

“Until the Sun of Science . . . the true Apollo of Medicine has risen”: Collective Investigation in Britain and America, 1880–1910

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In August, 1880, George Murray Humphry, in his presidential address to the British Medical Association (BMA), called for “collective action” by the country’s “eight thousand physicians” to accumulate observations concerning the role of “temperamental, climacteric, and topographical agencies upon disease”. Through participating in organized inquiries, practitioners would “deepen their interest in the science of medicine, and impart the charm of wider usefulness to the daily routine of life”.¹ By December 1881, the BMA had funded a Collective Investigation Committee, which over the next eight years would sponsor nearly a dozen inquiries into the natural history of disease.² Beyond Great Britain, Humphry’s appeal would launch an international movement for collective investigation, with physicians in Germany, the Netherlands, Norway, Sweden, and the United States following the British example.³

At first glance, there is little exceptional about the movement for collective investigation. Organized efforts to collect practitioner data on diseases and their treatment go back at least to the eighteenth-century: Félix Vicq d’Azyr led the Société Royale de Médecine in collecting data from French physicians on meteorological conditions and epidemics, while in England John Jurin surveyed correspondents of the Royal Society concerning their experiences with smallpox inoculation.⁴ The London Medical-Chirurgical Society, the

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¹ George Murray Humphry, ‘President’s Address’, *Br. med. J.*, 1880, **ii**: 241–4, on p. 244.

² Earlier brief accounts of collective investigation include R M S McConaghey, ‘The B.M.A. and collective investigation’, *Br. med. J.*, 1956, **i**

Supplement (25 February): 59–61; Ernest Muirhead Little, *History of the British Medical Association, 1832–1932*, London, British Medical Association, 1932, pp. 301–5.

³ William W Gull, ‘On collective investigation of disease’, *8e Congrès périodique international des sciences médicales. Compte-rendu*, Copenhagen, Libraries Gyldendal, 1886, vol. 1, pp. 54–66; N S Davis, ‘Report of the Committee on the Influence of Appreciable Meteorological Conditions on the Prevention of Acute Diseases’, *Trans. Illinois State Med. Soc.*, 1889: 41–5.

⁴ Jean-Pierre Peter, ‘Une enquête de la Société Royale de Médecine (1774–1794): malades et maladies à la fin du XVIII^e siècle’, *Annales: E.S.C.*, 1967, **22**: 711–51; Jean Meyer, ‘L’enquête de l’Académie de Médecine sur les épidémies, 1774–1794’, *Études Rurales*, 1969, **34**: 10–59. On Jurin, see Andrea A Rusnock, ‘The weight of evidence and the burden of authority: case histories, medical statistics and smallpox inoculation’, Roy Porter (ed.), *Medicine in the Enlightenment, Clio Medica*, 29, Amsterdam, Rodopi, 1995, pp. 289–315.

Clinical Society of London, the BMA, the Massachusetts Medical Society and the American Medical Association (AMA) each attempted analogous inquiries earlier in the nineteenth century with disappointing results.⁵ Yet the movement for collective investigation provides a unique window into late-nineteenth-century contests over medical science and medical society.

The historians Christopher Lawrence, John Harley Warner and George Weisz have analysed the intellectual and political tensions facing late-nineteenth-century elite physicians who sought to incorporate laboratory medicine into clinical practice. Contests over what the laboratory could contribute to diagnosis or treatment were central to a decades-long multinational debate about how future physicians should be trained and how current physicians should practise.⁶ Historians' emphasis on this "great transformation" has obscured a more immediate divide within the profession, between those practitioners who saw clinical encounters as opportunities for discovery and learning, and the great majority of working physicians who, their attentions focused on earning a living, had little time for new knowledge. The movement for collective investigation, which sought to harvest the experiences of general practitioners for medical science, demonstrates how uninterested such practitioners were in the scientific and social aspirations articulated by medical élites.

The present article examines the history of collective investigation in Great Britain and the United States, the two countries where the movement was most vigorous.⁷ In Great Britain, the sponsors of collective investigation were elite consultants who insisted that the experiences of general practitioners, properly gathered, could transcend the limitations of hospital medicine. Their own background in hospital practice notwithstanding, these consultants articulated a vision of a biographical medicine which might explain the trajectories of disease in ways no study of morbid anatomy could achieve.⁸ In the United States, collective investigations focused more on therapeutics than on the natural history of disease. Organized collective investigations were taken up by local medical societies,

⁵ Minutes of Council, Medical and Chirurgical Society of London, 30 Sept. 1831; 23 Oct. 1849; 11 Dec., 1849; 8 Jan. 1850, Royal Society of Medicine archives, London. For the Clinical Society, see Christian Baumler, Alfred B Duffin, Berkeley Hill, 'Report of the Committee on Temperature in Syphilis', *Trans. Clin. Soc. London*, 1870, 3: 170–9. On the BMA committee, see 'An investigation into the effects of remedies', *Br. med. J.*, 1862, ii: 175–6; 'Report of the Committee on the Action of Medicines', *Br. med. J.*, 1862, ii: 177; 'The therapeutical inquiry', *Br. med. J.*, 1862, ii: 284–5. For Massachusetts, see Ephraim Cutter, Alonzo Chapin and S A Toothaker, 'Report of the Committee . . . in the therapeutical action of medicinal agents', *Boston med. Surg. J.*, 1863, 68: 342–7. For the AMA, see Worthington Hooker and H D Buckley, 'Report on the epidemic diseases of New England and New York', *Trans. Am. med. Assoc.*, 1852, 5: 285–8.

⁶ Christopher Lawrence, 'Incommunicable knowledge: science, technology and the clinical art in Britain 1850–1914', *J. contemp. Hist.*, 1985, 20:

503–20; John Harley Warner, 'Ideals of science and their discontents in late nineteenth-century American medicine', *Isis*, 1991, 82: 454–78; *idem*, 'The fall and rise of professional mystery: epistemology, authority and the emergence of laboratory medicine in nineteenth-century America', in Andrew Cunningham and Perry Williams (eds), *The laboratory revolution in medicine*, Cambridge University Press, 1992, pp. 110–41; George Weisz, *The emergence of modern universities in France, 1863–1914*, Princeton University Press, 1983, pp. 359–68.

⁷ It would, none the less, be useful to have an analysis of the sociology and intellectual programme for collective investigation in Germany, which was initiated by the Berlin Medical Society. See 'Collective investigation movement', *Br. med. J.*, 1883, ii: 20–1.

⁸ John V Pickstone, *Ways of knowing: a new history of science, technology and medicine*, Manchester University Press, 2000; see also David Armstrong, 'The emancipation of biographical medicine', *Soc. Sci. Med.*, 1979, 13A: 1–8.

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national specialty groups and at least one drug company. Ephemeral as most such efforts were, collective investigation none the less survived two decades longer in the United States than in Great Britain. In both countries, collective investigation ultimately failed as a social movement for achieving professional cohesion and as a legitimate mode of investigation. The movement's failures illuminate the fragility of elite ideals about medical knowledge and medical community in the late nineteenth century.

Collective Investigation in Britain, 1880–1889

Collective investigation was the brainchild of Frederick Horatio Akbar Mahomed (1849–1884), “son of a Brighton Turkish-bath owner” who received his MD at Brussels and worked on a Cambridge MB while serving as medical registrar at Guy’s Hospital. While studying at Cambridge, he had been befriended by George Humphry, MB, FRS, FRCS, then Professor of Anatomy.⁹ Along with Humphry, Mahomed enlisted Arthur Ransome, MB, a Manchester general practitioner and public health reformer who had proposed a programme of organized medical inquiries to the BMA sixteen years earlier.¹⁰ The programme for collective investigation, as the three men presented it, had two goals. First, they hoped to transcend the limits of hospital “investigations” by enlisting general practitioners to observe the facts of disease in its natural setting.¹¹ Second, the organizers sought to involve “busy” practitioners in the scientific work of the profession, to bring “home to each man that he owed a duty to medicine as a science, which he was bound in honour to render in return for the privilege of using her as a trade”.¹²

The latter objective held special appeal for those, like Humphry, who saw the BMA as something more than a trade union. A university education was one means to draw physicians away from the “engrossing avocations of [a] money-making life” but so might the experience of participating in collective investigation.¹³ “If the [BMA’s]

⁹ On Mahomed’s career, see James F Goodhart and W H A Jacobson, ‘In memoriam: Frederick Horatio Akbar Mahomed’, *Guy’s Hosp. Rept.*, 1886, 43: 1–10; Samuel Wilks and George Thomas Bettany, *A biographical history of Guy’s Hospital*, London, Ward, Lock, Bowden, 1892, pp. 306–8. For Humphry, see D’Arcy Power, ‘Sir George Murray Humphry (1820–1896)’, *Dictionary of National Biography*, ed. Sidney Lee, London, Smith, Elder, 1907, vol. 22 (Supplement), pp. 883–5; Sir Humphry Rolleston, ‘Sir George Murray Humphry, MD, FRS’, *Ann. med. Hist.*, 1927, 9: 1–11. Rolleston credits Alex Hill, Humphry’s demonstrator, with the suggestion for collective investigation, but I have found no corroborating evidence for this claim.

