Trust, Protocol, Gender, and Power in Interwar British Biomedical Research: Kathleen Chevassut and the “Germ” of Multiple Sclerosis

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ABSTRACT. In March 1930, reports of the discovery of an organism causative of multiple sclerosis circulated in the British press. At the same time, news of a therapeutically efficacious vaccine also reached the ears of neurologists and patients afflicted with the debilitating degenerative disease. It was soon shown that no organism had been discovered. The events leading up to this ultimately painful episode reveal many of the central problems created when social conventions and a sense of decorum scripted received understanding of good scientific practice rather than actual regulatory frameworks. In the absence of such frameworks, few means were present to censor inappropriate scientific conduct. This story thus provides a window into an emergent world of state-sponsored biomedical research; a world where recrimination, gossip, misogyny, uncertainty, exaggeration, and dreams and delusions of scientific and therapeutic progress were collapsed together. **KEYWORDS:** multiple sclerosis, neurology, clinical research, women in science, women in medicine, science—sociology, vaccination, clinical trials—history, therapeutics—history, ethics—history.

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“**B** RILLIANT girl scientist,” trumpeted *The Star* newspaper in March 1930, “has just discovered a germ which may be the cause of nervous paralysis.”¹ “Naughtily-minded” was the phrase T. R. Elliott (1877–1961) the Professor of Medicine at University College London (UCL) used in a letter to surgeon Harvey Cushing when he alternatively described Kathleen Chevassut (1897–1985), the girl scientist.² “A very timid, retiring soul who has, by accident, recently discovered the cause of disseminated sclerosis” was how Grafton Elliott Smith (1871–1937) the Professor of Anatomy at UCL characterized her to Daniel P. O’Brien, the Rockefeller Foundation’s European Authority on Medical Science. O’Brien, for his part, heard contemptuous opinions from a professor in Utrecht against Sir James Purves-Stewart, Chevassut’s one-time supervisor.³ On Purves-Stewart, the Edinburgh neurologist Edwin Bramwell could only muster in his private dairy, “Scandalous. What can one do with a man like this?”⁴

These contrasting statements, from both contemporary and retrospective records, capture the ambiguities of this story. While early sources aggrandized Kathleen Chevassut, and foregrounded her gender as part of the important details of her “discovery,” later ones painted a picture of Chevassut’s findings as wrong, even “Fake!” as some allege in more recent sources.⁵ Comments like those by UCL neurologist Francis Walshe brimmed with misogynistic currents at her “feminine charm and inconsequence” which he took to be fatal endowments when combined with “a light-hearted disregard

1. Press Clipping in File, Anon., *The Star*, 19 March 1930, folder FD1/2746, National Archives Kew Garden (hereafter NA).

2. Letter from T. R. Elliott to Harvey Cushing, 14 November 1932, The Harvey Williams Cushing Papers in the Yale University Library, Manuscripts and Archives, Yale University Library. Microfilms Series II, Box 25, 469, Microfilm Reel 22.

3. Diary of Daniel P. O’Brien, entries entitled “Prof. G. Elliot Smith. Greetings and Gossip on matters Indian”; “Dr. and Mrs C F Harris for dinner and supper”; “Prof. Bowman—at the Institute of Neurology and Psychiatry, Utrecht, June 19th 1930,” Rockefeller Archive Centre, Sleep Hollow, New York, USA (hereafter RAC).

4. Diary of Edwin Bramwell, 13 October 1934, 40–41, Vol. 1 [Private Collection].

5. Norman Howard-Jones, “Fake!” *Br. Med. J.*, 1982, 284, 511; on women in medicine, see: *Women Physicians and the Cultures of Medicine*, ed. Ellen S. More, Elizabeth Fye, and Manon Parry (Baltimore: Johns Hopkins University Press, 2009); *Women, Science, and Myth: Gender Beliefs from Antiquity to Present*, ed. Susan V. Rosser (Santa Barbara, California: ABC-Clio, 2008); Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven; London: Yale University Press, 1985), especially 67–94.

of accuracy.”⁶ Girl, naughty, timidity, feminine—the language of gender difference in these records reveals socially significant hierarchies of dominance.⁷ Then again, the derision directed at Chevassut was also frequently accompanied with condemnatory remarks about her one-time supervisor and collaborator, and powerful establishment figure James Purves-Stewart. Thus, while in the more critical comments of the period, Chevassut’s gender served as a powerful tool for discrediting her work, less clear is whether Chevassut was the intended target or rather a useful tool for delegitimizing a more powerful figure—one who had lost the respect and trust of his peers in medicine but still possessed the social and cultural capital requisite for distinction in interwar British society.⁸

Recently discovered papers, letters, reports, as well as other archival documents from this episode justify such a re-appraisal. This is not because these sources vindicate Chevassut, who it seems clear exaggerated the importance of her work and research, but because they make manifest many of the significant social and political problems caused in the absence of regulatory standards in Britain governing over what historian Harry Marks has termed for the United States “the progress of experiment.”⁹ Gender as a category of analysis, Joan W. Scott has written, produces meanings that cannot be “easily assumed or translated.” In historical studies of science and medicine, asymmetries of power between men and men, and men and women, allow for active exploration of those meanings in the context of research.¹⁰ Such a contemporary reading of Chevassut’s story reveals that the subsequent formation of

6. Francis Walshe to Bernard Halley Stewart, 30 March 1931, folder FD 1/2750 Chevassut V, NA.

7. Aki Uchida, “When ‘Difference’ Is ‘Dominance’: A Critique of the ‘Anti-Power-Based’ Cultural Approach to Sex Difference,” in *The Feminist Critique of Language: A Reader*, ed. Deborah Cameron (London and New York: Routledge, 1998), 280–92.

8. On social and cultural capital, see Pierre Bourdieu, *Homo Academicus* (Cambridge: Polity Press, 2001); idem, *Distinction: A Social Critique of the Judgment of Taste* (Cambridge, Massachusetts: Harvard University Press, 1987).

9. Harry Marks, *The Progress of Experiment: Science and Therapeutic Reform in the U.S., 1900–1990* (Cambridge: Cambridge University Press, 1997); also see *The Right Tools for the Job: At Work in the Twentieth-Century Life Sciences*, ed. Adele E. Clark and Joan H. Fujimura (Princeton, New Jersey: Princeton University Press, 1992).

10. Joan W. Scott, “AHR Forum: Unanswered Questions,” *Am. Hist. Rev.*, 2008, 113, 1422–29, 1429; idem, *Gender and the Politics of History* (New York: Columbia University Press), 28–50.

regulatory standards in Britain ultimately brought with it protections for workers in science, and that by corollary their absence, *the focus of this essay*, entailed vulnerabilities.¹¹ At the same time, the sources of this story make it clear that in the absence of regulatory frameworks, deeply gendered constructions such as communally held notions of trust and social respectability, like those described in an earlier period for science by Steven Shapin, continued even in interwar Britain to underwrite commonly accepted practices in biomedical research.¹² To explore these issues, this essay proceeds from a consideration of the circumstances of interwar neurology and medical research, to a biographical analysis of the central figures involved in this story, a description of the events surrounding Chevassut's multiple sclerosis (MS) research, and it concludes with a consideration of the implications.

MEDICAL RESEARCH AND INTERWAR NEUROLOGY IN BRITAIN,
1919–39

Historians now consider the First World War to have transformed the social relationships between physicians, medical researchers, and the British State. The Medical Research Council, founded in 1919 from its wartime predecessor the Medical Research Committee and headed by Walter Morley Fletcher, became the chief institution "shaping a national system" of biomedical research that emerged in interwar Britain, and a significant agent in the testing and production of drugs and vaccines following the passing of the Therapeutic Substances Act of 1925.¹³ The emergence of this body coincided with the simultaneous pattern of the emergence of autonomous specialist departments in hospitals and clinical research units. Areas of clinical practice such as neurology saw significant increases in the number of inpatient beds and outpatient clinics allocated wholly to

11. *The "Racial" Economy of Science: Towards a Democratic Future*, ed. Sandra Harding (Bloomington: Indiana University Press, 1993).

12. Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago and London: University of Chicago Press, 1994); *Knowledge as Social Order*, ed. Massimo Mazzotti (Aldershot and Burlington, Vermont: Ashgate Publishing Company, 2008); Julia Penelope, *Speaking Freely: Unlearning the Lies of the Fathers' Tongues* (New York: Teachers College Press, 1990), 36–54.

13. Joan Austoker and Linda Bryder, eds., *Historical Perspectives on the Role of the MRC: Essays in the History of the Medical Research Council of the United Kingdom and Its Predecessor, the Medical Research Committee, 1913–1953* (Oxford and New York: Oxford University Press, 1989), v.

cases of nervous disease. With these increases in dedicated spaces came also a variety of junior and senior appointments for neurologists. The numbers of students and postgraduates entering the field steadily climbed, and with these increases in numbers came a corresponding upward shift in the numbers of papers published on neurological topics, which appeared in new venues such as the *Journal of Neurology and Psychopathology*, established in 1919.¹⁴

The practices of neurology epistemologically and therapeutically also changed in the interwar period, and the constant flow of dollars from the Rockefeller Foundation for clinical research with a biological emphasis ensured that these changes were mediated globally for neurology.¹⁵ Thus where prior to the interwar period neurologists had tended to focus on diagnostic evidence gathered directly from their patients' bodies, the interwar period saw the convergence of their empirical clinical knowledge with the increasingly reductive inclusion of biochemical, histological, physiological, and pathological terms of reference. In other words, neurologists globally, but especially in Britain and the United States, had begun reformulating their conceptions of numerous neurological conditions into physiological terms.¹⁶ Such new intellectual frameworks possessed great technical specificity. Neurological disorders that might have been described in the nineteenth or early twentieth century in purely gross anatomical and pathological languages became circumscribed, as Max Stadler has described, in the reductive languages of synaptic transmission, the normal and pathological properties of cellular membranes, and microscopic lesions.¹⁷

Despite this new formulation of neurological disease, many practitioners, including more than a few neurologists, viewed their field as therapeutically destitute.¹⁸ Neurologists diagnosed esoteric diseases with skill and finesse, yet many physicians considered

14. Stephen T. Casper, *The Idioms of Practice: British Neurology, 1880–1960* (Doctoral Thesis, University College London, 2006, 1–101).

