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The changing assessments of John Snow's and William Farr's cholera studies

Summary

This article describes the epidemiological studies of cholera by two major British investigators of the mid-nineteenth century, John Snow and William Farr, and it asks why the assessments of their results by contemporaries was the reverse of our assessment today. In the 1840s and 1850s Farr's work was considered definitive, while Snow's was regarded as ingenious but flawed. Although Snow's conclusions ran contrary to the exceptations of his contemporaries, the major reservations about his cholera studies concerned his bold use of analogy, his thoroughgoing reductionism, and his willingness to ignore what seemed to be contrary evidence. Farr's electic use of current theories, his reliance multiple causation, and his discovery of a mathematical law to describe the outbreak in London in 1849 was much more convincing to his contemporaries. A major change in thinking about disease causation was needed before Snow's work could be widely accepted. William Farr's later studies contributed to that acceptance.

Key-Words: History – Epidemiology – Cholera – Water – Elevation – John Snow.

The judgment of posterity is certainly unpredictable. Witness the changing acclaim for the cholera studies of John Snow and William Farr. Today John Snow is celebrated as the one who solved the mystery of cholera's transmission and as a founder of modern epidemiology. His work is held in the highest regard, and he is one of few nineteenth century medical figures whose name is likely to be known to most members of the health professions. William Farr, on the other hand, seems to be known today only to those very few in these professions who have well-developed historical interests. But in 1855, the year when the second and more famous edition of John Snow's "On the Mode of Communication of Cholera"[1] was published, the situation was reversed. Farr was then the recognised authority on vital statistics and epidemiology and the one whose report on the 1848–49 cholera epidemic was considered authoritative[2,3]. Snow's publications on cholera, on the other hand, were regarded as ingenious but seriously flawed[4 *p. 218, 221–36*; 5 *p. 525, 527*; 6 *p. 651–2*]. My purpose here is to suggest why the assessments of both men's work on cholera have changed so fundamentally since they wrote.

John Snow formulated his basic theory early in the 1848 outbreak by analogical reasoning on the pathology and therapeutics of the disease[7,8; 1 *p. 10–5*; 9 *p. 1643–4; p. 173–4*; 5 *p. 519–20; 523–4*, 6 *p. 649–50*; 4 *p. 204–5*]. Unlike other epidemic diseases which begin with general symptoms, such as fever, and whose morbid material was believed by most medical men to be present in the blood, cholera, Snow explained, began with local abdominal symptoms. This fact suggested to Snow that the disease was caused by a morbid material or poison which acted locally as an irritant on the surface of the stomach and intestines producing the pain, vomiting, diarrhoea, and dehydration that were the hallmarks of cholera. The fact that in its early stages cholera responded to treatments such as opium, chalk or catechu, which acted locally, seemed to confirm that cholera was a local disease of the gut. If the cholera poison acted solely on the surface of the alimentary canal, it seemed to follow that it must enter the body by being swallowed. It also suggested that the cholera poison ought to be present in the intestinal discharges of the sick. By his second edition Snow boldly drew on the examples of smallpox, cowpox and syphilis, inoculable diseases universally acknowledged to be contagious, to suggest that during an attack of cholera its morbid material was also multiplied in the body of the sick. If this were the case, as

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Snow clearly believed it was, then the cholera poison must be present in abundance in the intestinal discharge of cholera victims. Cholera, it followed, must be a communicable disease which spread when humans swallowed food or water contaminated with the dejecta of previous cholera victims.

Snow was not the only one to arrive at this conclusion in 1848–49. William Budd, later famous for demonstrating that typhoid fever was waterborne, reached a similar conclusion also by reasoning on pathological evidence. In a book published a mere 29 days after Snow's, Budd announced that he and a group of other Bristol medical men had found the agent of cholera, a fungus they had observed consistently in the stools of patients[10]. Snow never endorsed this theory, and the fact that it was soon discredited seems to have convinced him that his only hope of convincing others of his theory was to collect epidemiological evidence[4 p.157–88, 216–8, 5 p.521–3]. Snow was a general practitioner who had developed a practice in anesthesia and had conducted research in respiratory physiology and anesthesia, but he had no prior experience with epidemiology[9].