¹⁰ Arthur Ransome, ‘On the need of combined medical observation’, *Br. med. J.*, 1864, ii: 405–8; F A Mahomed, ‘Suggestions concerning the scientific work of the Association [letter]’, *Br. med. J.*, 1880, i: 74. Ransome, with a degree in chemistry from the University of Cambridge and clinical training in London and Paris, was not your usual general practitioner. Willis J Elwood, A Félicité Tuxford,

Some Manchester doctors: a biographical collection to mark the 150th anniversary of the Manchester Medical Society, Manchester University Press, 1984, pp. 93–7.

¹¹ F A Mahomed, ‘Suggestions concerning the scientific work of the Association’, *Br. med. J.*, 1880, i: 31–2, p. 31; Ransome, op. cit., note 10 above. As the official organ of the BMA, the *British Medical Journal* kept much closer tabs on collective investigation than the *Lancet*, especially when reporting on BMA branch activities.

¹² Mahomed, op. cit., note 11 above. On “busy men”, see G M Humphry, ‘Remarks on the collective investigation of disease’, *Br. med. J.*, 1883, i: 145–6, p. 146.

¹³ Humphry, op. cit., note 1 above. For Humphry’s views on the spiritual values of university medical training, see an earlier address to the BMA: G M Humphry, ‘Address on surgery’, *Br. med. J.*, 1864, ii: 175–86, p. 177. See also William W Gull, ‘An address on the collective investigation of disease’, *Br. med. J.*, 1883, i: 141–4, on p. 142; Samuel Wilks, ‘The address in medicine’, *Lancet*, 1872, ii: 213–16.

members could combine for social and political purposes,” Humphry maintained, “they ought certainly not to hesitate to do so for the promotion of the science and practice of medicine.”¹⁴

The programme’s greatest appeal lay in its scientific promise that general practitioners “can trace the life-history of [a] disease in a manner which no one else can possibly do”.¹⁵ To whom did this promise speak? The patrons of collective investigation were a group of prominent university professors and élite London clinicians with a long-standing interest in “constitutional” disease. Allied with Humphry (Cambridge University) were Henry Acland (Oxford University), William Withey Gull and Samuel Wilks (both affiliated with Guy’s Hospital), James Paget (St Bartholomew’s Hospital) and eight other prominent consultant physicians, all but one London-based.¹⁶ This clinical élite believed deeply in pathological anatomy as the bedrock of medicine: “Without morbid anatomy our work would be foundationless and in the air.”¹⁷ But they had come to recognize its limits in understanding clinical disease. As Gull put it:

One might as well hope to determine the physical geography of a country, by measuring and analysing the contents of its rivers as they fall into the sea, as to hope to reach a true pathology from studying alone the results of disease on the *post mortem* table.¹⁸

Part of their dissatisfaction was with the excesses of contemporary solidist pathology, especially its unproductive preoccupation with local disease. Even apparently localized diseases—of the kidneys, liver, heart, or lungs—were mere manifestations of “more general causes”.¹⁹ Unearthing such causes called for careful study of a patient’s constitution.²⁰

¹⁴Prof. Humphry, ‘A short history of the movement’, in *The Collective Investigation Record*, G M Humphry and F A Mahomed (eds), vol. 1, London, British Medical Association, 1883, pp. 1–6, on p. 2.

¹⁵Remarks of C Macnamara at a meeting of the BMA’s Birmingham and Midlands branch, in ‘Collective investigation of disease’, *Br. med. J.*, 1883, i: 32–4, on p. 33.

¹⁶For Acland’s role, see Humphry, op. cit., note 14 above, p. 5. As President of the General Medical Council, Acland’s support was welcome. Others active in the committee included Walter Cheadle (St Mary’s Hospital), Dyce Duckworth (St Bartholomew’s Hospital), Octavius Sturges (Hospital for Sick Children), Balthazar Foster (Birmingham), Stephen Mackenzie (London Hospital), I Burney Yeo (King’s College Hospital), Sidney Coupland (Middlesex Hospital) and Isambard Owen (St George’s Hospital Medical School), who replaced Mahomed as secretary after the latter’s death. For a full list of members and local committees, see ‘Organization for the Collective Investigation of Disease’, *The Collective Organization Record*, vol. 1, op. cit., note 14 above, pp. 160–8.

¹⁷Gull, op., cit., note 13 above, p. 141; Samuel Wilks, *A memoir*, London, Adlard & Son, 1911, pp. 124–9. Of the group, Wilks was the most strongly

committed to morbid anatomy. On the rise of pathological anatomy in the previous generation, see Russell C Maulitz, *Morbid appearances: the anatomy of pathology in the early nineteenth century*, Cambridge University Press, 1987.

¹⁸Gull, op. cit., note 13 above, p. 141; T Clifford Allbutt, ‘Introductory address delivered at the Leeds School of Medicine’, *Lancet*, 1871, ii: 531–5.

¹⁹English discussions form part of a broader, trans-European exploration of the relation between local and humoral factors in disease. A history of national pathological traditions, much needed, is beyond the scope of this article. See, however, Pedro Laín Entralgo, *La historia clínica: historia y teoría del relato patográfico*, facsimile edition, Madrid, Triacastela, 1998; Russell C Maulitz, ‘Rudolf Virchow, Julius Cohnheim and the program of pathology’, *Bull. Hist. Med.*, 1978, 52: 162–82.

²⁰Samuel Wilks, ‘Some remarks on the nature and causes of disease’, *Guy’s Hosp. Rept.*, 1859, 3rd series 14: 15–53, p. 17. Wilks put the blame for localist dogmas not on morbid anatomy but on organ-based specialists motivated by “mercantile” rather than scientific interests. See also James Paget, ‘Notes for the study of some constitutional diseases’, in *Clinical lectures and essays*, New York, D Appleton, 1875,

The “constitution” of Paget, Gull and Wilks was not that of earlier generations, invested in identifying “phthisical” or “gouty” bodily types:

There are few worse habits in practice than that of commonly saying of one case “It is all gout”, and of another that it is all scrofula, or all syphilis. We might as well say of any Englishman that he is all Norman, or all Anglo-Saxon or all Celt.²¹

Rather, they were preoccupied with identifying the interactions between such bodily predispositions and “accidents”, which might be a habit of body (diet, exercise) or an “external condition” such as an infection or rash. Such accidents made all the difference between a healthy person and a sick one, both with similar predispositions.²² Yet the antecedents of disease were frequently invisible to consultant practitioners. Understanding this interplay required “a much more complete and exact study of all the personal conditions of disease than is now usual”—a patient’s family history but also his or her social and medical biography.²³ Such natural histories were best studied not by consultants in hospital or even in private practice, but by the general practitioner:

It is his privilege to see the earliest beginnings of disease, and to have the opportunity of tracing its evolution and decline, or when so favourable a course does not happen, the steps of pathological progress are before him, whereas at the end of life when the whole organism crushes downwards into a chaos of pathological forms [so that] it is often impossible on the postmortem table to say where the failure began and how it has advanced. The family physician’s observations should thus supply a corrective to a too exclusive mechanical pathology.²⁴

For James Paget, the model was Charles Darwin: the patient and careful observer with “personal and exact knowledge” of family history could chart the ebb and flow of diseases

pp. 353–97; Humphry’s discussion of pyaemia in Humphry, ‘Address on surgery’, op. cit., note 13 above, pp. 179–80. For overviews of nineteenth-century constitutional doctrines, see Charles E Rosenberg, ‘The bitter fruit: heredity, disease and social thought’, in *idem*, *No other gods: on science and American social thought*, Baltimore, Johns Hopkins University Press, 1976, pp. 25–53; Erwin H Ackerknecht, ‘Diathesis: the word and the concept in medical history’, *Bull. Hist. Med.*, 1982, 56: 317–23; John C Waller, ‘“The illusion of an explanation”: the concept of hereditary disease, 1770–1870,’ *J. Hist. Med. Allied Sci.*, 2002, 57: 410–48.

²¹ Observing the variation among types was deemed more important than noting the types themselves. James Paget, ‘Some rare and new diseases [1882]’, in *Selected essays and addresses*, ed. Stephen Paget, London, Longmans, Green, 1902, pp. 352–80, quote on p. 372. On the anti-determinism of constitutionalist thinking, see Rosenberg, op. cit., note 20 above.

²² See Paget’s analysis of a nearly fatal septicaemia he developed after conducting an autopsy when he was “overtired” and his body no longer accustomed to the “poisons” encountered during dissection. James Paget, ‘Notes for a clinical lecture on dissection poisons’, *Lancet*, 1871, i: 735–6, 774–6; *idem*, ‘On disease of the

mammary areola preceding cancer of the mammary gland [1874]’, in *Selected essays*, op. cit., note 21 above, pp. 145–8. Compare Andrew Mendelsohn, who argues that such an interest in the dynamic role of external factors is a product of the fully realized germ theory. J Andrew Mendelsohn, ‘Medicine and the making of bodily inequality in twentieth-century Europe’, in Jean-Paul Gaudillière and Ilana Löwy (eds), *Heredity and infection: the history of disease transmission*, London, Routledge, 2001, pp. 21–80.