15. For example, see Stephen T. Casper, "Atlantic Conjunctions in Anglo-American Neurology: Lewis H. Weed and Johns Hopkins Neurology," *Bull. Hist. Med.*, 2008, 82, 646–71.

16. Lewis P. Rowland, *The Legacy of Tracy J. Putnam and H. Houston Merritt: Modern Neurology in the United States* (Oxford and New York: Oxford University Press, 2008).

17. Max Stadler, *Assembling Life. Models, the Cell, and the Reformations of Biological Science, 1920–1960* (Doctoral Thesis, Imperial College London, 2009).

18. I draw upon the excellent work of Delia Garvis here, especially her generously provided paper "Men of Dreams and Men of Action: Neurologists, Neurosurgeons, and the

neurologists’ talents in the art of medicine not paralleled by any similar achievements in therapy. Other than the development of the Wasserman test, a laboratory-based examination of the concentration of protein in cerebrospinal fluid, and the introduction of Salvarsan to treat syphilis in 1910, few new technologies or treatments had proved beneficial to neurologists. Only pyrotherapy, the treatment of syphilis by the deliberate induction of malarial fever, had shown marked success in the care of nervous and mental patients.

Part of the challenge in diagnosis and treatment of many neurological conditions lay in discovering their etiology.¹⁹ MS was one such condition. The disease—then known as disseminated sclerosis—was especially difficult to diagnose.²⁰ As early as 1881, the famous French physician Jean-Martin Charcot had noted that the condition bore strong similarities with other nervous disorders like locomotor ataxia, symmetrical sclerosis of the lateral columns, tabes dorsalis, and amyotrophic lateral sclerosis.²¹ Its geographic epidemiology presented a mystery. Although endemic throughout the globe, it was discovered that the number of people affected by the condition decreased with proximity to equatorial regions. Furthermore, the signs and symptoms of MS waxed and waned, causing uncertainty as to whether treatments were effecting cures or alleviating symptoms, or whether alternatively the disease was merely running its natural course.²² By 1919, many physicians felt that a microscopic organism caused the condition—a virus or

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Performance of Professional Identity, 1925–1950,” forthcoming in the *Bulletin of the History of Medicine*.

19. For an interesting discussion, see Arnold Edwards, *Disseminated Sclerosis* (Manchester: John Heywood, Deansgate and Ridgefield, 1895); he wrote on p. 19, “In disseminated sclerosis we find a diffused and vague picture, because the disease does not do enough to harm in any one part of the brain or spinal cord to destroy entirely its functions; but it teases and weakens various parts of the central nervous system, so that we have a great number of symptoms mixed up together.”

20. Colin L. Talley, *A History of Multiple Sclerosis* (Westport, Connecticut; London: Praeger, 2008).

21. Jean Martin Charcot, *Lectures on the Diseases of the Nervous System*, translated and edited by George Sigerson; with an introduction by Walter Reise, *History of Medicine Series 19* (1873; repr., New York: Hafner Publishing Company, 1962). See especially 7, 27, 235, 247.

22. The problem of uncertainty no doubt compounded with concerns about placebo effects. For a general discussion, see: Harry Collins and Trevor Pinch, *Dr. Golem: How to Think about Medicine* (Chicago and London: University of Chicago Press, 2005), 18–34.

germ—and, accordingly, as historian Katrina Gatley has described, physicians at London's leading center for neurology, the National Hospital for Nervous Diseases, Queen Square, had begun using fever therapies to treat the disorder.²³

A CAST OF CHARACTERS: THE BIOGRAPHICAL CONTEXT

Among the many physicians treating patients with MS in this way at the National Hospital in the late-1920s was Edward Arnold Carmichael (1896–1978). Carmichael, then also working at St. Bartholomew's Hospital, had been educated at the Edinburgh Academy. He had subsequently received his Bachelor of Medicine and Bachelor of Surgery degrees from Edinburgh University in 1921, where he had achieved academic distinction. In 1923, Carmichael had begun a nine-month training program in pathological and histological investigations under James Godwin Greenfield (1884–1958), pathologist at the National Hospital. Appointed Junior House Physician there in that same year, he quickly moved up the ranks to Resident Medical Officer by January 1924 and by 1926 held a post as Senior Medical Registrar.²⁴

Carmichael was a rising star, and there were many individuals pushing his advancement. Early on, it is important to note, Carmichael had made clear his intentions to pursue a career devoted to clinical research. Being young, highly trained, and well regarded, he exemplified the characteristics then being avidly recruited by funding bodies like the MRC and the Rockefeller Foundation. Powerful figures within the British medical establishment, men such as Francis Fraser and Thomas Lewis, had made representations on Carmichael's behalf to those agencies. By 1927, consequently, many people and forces were converging on Carmichael's destiny. For figures like Fraser and Lewis, Carmichael epitomized the young academic clinical researcher they desired to see established in universities and hospitals throughout Britain. In the same way, Walter Morley Fletcher would come to regard

23. Katrina Gatley, "When Book Medicine Meets Patient," Work-in-Progress Seminar, 17 May 2006, Wellcome Trust Centre for the History of Medicine at University College London, London.

24. Enclosure to Sir Walter Fletcher from Edward Arnold Carmichael, 1932, Development of the Research Unit at Queen Square, folder FD 1/2413, NA.

Carmichael as archetypical of the talent he wished the Medical Research Council to be seen recruiting.

In the same period as Carmichael’s investigations of fever therapy for MS were beginning, several individuals working across the city at the Westminster Hospital had begun investigating whether an organism could be isolated from the cerebrospinal fluid of patients with the condition. Kathleen Chevassut (1897–1985), James Purves-Stewart, then Consultant neurologist at the Hospital, and J. A. Braxton Hicks, the Director of the Westminster’s John Burford Carlill Pathological Laboratories were the central figures involved in this new research.²⁵

To us, Kathleen Chevassut appears different in many respects from Carmichael. From the few historical sources extant about her early life, it is possible to determine that she was the daughter of Frederick Chevassut, a Clerk in the Anglican Church and a Vicar in Blackburn, where Kathleen was born in 1897.²⁶ Her school records suggest that her parents urged her from early age to achieve academically.²⁷ Around 1916, when her father enrolled Chevassut at Bedford College, he expressed concern that his daughter’s education have practical value.²⁸ He consequently urged the Principal of Bedford College to steer his daughter away from her interest in mathematics, because “unless a person is a genius” he deemed it a difficult career.²⁹

25. J. G. Humble and Peter Hansell, *Westminster Hospital, 1716–1966* (London: Pitman Medical Publishing, 1966), 98.

26. Relatively little information about Kathleen Chevassut’s early life is available. While I found no information about her later life, I did succeed in having a phone conversation with one of her nephews. At the time when I was conducting this research, he was in his seventies. Sadly, he knew almost nothing about her. He could only tell me that he felt certain she had never married or had children. Form of Entrance (Bedford College for Women), folder BC AR200/1 1985 Kathleen Chevassut Archives, Royal Holloway, University of London (hereafter RHUL).

27. Gardner to Tuke, 21 April 1916, folder BC AR200/1 1985 Kathleen Chevassut, RHUL.

28. Frederick’s involvement in Kathleen’s education was typical for the period. The age of majority was twenty-one. What is interesting is that Frederick wanted her to be able to earn a living, which I take as a sign of how much the war in Europe was changing circumstances for women in British society. On this point, see: Lesley A. Hall, “‘Not a Domestic Utensil but a Woman and a Citizen’: Stella Browne on Women, Health, and Society,” in *Regenerating England: Science, Medicine, and Culture in Interwar Britain*, ed. Christopher Lawrence and Anna-K. Mayer (Amsterdam: Rodopi, 2000), 275–302, 276–77.

29. F. Chevassut to Tuke, 1 October 1916, folder BC AR200/1 1985 Kathleen Chevassut, RHUL.

Thus, when she entered Bedford College in October 1916, Chevassut began studying general science and medicine, one of three women in her course at the time.³⁰ She failed her exams in 1917 but passed them in 1918. She began her medical bachelor's degree (M.B.) that year, graduating in 1922.³¹ That same year she also completed a required University of London course in physiology with an emphasis on human anatomy and morphology, receiving a B.Sc. with a distinction.³² Subsequently she applied to Westminster Medical School, where she entered in autumn 1922.³³ Thereafter, the details of Chevassut's career as a medical student become less clear. Later letters from her life reveal that she never qualified in medicine, though she appears to have continued training there until 1925 and received her M.Sc. Degree with Honors in Physiology in 1926.³⁴ J. A. Braxton Hicks and James Purves-Stewart supervised her graduate research, which was a sophisticated biochemical analysis of cerebrospinal fluid. This research was published in the *Quarterly Journal of Medicine*.³⁵ For it, Westminster Hospital awarded Chevassut the Thomas Smyth Hughes Medical Research Prize in 1927, and she received a brief mention in *The Times*.³⁶

Why Chevassut remained medically unqualified is curious and probably has everything to do with the circumstances of women's medical education in the late 1920s and 1930s London. The only direct evidence on this point from her life appears in a long undated letter written by her brother, Frederick, sometime in late 1930 or early 1931 to Walter Fletcher at the MRC. In this letter,

30. Index Card in File (last updated 1983), folder BC AR200/1 1985 Kathleen Chevassut, RHUL; also see the later comments made by Grafton Elliot Smith to Tuke, 5 December 1929, folder BC AR200/1 1985 Kathleen Chevassut, RHUL; for the context of women in medicine, see Mary Ann Elston, "Run by Women, (mainly) for Women": Medical Women's Hospitals in Britain, 1866–1948," in *Women in Modern Medicine*, ed. Anne Hardy and Lawrence Conrad (Amsterdam: Rodopi, 2001), 73–107.