He relied on two types of evidence. The first was the result of outbreak investigations in which he could make the fecal-oral route of transmission plausible. Examples include his account of the cases at Albion Terrace in 1849 and the Broad Street outbreak in 1854[1 p.25–31, 38–54]. In the former, after inspecting the site, Snow reconstructed how the spring-fed water supply to this row of 17 houses could have become contaminated by leakage from the cesspools and surface water drains that served these same houses. Snow also examined a specimen of water from one of the domestic water tanks at Albion Terrace and found that it smelt like privy soil and contained bits of undigested food that had clearly passed through the alimentary canal. The Broad Street outbrak was much larger. In this instance Snow failed to find direct sensory evidence of fecal contamination of the water, although a subsequent investigation published by the parish suggested how the Broad Street pump may have been contaminated by the water used to wash the diapers of an infant who died of what may have been cholera[11]. The bulk of Snow's evidence in the Broad Street incident was drawn from his investigation of the circumstances of those who died of cholera in the vicinity. He could show that most of these had, or most likely had, consumed water from the public pump in Broad Street. Remarkably he could also implicate two cholera deaths in an area otherwise free from cholera, a widow in Hampstead and her daughter, who drank the water from the Broad Street pump after it was brought to them. Snow's colleagues found the two Hampstead deaths linked to the Broad Street pump water highly suggestive, but they did not find the evidence from Broad Street itself at all convincing. As one reviewer pointed out, Snow had not eliminated other explanations or the role of coincidence. Wells were so common in London that wherever the outbreak had occurred there probably would have been a well near its center[12; 6 p.651–2].

Snow's second type of evidence was the comparison of cholera mortality among large populations who had water supplies of varying degrees of sewage contamination. Circumstances provided an unusual opportunity for such an investigation, because in South London there were several areas served by two competing water companies[1 p.68–9, 74–5; 13 p.4–5]. In these districts the companies competed for customers house by house, so that the patrons of the two companies seemed indistinguishable except for their source of water. In 1854 one of these, the Southwark and Vauxhall Company, took its water from the Thames in central London at Battersea, while its competitor, the Lambeth Waterworks Company, had recently moved its inlet upstream to Thames Ditton, above Teddington Lock and hence beyond reach of most of London's sewage. In 1849 both companies had drawn their water from the Thames in central London. In neither 1849 nor 1854 did either company filter or treat its water. The circumstances seemed to be an ideal natural experiment for Snow's purposes. By comparing the mortality from cholera among the patrons of the two companies in 1854 and by comparing the experience of each set of patrons in 1849 with that of 1854, Snow obtained impressive results[1 p.68–92]. Analysing the results he obtained from the first four weeks of the outbreak in 1854 Snow concluded that cholera mortality was fourteen times higher among those served the more impure water[1 p.80]. During the epidemic the gap in mortality narrowed, but, as he figured it, the patrons of the Southwark and Vauxhall Company remained between eight and nine times as likely to die of cholera during the first seven weeks of the outbreak and five times as likely during the next seven weeks[1 p.86–8].

Unfortunately the study of the water supply in South London was not nearly as ideal as this brief description suggests. First of all, as Snow acknowledged, in the districts with the mixed supply it was difficult to learn the source of water for the houses in which cholera deaths had taken place. Tenants often did not know the name of the company that supplied water to their building. Attempting to surmount this difficulty Snow relied on a chemical test. He took apparently one sample of Lambeth Company water at Tharnes Ditton and one sample of Southwark and Vauxhall Company water and tested each for their common salt content using silver nitrate. He found a significant difference. The silver nitrate precipitated only 2.28 grains of silver chloride from a gallon of the Lambeth Company water but 91 grains from an equal quantity of the Southwark and Vauxhall Company water. This difference was so great that Snow felt he could distinguish the two water supplies at a glance by adding a little silver nitrate to the sample in question [1 p. 78–9]. Only later did he realise that the saline content of the river water varied widely over time, in the recent past by a factor of 20 for the Southwark and Vauxhall Company, which took its water from an area under tidal influence [9 p.211].

But much more important to contemporaries was the fact that Snow did not know the number of people at risk of cholera in his test case. In fact, when he composed the second edition of his book, he did not even know the number of households supplied by each company in the districts with the mixed water supply. It is not immediately obvious in reading this work that his estimates of relative risk of dying of cholera, such as those just quoted for the first four weeks of the epidemic, are based on houses not persons and are computed for all households supplied by the two companies and not on those households in the areas of mixed supply. In reviewing this work Snow's contemporary, E.A. Parkes, at first missed what Snow had done, but on rereading he discovered that Snow had ignored entirely the natural experiment offered by the mixed districts. A comparison of the experience of all patrons of the two companies was not convincing to Parkes, because the whole of the areas served by the two companies differed substantially in ways thought to be relevant to cholera's prevalence, in elevation, in family income, and in the quality of housing [12; 6 p.651–2].