²³ Paget, op. cit., note 21 above, p. 372. See also Wilks, op. cit., note 20 above, p. 50.

²⁴ William Gull, ‘An address on the international collective investigation of disease’, *Br. med. J.*, 1884, ii: 305–8, p. 306. See also Sir William Gull, ‘Address on clinical medicine’, *Lancet*, 1872, i: 139–40; *idem*, ‘Presidential address delivered before the Clinical Society of London’, *Lancet*, 1871, i: 145–7; James Paget to George Paget, 8 March 1880, in Stephen Paget (ed.), *Memoirs and letters of Sir James Paget*, London, Longmans, Green, 1901, p. 293; Samuel Wilks, ‘An address on collective investigation of disease’, *Br. med. J.*, 1883, ii: 1005; C J P Williams, ‘On the successes and failures of medicine’, *Lancet*, 1862, i: 345–7; J Magee Finny, ‘Collective investigation of disease’, *Dublin J. med. Sci.*, 1883, 75: 465–76, pp. 467–72.

over generations. Such knowledge could elucidate both the inheritance of specific diseases and the factors “which from generation to generation shall gradually obliterate the disease which one ancestor may have acquired”.²⁵ A deeper understanding of constitutions would lead to a truer pathology and a more precise therapeutics:

We need not only the diagnosis between diseases essentially different, but that between the different and varying forms of each of those [diseases] which we call by a generic name. . . . Better treatment will follow better diagnosis, and better diagnosis will certainly follow a more exact pathology.²⁶

Underlying Paget’s view that “better treatment” would follow “a more exact pathology” was a scepticism about contemporary therapeutic practice that was widely shared in the London consultant milieu, a suspicion that doctors ignorant of natural history were inclined to credit drugs for “what may be merely the decline of the disease itself”.²⁷

Authorized by the BMA in 1881, the Collective Investigation Committee (CIC) proposed a variety of inquiries, ranging from the natural history of specific diseases, to “life histories” of patients and their families, to studies of specific remedies, a special interest of Arthur Ransome’s.²⁸ Investigative work would be publicized through the BMA branches, which might also propose specific inquiries. The CIC would select the studies, collect and analyse the data, and then summarize the results. Most important were the data collection forms, “convenient cards which can be carried in the pocket and filled up in a spare minute of any time or place” in the busy practitioner’s life. “It has been accepted as a principle”, wrote the CIC secretary F A Mahomed, “that no written answers to questions beyond a single stroke of the pen . . . or occasionally a few words, must be asked for”.²⁹

The CIC’s first inquiry, on phthisis, revealed problems with this approach. Prompted by Robert Koch’s reports on the tubercle bacillus, the CIC asked simply, “Have you observed any cases in which pulmonary phthisis appeared to be communicated from one person to another?” Of 1078 physicians responding, 673 replied tersely, “No”. The remaining 405 provided the asked-for data on cases they had observed, mostly in the form of brief clinical

²⁵ James Paget, ‘An address on the collective investigation of disease’, in *The Collective Investigation Record*, vol. 1, op. cit., note 14 above, pp. 21–5, on p. 23. See *idem*, ‘Some rare and new diseases’, and ‘Elemental pathology’, both in *Selected essays*, op. cit., note 21 above, pp. 240–80. Paget belonged to Darwin’s far-flung network of correspondents on natural history but his interests in natural history long preceded their relationship. See Janet Browne, *Charles Darwin, vol. 2: The power of place*, New York, Alfred A Knopf, 2002, pp. 203, 286, 359; Paget, *Memoirs and letters*, op. cit., note 24 above, pp. 25–28, 32–38. See also Gull, op. cit., note 13 above.

²⁶ Paget, op. cit., note 21 above, pp. 369–72, on p. 369. See also Gull, ‘Address on clinical medicine [continued]’, *Lancet*, 1872, i: 175–7; Wilks, op. cit., note 24 above; *idem*, ‘The address in medicine’, *Br. med. J.*, 1872, ii: 146–53.

²⁷ W B Cheadle, ‘The progress of medicine’, *Fortnightly Rev.*, 1867, 6: 567–78, p. 576. See also

James Paget, ‘Address by the President,’ *Trans. Clin. Soc. London*, 1870, 3: xxxi–xxxix; William W Gull and Henry G Sutton, ‘Remarks on the natural history of rheumatic fever’, *Medico-Chirurgical Trans.*, 1869, 8: 43–82, pp. 75–82; Samuel Wilks, ‘On the syphilitic affections of the internal organs,’ *Guy’s Hosp. Repts.*, 1863, 3rd series 9:1–63, pp. 14–16.

²⁸ Humphry, op. cit., note 14 above, p. 3. Ransome’s interests may also be reflected in proposals for topographical and epidemiological inquiries. See Ransome, op. cit., note 10 above. In general, the topics selected reflect the interests of the London consultants in the natural history of disease, not Ransome’s interests in therapeutics and epidemiology.

²⁹ ‘The work of the Collective Investigation Committee’, *Br. med. J.*, 1882, i: 355–6, p. 355; ‘Collective Investigation Committee: the function of the local committees’, *Br. med. J.*, 1882, i: 674–6.

anecdotes.³⁰ Subsequent inquiries on pneumonia (350 replies), acute rheumatism (339 replies), chorea (200 replies) and diphtheria (138 replies) generated more detailed information but in lesser amounts.³¹ Stephen Mackenzie, author of the CIC report on chorea, acknowledged that the inquiry on the natural history of chorea “may to some appear disappointing”. Detailed tables on clinical antecedents of chorea, and on the age, sex, and social class of patients were admittedly “inconclusive”, resolving none of the existing debates on a subject of “considerable complexity and difficulty”. None the less, Mackenzie asserted weakly, the report demonstrated the willingness of “a large number of members of our profession” to participate in “the scientific investigation of disease”.³² A CIC report on pneumonia was similarly equivocal about its findings.³³

Enthusiasm for collective investigation, high at first, quickly diminished. By 1885, results from numerous inquiries had slowed to a trickle.³⁴ Practitioners complained about the amounts of detailed information demanded: “It has been said that no one attends a case of acute gout more than once a day, and that, therefore, no two temperatures can be secured in twenty-four hours.”³⁵ Practitioners complained that they were “too busy” in the day and “too tired” at night to spare time to write up their observations.³⁶ Thomas Dolan, a Yorkshire physician, sarcastically asked whether, if the results were truly so valuable, practitioners should not be paid for collecting them.³⁷ Even supporters of collective investigation wondered whether “the Committee may have too much lost sight of the conditions under which the practitioner works”.³⁸ The CIC considered various expedients, including (briefly) a greater reliance on hospital physicians for data.³⁹

Mounting expenditures combined with diminishing returns led to internal BMA inquiries: were CIC members authorized to spend BMA funds to promote collective investigation at the International Medical Congress?⁴⁰ Opposition was furthered by groups within

³⁰ Sub-committee on phthisis (Dyce Duckworth, Frederick Taylor, W J Tyson, I Burney Yeo, F A Mahomed), ‘A report on the communicability of phthisis’, in *The Collective Investigation Record*, vol. 1, op. cit., note 14 above, pp. 26–92.

³¹ These reports, all deemed “preliminary”, were published in *The Collective Investigation Record*, vol. 1, op. cit., note 14 above, pp. 93–133.

³² Stephen Mackenzie, ‘Report on chorea returns’, *The Collective Investigation Record*, London, British Medical Association, 1887, vol. 3, pp. 45–56, on p. 45.

³³ ‘Report on pneumonia’, *The Collective Investigation Record*, London, British Medical Association, 1884, vol. 2, pp. 5–71.

³⁴ British Medical Association archives, London (hereafter BMA). Minutes of the Collective Investigation Committee, 14 Jan. 1885, B/74/1/1. All further BMA archival references are to this collection unless otherwise noted.

³⁵ For gout, see Dyce Duckworth, ‘An address on collective investigation of disease’, *Br. med. J.*, 1884, i: 4–5, p. 4. See also the remarks of Dr Saunby (secretary of the local committee) and Dr Rickards at the Birmingham and Midland Counties Branch, ‘Collective investigation of disease’, *Br. med. J.*, 1883,

i: 33; remarks of Mr Vincent Jackson, ‘Staffordshire Branch’, *Br. med. J.*, 1885, i: 743; Report of the Collective Investigation Committee, 1 July, 1885, BMA. For a sample of an early and unusually detailed form, see Shirley F Murphy, ‘Memorandum on diphtheria’, *Br. med. J.*, 1882, ii: 1173–75, pp. 1174–75.

³⁶ Wilks, op. cit., note 24 above, p. 1005.

³⁷ T M Dolan, ‘Collective Investigation Committee’, *Br. med. J.*, 1881, ii: 101.

³⁸ Sidney Coupland, ‘Address on collective investigation’, *Br. med. J.*, 1884, i: 1197–99, p. 1198.

³⁹ Minutes, CIC committee, 14 Jan. 1885; Minutes, General Committee, CIC, 14 Oct. 1885, BMA.