31. Index Card in File (last updated 1983), folder BC AR200/1 1985 Kathleen Chevassut, RHUL.

32. Read to Tuke, 23 March 1922; Watson to Tuke, 16 May 1922; Tuke to Watson, 25 May 1922, folder BC AR200/1 1985 Kathleen Chevassut, RHUL.

33. Kathleen Chevassut to Tuke, 23 November 1922, folder BC AR200/1 1985 Kathleen Chevassut, RHUL.

34. Walter Fletcher to Kathleen Chevassut, 11 August 1928, folder FD 1/2746, NA.

35. Kathleen Chevassut, "Glycolysis in Cerebro-spinal Fluid and Its Clinical Significance," *Quart. J. Med.*, 1927–28, 21, 105.

36. "Medical Training, the Place of Science," *Times (London)*, 4 October 1927, 11.

Frederick Chevassut claims that her supervisor, James Purves-Stewart, had told her to give up pursuing medicine "because she would find no money there" and continue in research, a career better suited to her talents.³⁷ It is difficult to know why or even if Purves-Stewart gave this advice, but it seems not out of character, especially given comments in his autobiography described below. Generally circumstances for women medical students in interwar London had become precarious since the First World War. During the War, the London Medical Teaching Hospitals, seeking to replace missing men and bolster admissions, had opened their doors to women students, and by the end of the war the number of trained women doctors in Britain was higher than it had ever been. Yet by the time Chevassut had entered Westminster medical school in 1922, circumstances for women had begun taking a substantial departure. With the war over, one school after another had closed admissions to women, yielding to the pressure of male students. First the London Hospital in 1922, and then St. Mary's closed to women in 1924. By 1928, Charing Cross and Westminster Medical Schools had shut their doors as well, and even the comparatively liberal University of London had introduced a strict quota system.

Historian Carol Dyhouse identified multiple causes for this transition away from admitting women medical students:

The outlook for women seeking clinical experiences in London, in particular, looked bleaker, in many ways in the late 1920s and in the 1930s than it had appeared in 1918. These were years in which a "culture of masculinity" based on public school values, rugby and athleticism, and buttressed by freemasonry, operated to the disadvantage of women, and even those women who had distinguished themselves as students frequently found difficulty in obtaining hospital appointments of lucrative positions in general practice.³⁸

37. F. G. Chevassut to Walter Fletcher, c. December 1930 or Jan 1931, folder FD 1/2748 Chevassut III, NA.

38. Carol Dyhouse, "Driving Ambitions: Women in Pursuit of a Medical Education, 1890-1939," *Women's Hist. Rev.*, 1998, 7, 321-43, 337. The literature on the difficulties for women doctors in interwar London is extensive. See also Leah Leneman, "Medical Women at War, 1914-1918," *Med. Hist.*, 1994, 38, 160-77; Carol Dyhouse, "Women Students and the London Medical Schools, 1914-39: The Anatomy of a Masculine Culture," *Gender Hist.*, 1998, 10, 110-32; J. F. Geddes, "The Doctors' Dilemma: Medical Women and the British Suffrage Movement," *Women's Hist. Rev.*, 2009, 18, 203-18.

The views of Chevassut's supervisor apparently exemplified these trends. Of James Morgan Purves-Stewart, it is difficult to write sympathetically. Much of that difficulty derives from the fact that few had anything nice to say about him retrospectively. Even his obituaries harbor unkindness: one author of his *Lancet* obituary wrote, for example, that "his virtues had their defects: his manifestations of vitality could sometimes be interpreted as self-advertisement, and his almost exaggerated patriotism exposed him to a charge of vanity."³⁹ Macdonald Critchley (1900–97), neurologist and early historian of British neurology, once recalled the almost certainly apocryphal anecdote that when Purves-Stewart was a student, his teacher William Gowers had told him "to take up skins" and then get lost in the provinces.⁴⁰ He became, as Critchley recalled later but without explanation in an address before the Association of British Neurologists, "persona non grata" in British neurology.⁴¹

Purves-Stewart's contemporary, Edwin Bramwell, regarded him as a friend (albeit one best kept at a distance). Bramwell recalled how he antagonized his peers:

He was never popular with his colleagues and the reason was, I think, well expressed by Sir Thomas Walker . . . [who] . . . remarked that he and Purves Stewart had graduated at Edinburgh in the same year, that even in his student days "Purves Stewart was a marked man and that if he had taken up surgery he would have been Purves Stewart, KCMG, CB, FRCS. But, he said, Purves Stewart has missed his vocation for he should have been head of a great business concern" . . . Purves Stewart had to advertise, it was, I think, part of his upbringing.⁴²

It was thus not his genius that concerned his peers—it was his vulgarity. Bramwell mentions in his diary another occasion when

39. "James Purves Morrison Purves-Stewart, K.C.M.G., C.B., M.A., M.D., Edin., F.R.C.P.," *Lancet*, 1949, 2, 1122.

40. "Gordon Holmes: The Man and the Neurologist," in *The Divine Banquet of the Brain and Other Essays*, ed. Macdonald Critchley (New York: Raven Press, 1979), 228–34, 232.

41. Macdonald Critchley, "50th Anniversary Dinner Party of the Association of British Neurologists," folder "Origins," Archives of the Association of British Neurologists, London, Queen Square.

42. Edwin Bramwell, unpublished manuscript, *Rough Notes and Recollections 1945: Sir James Purves-Stewart KCMG CB MD FRCP*, 54–55 [Private Collection].

Purves-Stewart had proposed leaving a substantial portion of his estate to Edinburgh University for research scholarships and fellowships in neurology. Bramwell wrote in his diary "when one is dealing with a man of Purves-Stewart's make-up, one cannot help thinking of motives" and he went on to speculate that Purves-Stewart was vying for an honorary doctorate.⁴³

Despite this unflattering image, there was much to admire about Purves-Stewart. On more than one occasion, Bramwell defended him to other neurologists, and noted his remarkable endurance, "untiring brain," and retention of minute neurological details.⁴⁴ In Edinburgh, Purves-Stewart's successes as a student led to medals in chemistry, pathology, anatomy, medicine, and surgery. He was the Vans Dunlop Scholar, and won the Ettles Scholarship as the most distinguished Edinburgh graduate of 1894.⁴⁵ Following a House Appointment at Edinburgh Royal Infirmary under Thomas Grainger Stewart (1837–1900), he studied at the University of Jena and then proceeded to a residency at the National Hospital, Queen Square.⁴⁶ Subsequently, he was appointed to the Westminster Hospital at age twenty-nine, and shortly thereafter to the staffs of the West End Hospital for Nervous Diseases and the Royal National Orthopaedic Hospital. He became a Fellow of the Royal College of Physicians (FRCP) in 1906.⁴⁷

A military man and an expressive patriot, Purves-Stewart was involved in every war that occurred in his adult life (including at the age of seventy-two, the Second World War). He received many military honors, including Knight Commander of the Victorian Order (KCVO) and an honorary lifelong rank of Colonel. Moreover, Purves-Stewart apparently conversed ably in French, German, Italian, and Spanish. He was an incessant traveler and published a popular book titled *A Physician's Tour in Soviet Russia* (1933). Similar popular works followed, including: with the surgeon Charles Ballance, *Robert*

43. Edwin Bramwell, Diary Entry, 13 October 1934, 40–41, Vol. 1 [Private Collection].

44. Edwin Bramwell, Diary Entry, 161, Vol. 2 [Private Collection].

45. "James Purves Morison Purves-Stewart, K.C.M.G., C.B., M.A., M.D., Edin., F.R.C.P.," *Lancet*, 1949, 2, 1122.

46. Edwin Bramwell, unpublished manuscript, *Rough Notes and Recollections 1945: Sir James Purves-Stewart KCMG CB MD FRCP*, 54–55 [Private Collection].

47. "Sir James Purves-Stewart, Authority on Nervous Diseases," *Times (London)*, 16 June 1949, 7.

Burns—a *Medical Aspect* (1935); his own autobiography, *Sands of Time* (1939); and the patriotic volume *Over Military Age* (1942).⁴⁸ Few, and least of all those competing with him in neurological practice, were much impressed with his literary output. His erudition always appeared exasperatingly megalomaniac and aggrandizing in the way it represented his achievements.

Purves-Stewart's views of women are most difficult to decipher, but as alluded to above, they seem to have mirrored trends in inter-war Britain. In his autobiography, only one passage gives much away, and even this hardly qualifies as decisive.⁴⁹ Women, he noted, had been brought into the London medical schools during the First World War, mainly because almost all of the able-bodied men were fighting in the trenches. When the men returned, he felt it quickly became evident that women could not compete economically with men and were thus unable to establish themselves in positions commensurate with their abilities in either private practice or public posts. Accordingly, they were not encouraged to come to medical school, and in fact, he noted, many of the London medical schools had closed their doors to them:

This was not due to any academic educational deficiency, for, in fact, women students gained a large proportion of the competitive prizes. As a matter of experience... we teachers observed that, whereas 100 percent of the men students who succeed in qualifying remain for life in the medical profession, some 50 percent of the women leave the profession of medicine within a few years after graduation, to take up the superior calling of matrimony, usually a whole time job. Moreover... it is usually the ablest and best-qualified women (likewise the most attractive) who secure husbands, leaving the residue to compete as best they can with the 100 percent of men who stick to their profession for life. It is therefore uneconomic for medical teachers to train a mixed class to a high pitch of efficiency knowing that many of the women students, often the most brilliant, are unlikely to persevere with their medical

48. James Purves-Stewart, *A Physician's Tour in Soviet Russia* (London: G. Allen & Unwin Ltd., 1933). Similar popular works followed, including: with the surgeon Charles Ballance, *The Immortal Memory of Robert Burns—a Medical Aspect* (London: Matthew Drews & Company, 1935); his own autobiography, *Sands of Time, Recollections of a Physician in Peace and War* (London: Hutchinson & Co. Ltd., 1939); and the patriotic volume *Over Military Age: A Wartime Commentary on the First Two Years* (London: G. Allen & Unwin Ltd., 1942).