The reserve with which Snow's contemporaries greeted his results stemmed in part from the technical defects of his evidence and argument, but there was more to this skepticism. Witness the fact that his conclusions remained unconvincing even after Snow remedied some of these methodological problems, when additional information came into his hands. As Margaret Pelling has ably pointed out, Snow's colleagues did not so much oppose his theory as they objected to his dismissing other explanations for cholera's occurrence [4 p.205–6, 222–35]. It was the exclusiveness of his views that gave them pause. As we have seen, Snow began with his theory nearly formed and worked as an epidemiologist to collect evidence in its favor. He was untroubled by negative evidence, and he was overtly unsympathetic to the multifactoral theorizing about epidemic disease that was the hallmark of mainstream medical thinking at the time. His approach seemed to ignore what the profession had already learned in its experience with the disease. A few years later E.A. Parkes recalled his initial skepticism of Snow.

"There seemed at once an *a priori* argument adverse to this view, as, at that time, all evidence was against the idea of cholera evacuations being capable of causing the disease. They had been tasted and drunk (in 1832) by men, and been given to animals, without effect. Persons inoculated themselves in dissections constantly, and bathed their hands in the fluids of the intestines; in India, the pariahs who removed excreta, and everywhere the washerwomen who washed the clothes of the sick, did not especially suffer. And to these arguments must be added the undoubted fact, that there were serious deficiencies of evidence in Dr. Snow's early cases. Add to this the unfortunate circumstance, that Dr. Snow, with all the enthusiasm of a discoverer, adopted the view that cholera entered only by means of water, and not at all by air, an hypothesis which is quite irreconcilable with the history of cholera ..." [14; 5 p.527].

It is quite possible that some of them may have associated such determined unifactoral explanations with the glib explanations of quacks or the simplistic understanding of lay people.

The second edition of Snow's book was widely reviewed in the medical press, and it stimulated discussion and further investigation. But Snow's colleagues were more impressed with his evidence than with his conclusions, and true to form, they tried to accommodate this evidence into the multifactoral explanations of the time. The Committee of Scientific Inquiry of the General Board of Health, which investigated the 1853–54 outbreak, may serve as an example. The Committee was most impressed with the results of the cholera study in South London, and it was willing to concede that sewage-contaminated water was a contributing factor to the cholera tragedy. But it could not accept Snow's pathological theory of cholera or his contention of specific contamination or sufficient causation [4 p.222–35]. The Committee concluded that the exciting cause of cholera brews poison from air or water containing ample organic impurities [15]. John Simon, one of the most influential members of the Committee, regarded Snow's demonstration as very significant but concluded that Snow had established that both fecalized air and water were to blame for cholera's incidence [13 p. 9].

No one in the profession took Snow's work more seriously than William Farr [16; 17 p. 114–22]. Farr, the Statistical Superintendent of the General Register Office and a member of the Committee of Scientific Inquiries in 1854, devoted much attention to cholera and published important studies of three of England's cholera epidemics: monographs on the epidemics of 1848–49 and of 1866 and sections in his weekly and annual reports for the 1853–54 outbreak [2; 18 XXXVII; 19 p. 74–99]. If Snow was exclusive in his analysis, Farr was inclusive in the extreme. His much-acclaimed study of the 1848–49 epidemic consisted of 300 pages of tables, charts, and maps prepared in the General Register Office under his supervision and a 100 page introduction in which he analysed the outbreak. Using the mortality records at his disposal Farr traced the cholera in England over time and space, compared this epidemic to its predecessor in 1832 and to the plague epidemics of earlier centuries, and analysed the possible influence of a host of demographic, social and environmental factors: age, sex, temperature, rainfall, wind, day of the week, domestic crowding, or property value. What impressed Farr most was the fact the cholera mortality was geographically concentrated. He found that in 1849 80% of the registered cholera deaths occurred in only 137 of the nation's 623 registration districts among 40% of the population living on one-seventh of its territory[2 *I–III*]. Coastal districts had on average three times the cholera mortality of inland districts. Farr further analysed local influences in nine cholera fields, areas of intense cholera mortality, each centered on a large port town. One of these, the London cholera field, was subjected to the closest analysis.

