Section of the History of Medicine

President Lord Cohen of Birkenhead MD

Meeting June 12 1968

President's Address

John Snow– 'The Autumn Loiterer'?[1]

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The bare facts of John Snow's life are well known. Born at York in 1813, he qualified in medicine in 1838 as MRCSEng and LSA, and became an MD of London in 1844. He was a physician who pioneered ether and chloroform anæsthesia, and popularized the use of chloroform by administering it to Queen Victoria for the birth of Prince Leopold in 1853, and Princess Beatrice in 1857, though his name did not appear on the medical bulletins. His other major contribution was the hypothesis and epidemiological proof that cholera is water-borne. Thus as physician, anæsthetist and epidemiologist he has a firm place in the history of medicine.

The main biographical source from which subsequent writers on Snow have culled their data is Sir Benjamin Ward Richardson's memoir of the author, which forms the introduction to Snow's posthumous work 'On Chloroform and Other Anæsthetics', which was published in 1858, a year after Snow's death in his 46th year.

My purpose in this Address, however, is to review contemporary literature to seek an answer to many questions which are not touched on by Richardson, and to attempt, in the light of these, a more objective assessment of Snow's place in the history of medicine.

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I need not enter into the controversy which still rages over the early history of the introduction of ether anæsthesia in America. What seems indisputable is that Morton's success with ether was announced to the world in a paper by Henry J Bigelow published in the *Boston Medical and Surgical Journal* on November 18, 1846. Dr Frederick Fox Cartwright, in his John Snow Memorial Lecture of 1958, states that the news of the Boston success arrived in England in the middle of December 1846 by means of a letter from Bigelow to a Mr James Boott, who was an American dentist living in Gower Street. Boott is said to have consulted with a fellow dentist named Robinson, with a chemist named Squire, and with an instrument maker, Hooper of Pall Mall. On December 19, 1846, with an apparatus devised by Hooper, Boott extracted a molar tooth successfully in the presence of Robert Liston, then senior surgeon to University College Hospital of whose council Boott was a member. Two days later, on December 21, 1846, Robert Liston performed a mid-thigh amputation of the lower limb and an avulsion of the toe-nail successfully under ether anæsthesia, and ten days later there was a public demonstration of anæsthesia at King's College Hospital, when William Fergusson performed seven operations under ether administered by Squire and Robinson.

But it is by no means certain that this was the source from which Snow learned of ether anæsthesia. Nor indeed is there any valid evidence for what Richardson alleges was Snow's own account. This briefly was as follows:

'On a day early in January, 1847, he was leaving one of the London Hospitals when he met a druggist, a friend of his, who was hurrying along with an ether apparatus under his arm. "Good morning," said Snow.

"'Good morning to you, doctor,' replied his friend, "but don't detain me now. I am giving ether here, there and everywhere, and am getting quite into an ether practice. Good morning, doctor."

'An ether practice; rather peculiar, thought Snow'

and turned his mind to the possibility of starting one himself.

We do know, however, that Snow read and wrote for the *London Medical Gazette*. In 1846, he contributed to this journal papers on 'Strangulation of the Ileum in an Aperture of the Mysentery', and 'Alkalescent Urine, and Phosphatic Urinary Calculi'. On December 18, 1846, there appeared in the *Gazette* a note headed 'Animal Magnetism Superseded – Discovery of a New Hypnopoietic'. It read thus:

'We learn on the authority of a highly respectable physician of Boston, U.S., that a Dr Morton, a surgeon-dentist of that city, has discovered a process whereby in a few minutes the most profound sleep may be induced, during which teeth may be extracted, and severe operations performed, without the patient being sensible of pain, or having any knowledge of the proceedings of the operator. The process simply consists in causing the patient to inhale *the vapour of ether* for a short period, and the effect is to produce complete insensibility, – or, as the writer says, intoxication. We quote the following case on the same respectable authority: "I took my daughter last week to Martin's rooms to have a tooth extracted. She inhaled the (vapour of) ether about one minute, and feel (*sic*) asleep instantly in the chair. A molar tooth was then extracted without the slightest movement of a muscle or fibre. In another minute she awoke, smiled, and said the tooth was not out, had felt no pain, nor the slightest knowledge of extraction. It was an entire illusion."' (*London Medical Gazette* 1846).