49. Purves-Stewart, *Over Military Age*, 240–44.

career. There are other drawbacks, such as the athletic hospital activities, unsuitable for women, also the distractions of continuous association during working hours of attractive young men with attractive young women in classrooms, laboratories, and wards.⁵⁰

Obviously, to Purves-Stewart, competence and attractiveness seem to have gone hand in hand. The "residue," by which he had apparently meant neither competent nor pretty, would not be able to compete against their male colleagues. Underwriting his position was a meritocratic orthodoxy that spoke to a deeper hierarchy of power. There was thus an edge to his critique and advocacy of women physicians. Having described matrimony as woman's superior calling, he was making in his autobiography the *de facto* and in any case rhetorically tautological claim that women rejecting matrimony for medicine were not superior at all.

Yet it seems best to reserve our judgment about him—despite his unpleasant notions and the general contempt in which others held him. The contingencies in which he and Chevassut (and their research) were located were many and are difficult to understand now and their agency in these matters, like Edward Carmichael's, appears highly questionable. Many of the obstacles and turmoil they all confronted, resulted more from structural ambiguities in clinical research, the structure of neurology, and the broader forces slowly rationalizing British medical research and practice. More important for the purpose of this essay is that Carmichael and Chevassut both emerge initially to us as individuals with promising careers and each with advocates in 1927. Despite their similarities (sex and medical qualification notwithstanding), it is clear that both were on different trajectories and had different relationships with their mentors and patrons. In many respects, those differences were all that mattered. Chevassut was offered support, usually half-hearted, from the male scientists who surrounded her. Far from advancing her career, their support, paternalistic as it generally seems now, often undermined their confidence in her work. In addition, their support may well have contributed to the rosy gloss of over-confidence that appeared in her letters and ultimately led to the signal discrepancies in understanding that came to exist between her and her advisers, who she felt showed over-

50. Ibid., 241–42.

confidence in her findings. Carmichael was not in an entirely different situation. Although he had the advantages of his gender, and thus did not suffer under overbearing paternalistic sentiment, his career seems not to have blossomed in quite the way that his advocates might have hoped. Indeed, some of his advocates became decidedly unimpressed by his achievements by the late 1930s, but that was only after he had reaped the rewards of Chevassut's failures.

SCIENTIFIC DISCOVERY AND THE GERM OF MS

Kathleen Chevassut first came to the attention of the MRC in 1927, as the result of a letter from her former Anatomy Professor, Grafton Elliot Smith, to Walter Fletcher. In his letter, Smith described Chevassut's abilities.⁵¹ Noting that she was unaware that he was writing on her behalf, he attached an outline Chevassut had sent him regarding her next research project, a project hitherto only supported by Chevassut's former supervisor and sometimes patron Purves-Stewart.⁵² The enclosed document stated:

I [Chevassut] considered it useless to employ ordinary bacteriological methods in my attempt to throw light on the aetiology of [disseminated sclerosis]. I therefore attacked the problem from an entirely new point of view and employed methods and technique, which up to the present time have not been used in work on Disseminated Sclerosis. The results have been most encouraging, but there is an immense amount of work to be done before one can speak with certainty on the subject. The aim of my present research is, of course, to identify with certainty the causative organism and ultimately if possible, produce an anti-serum.⁵³

Chevassut's proposal was well within the mainstream of scientific research.⁵⁴ Using a protocol first pioneered by Joseph Edwin Barnard

51. Grafton Elliot Smith to Walter Fletcher, 7 October 1927, folder FD 1/2746 Chevassut I, NA.

52. "A request from Sir James Purves-Stewart that I should investigate the problem of the causation of disseminated sclerosis was responsible for the initiation of this research." Kathleen Chevassut, "The Aetiology of Disseminated Sclerosis," *Lancet*, 1930, 1, 552-60, 560.

53. Kathleen Chevassut to Grafton Elliot Smith, 5 October 1927, folder FD 1/2746 Chevassut I, NA.

54. In his 1955 textbook on disseminated sclerosis, Douglas McAlpine noted that between 1910 and 1939, there were approximately fifty studies in Britain, France, Germany, and the United States similar to those proposed by Chevassut. Douglas McAlpine, Nigel Compston, and Charles E. Lumsden, *Multiple Sclerosis* (Edinburgh and

(1870–1949) in a 1925 study of filterable viruses associated with malignant growths, Chevassut proposed using an ultra-fine filtration assay to purify the cerebrospinal fluid of patients with disseminated sclerosis.⁵⁵ Her hypothesis was that fine filtration would remove proteins and other contaminants detrimental to the growth of any residual organism. Upon removing those contaminants, she hypothesized that an organism could be cultured on a nutritious medium. In her preliminary tests, Chevassut claimed to have seen "spherules" that changed shape, possibly indicating an organism with a life cycle. Joseph Barnard, she noted, had confirmed her observations.⁵⁶ Chevassut's work, in short, held out a real hope for a vaccine. Her lack of medical qualification, however, meant that she required the assistance of a physician.

After receiving Elliot Smith's letter, Fletcher consulted Joseph Barnard for his opinion of Chevassut's work. Admitting that he had advised Chevassut, Barnard recommended that Fletcher wait until "her work . . . is proceeding on sound lines."⁵⁷ By June of 1928, eight months following Fletcher's initial enquiry, Barnard recommended support. Fletcher asked him then specifically about the quality of her microscopic work.⁵⁸ Barnard stated, "so far as her microscope work is concerned, I have not seen a great deal of it but what I have seen has appeared to me to be carefully done."⁵⁹ Barnard's equivocal comment is important to note—it became one source of great misunderstanding to Chevassut, who took to citing him as a witness to her observations.⁶⁰ She reported, for instance, in

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London: E. & S. Livingston Ltd., 1955), 257–58; also see, "Neurological Therapeutics," *J. Neurol. Psychopathol.*, 1934, 14, 369–73; and A. Landsborough Thomson, *Half a Century of Medical Research*, 2 vols. (London: HMSO, 1973–75), 2: 114–17.

55. For the technique, see Joseph Barnard, "The Microscopical Examination of Filterable Viruses Associated with Malignant Growths," *Lancet* 1925, 1, 117–23; on Barnard, see "Obituary Mr J E Barnard," *Times (London)*, 26 October 1949, 7; on Barnard's work for the MRC, see Thomson, *Half a Century of Medical Research*, 2: 115.

56. See both: Kathleen Chevassut to Grafton Elliot Smith, 5 October 1927; Grafton Elliot Smith to Walter Fletcher, 7 October 1927, folder FD 1/2746 Chevassut I, NA.

57. Joseph Barnard to Walter Fletcher, 13 October 1927, folder FD 1/2746 Chevassut I, NA.

58. Walter Fletcher to Joseph Barnard, 25 June 1928, folder FD 1/2746 Chevassut I, NA.

59. Joseph Barnard to Walter Fletcher, 28 June 1928, folder FD 1/2746 Chevassut I, NA.

60. Kathleen Chevassut to Grafton Elliot Smith, 5 October 1927, folder FD 1/2746 Chevassut I, NA.

July 1928, "the whole research would have been non-existent but for all of [Barnard's] instructions and assistance. It is because I have had all this invaluable aid from him and that advantage of his expert knowledge in this field of research that it has been possible to carry out the research."⁶¹

Larger problems quickly became evident. Chevassut's position was professionally tenuous. An earlier letter from Barnard to Arthur Landsborough Thomson foreshadowed challenges at the Westminster Hospital:

The main difficulty I see in this matter is that Sir James Purves Stewart has been supplying her with a good deal of the apparatus she needs, and I think there is no doubt that he has very liberally supported her in this particular work on Disseminated Sclerosis. He appears, however, to regard this as a sufficient reason for appropriating the results of the work and, if I am rightly informed, is already making some use of them in his practice.⁶²

A letter from Elliot Smith to Fletcher stated: "the difficulty at the present moment is that she has neither the financial resources nor the scientific standing to be independent of others." Highlighting the professional security MRC support might provide Chevassut by granting external financial support, Smith added: "it is important that she be extricated from the vicious circle which prevents her from carrying on the strictly scientific investigation."⁶³ It soon emerged that Purves-Stewart had asked Chevassut to prepare vaccines against the organism, which he tested on his private patients.

With James Purves-Stewart making use of her research in his private practice, the uncertainty of Chevassut's professional circumstances, and Barnard's recommendation that she be supported, it became clear to Fletcher that some outside arbitrator was necessary. Consequently, in August 1928, the members of the council of the MRC allocated Chevassut support, providing her a salary but stipulating that she take the medical qualifying exam before the year ended (as described above, it is likely that the hospital had stopped

61. Kathleen Chevassut to Walther Fletcher, 19 July 1928, folder FD 1/2747 Chevassut II, NA.

62. Joseph Barnard to A. Landsborough Thomson, 5 May 1928, folder FD 1/2747 Chevassut II, NA.

63. Grafton Elliot Smith to Walter Fletcher, 7 June 1928, folder FD 1/2746 Chevassut I, NA.

granting women medical degrees but there are no records that demonstrate one way or the other).⁶⁴ In any case, Chevassut failed to qualify.⁶⁵ Acknowledging her failure (or more likely the impossible circumstances), Fletcher replied that, "The council have certainly not disowned you."⁶⁶ Barnard and Elliot Smith's apprehensions about the charged atmosphere at the hospital were quickly realized. Hitherto, the difficulties had been between Chevassut and the administrators and physicians working at the hospital, but with her new autonomy, tensions grew between the MRC and the administration at the Westminster Hospital.⁶⁷ The Chairman of the Hospital, Kenneth Wolfe Barry (1879–1936), objected to Landsborough Thomson at "Miss Chevassut being called "independent" when she had for years had so much help from the Hospital, direction from Dr [Braxton] Hicks, and [clinical] material from Sir James Purves-Stewart."⁶⁸ Later that year, Chevassut detailed to Fletcher how discontented the hospital administrators and physicians were with the situation, and noted that Purves-Stewart had "made the surprising suggestion that he should first see any report which I intend to make to [to the MRC] . . . on the work. I replied that I could not possibly agree to this and point out [*sic*] that any such report would concern the Medical Research Council and myself only. It seemed to me the most absurd suggestion."⁶⁹

Chevassut was nevertheless under pressure to publish, and she was fully dependent upon the authorities of the hospital and Purves-Stewart, the only physician then providing her with cerebrospinal fluid samples. The withdrawal of clinical material would have meant an end to her experiments. Nevertheless, Fletcher insisted that Chevassut think of herself as a free agent. He reminded

64. A. Landsborough Thomson to Joseph Barnard, 25 April 1928; Joseph Barnard to A. Landsborough Thomson, 7 May 1928; folder FD 1/2746 Chevassut I, NA; also Walter Fletcher to Kathleen Chevassut, 11 August 1928, FD 1/2747 Chevassut II, NA.