It was in the London cholera field that Farr made his most prized discovery, that cholera mortality is inversely related to elevation of the soil. He found that, if he arranged the districts of London into terraces by mean elevations above the high water mark of the Thames, cholera mortality varied according to a simple formula $C = C' (e' + a)/(e + a)$ where C and C' are cholera mortality rates per 10,000 in two districts having mean elevations in feet of e and e', and a is a constant, (12.8)[2 *lxiii;* 20]. He demonstrated his elevation law by calculating for elevations 0, 10, 30, 50, 70, 90, 100, 350 a theoretical series of mortalities 174, 99, 53, 34, 27, 22, 20, 6 that agreed remarkably well with the observed series 177, 102, 65, 34, 27, 22, 17, 7.

This was exactly the sort of result Farr was looking for, demonstrating as it did that human mortality was regular, predictable, and capable of description in mathematical terms[21]. It also was a result in keeping with the understanding of epidemic disease held by Farr and by many in the profession. Farr's statements of his disease theory are most explicit, when he was presenting or defending new versions of the official nosology he had prepared to bring order and usefulness to the national system of death registration and, when he discussed the uses to which the registration material could be put in the campaign to prevent disease[17 *p.53–60*]. Of greatest interest to us here is the first Class of Farr's nosology, the Epidemic, Endemic, and Contagious Diseases. As I have explained in detail elsewhere, Farr, like Snow, used analogies to the inoculable diseases – smallpox, cowpox, syphilis – to insist that diseases of this class were specific, disease entities produced by specific material causes that were reproduced in the blood of the victim[16 *p.81–7*; 17 *p.97–108*]. But unlike Snow, in the 1840s, Farr was certain that these material causes, while organic, were non-living, and he drew heavily on the writings of the German organic chemist Justis Liebig to explain the processes of fermentation or putrefaction[4 *p.113–45*]. To emphasise the similarity between these chemical processes and disease processes, Farr called the epidemic, endemic and contagious diseases zymotic, and held that a specific, non-living zymotic material caused each one. Cholera, for example, was caused by the as yet unidentified zymotic material "cholerine".

While Farr recognised that a few zymotic materials were inoculable, he held that most entered the body through the lungs. A long medical tradition of miasmatic explanations allowed him to explain why the epidemic, endemic and contagious diseases were most prevalent in urban slums, in prisons, or in squalid tropical port towns. In such places the air was ladened with organic material from respiration, perspiration, decomposition, or putrefaction. Zymotic material was abundant in the air in such circumstances. Furthermore, under extreme conditions zymotic material might be produced from ordinary organic material by chemical means without the presence of a prior case of the disease[22,23]. Farr's purpose throughout his mortality studies was to demonstrate that the prevalence of the epidemic, endemic and contagious diseases was dependent on local environmental conditions and to provide compelling evidence that environmental, particularly sanitary reform, was essential.

Farr's discussion of the zymotic theory demonstrates not only the eclectic and inclusive nature of his thought, a feature of his work that contemporaries found appealing, but it also suggests why he was so delighted with his elevation law for cholera[2 *lxxx–lxxxlii*]. The low, moist soil on margins of the Thames, the filth and debris on the banks of a tidal river in a large industrial city provided ample organic material for putrefaction and decay. The concentration of miasmata, the airborne organic particles, resulting from these processes was greatest at lower elevation, and it could be expected to decrease at a regular rate as one ascended the sides of the river basin. The elevation law he discovered in analysing the epidemic of 1848–49 was a result that was easy for Farr's contemporaries to accept, so consistent did it seem to be with the majority opinion on the nature and cause of the epidemic diseases. They soon affirmed his discovery. William Duncan, Medical Officer of Health for Liverpool, wrote that when he grouped the districts of his city by elevation as Farr had done, that cholera mortality in the last epidemic obeyed Farr's elevation law for Liverpool as well[24].