The *Gazette* proceeded to comment favourably on the frank disclosure by Dr Morton of his methods in contrast to the mystery proceedings of 'the tribe of hypnotic quacks who have lately perambulated the country', but urged that caution should be exercised in as much as ether is a poison, and it was difficult to understand how the patient was unaware that the tooth was out and had experienced no post-operative pain.

John Snow could well have learned of ether anæsthesia from this note, for the news from America was to fall on a prepared mind. When at the age of 24 Snow came to London from York, he spent a year at the Windmill Street School, founded by William Hunter, where he was well grounded in chemistry. On December 16, 1837, while still a student, he reported to the Westminster Medical Society, which was later to merge with the Medical Society of London, the results of some experiments he had performed to ascertain the effects on animal life of combustion of candles containing arsenious acid. By 1839, the year after he qualified, he had performed and reported experiments to show (1) that carbon dioxide gas was toxic when inhaled; (2) that a decrease of oxygen in the atmosphere was dangerous to life; and (3) that if both were combined then the effects proved more rapidly fatal.

He had undertaken also quantitative experiments to show that 5–6% was the concentration above which carbon dioxide might give rise to alarming symptoms. Moreover, he had succeeded in controlling the concentrations of carbon dioxide in closed spaces, first with lime water, and later with caustic potash solution. There can be no doubt that it was these studies on respiration and asphyxia which led him to recognize the significance of ether anæsthesia.

He carried out many experiments with ether, on animals and on himself, and within a month had devised an 'Apparatus for Inhaling the Vapour of Ether', which on January 23, 1847, he demonstrated to the Westminster Medical Society. It consisted of a round tin box, two inches (5 cm) deep, and four or five inches (10–12·5 cm) in diameter, with a tube of flexible white metal, half-an-inch (1·25 cm) in diameter and about a foot and a half (45 cm) in length, coiled round and soldered to it. There was an opening in the top of the vessel at its centre for pouring in the ether, and to this was afterwards attached the flexible tube belonging to the mouthpiece. In the interior was a spiral plate of tin, soldered to the top and reaching almost to touch the bottom. When used the inhaler was to be put in a hand basin of water, mixed to a particular temperature corresponding to the proportion of vapour that the operator might desire to give; the caps being removed and the mouth tube attached when the patient began to inhale, the air would gain the desired temperature in passing through the metal pipe; it would then rise to the surface of the ether where it would have to pass round three or four times before entering the tube going to the mouthpiece, thus ensuring its full saturation and preserving it at the desired temperature.

At a meeting of the Westminster Medical Society a month later, the inhalation of ether was again discussed. Hale Thomson regarded it as 'a most valuable boon to humanity' from which he had seen no ill results, whilst Hancock, another surgeon, reported that he had seen it produce convulsions in one instance and severe irritation of the throat in another, which remained until the patient, who was previously in a very low condi-

tion, died. There was much praise for Snow's apparatus, and Snow himself reported that he had completed further experiments by which he had ascertained that the vapour of ether given out from the lungs was unchanged, and that the amount of carbon dioxide produced during the inhalation of ether was less than at other times, circumstances which he considered to confirm his explanation of how ether worked. A little later we find Dr Gull, subsequently Sir William Gull of Guy's Hospital, one of the greatest physicians of his time, reading a paper to the South London Medical Society on the effects of ether on different classes of animals, in which he surveyed what was known of the physiology of ether up to that time, but ended his communication with three questions which indicated the major problems confronting his surgical colleagues. Firstly, is it useful to abolish pain during a surgical operation? Secondly, can this be done safely by ether? Thirdly, does the presence of ether in the blood modify the healing process? Most surgeons would by then have answered all three questions affirmatively, but there were still many who were doubtful. Amongst these was the distinguished Guy's surgeon, Mr Bransby Cooper, who remarked that 'pain was a premonitory condition no doubt fitting parts, the subject of lesions, to reparatory action, and therefore he should feel averse to the prevention of it. In parts operated upon under the influence of ether there was no muscular contraction, no retraction of the larger vessels, and the small ones continued bleeding', and so forth in denunciation. By February 1847 James Young Simpson, a most distinguished professor of obstetrics at Edinburgh, was using ether in his practice. It is said that when the news of ether was first received he cried: 'It is a glorious thought. I can think of nought else.' But there was much evidence of its misuse by the inexperienced and in many places, particularly in Europe, orders were issued prohibiting its use.