65. Kathleen Chevassut to Walter Fletcher, 30 November 1928, folder FD 1/2746, Chevassut I, NA.

66. Walter Fletcher to Kathleen Chevassut, 21 February 1929 in response to Kathleen Chevassut to Walter Fletcher, 19 February 1929, folder FD 1/2746 Chevassut I, NA.

67. I spent several days searching The Westminster Hospital's Archives in the London Metropolitan Archives and found no evidence pertaining to these administrative problems specifically.

68. Personal note from A. L. Thomson, c. Aug. 1929, folder 1/2746 Chevassut I, NA.

69. Kathleen Chevassut to Walter Fletcher, 30 November 1928, folder 1/2746 Chevassut I, NA.

her paternalistically that, “in science there is no priority in ideas but only in the production of scientific evidence.”⁷⁰ His remark ignored much. With her results unverified and unpublished, and a physician—ostensibly her former supervisor—using her hypothesis as justification to produce vaccines to treat desperate patients against an organism not yet proven to exist, Chevassut likely felt her situation keenly. Meanwhile her MRC salary—which represented most of her livelihood—was conditional upon results she was not certain she could even produce. Continual “failures” were all she reported to the MRC. Yet the Council renewed her grant in the spring of 1929.⁷¹

In the summer of 1929, the true extent of Purves-Stewart’s involvement finally became clear to the MRC. Fletcher observed in a note: “[Chevassut] has been supplying vaccines for those on the staff at Westminster and thinks this monopoly . . . most undesirable. She would like to widen the field of inquiry by getting in touch with men at Queen Square and elsewhere. She would welcome a properly organised inquiry, if feasible, into the value of this vaccine, conducted by suitably chosen clinicians at more than one centre.” He also noted, the “Westminster people were pressing her to publish and were making publication a condition of renewing her research studentship.”⁷² Shortly later, Chevassut outlined the reasons for her reluctance to publish to Fletcher:

I saw Mr Wolfe-Barry, the Chairman of the hospital today. . . . He asked me, could it be true that I had refused to have any association with Sir James Purves Stewart as regards the publication of the work. I said that the question had not yet actually arisen, but I understood that Dr Hicks had had a letter from Sir James asking him to collect the laboratory material together so that it could be published as a joint publication. . . . I said that I had not been directly spoken to on the subject at all but that certainly a joint publication was out of the

70. Walter Fletcher to Kathleen Chevassut, 1 January 1929, folder FD 1/2746 Chevassut I, NA.

71. Report on Research on the Etiology of Disseminated Sclerosis; A. L. Thompson to Kathleen Chevassut, 27 March 1929, folder FD 1/2746 Chevassut I, NA.

72. Joseph Barnard to A. L. Thomson, 5 May 1928. The record from Barnard’s letter reveals on-going difficulties of her position. See, for example: Personal Note in the same file from Fletcher, dated 11 July 1929, although numerous letters appear in between these dates (and also afterwards) explaining more fully the circumstances. Both documents in folder FD 1/2746 Chevassut I, NA.

question so far as I was concerned. I pointed out that in the first place I do not consider that the clinical side should be published at all and secondarily nothing would induce me to have my name associated with [Purves-Stewart's].⁷³

This letter appears evidence of several points. It is clear that Chevassut did not think the time right to publish. Secondly, various figures at the Westminster Hospital were pushing her toward publication. It is easy to guess why: a novel treatment without publication smelled of either quackery or monopoly and both were ugly charges. Thirdly, Chevassut held a strong enough opinion against the clinical treatments that she was opposed to their publication. Fourthly, in spite of her dependencies on Purves-Stewart for clinical material, she and he were no longer on civil terms.

For her part, Chevassut had continued equivocating about the existence of a micro-organism: she noted in one official report: "research is being carried out on the etiology of Disseminated Sclerosis with special reference to the possibility of the causative agent being a filterable virus." She added, "this apparent virus, which can be cultured from the cerebrospinal fluid from cases of disseminated sclerosis, is specific in that it is practically invariably present in such cultures and has never been demonstrated in controls."⁷⁴ This statement sounded like a distinct declaration of the organism's existence. But words like "apparent" and "practically invariably" make her views rather more ambiguous. It is perhaps instructive to consider the circumstances had she said that she had no definitive evidence demonstrating one way or the other the organism's existence. The physicians and administration of the Westminster Hospital would certainly have found themselves in a difficult position. The situation appears to have been one where everyone involved—from Chevassut to Fletcher—had begun to see the desirable rather than the real.

Nevertheless, when the MRC renewed her grant, the Council members insisted that she produce a preliminary notice of her findings. In his letter informing her of this, Fletcher wrote, "the

73. Kathleen Chevassut to Walter Fletcher, 24 July 1929, folder FD 1/2746 Chevassut I, NA.

74. Report on Research on the Etiology of Disseminated Sclerosis, 28 September 1928, folder FD 1/2746 Chevassut I, NA.

Council have now been able to consider the question of your research grant in all the quite exceptional and somewhat embarrassing circumstances that surround it," and:

When . . . publication is made, the Council will be free to consider what further support, if any, they should give to you in this inquiry. If they can be satisfied that the facts so far reported have real significance and promise, and if they can be satisfied that you are personally competent to conduct it further, they will be prepared, of course, to give you generous financial aid . . . the Council do not feel free to secure this now until causes of possible embarrassment and misunderstanding are removed by your making preliminary publication.⁷⁵

Though at odds with this decision, Chevassut agreed to publish. Her paper, "The Aetiology of Disseminated Sclerosis," finally appeared in *The Lancet* in March 1930.⁷⁶ Joseph Barnard, according to Purves-Stewart, had edited the final draft (if true, surely indicative of Barnard's tacit acceptance of her results).⁷⁷ In her article, she reported the appearance of spherules in cell-cultures taken from the cerebrospinal fluid of patients with the disease and suggested that they were unique to cases of disseminated sclerosis.⁷⁸ Others, she stated, could testify to her observations, including Barnard. Additionally, Chevassut noted that Lange Tests (an analysis of protein concentrations) of the cerebral spinal fluid of diseased patients produced unique gold curves relative to controls.⁷⁹ These preliminary results, she remarked, held out some hope for a vaccine

75. Walter Fletcher to Kathleen Chevassut, 6 December 1929, folder FD1/2746 Chevassut I, NA.

76. Walter Fletcher to Kathleen Chevassut, 10 December 1929; Kathleen Chevassut to Walter Fletcher, 18 December 1929; Kathleen Chevassut to Walter Fletcher, 13 January 1930; Kathleen Chevassut to Walter Fletcher, 3 March 1930, folder FD1/2746 Chevassut I, NA.

77. Kathleen Chevassut to Walter Fletcher, 13 January 1930; James Purves-Stewart to Walter Fletcher, 24 January 1930, folder FD1/2746 Chevassut I, NA.

78. Chevassut, "The Aetiology of Disseminated Sclerosis," 552.

79. Ibid., 556–57; later it was noted in McAlpine, Compston, and Lumsden, *Multiple Sclerosis*: "It should be emphasized at the outset that in multiple sclerosis the cerebrospinal fluid (C.S.F.) may be normal in all respects. Furthermore, in the fluid there is no characteristic abnormality specific of the disease, although a combination of pleocytosis, an increase of protein, and a positive Lange colloidal gold curve, in conjunction with a negative Wassermann reaction, is highly suggestive," 123. A gold curve is a now obsolete colorimetric test. Gold solution was added to serial dilutions of spinal fluid. In the presence of altered proteins produced by the syphilis bacillus, the gold causes a precipitation reaction to occur leading to change in the solution's opacity, creating to a novel reading relative to control samples.

for the disease, and this hope was realized still further with Purves-Stewart's publication, which appeared in the same issue of *The Lancet* and indicated successful results from vaccine trials in his patient population.⁸⁰

Unsurprisingly given the severity of the disorder and the published results, a wave of publicity followed the publication of these papers. Overnight, clippings contained in the MRC archives reveal, both Purves-Stewart and Chevassut became celebrities with the lay and medical press.⁸¹ For her part, Chevassut claimed to be disdainful of press attention, although she noted that a philanthropic trust had indicated interest in the work.⁸²

This storm of publicity did little to improve social relations at the Westminster Hospital between the spring of 1930 and late winter. Chevassut described control of her project steadily eroding.⁸³ As she was the only one able to produce the cell-cultures, she spent increasing time producing vaccines.⁸⁴ Purves-Stewart had complained about this earlier, ostensibly noting that it was taking up too much of her time, but perhaps actually voicing an entirely different concern. He told Fletcher, "I wrote to the Chairman stating that unless he withdrew his instructions to the pathological laboratories to supply upon the prescription of another physician experimental vaccines to patients whom I had not seen or examined, I would resign."⁸⁵ Purves-Stewart's motives must have been under suspicion.

80. James Purves-Stewart, "A Specific Vaccine Treatment in Disseminated Sclerosis," *Lancet*, 1930, 1, 560–64.

81. See Press Clippings in File, for example, Anon., *The Star*, 19 March 1930, folder FD1/2746 Chevassut I, NA; also see, "The Germ of Multiple Sclerosis," *Science* 1930, 72, x–xi.