In his report on the 1848–49 cholera Farr labeled Snow's the most important of the various cholera theories he reviewed, and he quoted from Snow's recent pamphlet and accurately summarised Snow's explanations [2 lxxvi–lxxviii]. Farr acknowledged that river water was highly polluted, but he was unconvinced by Snow's conclusions [2 liii, lxix–lxx, lviii–lxii]. He recognised that river towns inherited the sewage and other organic debris of the towns upstream and invariably had higher cholera mortalities than the towns upstream. When he analysed cholera mortality in London's water fields, the areas of the Metropolis served by water of different origins, he found that the water fields with the highest cholera mortalities were not only those at the lowest elevation but also those that drew their water from the Thames furthermost downstream. But in 1852 Farr viewed sewage-contaminated water as only another source of miasmata. Sewage contaminated river water, he emphasised, contained abundant organic material undergoing putrefaction and decomposition. Particularly telling is his use of the Royal Observatory's estimates of the quantity of vapor evaporated from the surface of the Thames in London and his emphasis on air and water temperatures during the weeks of the cholera epidemic in 1849 to demonstrate that "the wide simmering waters were breathing incessantly into the vast sleeping city tainted vapors, which the temperature of the air at night would not sustain" [2 lix].

While he could not accept Snow's conclusions in 1852, Farr had become intensely interested in his theory, and when cholera broke out in England again in 1853, he wasted no time investigating. On 13 October at his suggestion the General Register Office requested from each water company information on the source(s) of the water it supplied, on the methods of filtration or purification used, and any change in source since the last epicemic [19 p.91]. The responses alerted Farr to the change in the Lambeth Waterworks Company's source and hence to the importance of the mortality patterns in South London. He began publishing in his weekly reports data on cholera mortality by water field as he had done retrospectively for the previous outbreak [19 p.91–7]. His weekly report for 26 November 1853 carried the table that probably alerted Snow to importance of the arrangements in the water supply in South London [1 p.69; 19 p.75–6]. Farr fully co-operated with Snow's efforts to test his theory, giving Snow unpublished material from the death registers, and, when Snow's preliminary results appeared promising, ordered local registrars in South London to inquire into the source of water, when a cholera death was registered [1 p.76–7; 19 p.94]. It was Farr who suggested that the Committee for Scientific Inquiries of the General Board of Health pay particular attention to cholera in South London in its report on the 1853–54 epidemic [25].

Farr's own investigations of this outbreak appeared in his weekly reports and in a section of his letter in the Registrar-General's Seventeenth Annual Report [19]. He repeated the sort of analysis of the outbreak that he had published for the previous epidemic, and again he found cholera mortality to depend on local conditions. What was new was his conviction that sewage-contaminated water had exercised a decisive influence. He ended his report by saying that the collective efforts of Snow, the G.R.O., and the Board of Health had proven that "cholera matter or *cholerine*, where it is most fatal, is largely diffused through *water*, as well as through other channels" [19 p.99]. This statement might be taken to mean that Farr now agreed with Snow. But the fact is that Farr simply added polluted water to contaminated air as a vehicle for morbid matter. He continued to believe that zymotic material entered the body primarily through the lungs. He pointed out that evaporation from cisterns, taps, drains, and local reservoirs took place constantly. Sewage-contaminated water "comes into contact with the body in many ways and it gives off incessantly at its temperature, ranging from the freezing point to summer heat, vapors and effluvia into the atmosphere that is breathed in every room" [19 p.95]. Thus the differences between Snow and most of the profession, including Farr, continued to be over Snow's pathological theory and his exclusiveness. Strictly speaking, what had been established in South London was only that patrons of a company supplying highly contaminated water were at greater risk of dying of cholera. No one had proved how that contaminated water acted.

By the time of the 1866 epidemic of cholera John Snow was dead, and William Farr had become one of the waterborne theory's few champions. Farr's sympathy for Snow's hypothesis can be properly gauged, when his views are contrasted with the skepticism of most Medical Officers of Health in the Metropolis when the 1866 epidemic began [26]. Farr had been deeply impressed by the analyses of cholera mortality in South London in 1853–54, but the next epidemic provided a case study that was decisive. Farr's analysis of the cholera returns pointed very early in the epidemic to excessive cholera mortality in the East London Waterworks Company's water field. Farr not only made this information public, but he lobbied to have the causes of the outbreak and the quality of the East London Company's water investigated. The series of official investigations that followed showed that the company had been illegally pumping water from its reservoir at Old Ford, which it claimed was no longer in use, and which had been contaminated from the new sewage system of West Ham [18 xii, xvii-xx; 26].