In 1847, less than a year after the introduction of ether, Snow had published his first monograph, 'On the Inhalation of the Vapour of Ether in Surgical Operations'. This described the various stages of etherization, and reported the 'Result of nearly Eighty Operations in which Ether has been employed in St. George's and University College Hospitals'. In it he differentiated the five stages or degrees of etherization and what in his view were the indications for, and contraindications to, its use. On November 10, 1847, within a few weeks of its publication, Simpson announced to the Medico-Chirurgical Society of Edinburgh that he had discovered a new anæsthetic agent – chloroform. This he had tried on the advice of Waldie of Liverpool. Ten days later, on Saturday, November 20, Snow was reporting to the Westminster Medical Society that chloroform 'was preferable to ether in some respects, although it was impossible that anything could be more efficient than ether, as it was capable of totally preventing the pain in every operation in which it might be properly applied'. He told of one operation on the preceding Thursday, November 18, for amputation of the breast at St George's Hospital in which he had used chloroform successfully, and pointed out that whereas ten drachms of ether would probably have been used in the operation, only one drachm of chloroform was necessary. He proceeded to describe experiments on himself which led to unconsciousness and he elaborated also on the physical properties of chloroform.

During the ensuing years, Snow gave anæsthetics for Benjamin Brodie, for Liston and the Hawkins brothers, for James Paget, Bowman, Spencer Wells, Partridge and Quain amongst many others. But from 1850 until 1858 most of his experimental work and hospital practice was carried out with William Fergusson at King's College Hospital. Fergusson had been a pupil of, and prosector for, Robert Knox in Edinburgh, and in 1839 became surgeon to the Edinburgh Royal Infirmary. But he was attracted to London, largely it is said because of James Syme's success, and there, when virtually any disease of the limbs was regarded as a sufficient reason for amputation, he founded a school of conservative surgery in which he sought to preserve parts of the body which were needlessly sacrificed by earlier operators. Parochial loyalty compels me to record that Fergusson was not the first to advocate the surgery of 'conservation'. The credit belongs to Henry Park of Liverpool Royal Infirmary who, seventy years earlier, was treating by excision chronic infections of the knee- and elbow-joints, which had hitherto been treated by amputation. His work entitled 'Cases of the Excision of Carious Joints' (Park & Moreau 1806) ran into many editions, and made his reputation as a surgeon, though his methods found favour sooner in France than in this country. He urged that by total extirpation of infected joints limbs might 'be preserved, with such a share of the motions which Nature had originally allotted to them, as to be considerably more useful than any invention which art has hitherto been able to substitute in their stead'.

Thus Park anticipated Fergusson's more succinct comment that it is 'a grand thing when by prescience even the tip of a thumb can be saved'. Little wonder that Fergusson showed enthusiasm

for anæsthesia or that he chose the skilful Snow as his colleague. For he realized that anæsthesia increased the scope of surgery by adding to it not only loss of pain, but also the dimension of time, for whereas before anæsthesia the range of surgery was limited by the endurance of the patient, with anæsthesia speed could be sacrificed o care and accuracy.