⁴⁰ In its first year of operation (1882), the CIC spent £474 8s 9d, a substantial sum in an organization recently returned to financial health. See Report of the CIC to the Annual Meeting, 3 July 1883, BMA. In 1884, they sought £600 for their second year of operations. See G M Humphry, ‘Report of the Collective Investigation Committee’, *The Collective Investigation Record*, vol. 2, op. cit., note 33 above, pp. 1–4, on p. 1. On the BMA’s finances, see Peter Bartrip, *Themselves writ large: the British Medical Association 1832–1966*, London, BMJ Publishing

the BMA who apparently resented the CIC's autonomy and prosperity. The Section on Therapeutics wished for a greater role in therapeutic inquiries; various BMA branches questioned the CIC's "premature" publication of reports and the allegedly centralized conduct of the inquiries.⁴¹ The CIC's short-term political difficulties were none the less surmountable, once new budgetary controls were imposed and compromises with the Section on Therapeutics negotiated.⁴²

The decisive challenge came from those who questioned the basic premises of collective investigation. "Among the thousands of practitioners who took part in the work," George W Potter averred, "only a small proportion were competent, by natural capability and education, to conduct scientific inquiries."⁴³ Such inquiries were little more than "scientific book-keeping", added Thomas Dolan: "If book-keeping were all that was required, we should have long since arrived at a knowledge of diphtheria". Collective investigation inevitably lacked the insight provided by the "personal equation of the discoverer", a discriminating observer, Dolan implied, like himself.⁴⁴

Collective investigation's critics insisted that true medical knowledge, like tact and manners, was an interpretive skill demanding discernment and character. As Christopher Lawrence has argued, such "incommunicable knowledge" was seen as the product of a lengthy moral apprenticeship, accessible to the few and not the many.⁴⁵ Collective investigation, by contrast, was aimed at the gathering of facts which "are of value only from their number, and not from the importance of individual observations".⁴⁶ Enthusiasts insisted that harvesting "the common everyday facts" of general medical practice was "as valuable and important work as any that can be done just now for medicine".⁴⁷ It did not help that some CIC facts, such as degrees of "abstinence" from drink, were poorly defined.⁴⁸ But as CIC reports reluctantly acknowledged, even well-gathered facts cannot

Group, 1996, pp. 54–6. On the Berlin expenditures, see Minutes, Committee of Direction (CIC), 8 Apr. 1885, 14 Oct. 1885; Minutes, BMA Council, 8 July 1885, B/55/2/2, Minutes of Council and Subcommittees, 1855–1887, BMA.

⁴¹For branch criticisms, see Minutes, Collective Investigation Committee, 18 Jan. 1885; Minutes, Committee of Direction, 21 July 1886, BMA. For additional criticisms, see 'The preliminary report of the Collective Investigation Committee on Diphtheria', *Br. med. J.*, 1884, i: 1111–13. On the complaints of the Section on Therapeutics, see Committee of Direction (CIC), Minutes, 1 July 1885; Joint Meeting of CIC members and President of BMA plus President and officers of sections, Cardiff, 29 July 1885; 'Investigation of the action of medicines', *Br. med. J.*, 1885, ii: 313. Balthazar Foster, initially a proponent of collective investigation, played a key role in promoting the interests of the Section on Therapeutics within the BMA.

⁴²On funding, see Minutes, Committee of Direction, 14 April 1886. Relations with the Section on Therapeutics continued to be conflictual, despite a formal agreement. See Joint Meeting of CIC members

and President of BMA plus President and officers of sections, Cardiff, 29 July 1885; Minutes, CIC General Committee, 14 Apr. 1886, 21 July 1886.

⁴³G W Potter [presentation on the dangers of collective investigation], 'Metropolitan Counties Branch: Northern District', *Br. med. J.*, 1884, i: 386; see also the criticisms reported in Committee of Direction, Minutes, 19 Jan. 1887, BMA; Octavius Sturges, 'The Collective Investigation Committee's report on acute pneumonia', *Br. med. J.*, 1885, i: 348–49, p. 348.

⁴⁴Thomas M Dolan, 'Collective investigation', *Br. med. J.* 1884, i: 1249–50, p. 1249.

⁴⁵Lawrence, op. cit., note 6 above.

⁴⁶'The Collective Investigation Movement', *Br. med. J.*, 1883, ii: 20.

⁴⁷'Collective investigation', *Br. med. J.*, 1883, i: 22; see also 'Collective investigation of disease [Birmingham and Midlands Branch]', *Br. med. J.*, 1883, i: 32.

⁴⁸'Report on pneumonia', op. cit., note 33 above, p. 32; Isambard Owen, 'Report on the inquiry into the connection of disease with habits of intemperance', in *The Collective Investigation Record*, London, British Medical Association, 1888, vol. 4, pp. 100–111.

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speak for themselves. Facts, like opinions, were diverse and contradictory. As the authors of the CIC's report on pneumonia diffidently put it:

The large body of facts here brought together point to certain conclusions which, while they cannot be said in any case to reach the level of demonstration, are based upon different degrees of evidence whose precise value will be variously estimated according to the prepossessions of individual readers.⁴⁹

What, then, had collective investigation achieved?

Collective investigation, committee members conceded, had “not realized the sanguine expectations of some of its promoters”. None the less, it would be “an error to regard its career in the past five years as a failure”.⁵⁰ Collective investigation, proponents claimed, had done something far more valuable than produce definitive “positive” knowledge. In several cases, inquiries shed doubt on well-accepted beliefs in the profession.⁵¹ More importantly, the careful note-taking required by collective investigation honed participants' observational skills, a result outlasting the reports themselves.⁵² In teaching general practitioners how to observe, collective investigation had created a “small ‘army of observation’ within the profession”.⁵³ Such methodical note-taking and observation was highly valued within the consultant milieu.⁵⁴ In the eyes of most BMA members, however, “collective” as opposed to “individual” investigation, was moribund. By January 1887, the BMA began winding down its support for collective investigation.⁵⁵ The last report, on medical histories of the aged, was distributed at Humphry's personal expense in 1889.⁵⁶ The BMA had better uses for its money, including subsidies for a newly created committee on therapeutics.⁵⁷

Collective investigation did not lack for external enemies: jealous provincial practitioners like Thomas Dolan, consultants like Birmingham's Balthazar Foster advancing the interests of the Section on Therapeutics, or BMA members who were simply looking for more practical guidance on treatment. Their criticisms were fuelled by pre-existing tensions between BMA branches and the London consultant milieu. Yet collective

⁴⁹ ‘Report on pneumonia’, op. cit., note 33 above, p. 64. See also the anonymous criticism in ‘Preliminary report of the Collective Investigation Committee on Diphtheria’, *The Collective Investigation Record*, vol. 1, op. cit., note 14 above, pp. 28–33.

⁵⁰ Memorandum on collective investigation, Dec. 1886, BMA.

⁵¹ G M Humphry, ‘Report on aged persons’, *The Collective Investigation Record*, vol. 4, op. cit., note 48 above, pp. 85–99, on p. 90; Memorandum on collective investigation, Dec. 1886, BMA.

⁵² ‘Collective investigation’, *Br. med. J.*, 1885, i: 196–7; Finny, ‘Collective investigation’, op. cit., note 24 above, p. 472; Coupland, op. cit., note 38 above, p. 1198; Philip H Kidd, ‘The late Mr Charles Palmer and collective investigation’, *Br. med. J.*, 1885, i: 208. Kidd was none the less critical of collective investigation for not insisting that practitioners record their notes while observing the patient.

⁵³ Memorandum on collective investigation, Dec. 1886, BMA.

⁵⁴ Sir James Paget, *Studies of old case-books*, London, Longmans, Green, 1891, pp. v–viii; Duckworth, op. cit., note 35 above; A[rchibald] E G[arrod], ‘Sir Dyce Duckworth, Bart., M.D., 1840–1928’, *St. Bartholomew's Hosp. Repts*, 1929, 62: 18–41, p. 27.

⁵⁵ Francis Fowkes to Isambard Owen, 19 Jan. 1887; Quarterly report of the Standing Subcommittee, Jan.–March 1887, BMA.

⁵⁶ George Murray Humphry, *Old age: the results of information received respecting nearly nine hundred persons who had attained the age of eighty years, including seventy-four centenarians*, Cambridge, Macmillan and Bowes, 1889. On distribution, see ‘The British Medical Association and collective investigation’, *Br. med. J.*, 1928, ii (Supplement): 245–8, p. 248.

⁵⁷ Minutes, BMA Council, 18 July 1888, 12 July 1888, b/54/2/4, BMA.

investigation's injuries were ultimately self-inflicted. Humphry and his associates had tried to enlist general practitioners in an intellectual project born of their own experience as consultant physicians. Raised in the hospital milieu of morbid anatomy, they had encountered the limits of anatomical pathology for explaining and managing clinical disease. Believing that the key to pathology (and therapy) lay in tracing the manifestations of disease across the generations, they had envisioned a series of "life-history" albums through which general practitioners could record the natural history of diseases in individuals and their families.⁵⁸ This most ambitious of collective investigations was stillborn: general practitioners had neither the opportunity nor, in most cases, the interest to sustain such a project.