Farr's monograph on the 1866 epidemic, like that on the 1848–49 eidemic, provided a comprehensive analysis of the epidemic, treating it as a complex social and medical phenomenon. But this time it passed quickly over the influence of age, sex, weather, income, occupation, and housing density in order to concentrate his analysis on water, particularly on the outbreak in East London. Farr demonstrated not only that cholera mortality was extremely high only in the East London water field, particularly among those people receiving water from the Old Ford reservoir, but he also showed that the law of the epidemic, the rates of the outbreak's rise and fall, for the East London water field in this epidemic was different from the law that prevailed in either previous London cholera outbreaks or in other water fields in 1866[18 *xx–xxiv; xxx–xxxiii*]. One of the anomalies in the East London outbreak was its sudden decline. Farr attributed this peculiarity to changes the company made in its supply once the high mortality among its patrons was exposed.

Farr now concluded that cholera was spread by cholera flux, the intestinal discharges of cholera patients, and that it could be transmitted in four ways: by personal contact, by air, by sewer vapor, and by water. This list was familiar. What was new was how he ranked these means. The first three, Farr thought, may have exercised a slight influence over all of London, not exceeding five deaths per 10,000. But in a city like London the first three were insignificant in comparison to the fourth. The character of the outbreak was determined by waterborne contagion[18 *xv–xvII, lxxx*]. Except for a vestige of miasmatic notions, represented by air and sewer vapor as media that were retained to explain a few exceptional cholera cases that seemed intelligible in no other way, Farr had accepted Snow's explanation of cholera's transmission[18 *xiv.xvi*].

The intellectual climate in the mid-1860s was more congenial to Snow's pathological theory of cholera than the climate of the 1840s had been, and that the change in theoretical context was instrumental in Farr's conversion to Snow's position. In Farr's discussion of disease in his monograph on the 1866 cholera epidemic we find that biological evidence replaced much of the former reliance on chemical authorities. Liebig, Dumas, and Thomas Graham are displaced by Pasteur on fermentation and by pathologists such as Filippo Paccini and Lionel S. Beal, who reported seeing corpuscles in the intestinal discharges of cholera patients and in the cases of the cattle plague, respectively. In this Farr was reflecting a heightened interest of the profession in elementary units in biology and a suspicion that they were particulate[27]. I have shown elsewhere that Farr began to hold that zymotic material consisted of elementary particles he called zymads. Thus the agent of cholera, cholrine, consisted of cholrads. Over the next few years he responded to contemporary biological research by endowing these zymads with additional properties of life, until they became nearly indistinguishable from living organisms[17 *p. 105–7*]. What was true for Farr may have been true of others in the profession as well. The *Lancet* suggested that Farr had led the way by making the waterborne theory "irresistible"[28].

The contrast of Farr's and Snow's approaches to the study of cholera highlights the importance of disease theory in epidemiological investigations. The studies of both men were predicated on their understanding of the nature and causation of disease, and their methodology reflected those theoretical differences. Snow was exclusive or reductionist in theory, and he focused his empirical investigation on finding collaborating evidence and ignored negative evidence or anomalous cases. For him epidemiology was a means of verification; for Farr it was also a means of discovery. Farr was eclectic and inclusive in his theory, and he approached his cholera studies by trying to weigh a large list of social, environmental, and biological factors in accounting for cholera's behaviour. These qualities of mind made Farr responsive to new ideas and adaptable, as we can see in both the changing emphasis and the conclusions in his investigations of three cholera epidemics. A recent biographer of Snow briefly compares Snow and Farr and praises Snow for his openmindedness[9 *p. 179–80*]. By implication Farr was closed-minded. On the cholera question I would conclude just the opposite. Judged by the standards of his time Snow was the dogmatic contagionist and premature reductionist. Farr was the more cautious in weighing all evidence.