82. Kathleen Chevassut to Walter Fletcher, 21 March 1930, folder FD1/2746 Chevassut I, NA.

83. Ibid.; Kathleen Chevassut to Walter Fletcher, 28 March 1930, folder FD1/2747 Chevassut II, NA; here she reports that Braxton Hicks had ceased to be her Director; Kathleen Chevassut to Walter Fletcher, 9 April 1930; James Wolfe-Barry to Walter Fletcher, 10 April 1930 under Westminster Hospital Proceedings; Kathleen Chevassut to Fletcher, 11 April 1930, and so on throughout folder FD1/2747 Chevassut II, NA.

84. Grafton Elliot Smith to Walter Fletcher, 2 April 1930; Walter Fletcher to Grafton Elliot Smith, 4 April 1930, folder FD1/2747 Chevassut II NA. Importantly, note that Elliot Smith's letter suggests, "My reason for writing to you is to ask whether you—in your private capacity—would approve of an appeal to the Rockefeller Foundation for financial assistance to secure her independence." It may be that Smith advised her to seek external sources of support.

85. James Purves-Stewart to Walter Fletcher, 29 January 1930, FD1/2747 Chevassut II, NA.

Yet with the commotion now ensuing at the hospital and in the press, and given the disease's manifestations, it is small wonder that other physicians might have wanted access to the vaccines.

Circumstances seem to have been at their worst. Chevassut's research had yet to be verified and on Purves-Stewart's work Fletcher could muster only the words "premature" and "to be deplored in the public interest" as they now gave rise potentially to false hope.⁸⁶ (Chevassut, remember, from the start, had been against the publication of Purves-Stewart's results and even reluctant to publish her own research.⁸⁷) It was the worst of possible situations. Ominously, a week after Chevassut and Purves-Stewart's publications appeared, Fletcher received a letter from Charles Symonds, then Consultant physician for Nervous Diseases at Guy's Hospital.

In common doubtless with other neurologists, I am receiving, from private patients with disseminated sclerosis, enquiries about the "new cure"; whether I think it is any good (to which I can only answer that I do not know) and how, where, and at what expense it is to be procured. The latter questions I do not quite know how to answer. I understand that the work recently published by Miss Chevassut and Sir James Purves-Stewart has been carried out under the auspices of the Medical Research Council and I write to you for guidance.⁸⁸

It was now obvious that Chevassut's results required immediate verification. Needed for this was a credible clinical scientist, and a young neurologist working at St. Bartholomew's Hospital and the National Hospital, Queen Square, appeared to be an ideal candidate.⁸⁹

86. Walter Fletcher to Webb, 3 May 1930, folder FD1/2747 Chevassut II, NA; in a letter to Chevassut, Fletcher wrote, "The scientific results already publicly reported by Sir James Purves-Stewart own their only value to his individual experience, and he would be the first to admit that so far as they go they supply not trustworthy evidence for any curative value in a disease like this, where diagnosis may be doubtful and where no judgement can be fairly formed until the lapse of several years. They regard it as of the utmost importance in the public interest that no false hopes should be raised and if they take any steps now to promote further trials of the vaccines it must be clearly understood that this does not imply any confidence that a useful therapeutic agent has been found." Walter Fletcher to Kathleen Chevassut, 14 April 1930, folder FD1/2747 Chevassut II, NA.

87. Private and Confidential for Members of the Medical Research Council Only, 1930, folder FD1/2747 Chevassut II, NA, "The results gained experimentally with monkey, and other observations, by Braxton Hicks, Hocking, and Purves-Stewart were published I understand without Miss Chevassut's approval and against her wishes."

88. Charles Symonds to Walter Fletcher, 31 March 1930, folder FD1/2746 Chevassut I, NA.

89. Walter Fletcher to Henry Dale, 14 April 1930, folder FD1/2746 Chevassut I, NA.

THE SOCIAL RELATIONS OF SCIENTIFIC VERIFICATION

In a letter marking the beginning of Edward Carmichael's twenty-nine year relationship with the MRC, Fletcher wrote to Joseph Barnard, "I feel pretty sure that you will like him personally" and added that though Carmichael was not completely qualified for his appointment, he had two attractive qualities. "No doubt he has much to learn on the technical laboratory side, though he has already shown competence there. Quite apart from this particular problem . . . it must, I think, be useful to get an able young man at work in this direction who can combine experimental work in the laboratory with skilled clinical work."⁹⁰ Note the language of difference and the meanings that Fletcher attached to terms like technical, experimental, and clinical. Then there was the social categorization—Carmichael was likeable. Remember too that Fletcher had written specifically to Barnard to inquire about Chevassut's technical competence. These were differential experiences.

The MRC planned a two-prong approach to verify Chevassut's findings. Carmichael was to replicate her bacteriological research, while limited vaccine trials conducted by other neurologists would begin with the purpose of confirming Purves-Stewart's results. Additionally these neurologists would provide Chevassut with cerebrospinal fluid.⁹¹ It seems from comments Fletcher made that they considered vaccine trials premature. But the MRC publicly adopted a pragmatic position neither endorsing Purves-Stewart's work nor ignoring it.⁹²

Fletcher's reasons for reserve can be further distilled. It may have been clear to the Council members of the MRC that Chevassut had been overly reliant upon Purves-Stewart for her supply of cerebrospinal fluid from patients with disseminated sclerosis. This created two problems: first, Purves-Stewart's ability to diagnose patients accurately with the condition had to be accepted (no one

90. Walter Fletcher to Joseph Barnard, 16 April 1930; also see, "Private and Confidential for Members of the Medical Research Council Only," 1930, folder FD1/2747 Chevassut II, NA.

91. Private and Confidential, 1930, folder FD1/2747 Chevassut II, NA.

92. Walter Fletcher to Kathleen Chevassut, 14 April 1930, folder FD1/2747 Chevassut II, NA.

ever questioned his ability to do so). Yet because of the nature of the disorder, it was accepted that his (or anyone's) diagnosis was accurate to a degree only. Secondly, because Purves-Stewart had been telling Chevassut which samples were positive (from patients with the condition) and which were controls, he created two sources of bias, because Chevassut knew from which samples she should expect positive results.⁹³ Under the new research protocol Chevassut would be blinded to the diagnosis of the source patient, so she would have to demonstrate that the Lange test accurately identified cases of disseminated sclerosis.

An unstated implication throughout these records is the concern that Purves-Stewart had been overly influencing her experimental results. A letter from Purves-Stewart to Fletcher was not encouraging:

I understood from your telephone message . . . that certain physicians will be nominated by the Medical Research Council to be associated with me in the clinical part of the research and that their instructions are to confine themselves to hospital patients, whilst I am to be permitted to select suitable private patients. I confess I feel rather uneasy if I am to be the only person to have private cases investigated. Quite half of the patients referred to in my own recent article were private cases. You can readily understand that selected cases of this sort are often the best for purposes of adequate supervision and treatments. I do not wish it to be felt that I am having an unfair advantage over the other physicians, by being allowed to select suitable private cases for experimental treatment.⁹⁴

From one point of view, Purves-Stewart's remarks appear ingenious. He appeared to be arguing that private patients presented fewer obstacles in treatment and supervision but was actually implying that diagnosis of hospital patients might be problematic.

The MRC and his peers in neurology, however, took a different view:

On the clinical side it is the unanimous opinion of other neurologists that Purves Stewart's clinical results so far as they go, allow no conclusions to be drawn. There is probably no disease in which results

93. For a discussion on types of research bias, see Collins and Pinch, *Dr. Golem*, 32–34.

94. James Purves-Stewart to Walter Fletcher, 22 April 1930, folder FD1/2747 Chevassut II, NA.

given of any given therapeutic treatment are so difficult to assess on a sound basis; other disorders, whether post-syphilitic or non-syphilitic may mimic it. No one can doubt Purves Stewart's competence here, and his diagnosis of disseminated sclerosis maybe accepted with a high degree of probability, but, as is well known, in this disease, conditions giving the temporary and even protracted appearance of cure may be caused by treatment of any kind or may occur without any treatment.⁹⁵

It was for these reasons that Fletcher sought to identify neurologists of sufficient skill and status to provide Chevassut with samples of cerebrospinal fluid. If from these samples, Chevassut could both confirm diagnosis of the disease and grow an organism, then there would be no doubt that she had made a great discovery. Only further replication and independent verification of her results would be required, and that was Edward Carmichael's task. Accordingly, Fletcher wrote to four neurologists asking them for assistance: Charles Symonds, Francis Walshe, Eric Blake Pritchard, and George Riddoch.⁹⁶ It is worth noting that all four men were ex-servicemen, and it seems not wholly conjecture to wonder if they might not have, as historian Carol Dyhouse described, felt "unease and volatility" and even "indignity" at having to compete with a talented woman.⁹⁷

All were invited to a meeting with Fletcher at the MRC headquarters in late May 1930 (Purves-Stewart and Chevassut were not there). The minutes of this meeting now seem vague and difficult to understand, but clearly no one was optimistic about the experiments hitherto conducted. Francis Walshe observed rather that Chevassut was spending "four-fifths of her time" preparing vaccinations for

95. Walter Fletcher to Webb, 3 May 1930, folder FD1/2747 Chevassut II, NA.

96. Fletcher's letters to them, 2 May 1930. These neurologists were among the elite of London neurology. Symonds, an Oxford MA and DM, had spent a year at Johns Hopkins Hospital with Adolph Meyer and had returned to appointments at Queen Square and Guy's Hospital. Francis Walshe, already mentioned and educated at University College Hospital, had worked with Victor Horsley (1857–1916) in Africa during the War, and following that had been a lecturer at Oxford, where he worked with Charles Sherrington (1856–1952); like Symonds, he was a physician at the National Hospital and had spent time in Baltimore as a visiting professor in neurology in 1925. Blake Pritchard, educated at Kings College London and Cambridge, currently held a position at the Maida Vale Hospital for Nervous Diseases. George Riddoch, trained at Aberdeen University, had during the War, collaborated with Henry Head (1861–1940) at the London Hospital, where he eventually became Consultant physician. Riddoch, like Blake Pritchard, held a position at Maida Vale, folder FD1/2747 Chevassut II, NA.

