBN 978-0-7546-5747-7.

Writing with reference to nineteenth-century scientific debates, Mussell proposes a new methodology for understanding the periodical press in terms of its movements in time and space.

Genesis Redux: Essays in the History and Philosophy of Artificial Life. Edited by Jessica Riskin (The University of Chicago Press, Chicago, 2007). Pp. xviii + 389. \$65/£41 (hardcover), \$25/£16 (paperback). ISBN 978-0-226-72080-7 (hardcover), 978-0-226-72081-4 (paperback).

The seventeen essays that make up this book are the result of a meeting held at Stanford University in 2003 to discuss the attempts down the ages to understand life by reproducing it. About half the book is written by historians and philosophers, and half by computer scientists and engineers.

Science Talk: Changing Notions of Science in American Culture. Daniel Patrick Thurs (Rutgers University Press, New Brunswick and London, 2007). Pp. xii + 237. £28.95. ISBN 978-0-8135-4073-3.

‘Science’ in modern America has huge prestige, yet is largely ignored by the bulk of the population. Thurs examines how the meaning of ‘science’ has developed over time, by examining in turn phrenology, evolution, relativity, UFOs, and intelligent design.

Finding Time for the Old Stone Age: A History of Palaeolithic Archaeology and Quaternary Geology in Britain, 1860–1960. Anne O’Connor (Oxford University Press, Oxford, 2007). Pp. xl + 423. £75. ISBN 978-0-19-921547-8.

A scholarly examination of the attempts made since the mid-nineteenth century to date early stone implements found in southern England, in various contexts including geology, palaeontology, anthropology and archaeology.

NOTES ON CONTRIBUTORS

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INDEX TO VOLUME 46

- Ashworth, William J., *The Ghost of Rostow: Science, Culture, and the British Industrial Revolution*, 249
- Beasts of the New Jerusalem, by Gordon L. Miller, 203
- Boner, Patrick J., *Life in the Liquid Fields: Kepler, Tycho and Gilbert on the Nature of the Heavens and Earth*, 275
- Chapman, Allan, rev. of *The Herschel Partnership, Caroline Herschel's Autobiographies, and The Herschels of Hanover*, by Michael Hoskin, 365
- Clucas, Stephen, *Galileo, Bruno and the Rhetoric of Dialogue in Seventeenth-century Natural Philosophy*, 405
- Collective Construction of Scientific Memory, The, by Yves Gingras, 75
- Cooper, Alix, *Inventing the Indigenous*, rev. by Emma Spary, 369
- Crozier, Ivan, *Pillow Talk: Credibility, Trust and the Sexological Case History*, 375
- Ernst Haeckel and the Theory of the Cell State, by Andrew Reynolds, 123
- Fragmentation of Renaissance Occultism and the Decline of Magic, The, by John Henry, 1
- Galileo, Bruno and the Rhetoric of Dialogue in Seventeenth-century Natural Philosophy, by Stephen Clucas, 405
- Gavroglu, Kostas, and colleagues, *Science and Technology in the European Periphery*, 153
- Gay, Hannah, *Science, Scientific Careers and Social Exchange in London*, 457
- Ghost of Rostow, The: Science, Culture, and the British Industrial Revolution*, by William J. Ashworth, 249
- Gingras, Yves, *The Collective Construction of Scientific Memory*, 75
- Gooday, Graeme, *Liars, Experts and Authorities*, 431
- Henry, John, *The Fragmentation of Renaissance Occultism and the Decline of Magic*, 1
- Herschel Partnership, The, Caroline Herschel's Autobiographies, and The Herschels of Hanover*, by Michael Hoskin, rev. by Allan Chapman, 365
- Hoskin, Michael, *The Herschel Partnership, Caroline Herschel's Autobiographies, and The Herschels of Hanover*, rev. by Allan Chapman, 365
- Intellectual Property and Narratives of Discovery/Invention, by David Philip Miller, 299
- Inventing the Indigenous*, by Alix Cooper, rev. by Emma Spary, 369
- Last Word, The: John Wallis on the Origin of the Royal Society, by Jason M. Rampelt, 177
- Liars, Experts and Authorities, by Graeme Gooday, 431
- Life in the Liquid Fields: Kepler, Tycho and Gilbert on the Nature of the Heavens and Earth*, by Patrick J. Boner, 275
- Lodestones and Gallstones: The Magnetic Iatrochemistry of Martin Lister (1639–1712), by Anna Marie Roos, 343
- Miller, David Marshall, *The Thirty Years War and the Galileo Affair*, 49
- Miller, David Philip, *Intellectual Property and Narratives of Discovery/Invention*, 299
- Miller, Gordon L., *Beasts of the New Jerusalem*, 203
- Notes on Contributors, 122, 247, 373, 499
- Notices of Books, 115, 245, 371, 497
- Pillow Talk: Credibility, Trust and the Sexological Case History*, by Ivan Crozier, 375
- Rampelt, Jason M., *The Last Word: John Wallis on the Origin of the Royal Society*, 177
- Reynolds, Andrew, *Ernst Haeckel and the Theory of the Cell State*, 123
- Roos, Anna Marie, *Lodestones and Gallstones: The Magnetic Iatrochemistry of Martin Lister (1639–1712)*, 343
- Science and Technology in the European Periphery*, by Kostas Gavroglu and colleagues, 153
- Science, Scientific Careers and Social Exchange in London*, by Hannah Gay, 457
- Spary, Emma, rev. of *Inventing the Indigenous*, by Alix Cooper, 369
- Thirty Years War and the Galileo Affair, The*, by David Marshall Miller, 49