One final comment on Farr must be made. He may have been eclectic and flexible in his understanding of the mechanism of disease, and he might have been open to new hypotheses, but he was firmly committed to his conviction that disease, in fact all vital phenomena, were law-abiding. He was convinced that a statistical law with as much predictive value as his elevation law must reveal something fundamental about disease. He never accepted Snow's claim that the elevation law Farr discovered in the data for the 1848–49 epidemic was merely a "remarkable coincidence"[1 *p. 97–8*]. Farr's increased sympathy for Snow's theory did not mean that he abandoned the elevation law. In fact it appears, suitably modified, in his reports on both subsequent epidemics[19 *p. 88–90;* 18 *xiv–xv.xx*]. The tenaciousness and the ingenuity with which Farr worked to salvage an elevation law in each of his later cholera reports speaks volumes for his faith in the power of statistical inquiry. This faith made numerically strong relationships that confirmed expectations irresistible to him. Today it is his results and not Snow's that are considered merely ingenious, and Snow's publications are read perhaps more sympathetically than they deserve, because the modern medical reader can "fill in the gaps in his reasoning with the comforting knowledge that Snow was, after all, right"[6 *p. 652;* 4 *p. 209–10*].

Zusammenfassung

Das sich wandelnde Verständnis der Cholera-Studien von John Snow und William Farr

Dieser Artikel befasst sich mit den epidemiologischen Cholera-Studien von zwei bedeutenden Britischen Forschern der Mitte des 19. Jahrhunderts, John Snow und William Farr. Es wird hinterfragt warum deren Zeitgenossen die Studienresultate entgegengesetzt beurteilten als wir dies heute tun. In den 1840er- und 50er-Jahren wurde die Arbeit von Farr als massgeblich betrachtet, während die Arbeit von Snow zwar als originell, aber fehlerhaft galt. Auch wenn die Schlussfolgerungen von Snow den Erwartungen seiner Zeitgenossen widersprachen, die grössten Bedenken im Hinblick auf seine Cholera-Studien galten der gewagten Verwendung von Analogien, seinem extremen Reduktionismus und seiner Neigung zu ignorieren, was ein Beweis des Gegenteils schien. Farrs selektiver Gebrauch bestehender Theorien, sein vertrauensvolles mehrfaches Kausalprinzip und seine Entdeckung eines mathematischen Zusammenhangs zur Beschreibung des Cholera-Ausbruchs in London 1849, hatten grössere Überzeugungskraft auf seine Zeitgenossen. Es war zunächst notwendig, dass sich das Verständnis über die Verursachung von Krankheiten grundsätzlich änderte, bevor Snows Arbeit überhaupt akzeptiert werden konnte. William Farrs spätere Studien trugen dazu bei.

Résumé

Evolution historique de l'appréciation des études sur le choléra de John Snow et de William Farr

Cet article décrit des études épidémiologiques sur le choléra menées par deux importants chercheurs britanniques du milieu du 19ème siècle,John Snow et William Farr. Il s'interroge sur les raisons pour lesquelles leurs contemporains apprécièrent leurs résultats de façon inverse à notre appréciation contemporaine. Au curs des années 1840 et 1850, les travaux de Farr furent considérés comme définitifs, alors que ceux de Snow étaient jugés ingénieux mais biaisés. Bien que les conclusions de Snow allaient à l'opposé des attentes de ses contemporains, les principales réserves par rapport à ses études sur le choléra provenaient de ses analogies périlleuses, de son réductionnisme implacable et de sa tendance à ignorer les faits qui contredisaient sa théorie. Ses contemporains étaient beaucoup plus convaincus par l'utilisation éclectique, par Farr, des théories en vogue, son acceptation de l'existence de causes multiples et sa découverte d'une loi mathématique qui décrivait l'épidémie de Londres en 1849. Il fallait que se produise un changement majeur dans le raisonnement causal en médecine pour que les travaux de Snow puissent être largement acceptés. Les dernières études de William Farr ont contribué à la reconnaissance de Snow.

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Appendix

A Note on Sources

The second edition of Snow's "On the mode of communication of cholera" of 1855 is cited from the reprint in Snow on cholera: Being a reprint of two papers by John Snow, M.D. together with a biographical memoir by B.W. Richardson, M.D. and an introduction by Wade Hampton Frost, M.D. London and New York, 1936 and is cited as Snow, "On the Mode of Communication (1855)". Farr's annual letter in the "Annual Report of the Registrar-General of Births, Deaths, and Marriages in England and Wales" is cited as Farr, "Letter, nth A.R.R.G". This important source was reprinted in the British Parliamentary Papers, here abbreviated as "B.P.P." In some years the pagination differed between the separately published version and the version in the Parliamentary Papers. The version in "B.P.P." is cited, but when the pagination differs, the page number in the separately published version is given in parenthesis.