97. Dyhouse, "Women Students and the London Medical Schools, 1914–39."

Purves-Stewart. George Riddoch was of the opinion that the vaccination experiments were “‘too empirical’ in the present uncertain state of knowledge [of] the ‘virus.’” Symonds “thought that therapeutic tests were ethically justified if they could be carried out on a sufficiently large scale to give early indication of the value of the vaccine” but added that “in view of the fact that at present only Miss Chevassut was capable of making the vaccines, he thought that this was impossible.” At the end of the meeting, the neurologists agreed to provide Chevassut with cerebrospinal fluid but were undecided as to whether they would attempt vaccination trials.⁹⁸

The minutes of this meeting are more notable for what they do not say. Since Fletcher had suggested independent vaccination trials, Purves-Stewart had seemed too enthusiastic about the participation of his peers. He had endeavored, for example, to meet with the other neurologists in order to ensure that it not be felt he had “unfair advantage.”⁹⁹ He had also started holding dinner parties at his house for the other neurologists, of which, on one occasion, an absent Francis Walshe sarcastically commented to Fletcher, “I gather that it was a splendid occasion: a precedent that the ‘whole time’ scientist will hasten to follow when inaugurating a research.”¹⁰⁰ A private diary entry by Edward Carmichael’s former advisor at Edinburgh, Edwin Bramwell, clarifies such sentiments further. “Purves-Stewart,” Bramwell noted retrospectively, “was incurring the rancour of his colleagues for large numbers of cases of disseminated sclerosis were being sent him from, I believe, all over the world, and he must have been making large sums of money for he charged high fees.”¹⁰¹ Fletcher was straightforwardly condemnatory to Purves-Stewart:

The Council have entrusted the [vaccine] trials to men not engaged in competitive private practice, but having fully clinical facilities in their hospitals. In this way, the Council have avoided the embarrassment which might come from the invidious task of selecting among rival

98. Memorandum: *Disseminated Sclerosis*, 5 May 1930; on this point as well, see Walter Fletcher to Charles Symonds, 5 May 1930, George Riddoch to Walter Fletcher, 5 May 1930, folder FD1/2747 Chevassut II, NA.

99. James Purves-Stewart to Walter Fletcher, 22 April 1930, folder FD1/2747 Chevassut II, NA.

100. Francis Walshe to Walter Fletcher, 17 May 1930, folder FD1/2747 Chevassut II, NA.

101. Edwin Bramwell, Diary Entry, 13 October 1934; Vol. 1, 33–43, 37 [Private Collection].

practitioners those who might well be supposed to gain an immediate professional advantage from early supplied and early experience of some new and important therapeutic substance. The same principle, though perhaps in less degree, is certainly operative in the present instance. Another reason of different kind is that by restricting the field of inquiry to hospital cases, the Council protect the clinicians engaged from the importunities with which they might be pursued by anxious relatives and friends seeking in their desperation any help, however, hypothetical, for the sufferers in which they are interested.¹⁰²

These men “not engaged in competitive private practice” seem also to not have evinced much enthusiasm for the whole project. Symonds, Walshe, Pritchard, and Riddoch provided Chevassut with very few samples during the months that followed.¹⁰³ In a phone conversation with someone at the MRC, Chevassut voiced her concerns: “the supply from the 4 clinicians has not been satisfactory so far, and she has not obtained a positive result on those she has received. She suggested tentatively that carriage of the fluids might affect cultures” negatively.¹⁰⁴ In other words delay in the movement of spinal fluid from patients to her laboratory might have altered its composition. By early December 1930, Chevassut had received thirty-two samples in total from the neurologists. In contrast, Purves-Stewart had sent her 150 vials of spinal fluid between 1929 and 1930. Chevassut did not correctly identify any of the fourteen positive samples, and in one case mistook a case of chorea for the disease (here the diagnostic imprecision with regard to disseminated sclerosis should be borne in mind; perhaps the “positives” were not true cases at all).¹⁰⁵

Carmichael’s results were equally negative. A memorandum records a tense evening at the offices of the MRC, where Kathleen Chevassut met with twelve men including, T. R. Elliot, a Mr. Church, Joseph Barnard, Peter Laidlaw, Mr. Elford, Carmichael, Walshe, Symonds, Riddoch, Purves-Stewart, Blake Pritchard, and Fletcher.¹⁰⁶ Carmichael reported being unable to verify a single

102. Walter Fletcher to James Purves-Stewart, 9 May 1930, folder FD1/2747 Chevassut II, NA.

103. See letters from July through November 1930, folder FD1/2747 Chevassut II, NA.

104. Transcript, 10 October 1930, folder FD1/2747 Chevassut II, NA.

105. Ibid.

106. Memorandum: *Disseminated Sclerosis*, 5 May 1930, folder FD1/2747 Chevassut II, NA.

aspect of her work. He, along with James Godwin Greenfield, pathologist at the National Hospital, had attempted to find a Gold Colloidal Curve similar to the one she had reported. They had not succeeded. Nor had they followed her protocols. The Lange test, a basic spectroscopic examination, was a technique relying upon changes in light passing through a solution. Changes in light relative to controls suggested a deviation relative to protein concentration, and Chevassut had argued that this deviation was unique for disseminated sclerosis. Importantly, Carmichael controlled his experiments with cerebrospinal fluid from patients with syphilis, something Chevassut had not done and could not do without the aid of Purves-Stewart. The minutes recorded:

Greenfield and Carmichael failed to find a characteristic Lange curve in disseminated sclerosis. They standardised their gold solution by a biological test (i.e. against a standard G.P.I fluid). Miss Chevassut used a different gold solution, standardised by chemical means. Dr. Carmichael suggested that the solution used by Miss C was too sensitive to give results of any value.¹⁰⁷

Carmichael had used the Wasserman test to confirm diagnosis of General Paralysis of the Insane (GPI), a condition appearing in advanced stages of syphilis. Thus by standardizing the gold solution using cerebrospinal fluid from GPI patients as a control, Carmichael and Greenfield could be sure that they were producing results. The curve produced in a patient without GPI would be the normal control and would demonstrate that the test worked. Logically, if cerebrospinal fluid from patients with disseminated sclerosis produced a unique curve relative to the controls, they would have confirmed Chevassut's results. Yet, Carmichael reported failure. However, his result did not disprove Chevassut's claim; at best, all using different solutions and methods of standardization proved, was that Carmichael and Greenfield's results varied from her own.

Carmichael challenged other technical aspects of her work: he had been unable to receive reliable pH readings with the bubbling hydrogen meter she used and suggested that the hydrogen electrode was not adequate to the task.¹⁰⁸ Chevassut rejoined that it was a

¹⁰⁷. NA, FD1/2747 Chevassut II, Memorandum of meeting held at 38 Old Queen Street on Friday 19th December 1930, folder FD1/2747 Chevassut II, NA.

¹⁰⁸. Ibid.

matter of practice, insinuating the issue was Carmichael’s competence. Carmichael’s next criticism was less of a shock, but Joseph Barnard’s added comment was in retrospect catastrophic.

Carmichael had “cultured” 19 fluids from declared cases of disseminated sclerosis and had seen no evidence of a “spherule”. Mr Barnard had personally examined 9 of those fluids without success. Mr Barnard referred to his original assistance to Miss Chevassut. He said that all he had stated was that in some of the cultures shown to him he saw an appearance different to anything he had seen before when working with the same technique: he was not prepared to say that this was characteristic of DS [disseminated sclerosis] or implied a living virus.¹⁰⁹

Barnard’s qualification must have been somewhat bewildering to Chevassut. He had, after all, participated in the editing of her paper. Moreover, she had frequently admitted there was no reason to suspect the organism was causative—one reason she had opposed Purves-Stewart’s vaccination experiments and his editing of her *Lancet* paper. She could easily explain Carmichael’s inability to detect the organism, because he had not managed to confirm any of her results at all, but Barnard’s seemingly and suddenly discovered pragmatism must have caught Chevassut off guard.

The final blow, however, was still to come:

Professor Elliott referred to Miss Chevassut’s failure to repeat her work when supplied by the neurologists present with unknown fluids from different sources. 32 fluids had been sent to her, 14 of which came from patients with disseminated sclerosis. 2 of these were returned as “contaminated” the others were all “negative”. Only one fluid had been returned as “positive” and that came from a child with chorea.¹¹⁰

Chevassut, Elliott intimated, had exaggerated her case. Chevassut admitted the validity of these points, but successfully deflected some criticisms. Over time, she knew (she had demonstrated it in her graduate thesis), the pH of cerebrospinal fluid became more alkaline. Even an hour in transit between hospitals or from the bedside to a laboratory she suggested might kill the organism. She

109. Ibid.

110. Ibid.

then pointed out in a rebuttal to Carmichael: “often, she said, she got negative cultures ‘for months on end.’ It might be necessary in a given case of DS to examine 5 or 6 specimens of fluid before ultimately obtaining a ‘positive’ culture.” Additionally, she pointed out that Henry Cohen, a young neurologist at the Liverpool Royal Infirmary, had repeated her experiments “on a large series of cases of disseminated sclerosis at the Ministry of Pensions Hospital there. They had obtained “positive cultures” in nearly 100% cases of D.S., and in none of equal numbers of controls.” Moreover, she had—an earlier letter indicated—visited Liverpool and seen them for herself.¹¹¹

Unable to contest her defense, the members of the committee finally proposed that Chevassut move from the Westminster Hospital to the National Hospital—there she would have the materials, equipment, and proximity to patients necessary to settle the questions.¹¹² To these stipulations, Chevassut unhappily agreed. She changed her mind soon after. The deciding factor apparently was another more public round of devastating criticisms on her research by Carmichael at the Neurological Section of the Royal Society of Medicine in January 1931.¹¹³ At this meeting, Carmichael’s critique was sharp, and though Purves-Stewart and Chevassut were given opportunities to respond, most present sided with Carmichael. Chevassut was remembered to have left the meeting in tears, not to be “encountered in neurological circles again.”¹¹⁴

CONCLUSION

Kathleen Chevassut continued her research until 1933. For a time, she was supported by the Halley Stewart Trust, which provided her with a salary and laboratory facilities. Her decision to quit working for both the MRC and at the Westminster Hospital led James

111. 1573 Transcript, 10 October 1930, folder FD1/2747 Chevassut II, NA.