General practitioners in late-nineteenth-century Britain faced a harsh marketplace in which professional and economic difficulties loomed large. Local colleagues were easily regarded as competitors, not as potential collaborators.⁵⁹ Even the more idealistic practitioners were not in a position to wait for that "truer" pathology which would direct them towards a sounder therapeutics. The more cynical among them may have seen collective investigation as little more than a device for publicizing the names and reputations of their competitors, as did Dolan:

It is a great advantage to those practitioners whose names have been printed on the Collective Investigation cards. They have been thus brought before the profession in a special manner connected with a special disease, so that an inference might be drawn that they were authorities on this disease—an inference not always correct.⁶⁰

For consultants and general practitioners alike, the day when the "sun of science, which is the true Apollo of Medicine" would rise remained as remote as ever.⁶¹ In Britain, the decade of collective investigation was over.

In Search of Therapeutic Authority: Collective Investigation in the United States

The story of collective investigation in the United States is more episodic. At various times, "collective investigation" was taken up by state and local medical societies, by the

⁵⁸ F A Mahomed, 'On medical life-histories', *Br. med. J.*, 1882, **ii**: 1295–96; Minutes, Collective Investigation Committee, 20 March 1882, 1 Aug. 1883, 16 Oct. 1883, BMA; Minutes, Collective investigation of disease, Subcommittee Minutebook, 13 Oct. 1885, b/764/2/1, BMA; Karl Pearson, *Life, letters and labours of Francis Galton*, 3 vols, Cambridge University Press, 1924, vol. 2, pp. 360–7; Charles Roberts, 'The life-history album [letter]', *Br. med. J.*, 1884, **ii**: 1166.

⁵⁹ Anne Digby, *Making a medical living: doctors and patients in the English market for medicine, 1720–1911*, Cambridge University Press, 1994. Digby suggests that economic pressures were increasing in the 1880s, see *ibid.*, pp. 136–48. On jealousy and suspicion among practitioners, see Alfred Cox, *Among the doctors*, London, Christopher Johnson, [1950], pp. 54–6. I am grateful to Dr Andrew Morrice for calling this source to my attention.

⁶⁰ Dolan, *op. cit.*, note 44 above, p. 1249.

⁶¹ The phrase is William Gull's, announcing early plans for collective investigation. Gull, *op. cit.*, note 13 above, p. 141. Chris Lawrence suggests that resemblances between James Mackenzie's work at Aberdeen and collective investigation deserve further exploration (personal communication); see Jane Macnaughton, 'The St Andrews Institute for clinical research: an early experiment in collaboration', *Med. Hist.*, 2002, **46**: 549–68. In the 1920s, the BMA used the term "collective investigations" to describe its surveys of hospital consultants' results in using surgery to treat ulcers. Other than the name, these surveys have little in common with the purposes and methods of collective investigation. See Arthur P Luff, 'Report on collective investigation into the after-history of gastro-enterostomy', *Br. med. J.*, 1929, **ii**: 1074–78, 1125–29; *Br. med. J.*, 1930, **i**: 348–54.

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American Pediatric Society, by individual practitioners, and by the Parke, Davis drug company. Between 1883 and 1910, any survey of physicians might be deemed a “collective investigation”. The term, along with the British example, was even invoked by a representative of the Michigan State Board of Health, in an effort to promote sickness reporting by physicians.⁶²

The multiple invocations of collective investigation reflect the heterogeneous character of American medicine itself. Collective investigation was introduced to a medical profession still fractured along multiple lines: by training, by region and by generation. The American Medical Association was strongest in the north-east where members of a putative medical élite still expressed open contempt for the training and ability of physicians in the south and mid-west.⁶³ Within the north-east, younger laboratory-oriented physicians and an older generation of clinicians were similarly divided by interest and conviction.⁶⁴ Against this background of heterogeneity, collective investigation appealed to both local and national medical leaders looking to unify the profession. But such appeals meant little to most working practitioners, and the resulting hopes for collective investigation would frequently be disappointed.

As in Britain, collective investigation in the United States had precedents in medical society inquiries on therapeutics and in US Army studies of climatology and epidemic disease.⁶⁵ The immediate impetus came from an 1883 BMA invitation for the American Medical Association to organize a series of collective investigations. “In a country embracing so great an extent of territory as ours”, AMA representatives opined, state medical societies were “more likely to secure results of value, both in regard to quantity and quality” than any national committee.⁶⁶

For professional leaders, scientific uplift of the profession at large was seen as a major benefit of the enterprise. In the more pluralist, less stratified medical profession of North America, the observations of all were invited:

Disease is many-sided; and we wish to include in our organization those who see it from every side. All, therefore, whether hospital physicians, family and school attendants, specialists, medical officers of the Army and Navy, and of workhouses and asylums, will be asked to contribute their quota of observation to the common fund.⁶⁷

The AMA’s call met with a prompt, enthusiastic response from local medical leaders around the country but little interest from the profession’s rank and file. In 1884, the Medical Society of the State of Pennsylvania began organizing a collective investigation of

⁶² Henry B Baker, ‘Scientific collective investigation of disease’, *J. Am. med. Assoc.*, 1887, **9**: 486–90.

⁶³ John Shaw Billings, ‘Medicine in the United States, and its relations to co-operative investigation’, *Br. med. J.*, 1886, **ii**: 299–307, esp. pp. 300–4; William G Rothstein, *American physicians in the nineteenth century: from sects to science*, Baltimore, Johns Hopkins University Press, 1972, pp. 201–7.

⁶⁴ Warner, ‘Ideals of science’, op. cit., note 6 above; Toby A Appel, ‘Biological and medical societies and the founding of the American Physiological Society’, in Gerald L Geison (ed.), *Physiology in the American*

context, 1850–1940, Bethesda, American Physiological Society, 1987, pp. 158–62.

⁶⁵ See the sources cited in note 5 above, and James H Cassedy, *Medicine and American growth, 1800–1860*, Madison, University of Wisconsin Press, 1986, pp. 44–8.

⁶⁶ ‘Collective investigation of diseases’, *J. Am. med. Assoc.*, 1883, **1**: 216–18, p. 218. See also Davis, op. cit., note 3 above.

⁶⁷ ‘International collective investigation’, *J. Am. med. Assoc.*, 1884, **3**: 442–3, p. 442. On uplift, see also ‘Collective investigation of diseases’, op. cit., note 66 above.

pneumonia, modelled on the British inquiry. Of the 2000 cards distributed, only sixty were returned in the first year. These “very meagre” results were barely improved on with the ninety-one results from the rheumatism inquiry reported in 1886. After limping along for two additional years, the committee asked to be “discharged”.⁶⁸ In metropolitan New York, the county medical society sent out 800 cards for an investigation of fibrous pneumonia: “only forty-six practitioners and four hospitals” took an “active part” in producing the eighty-seven case reports.⁶⁹ Ohio’s committee, in existence from 1883 to 1887, apparently produced no results worth reporting.⁷⁰ Illinois reported that the number of results was “so small that a tabulation . . . would possess no practical value”.⁷¹ Missouri’s epidemiological investigation of malarial fever yielded replies from thirty-seven of the state’s 115 counties.⁷²

Most local medical societies gave up collective investigation after a few years’ frustration. In Connecticut, the state medical society persisted into the new century, reporting inquiries on new drugs (1885, 1889), albuminuria (1888), syphilis (1890), appendicitis (1894), diphtheria antitoxin (1895), treatment for typhoid (1897), malaria in children (1898), rheumatism (1902) and pulmonary tuberculosis (1903). Yet the returns in Connecticut were no stronger than elsewhere, between seventy to ninety of the state’s 600-plus physicians.⁷³ As the committee asking about typhoid therapies complained of its seventy-one replies:

With such a theme as this before them it was hoped that the interest of every practitioner would be enlisted, especially that men who had seen most of the disease . . . would contribute to the solution of the unsettled questions. But it has not been so. From Stamford where there has been within two years an epidemic of four hundred and six cases only one response was received; very many of our leading practitioners in both city and county have kept their knowledge to themselves. This is not what the Connecticut Medical Society was organized for; this is not the normal attitude of the profession. It required only a little thought and time on the part of each one to make a strong showing that would be creditable. Something cannot come from nothing in medical investigations any more than in agriculture or commerce.⁷⁴

A poor yield aside, the information reported in collective investigations did little to bolster the confidence asserted “in the acumen and wisdom of the general

⁶⁸ James Tyson, Charles K Mills, R H Chase, ‘Report of the Committee on the Collective Investigation of Disease’, *Trans. Med. Soc. State Penn.*, 1885, **17**: 66–73; William A Edwards, ‘Report of the Committee on the Collective Investigation of Disease. Report on acute rheumatism’, *Trans. Med. Soc. State Penn.*, 1886, **18**: 84–8. For the creation of the committee, see ‘Minutes of the annual meeting’, *Trans. Med. Soc. State Penn.*, 1884, **16**: 1–50, pp. 30–1; for dissolution, see ‘Minutes of the annual meeting’, *Trans. Med. Soc. State Penn.*, 1888, **20**: 1–33, p. 19.

⁶⁹ F A Seibert, ‘A collective investigation regarding fibrous pneumonia,’ *New York med. J.*, 1885, **41**: 697–8.

⁷⁰ See *Trans. Ohio State Med. Soc.*, vols 38–42.