In the Royal College of Physicians are to be found the medical case books of John Snow which contain a record in diary form of all his cases from July 17, 1848, to June 5, 1858 (Snow died on June 17, 1858). These three quarto volumes record all but the first 47 cases to which he administered chloroform anæsthesia together with, in a few instances, the use of ether, 'Dutch liquid' and 'chlaurett chloride of ethyle', a French preparation. These volumes would well repay a more careful study than they have yet received. In them is recorded the administration of chloroform to Queen Victoria on April 7, 1853, at the birth of Prince Leopold and on April 14, 1857, at the birth of Princess Beatrice; and to many other distinguished patients. There is one entry which is of interest because of the theological controversy aroused by Simpson's use of anæsthetics in childbirth. The curse of Eve, the Churchmen asserted, is clear: 'In sorrow thou shalt bring forth children'. But Simpson was unmoved, and in 1847 he wrote a powerful pamphlet as 'An Answer to the Religious Objections Advanced against the Employment of Anæsthetic Agents in Midwifery and Surgery'. That many theologians in high clerical office did not share some of the alleged religious objections to anæsthesia is shown by an entry in Snow's case book on October 20, 1853: 'The confinement of a daughter of the Archbishop of Canterbury at Lambeth Palace.'

These records form the basis of Snow's major work published shortly after his death – 'On Chloroform and Other Anæsthetics: Their Action and Administration' – and a fitting memorial it is to his pioneer work. It describes the physical and chemical properties of chloroform, its physiological effects, the circumstances which modify these, such as age, habits, and disease, the mode of administration, the causes of death which may follow chloroform, and the indications for the use of chloroform in medical conditions, such as neuralgia, asthma, whooping-cough, convulsions, &c. He analyses similarly ether, amylene (the anæsthetic of which he once had the greatest hopes) and other volatile agents. Perusing its pages, which reveal his industry and meticulous attention to detail, we are reminded of what Mr Joshua Parsons, a fellow student of Snow's in 1836 at the Hunterian School of Medicine in Windmill Street, wrote of Snow to Sir Benjamin Richardson:

'Not particularly quick of apprehension, or ready in invention, he had always kept in the foreground by his indomitable perseverance and determination in following up whatever line of investigation was open to him. The object of this steady pursuit with him was always *truth*: the naked truth for its own sake, was what he sought and loved. No consideration of honour or profit seemed to have power to bias his opinions on any subject' (Richardson 1858).

Nearly a century was to elapse before any work on anæsthesia was published, commensurate in scope, quality and originality with that of Snow, and before anæsthesia was to be acknowledged as a specialty and academic discipline worthy of parity of esteem with the other specialties.

Snow was interested also in local anæsthesia, and on April 10, 1854, he read to the Physiological Society of which he was a Vice-President, a paper on the production of local anæsthesia, in which he referred to Dr James Arnott's method of obtaining anæsthesia by the application of crushed ice. Snow tried wetting a piece of folded lint with chloroform and applying it to the skin, preventing evaporation by a covering of oiled silk. But there was no complete anæsthesia except when the skin was denuded of its cuticle by a blister. The denuded surface could then be made readily insensible even by the vapour of chloroform. He referred also to the method of cooling the skin by dropping ether on it and increasing the rate of evaporation by a current of air from bellows. This had been introduced by the French, and Snow had used it with some success for the incision of the callous edges of a leg ulcer. But Snow was never satisfied with the results of these trials and discontinued them in order to 'concentrate his energies on the discovery of what he felt sure must be discovered ultimately – an anæsthetic which might be inhaled with absolute safety, and which would destroy common sensation without destroying consciousness'. He thought that he had succeeded for a time in this venture with amylene, but soon deaths began to occur. These he faithfully recorded and as a result of these fatalities discontinued its use.

Cholera

Snow's work on cholera has perhaps attracted wider attention than that on anæsthesia because it led not only to the saving of tens of thousands of lives, but was powerfully to strengthen the infant sciences of epidemiology and medical statistics. It bears further witness to his powers of accurate observation, recording, and scientific inference.

