“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

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

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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. 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 élite 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 élite 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.
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. 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. 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. 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.”

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. “If the [BMA’s]
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


16 For 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.


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


23 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.


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

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”. 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.

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”.

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. 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”.

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

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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.

28 Humphry, op. cit., note 14 above, p. 3.

anecdotes. 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. 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”. A CIC report on pneumonia was similarly equivocal about its findings.

Enthusiasm for collective investigation, high at first, quickly diminished. By 1885, results from numerous inquiries had slowed to a trickle. 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.” Practitioners complained that they were “too busy” in the day and “too tired” at night to spare time to write up their observations. Thomas Dolan, a Yorkshire physician, sarcastically asked whether, if the results were truly so valuable, practitioners should not be paid for collecting them. Even supporters of collective investigation wondered whether “the Committee may have too much lost sight of the conditions under which the practitioner works”. The CIC considered various expedients, including (briefly) a greater reliance on hospital physicians for data.

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? Opposition was furthered by groups within

31 These reports, all deemed “preliminary”, were published in The Collective Investigation Record, vol. 1, op. cit., note 14 above, pp. 93–133.
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.
39 Minutes, CIC committee, 14 Jan. 1885; Minutes, General Committee, CIC, 14 Oct. 1885, BMA.
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. 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.

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.” 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.

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. 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”. 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”. It did not help that some CIC facts, such as degrees of “abstinence” from drink, were poorly defined. But as CIC reports reluctantly acknowledged, even well-gathered facts cannot...
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 partici-
pants’ 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 practi-
tioners 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[Garrod], ‘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.
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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. 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. 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.

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. 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


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.


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 elite 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

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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

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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.


practitioner''. 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.” 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.

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”. 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. 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.

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”. 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.

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. The paediatric specialists leading the APS had a different

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.
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. The APS surveyed 613 physicians in fifteen states across the nation, accumulating data on 3,384 cases. 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. 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.

To some members, however, the APS’s recommendation on antitoxin “looks like establishing scientific truths by legislation”. 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. 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

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85 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.

86 Ibid. The committee took little note of the fact that fruit juice was included in most (257) of the successfully treated cases. It 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.


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

90 Remarks 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.
91 Remarks of Dr Griffith, ibid., p. 507.
94 Ibid., p. 502.
95 Ibid., p. 508.
97 On 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”. 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. 

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. With few exceptions, the studies were neither collective nor investigations, but endorsements gathered to promote the company’s products.

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. 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. 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 elites 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...
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.  

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. 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; or when salaried medical officers of health used vital statistics to persuade local rate-payers in England to invest in sanitary improvements. 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.

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. 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

109 See 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.\footnote{Harry M Marks, ‘Notes from the underground: the social organization of therapeutic research, 1920–1950’, in Russell C Maulitz and Diana Long (eds), \textit{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.}

As important as the recurrent failures of collective action are the seemingly unceasing 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.\footnote{John Harley Warner, \textit{Against the spirit of system: the French impulse in nineteenth-century American medicine}, Princeton University Press, 1999; Marks, op. cit., note 104 above.} 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.\footnote{Prescott Gardener Hewett, ‘Address by the President’, \textit{Trans. Clin. Soc. London}, 1873, 6: xxxv–xliv, p. xxxv.}

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”.\footnote{Paget, op. cit., note 20 above, p. 376.} 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 ‘fester’s’” and “every strained joint inflames” while in others such accidents leave no mark.\footnote{For one example of an anthropology of medical labour and knowledge, see Stephen M Stowe, ‘Seeing} By enlisting general practitioners to observe \textit{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, scrabbling 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.\footnote{For one example of an anthropology of medical labour and knowledge, see Stephen M Stowe, ‘Seeing} Such a historical anthropology of
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. America’s pathologists did not consider collective investigation of any particular value to their enterprise. 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. 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 knowledge.


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. 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.