⁷¹ J F Todd, ‘Report of the Committee on Original Investigation’, *Trans. Illinois State med. Soc.*, 1884, 324–5.

⁷² B F Hart, ‘Report of Special Committee on Collective Investigation of Disease,’ *St. Louis Courier Med.*, 1886, **16**: 1–22. The organization of the Missouri inquiry is unclear; it seems as if questionnaires were sent to the county medical societies, who chose only one physician to reply.

⁷³ See the various ‘Report[s] of the Committee on Matters of Professional Interest in the State’, *Proc. Conn. med. Soc.*, 1883–1905.

⁷⁴ ‘Report of the Committee on Matters of Professional Interest in the State’, *Proc. Conn. med. Soc.*, 1897, **105**: 97–119, p. 102. See similar complaints for the syphilis and appendicitis inquiries: ‘Report of the Committee on Matters of Professional Interest in the State’, *Proc. Conn. med. Soc.*, 1890, **99**: 251–69, pp. 251–3; ‘Report of the Committee on Matters of Professional Interest in the State’, *Proc. Conn. med. Soc.*, 1894, **102**: 91–111, pp. 91–3.

practitioner”⁷⁵ Poor record keeping meant unanswered questions. Poorly reported data compounded the problem: “It is impossible to tell how many cases the replies represent or with what care the observations were made.”⁷⁶ The greatest difficulty came from the diversity of opinion among those surveyed. *Convallaria majalis* (lily of the valley) might be “much more uniform” or “less certain” than digitalis, “more reliable than digitalis as a tonic” or “not so reliable as digitalis”, depending on which anonymous opinion one believed.⁷⁷

After two decades of collective investigation in Connecticut, J E Loveland, a young Middleton practitioner (Harvard Medical School, 1892) wondered about its value for questions better studied in the “hospital ward”. For some questions, collective investigation none the less had unique advantages: “we can only learn from the physician himself, if we can learn at all, how often he has been a carrier of the contagion of Scarlet Fever”. For his survey, Loveland sought out only “men who were forty years or over, who had large general practices and who were accurate observers”.⁷⁸ While the third-party reports received were too sketchy to be of value, he found the few first-hand reports, most from “intimate friends in the county”, fully persuasive.⁷⁹ Information about the practices Connecticut physicians used to prevent transmission was less informative. Loveland’s greatest “surprise” was that 11 per cent of practitioners did not believe that they would transmit scarlet fever via their clothes or persons. Apparently, one could and did learn from collective investigation how poorly physicians kept up with current knowledge.⁸⁰

For a group of Philadelphia physicians, fifty-four cases seemed sufficient to demonstrate “the positive efficaciousness of sweet-oil [olive oil] in the treatment of gall-stone colic”.⁸¹ Their colleagues remained unconvinced. The committee was either mistaken—without post-mortem data, how did they know pains were due to gallstones?—or deceived—any lubricant might temporarily relieve a spasm but would do nothing to dissolve stones.⁸²

The most successful of all collective investigations was the American Pediatric Society’s (APS) inquiry on diphtheria antitoxin. Introduced into the United States from Europe in 1894–95, antitoxin’s value was challenged by physicians who questioned the bacteriological case-definitions used in the initial evaluations. Sceptics asserted that such cases were not “true” clinical diphtheria, and the favourable results reported for antitoxin were not to be believed.⁸³ The paediatric specialists leading the APS had a different

⁷⁵ ‘Report of the Committee on Matters of Professional Interest in the State’, *Proc. Conn. med. Soc.*, 1896, **106**: 111–24, p. 111.

⁷⁶ ‘Report of the Committee on Matters of Professional Interest in the State’, *Proc. Conn. med. Soc.*, 1885, **94**: 47–95, p. 60.

⁷⁷ *Ibid.*, pp. 61–2. See also the inquiry on antipyretics: ‘Report of the Committee on Matters of Professional Interest in the State’, *Proc. Conn. med. Soc.*, 1889, **98**: 73–82, pp. 78–9.

⁷⁸ J E Loveland, ‘The physician as a carrier of the contagion of scarlet fever: a collective investigation’, *Proc. Conn. med. Soc.*, 1904, **112**: 173–208, quotes on p. 175.

⁷⁹ *Ibid.*, pp. 183, 187–8.

⁸⁰ *Ibid.*, p. 179. See also the discussion of a lack of innovation in typhoid treatment: ‘Report of the

Committee on Matters of Professional Interest’, *Proc. Con. med. Soc.*, 1897, **105**: 97–119.

⁸¹ ‘A collective investigation by the Therapeutic Section of the Philadelphia Polyclinic Medical Society. Sweet-oil in the treatment of Gall-stones’, *The Times and Register*, 1891, 260–7, p. 265. The committee’s idea of collective investigation was capacious: seventeen of the fifty-four cases were from citations to the published literature.

⁸² *Ibid.*, pp. 265–7.

⁸³ Evelyn Maxine Hammonds, *Childhood’s deadly scourge: the campaign to control diphtheria in New York City, 1880–1930*, Baltimore, Johns Hopkins University Press, 1999, pp. 122–31. See also the discussion of the case-mix issue in William H Welch, ‘The treatment of diphtheria by antitoxin’, *Trans. Assoc. Am. Phys.*, 1895, **10**: 312–75.

concern. The earliest studies, done in municipal hospitals and on tenement populations, tilted towards advanced cases, treated several days into the illness. Such studies were of limited value to the community practitioner:

There are very few hospitals in America that receive diphtheria patients and the conditions under which patients are admitted to hospitals and the surroundings while there are so different from those of private practice, that the measure of success in hospital cases cannot be taken as an index of the results which have been obtained upon this side of the Atlantic with the new treatment.⁸⁴

The APS surveyed 613 physicians in fifteen states across the nation, accumulating data on 3,384 cases.⁸⁵ Analyses distinguished results by age, co-morbidity, severity of disease and date of treatment, as well as by whether or not the diagnoses had been bacteriologically confirmed. The results were striking: antitoxin treatment lowered mortality to 13 per cent and, for patients treated on the first day of illness, to an unprecedented 4.9 per cent.⁸⁶ The Society's recommendation that diphtheria antitoxin be used as early as possible was "published in virtually every medical journal" in the country, largely ending debates about antitoxin's value.⁸⁷

To some members, however, the APS's recommendation on antitoxin "looks like establishing scientific truths by legislation".⁸⁸ The occasion for this complaint was a heated debate over a second APS collective investigation, on the causes of infantile scurvy. The committee reported that in 275 of 379 cases, a faulty diet was involved, and that a change from commercial foods to a more "natural" diet seemed to reverse the condition.⁸⁹ Though the committee was at pains to insist that it was simply summarizing the experiences of others, some APS members saw the report as an attack on sterilized

⁸⁴L Emmett Holt, W P Northrup, Joseph O'Dwyer and Samuel S Adams, 'The report of the American Pediatric Society's collective investigation into the use of antitoxin in the treatment of diphtheria in private practice', *Trans. Am. Ped. Soc.*, 1896, **8**: 21–45, p. 21. Both antitoxin's proponents and critics could point to the non-representative character of hospital studies: John W Branna, 'A critical analysis of Dr. Winters' clinical observations on the antitoxin treatment of diphtheria', *Med. News*, 1896, **48**: 691–4; John W Kyger, 'A protest against accepting the conclusions of hospital physicians as to the value of antitoxine in diphtheria', *New York med. J.*, 1895, **62**: 151.

⁸⁵The study also reported cases from 942 cases treated by the New York City Health Department and 1,468 cases treated by the Chicago Health Department. Holt, *et al.*, 'Report', *op. cit.*, note 84 above.

⁸⁶*Ibid.* The stratified analysis follows closely the earlier discussion by William Henry Welch concerning factors which had confounded interpretation of the hospital results. Welch, *op. cit.*, note 83 above. It is difficult to compare these results directly with pre-antitoxin experience; in-hospital case-fatality rates could run at 50 per cent or higher but there was limited data on community practice prior to the APS study. The results were universally acclaimed as dramatic, however.

⁸⁷Barbara Gutmann Rosenkrantz, 'Cart before horse: theory, practice and professional image in American public health, 1870–1920', *J. Hist. Med. Allied Sci.*, 1974, **29**: 55–73, p. 70. See also Hammonds, *op. cit.*, note 83, above, pp. 132–6. A second favourable report, on cases involving partial or total blockage of the larynx, was published the following year. W P Northrup, Joseph O'Dwyer, L Emmett Holt and Samuel S Adams, 'The American Pediatric Society's report on the collective investigation of the antitoxin treatment of laryngeal diphtheria in private practice, 1896–1897', *Trans. Am. Pediatr. Soc.*, 1897, **9**: 32–8.

⁸⁸Comments of Dr [Walter Shield] Christopher, in 'The American Pediatric Society's collective investigation on infantile scurvy in north America', *Arch. Pediatr.*, 1898, **15**: 481–508, p. 500. Nathan Davis made a similarly disparaging analogy to politics, describing collective investigations as like deciding "scientific questions by popular vote". See Davis, *op. cit.*, note 3 above, p. 44.