The Indian or Asiatic cholera first appeared in Britain in houses on the quay in Sunderland in October 1831, although the disease was known here for some years before, and a lengthy description of it had appeared in the 1825 edition of Dr Mason Good's 'Study of Medicine'.

Snow's interest in cholera had been early aroused. In 1827, at the age of 14, he had left York, his birthplace, to be apprenticed to William Hardcastle in Newcastle upon Tyne. In 1831–2 cholera visited Newcastle in a most fatal form and Hardcastle sent Snow to Killingworth Colliery to attend sufferers there. The rest of Snow's pupillage coincided with the growing interest aroused in the conditions of living, especially of the poor, and their contribution to disease, which led to Edwin Chadwick's work on the health of the labouring classes (Poor Law Commissioners 1842), which initiated the 'sanitary era' of public hygiene, and inspired the efforts of social reformers who urged the eradication of the evils of poverty – crowding, squalor, dirt, lack of sanitation and ventilation and warmth, malnutrition and inadequate clothing. At that time the question of how cholera spread was being widely debated. The view generally held was that emanations from the sick diffused through the atmosphere (*miasmata*), were inhaled, absorbed by the blood, passed through the lungs, and then poisoned the alimentary tract, giving rise to diarrhœa and vomiting with ultimate collapse and death. Snow countered this view in a short essay 'On the Mode of Communication of Cholera' (1849), in which he stated that cholera commences *primarily* in the alimentary canal, and is due to swallowing of morbid material from the excretions of the victim, usually in drinking water, though it can be transmitted to a distance, e.g. on soiled linen. He elaborated this view at a meeting of the Royal Medical and Chirurgical Society on January 22, 1850, with Dr Thomas Addison of Guy's in the Chair. There Snow emphasized that, unlike influenza which spread as swiftly as a piece of bad news and was communicated by the breath, the cholera poison had to be swallowed to produce its effects, and thus the disease necessarily spread more slowly, and that it was found to spread most extensively where there were the greatest facilities for swallowing the excretions. He emphasized that the recent epidemic had proved most fatal in those districts of London supplied with water obtained from the Thames in the neighbourhood of the chief sewers which would, when cholera was prevalent, contain the evacuations of the patients. This 1848–9 epidemic, on which Snow's observations were based, had started in Bethnal Green and other outparishes in the autumn of 1848, but it was not until the summer and autumn of 1849 that the ravages appeared. In London, in July 1849, there were 1,952 deaths; in August, 4,251; in September, 6,644; then came a rapid decline. In October there were 464 deaths and in November, 27.

The critical proof of his theory was, however, to await the more sudden though less severe epidemic of 1854–5 in London, and is on record in his classic treatise on cholera published in 1855, and a later publication in 1856. Snow mapped out every death from cholera occurring in the districts deriving their water supplies from two companies, namely, those served by the Southwark and Vauxhall Company, and the three districts served wholly by the Lambeth Company. Although both companies had until 1849 drawn their water unfiltered from the Thames at London Bridge, the Lambeth Company in 1852 had removed its waterworks to Thames Ditton, where the supply of water was quite free from the sewage of London. Richardson (1858) records that:

'The result of his [Snow's] endeavours, in so far as scientific satisfaction is a realization, was truly realized, in the discovery of the statistical fact, that of 286 fatal attacks of cholera, in 1854, occurring in south districts of the metropolis, where one water company, the Southwark and Vauxhall, supplied water charged with the London fæcal impurities, and another company, the Lambeth, supplied a pure water, the proportion of fatal cases to each 10,000 houses supplied by these waters, was to the Southwark and Vauxhall Company's water 71, to the Lambeth 5.'