112. Memorandum of meeting held at 38 Old Queen Street on Friday 19th December 1930, folder FD1/2747 Chevassut II, NA.

113. Edward Carmichael, “The Aetiology of Disseminated Sclerosis: Some Criticisms of Recent Work, Especially with Regard to ‘Spherula Insularis,’” *Proc. Roy. Soc. Med.*, 1931, 24, 591–606; draft and correspondence also available in FD 1/2748 Chevassut III NA.

114. Alastair Compston, “The Story of Multiple Sclerosis,” in *McAplaine’s Multiple Sclerosis*, ed. George Ebers, Hans Lassmann, Ian McDonald, Bryan Mathews, Hartmut Wekerle (London: Churchill Livingstone, 1998), 3–68, 33.

Purves-Stewart to officially dissociate himself from her research—although he continued to argue that his vaccines (based on her research) treated the dreaded condition.¹¹⁵ The Halley Stewart Trust eventually severed their connection with Chevassut. They claimed that Chevassut had lied about her abilities and had produced no scientific data worthy of reporting. Their latter claim certainly appears true. The former is harder to judge: the MRC had been making considerable efforts to discredit Chevassut’s work in the eyes of her new benefactor while simultaneously lobbying them to support Edward Carmichael.¹¹⁶ Eventually, the Halley Stewart Trust’s laboratory equipment, along with control of a new fellowship for neurological research, passed to him. Carmichael then also became Director of the MRC Clinical Neurological Research Unit, founded at the National Hospital Queen Square in 1933.¹¹⁷ After the Trust parted company with her, Kathleen Chevassut simply disappears from the historical record. Other than occasional appearances in telephone directories, no further sources to the remainder of her life appear extant.¹¹⁸

Obviously, Chevassut did not discover a virus or germ causing MS—at the time of this writing no causative organism has ever been found for the disease. Yet although Fletcher came to regard her as possessing a pathologically “criminal mind,” several points may be made about her circumstances during this period.¹¹⁹ Without a medical qualification (and as of sometime in 1928 probably having little hope of becoming medically qualified), Chevassut had few means of providing herself with an income (her family was not wealthy).¹²⁰ The results of her research—which others initially lauded much more than she—were thus entangled with questions about her material well-being. To be more precise, Chevassut’s

115. Edwin Bramwell, Diary Entry, 13 October 1934, Vol. 1, 33–43 [Private Collection]; James Purves-Stewart, “Disseminated Sclerosis: Experimental Vaccine Treatment,” *Lancet* 1931, 1, 440–41.

116. Bernard Halley Stewart, “Letter to the Editor,” *Lancet*, 1932, 1, 751–52.

117. See folder FD1/2751 Chesterford Gardens Scheme, NA.

118. I thank James Casper for having had such interest in the later features of Kathleen Chevassut’s story as to pursue these genealogical inquiries with gusto. His persistence in pursuit of these trace details was of great inspiration and education to me.

119. Walter Fletcher to Bernard Halley Stewart, 13 January 1932, folder FD1/2750, NA; to her face he stated that he considered her to be “so hysterical about her work that she could not see it honestly.” Fletcher Memo, 16 December 1931, folder FD1/2750, NA.

120. Her father’s estate at the time of his death was less than three hundred pounds.

livelihood was dependent upon her discoveries in laboratory research. If she exaggerated aspects of her work, then one reason for this might well have been the profound sense of insecurity obviously entailed in a future of questionable economic security. Secondly, the sources to her story describe events salient to her life and relationship with the MRC. Yet, it is clear from the records that much of her story has more to do with widely held animosities directed against Purves-Stewart. It was to Purves-Stewart that Fletcher would later write to enquire about “emoluments” received as a result of the vaccination of private patients.¹²¹ Purves-Stewart denied any such ethical lapse in judgment, but in 1934 Edwin Bramwell recorded in his diary direct knowledge that Purves-Stewart had continued seeing patients with MS and charging them high fees for a vaccine treatment. It was this discovery that prompted Bramwell to describe Purves-Stewart’s behavior as “scandalous.”¹²² It also seems clear then that Chevassut was held more to account for Purves-Stewart’s behavior than for her own. A man as powerful as Purves-Stewart, however, was not so easy to publicly attack. Chevassut, by contrast, was an easy target and one capable of being used indirectly to embarrass Purves-Stewart.

121. Walter Fletcher to James Purves-Stewart, 23 September 1932, folder FD1/2751 Chesterford Gardens Scheme, NA.

122. James Purves-Stewart to Walter Fletcher, 26 September 1932, folder FD1/2751 Chesterford Gardens Scheme, NA; Diary of Edwin Bramwell, 13 October 1934, 40–41, Vol. 1 [Private Collection]. According to Bramwell: “I had thought that Stewart had abandoned the treatment of disseminated sclerosis by vaccines, but a few weeks ago I had a letter from Dr Ross Haddon of Hawick in which he told me that a patient of his, a lad of 20, was being treated by Purves-Stewart for disseminated sclerosis with vaccines, and that the lad’s mother, a Scottish minister’s wife, in whose case I had diagnosed disseminated sclerosis, wanted to know whether I would advise her to do to London and have the same treatment? Ross Haddon enclosed copies of several letters from Purves Stewart to the doctor in London who had sent the lad to see him. In the first letter Purves Stewart narrated the facts of the case, and these on paper were suggestive, though not perhaps conclusive, of disseminated sclerosis. Finally he wrote on examining the cerebrospinal fluid, ‘we found a growth or culture and I have advised an autogenous vaccine which I have prepared and propose to give. I should like to see the patients again in three months.’ In a second letter he writes, ‘I have again examined the patient; the cerebrospinal fluid is now sterile and we may I think discontinue the vaccine’. ‘It is important (I think these were his words) that I should see this patient again in six months’. I wrote to Ross Haddon telling him that there was no published proof of the existence of the organism in the cerebrospinal fluid of disseminated sclerosis and that I did not advise the boy’s mother to undergo the treatment. Further I asked Ross Haddon whether Purves Stewart was charging any fee for treating the lad for if he was not doing so I felt that Stewart had an excuse. Haddon replied that Stewart was charging the boy, a minister’s son, fifteen guineas each time he went up to London.”

Chevassut's story captures the ambiguities of biomedical research in interwar Britain. With no real regulatory structure guiding the production of clinical knowledge, a series of socially understood tacit rules ordered clinical research practices. Purves-Stewart had broken those rules; it seems doubtful that Chevassut even understood them. She was in no position in terms of merit or status to be able to use those tacit rules to her advantage, and indeed as a woman in research, it could be suspected that the novelty of her sex had already broken some sacrosanct line in the male-dominated world of medicine and clinical research. But the still larger implication of her story is that the eventual formation of transparent rules governing the structure of clinical research at the MRC brought with them a more egalitarian structure, even if this was probably an unintended outcome. This formation protected vulnerable workers from rules of practice that had hitherto been imagined common knowledge but were actually the continuing vestige of masculine knowledge, that is of hierarchies of dominance. That older formation was a “theodicy of a privilege,” a mode as sociologist Pierre Bourdieu has described, by which the social order mobilized a cultural infrastructure of domination and discourse to structure through symbolic violence individual belonging and empowerment.¹²³ The rules and regulations surrounding conduct in experimental protocol and experimental vaccination trials did not come into existence in a precise way in Britain until the 1950s.¹²⁴ That more egalitarian formation probably trailed behind.

Kathleen Chevassut's story is challenging to tell. It is difficult to use historical analysis convincingly to record silences, to reveal lacunae, and to reconstruct meanings. Her story is both self-evidently and not self-evidently a story about gender. The frequent and overtly sexual language used by men in epistolary or literary description or in reminiscence is perhaps where gender differences are clearest. Words like “girl,” “seamy,” or phrases like “naughtily-minded” cannot be construed as silent—they are signifiers ringing with assumptions. Yet beyond these articulations, so obviously derogatory,

123. Pierre Bourdieu, *The Logic of Practice*, trans. Richard Nice (Stanford: Stanford University Press, 1990), 133.

124. Benjamin Toth, *Clinical Trial in British Medicine 1858–1948, with Special Reference to the Development of the Randomized Controlled Trial* (Doctoral Thesis, University of Bristol, 1998, 124–279).

are quieter indices of experience. Various social metrics—facts, for example, about the difficulty of the admissions process into medical schools for women in interwar London or the inequity in women's childhood and adolescent education experiences when compared with men—do not come pre-packaged in the language and sources of the past. We may infer their existence and assume a context and meaning from them or we may choose not to. Likewise claims about differential cultural experiences—almost all of the men in this story were returning the First World War veterans—do not conjure clearly the meaning of difference, even though scholars have shown that many veteran medical men considered it intolerable to work under or with intellectually competent and competitive women in the interwar period. One critical interpretation might be that war has traditionally been construed a male enterprise, tragedy, and destiny; so too scientific research, medical practice, career, learning, and opportunity. Another critical interpretation might be that asymmetries of power create experiences like Chevassut's every day—that class, gender, trust, and protocol are made in these asymmetries.

None of these observations brings us any closer to knowing whether Chevassut was, as Norman Howard-Jones suggested in 1982, suffering from a paranoid personality disorder. Leaving to one side the long history of pronouncing "difficult" women hysterical or mad, little evidence supporting Howard-Jones' claim exists. While we should not see Chevassut as a martyr (she is no Rosalind Franklin figure for biomedical research), we should acknowledge that Howard-Jones' assessment (and others like it) rarely show evidence of any effort to comprehend the difficult social circumstances faced by women medical students (and researchers, physicians, and scientists) in this period. Indeed their lack of empathy is the noteworthy feature of such sources. What we do know is that throughout the whole of Chevassut's life following this episode, her story was told over and over again in medicine. It appears in histories of MS, in text-books on the disease, in pithy anecdotes recorded at dinner-time conversation as doctors sought to communicate what other younger *men* should *not* do as researchers. We also know that there are almost no records about her life afterwards. That silence is gender; it is also class. The lack of empathy is power.

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