⁸⁹*Ibid.* The committee took little note of the fact that fruit juice was included in most (257) of the successfully treated cases. On doctors and "artificial" feeding, see Rima D Apple, *Mothers and medicine: a social history of infant feeding, 1890–1950*, Madison, University of Wisconsin Press, 1987, pp. 23–34, 53–71.

milk, which had been used in many cases to prepare the foods. Milk sterilization had been heavily promoted by paediatricians as “one of the greatest advance[s] in the last half of this century”. Dr August Caillé pressed for a minority report which would exonerate sterilization “*per se*” from promoting scurvy.⁹⁰

The dispute over sterilization soon turned into a debate on the value of collective investigation. As J P Griffith, the committee’s chair explained, they were reluctant to draw conclusions based upon the reports of “observers we do not know. . . . We do not know how many [reports] are accurate, of course, and so we only took the figures received, added them up and gave you the results”.⁹¹ The report’s lack of conclusions bothered some members, while its implied indictment of sterilized milk troubled others even more.⁹² Further doubts about the nature and status of collective investigations soon emerged. Supposing conclusions *were* desired, should the report incorporate the personal experience and judgements of committee members or should the findings remain “not clouded by individual opinions?”⁹³ As Dr Edward M Buckingham, Instructor in the Diseases of Children at the Harvard Medical School, observed:

When Dr. Caillé presented his minority report it seemed to me that what he did was to proffer the evidence that has come from his personal knowledge and the knowledge of his personal friends rather than the observations of people that he knows very little about. There are just two conclusions that can be drawn: Either sterilization of milk produces scurvy or collective investigations are not a safe way of getting information.⁹⁴

The APS accepted the committee’s inconclusive report on infantile scurvy, but collective investigation itself had proved untrustworthy.⁹⁵ A mistrust of data from “unknown” individuals, combined with anxieties that institutional authority might pre-empt individual clinical judgement, ended collective investigations at the APS.⁹⁶ Paediatric meetings returned to the norm: detailed clinical reports from seasoned clinical observers were assessed by the private judgements of individual practitioners.

From the start, collective investigations in the United States had tilted towards practical therapeutic questions. No surprise that collective investigation was taken up by a progressive drug firm, Parke, Davis & Company. The company published its results in a house organ, the *Therapeutic Gazette* and in a series of working bulletins on specific drugs.⁹⁷ With drugs sent to “a large number of practitioners scattered over the land”, the company did “not claim that the information gathered in this way is conclusive” but

⁹⁰Remarks of Dr [August] Caillé in ‘Collective investigation of infantile scurvy’, op. cit., note 88 above, p. 506; for Caillé’s minority report, see *ibid.*, p. 500. On the Committee’s demurrals, see *ibid.*, pp. 485, 495, 507.

⁹¹Remarks of Dr Griffith, *ibid.*, p. 507.

⁹²Compare the comments of Drs [Walter Shield] Christopher and [William Perry] Northrup, *ibid.*, p. 500.

⁹³Remarks of Drs [Samuel S] Adams, [John Lovett] Morse and [Abraham] Jacobi, *ibid.*, pp. 504–5.

⁹⁴*Ibid.*, p. 502.

⁹⁵*Ibid.*, p. 508.

⁹⁶The APS did publish editorial notices of two subsequent collective investigations conducted by other groups: ‘Collective investigation by the Gesellschaft für Kinderheilkunde of Barlow’s Disease (Infantile Scurvy)’, *Arch. Ped.*, 1904, **21**: 212–14; ‘Collective investigation of anterior poliomyelitis’, *Arch. Ped.*, 1907, **24**: 849–50.

⁹⁷On George Davis’ pioneering use of journals to promote his products, see Tom Mahoney, *The merchants of life: an account of the American pharmaceutical industry*, New York, Harper & Brothers, 1959, pp. 71–2.

insisted that the “method is a very valuable one for collecting evidence”.⁹⁸ The resulting bulletins provided basic descriptions of the drug, followed by physicians’ “reports”:

I have given the pound of Bladderwrack you sent some time ago, to a very corpulent lady who was suffering from a suppression of the menses. After she had taken the medicine for two or three days, her menses started up again, and her health commenced improving, while at the same time her flesh began to diminish until, at the present, the diminution is perceptible to the most casual observer.⁹⁹

A handful of reports came from physicians in hospital practice and pharmacologists whose work the company acknowledged as “more scientific”, but most resembled the testimonials common to the era’s “ethical” drug industry.¹⁰⁰ With few exceptions, the studies were neither collective nor investigations, but endorsements gathered to promote the company’s products.¹⁰¹

Physician-organized surveys continued into the early twentieth century. In 1909, George Richards surveyed “prominent laryngologists in this country and Europe” about their beliefs and practices regarding tonsillitis.¹⁰² In 1910, a joint committee of the New York Neurological Society and the New York Academy of Medicine published a monograph on their “collective investigation” of the 1907 poliomyelitis epidemic. Rather than publish “a mere array of statistics”, the organizers presented analyses from a select group of observers. Selective in its presentation of clinical and pathological data, the study resembles a traditional scientific report in tone and format. Its brief discussions of therapy and rehabilitation are circumspect and uncontroversial.¹⁰³ It is difficult to say when the last collective investigation in the United States took place. By the First World War, the term seems to have gone out of favour, but it had by then lost any special meaning it might have had in the movement’s first decade.

Collective investigations in the United States lacked the ideological inspiration manifest in the British example. Although local medical societies, composed largely of general practitioners, sought out the experiences of ordinary physicians, no one objected when specialty societies surveyed a more selective group. Medical knowledge, like medical society, was heterogeneous. The meagre results from local collective investigations revealed to medical élites just how unevenly developed that society was. Most general practitioners held onto their clinical experience as valuable private property. Even collective ventures successful at generating data, such as the APS’s inquiries, trod on

⁹⁸ Parke, Davis & Co’s collective investigation of drugs by the Working Bulletin system’, in *Working Bulletin for the Scientific Investigation of Manaca*, Detroit, Scientific Department Parke, Davis & Co, 1884, p. iii.

⁹⁹ Bladder-wrack: clinical reports from private and hospital practice’, in *The pharmacology of the newer materia medica*, Detroit, George S Davis, 1892, p. 107.

¹⁰⁰ See [Issac Ott], ‘The physiological action of *Urechites suberecta*’, in *The pharmacology of the newer materia medica*, op. cit., note 99 above, pp. 1195–98.

¹⁰¹ The company also supplied drugs to individuals or organizations conducting collective investigations. See John Aulde, ‘Arsenite of copper—the results of

collective investigation’, *Trans. med. Soc. State Penn.*, 1890, **21**: 200–2.

¹⁰² George L Richards, ‘The present status of the tonsil operation: a collective investigation’, *Ann. Otol. Rhinol. Laryngol.*, 1909, **18**: 739–85.

¹⁰³ Of the 4,000 physicians surveyed, 470 of the 1,100 doctors answering replied that they had seen poliomyelitis cases. Their 752 case reports were then reviewed by the committee, which selectively reported on their findings. *Epidemic poliomyelitis. Report on the New York epidemic of 1907 by the Collective Investigation Committee*, New York, Journal of Nervous and Mental Disease Publishing Company, 1910, pp. 4–9, 29–54, quote on p. 9.

unsure ground when they based clinical recommendations on the findings. The medical collective remained resolutely laissez-faire, better able to accommodate drug company testimonials than organized efforts to guide clinical practice.¹⁰⁴

Collective Investigation: Medical Community in the Long and Short Run

The story of collective investigation belongs to the long-run history of professional collective action, which extends from organized epidemiological inquiries in the eighteenth century to the late-twentieth-century movement for evidence-based medicine. Such efforts depend on substantial contributions of voluntary labour, much of it from hard-pressed working practitioners. As with all voluntary organizations, individuals must be strongly motivated to undertake activities with little or no immediate material reward.¹⁰⁵ On this theoretical account, collective action is difficult at best. What are the conditions that favour success?

Historians have given the most attention to organized epidemiological investigations. These inquiries succeeded when data collection was an intrinsic part of the job, as it was for the reporting physicians in the US Army, whose medical officers demanded such reports;¹⁰⁶ or when salaried medical officers of health used vital statistics to persuade local rate-payers in England to invest in sanitary improvements.¹⁰⁷ Getting private practitioners to contribute even to a relatively simple task like disease reporting was a struggle. English public health authorities experimented with fining non-compliant physicians before deciding to pay for each report.¹⁰⁸

We lack similarly fine-grained historical accounts of efforts to extend the epidemiological model to clinical questions of disease treatment and aetiology. Local medical societies in Britain and the United States repeatedly attempted such organized inquiries in the nineteenth century, although few were as long-lived as the movement for collective investigation.¹⁰⁹ Both collective investigation and its predecessors shared a common difficulty—that of mobilizing practitioners to devote time and attention to clinical observation. Well into the twentieth century, organized therapeutic evaluations faced analogous

¹⁰⁴On the subsequent history of such efforts, see Harry M Marks, *The progress of experiment: science and therapeutic reform in the United States, 1900–1990*, Cambridge University Press, 1997.

¹⁰⁵Mancur Olson, *The logic of collective action: public goods and the theory of groups*, Cambridge, MA, Harvard University Press, 1965.