The Broad Street Pump episode on which, in the minds of many, Snow's claim to fame rests, is described by Richardson thus:

'In the latter part of August 1854, a terrific outbreak of cholera commenced in and about the neighbourhood of Broad-street, Golden-square. Within two hundred and fifty yards of the spot where Cambridge-street joins Broad-street, there were upwards of five hundred fatal attacks of cholera in ten days. To investigate this fearful epidemic was at once the self-imposed task of Dr Snow. On the evening of Thursday, the 7th of September, the vestrymen of St James's were sitting in solemn consultation on the causes of the visitation. They might well be solemn, for such a panic possibly never existed in London since the days of the great plague. People fled from their homes as from instant death, leaving behind them, in their haste, all the mere matter which before they valued most. While, then, the vestrymen were in solemn deliberation, they were called to consider a new suggestion. A stranger had asked, in modest speech, for a brief hearing. Dr Snow, the stranger in question, was admitted, and in a few words explained his view of the "head and front of the offending". He had fixed his attention on the Broad-street pump as the source and centre of the calamity. He advised the

removal of the pump-handle as the grand prescription. The vestry was incredulous, but had the good sense to carry out the advice. The pump-handle was removed, and the plague was stayed.'

But, as Bradford Hill (1955) has pointed out, the effect was *post hoc* and not *propter*. At least five days before the removal of the pump handle, the peak incidence had occurred and deaths had been falling rapidly (Table 1), a fact which Snow himself had recognized. Had the pump handle remained *in situ* it is almost certain that more deaths would have occurred, but the end of the epidemic was certainly not brought about by its removal. Snow himself had recorded in 1850 an earlier example of closure of a pump. It occurred when, as a result of a fatal series of cases in the neighbourhood of Bridge Street, Blackfriars, St Bride's pump, from which the inhabitants obtained their drinking water, was closed at the insistance of Mr Hutchinson, the surgeon (not the famous Jonathan Hutchinson, who was then 22 years old), it having been ascertained that the well was contaminated by a sewer running into the Fleet ditch.

Table 1

Sequence of deaths from cholera during the Broad Street Pump episode (Hill 1955)

Date	*No. of fatal attacks commencing on each day*
August 26	1
August 27	1
August 28	1
August 29	1
August 30	8
August 31	56
September 1	143
September 2	116
September 3	54
September 4	46
September 5	36
September 6	20
September 7	28
September 8 ●	12
September 9	11
September 10	5
September 11	5
September 12	1
September 13	3
September 14–30	18
Date unknown	45

● Pump handle removed

Isaac D'Israeli declared that 'the defects of great men are the consolation of dunces' but, in the interests of historical accuracy, it is right to refer to Snow's attitude towards the microbic and fungus theories of cholera. A few weeks before Snow's essay in 1849, Brittan and Swayne, at a meeting of the Bristol Medico-Chirurgical Society, demonstrated 'peculiar microscopic objects in the characteristic rice water discharges of persons affected with malignant cholera'. These objects, believed to be living organisms, were later detected by William Budd in the drinking water of cholera districts in Bristol. Snow recognized that if these cells existed they tended to confirm his view of the nature of cholera, though he was not sure that they were the real cause, and a little later he threw doubt on their existence and on the motives of the original discoverers of these organisms. In this he was in good company, for not only Sir James Paget, but also Drs Bailey and Gull, who spoke for the Sub-Committee of the College of Physicians on cholera fungi, concluded that 'the whole theory of the disease which has recently been propounded is erroneous as far as it is based on the existence of the bodies in question'. It has been said that one of the reasons why Snow withheld his support from this discovery was that it might detract from his claims to be the originator of the theory that cholera spread through imbibing water contaminated with infected excreta. Evidence that he held jealously to this claim is found in the *Lancet* of 1856. In an address on January 21, 1856, to the Manchester School of Medicine on the progress of medical science, Sir James Kay Shuttleworth, whose contributions to education and public health in England had earned him a Baronetcy, said:

'The discoveries of Dr Semelweiss [*sic*], as to the mode of the propagation of puerperal fever by the cadaverous poison, and Dr Budd's discovery of one mode of the dissemination of cholera, by a poison evolved in the early stage of the decomposition of the specific excretions, are facts which indicate, though they may not prove, the influence of specific virus on the constitution of the blood, and the action of that fluid in a state of disease in the solid parts.'