¹⁰⁶On the US Army studies, see Cassedy, op. cit., note 65 above, pp. 44–8; see also Peter Mathias' discussion of organized inquiry in the British military in 'Swords and ploughshares: the armed forces, medicine and public health in the late eighteenth century', in *idem*, *The transformation of England: essays in the economic and social history of England in the eighteenth century*, London, Methuen, 1979, pp. 265–85.

¹⁰⁷John M Eyler, *Victorian social medicine: the ideas and methods of William Farr*, Baltimore, Johns

Hopkins University Press, 1979, pp. 123–49; Simon Szreter, 'The GRO and the public health movement in Britain, 1837–1914', *Soc. Hist. Med.*, 1991, **4**: 435–63; Graham Mooney, 'Professionalization in public health and the measurement of sanitary progress in nineteenth-century England and Wales', *Soc. Hist. Med.*, 1997, **10**: 53–8.

¹⁰⁸Graham Mooney, 'Public health versus private practice: the contested development of compulsory infectious disease notification in late-nineteenth-century Britain', *Bull. Hist. Med.*, 1999, **73**: 238–67. As Mooney notes, opposition to disease reporting was based on more than just the lack of financial incentives. See also Daniel M Fox, 'Social policy and city politics: tuberculosis reporting in New York, 1889–1900', *Bull. Hist. Med.*, 1975, **49**: 169–95.

¹⁰⁹See the sources cited in note 5 above.

problems in overcoming a medical culture of individualism. Research physicians lacked both the time and inclination to complete cooperative studies according to agreed-upon protocols.¹¹⁰

As important as the recurrent failures of collective action are the seemingly ceasing attempts at collaborative investigations of disease and treatment. Sociologically, these efforts flourished more readily in metropolitan areas where professional networks were dense, and the opportunities for scientific exchange were greatest. The inquiry on myxoedema, organized by the Clinical Society of London in the 1880s, built on the Society's existing network of consultant physicians.¹¹¹ More significant in the long run were the various communities which transcended geographically fixed medical societies. National specialty groups are the most obvious example: the American Pediatric Society was far more successful in obtaining data for its collective investigations than any of the state medical societies. No less important were the "intentional communities" formed around a particular vision of medical knowledge: the American alumni of the Paris hospitals, studied by John Warner, or the community of "therapeutic reformers" in the twentieth-century United States, organized around a shared programme of therapeutic truth.¹¹²

Organized collective investigation in Britain was one such intentional community, built around the idea that only general practitioners could track the complete natural history of a disease, and thereby "catch mischief at its very dawn" and pursue "the various evils to which it may ultimately lead".¹¹³ General practitioners did not conceive this project, which arose in the heart of the London consultant milieu among individuals who taught and practised morbid anatomy. Their programme for collective investigation was both scientific and social. Observations in the dissecting room and the hospital ward could not explain why it was that in some children "every scratch 'festers'" and "every strained joint inflames" while in others such accidents leave no mark.¹¹⁴ By enlisting general practitioners to observe *in situ*—at the bedside and in the home—Humphry and his associates hoped to elucidate the mysteries of variation in susceptibility to disease. At the same time, they hoped to refashion general practitioners in their own image as methodical and patient observers of disease.

In the event, collective investigation failed to bridge the profound gaps between the world of the hungry, scabbling practitioner and that of the inquiring, flourishing consultant. To understand fully the story of collective investigation in Britain, we would have to know far more than we do at present about the material, moral and intellectual worlds of both consultant and general practitioner communities.¹¹⁵ Such a historical anthropology of

¹¹⁰ Harry M Marks, 'Notes from the underground: the social organization of therapeutic research, 1920–1950', in Russell C Maulitz and Diana Long (eds), *Grand rounds: one hundred years of internal medicine*, Philadelphia, University of Pennsylvania Press, 1988, pp. 297–336; Marks, op. cit., note 104 above, pp. 53–60, 98–128.

¹¹¹ 'Report of a Committee of the Clinical Society of London . . . to investigate the subject of myxoedema', *Trans. Clin. Soc. London*, 1888, 21 (Supplement). For background, see Clark T Sawin, 'Introduction', *Report on myxoedema*, facsimile

edition, Boston, Francis A Countway Library of Medicine, 1991, pp. 1–14.

¹¹² John Harley Warner, *Against the spirit of system: the French impulse in nineteenth-century American medicine*, Princeton University Press, 1999; Marks, op. cit., note 104 above.

¹¹³ Prescott Gardener Hewett, 'Address by the President', *Trans. Clin. Soc. London*, 1873, 6: xxxv–xliv, p. xxxv.

¹¹⁴ Paget, op. cit., note 20 above, p. 376.

¹¹⁵ For one example of an anthropology of medical labour and knowledge, see Stephen M Stowe, 'Seeing

medical knowledge might tell us whether general practitioners were simply indifferent or actively hostile towards collective investigation. It might better inform us about the trajectory of morbid anatomy in an élite medical milieu that was, historians tell us, largely dominated by the contests for power and authority between laboratory medicine and clinicians.

The story of collective investigation in the United States is less complex. Collective investigation had no distinctive intellectual identity there. Topographical and climatological studies, which might have loomed large at mid-century, were not pursued.¹¹⁶ America's pathologists did not consider collective investigation of any particular value to their enterprise.¹¹⁷ Accordingly, the American history of collective investigation is largely a story about the difficulties of realizing medical community. Like their British counterparts, American national and local medical élites saw collective investigation as a way of involving ordinary practitioners in the scientific life of the profession. But few practitioners heeded the call, even in an urbanized state such as Connecticut and its principal cities, New Haven, Hartford and Stamford. Therapeutic knowledge was a form of private property, jealously guarded. Only where practitioners saw a material advantage from publicity did they participate, as in Parke, Davis's *Therapeutic Gazette*. And only in Connecticut did local medical societies persist with their inquiries; if collective investigation was meant to build medical community, it did the job poorly.

In national specialty groups, where the professional rewards for sharing knowledge were better established, the history of collective investigation developed differently. Among specialists, the community which collective investigation sought to create already existed. Specialists acknowledged a common interest in collecting and sharing data, a familiar exercise for those in the scientific élite. But the limits to community were equally clear.¹¹⁸ At the American Pediatric Society, adjudicating practice on the basis of data gathered from "observers we do not know" went beyond those limits. A collective investigation like the New York report on poliomyelitis, by contrast, made no such demands on practitioners. Cautiously edited, the report's authors weighed additions to the store of medical

themselves at work: physicians and the case narrative in the mid-nineteenth-century American South', *Am. Hist. Rev.*, 1996, 101: 41–79.

¹¹⁶It is worth noting that John Shaw Billings' call for collective investigation of race and climate in medical geography went unheeded: see Billings, *op. cit.*, note 63 above, pp. 305–6. Nathan Davis' desire to pursue on-going meteorological investigations via collective investigation was similarly disappointed. See Davis, *op. cit.*, note 3 above. On the geographical tradition, see Ronald L Numbers, 'Medical science before scientific medicine: reflections on the history of medical geography', in Nicolaas A Rupke (ed.), *Medical geography in historical perspective*, London, Wellcome Trust Centre for the History of Medicine at UCL, 2000, pp. 217–20.

¹¹⁷A deeper exploration would be needed to explain why American pathologists did not take up collective investigation with the fervour of British morbid anatomists. Russell Maulitz suggests that by the late

nineteenth century, American pathologists had assumed the identity of a specialized scientific discipline whose preoccupations with clinical medicine were no longer central to its mission. Russell C Maulitz, '“The whole company of pathology”—pathology as idea and as work in American medical life', in Teizo Ogawa (ed.), *History of Pathology. Proceedings of the 8th International Symposium on the Comparative History of Medicine—East and West*, Osaka, Taniguchi Foundation, 1986, pp. 139–61; Russell C Maulitz, 'Pathologists, clinicians, and the role of pathophysiology', *Physiology in the American Context, 1850–1940*, Bethesda, MD, American Physiological Society, 1987, pp. 209–36.

¹¹⁸On gift cultures and peer review more generally, see Harry M Marks, 'Local knowledge: experimental communities and experimental practices, 1918–1950', Paper presented at the conference on Twentieth Century Health Sciences: Problems and Interpretations, University of California, San Francisco, May, 1988.

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knowledge but required no changes in clinical practice. Normal science of this sort posed no direct challenges to the social order of medicine.

The movement for collective investigation ended in Britain by 1890, lingering on in the United States through the first decade of the twentieth century. If the movement is more than a historical oddity, it is because of what its history can tell us about the fabric of medical community in these two societies. General historians in recent decades have given us an idea of the complexity of the cultural and social processes by which a sense of national identity is achieved.¹¹⁹ The history of collective investigation suggests that achieving a sense of shared purposes and mutual obligation within medical society was no less difficult or complex.

¹¹⁹ As examples from an enormous literature, see Peter Sahlins, *Boundaries: the making of France and Spain in the Pyrenees*, Berkeley, University of California Press, 1991; David A Bell, *The cult of the nation in France: inventing*

nationalism, 1680–1800, Cambridge, MA, Harvard University Press, 2001; Gyanendra Pandey, *Remembering partition: violence, nationalism and history in India*, Cambridge University Press, 2001.