This quickly evoked a reply (February 16, 1856) from Snow claiming priority, which concludes by stressing that the correction he was making was not

'in the way of complaint; but as my researches respecting cholera were conducted with great labour, and very much to the detriment of my more immediate interests, I feel it a duty not to allow the credit of them to pass from me by a mere mistake.'

Brown (1961) revives claims to priority which from time to time have been put forward on behalf of Budd, Swayne and Brittan. But Budd himself acknowledged Snow's priority. He wrote in a footnote (p 19) of his 'Malignant Cholera: Its mode of Propagation, and its Prevention' (1849):

'Dr Snow, whose ingenious pamphlet on Cholera fell into my hands while these materials were preparing for publication, has been led, by the consideration of particular instances of some of the facts above alluded to, to the same conclusion as to the part which water

plays in the diffusion of the disease. Of being the first to develope [*sic*] and to publish this very important conclusion he must, therefore, have the whole merit. To no part of this merit do I lay the slightest claim. In Dr Snow's illustration of the entire subject of the propagation and prevention of cholera there is, besides, much that is so apt, and in such entire accordance with the truth, that the detection of the actual cause of the disease, and the determination of its nature, were all that was wanting to convert his views into a real discovery.'[1]

The fungi, which Budd and his colleagues claimed to have seen through the microscope when examining the stools of cholera patients, were almost certainly epithelial cells, starch granules and fat globules. Not until 1853 did Koch discover the cholera vibrio.

Brown (1961) refers to Snow as 'the autumn loiterer' from Browning's verse:

And men have grown old among their books
To die case-hardened in their ignorance,
While autumn loiterers . . . have chanced on truth.

But Snow did not chance on truth; he searched for it and found it. Hence the question mark in the title of this Address. Snow's method of epidemiological investigation justifies the well-nigh universal admiration which he has attracted for over a century. He sometimes, and he was by no means the first to do so, extrapolated prematurely. For example, he stated at the Epidemiological Society on June 2, 1851, that:

'He believed that several diseases, including abdominal typhus and the plague, were communicated in the same way as cholera, and that ague was often caused by drinking the ditch water of marshy places.'

As a physician, the width of his interests is shown by an interesting paper he published in the *Lancet* in 1857 on the 'Adulteration of Bread [by Alum] as a cause of Rickets'. His theory was both attractive and plausible. He stated that bones owed their hardness to phosphate of lime, which exists ready formed in many articles of food from which it is assimilated, whilst in rickets the phosphate of lime in the bones is known to be deficient. He says that it is likely that the sulphuric acid of the alum, which is added to bread, would decompose the phosphate of lime of wheat forming sulphate of lime which would not be available as nourishment for the bones. Although we now know his theory is groundless, he put it forward with a wealth of epidemiological and chemical data which reveal once again his chemical knowledge and his scientific approach.

Indeed, he was ever prepared to defend the scientific method in medicine against those medical men whose systems of practice were based on general impressions and mere report. He wrote in 1846, for example, a strong letter to the Editor of the *Lancet* condemning him for using the name 'Allopathy' to cover the mode of practice of the eclectics which the

'homœopathists have tried to impose on the profession; for there is nothing which would, in my opinion, tend so much to prolong the brief day of the fatal and extravagant system of homœopathy as an acquiescence in the term *allopathy*, so inapplicable to the science of medicine. Its adoption would greatly increase the importance of the framers of both terms, and would assist to hide from the public the fact that their practice is opposed by the accumulated experience of all nations, not only that of medical men, but of the people at large. A person knowing but little of medical science, (and this must ever be the case of the greater number of patients,) would say – allopathy – homœopathy – well, doctors disagree, I have tried one pathy, now I'll try the other.'

It is gratifying to report that the Editor, the redoubtable Thomas Wakley, cordially concurred that the word 'allopathy' must be admitted to be a misnomer, and that the less it was used by professional men the better.

In Garrison's standard 'History of Medicine', John Snow is accorded a dozen lines in which is acknowledged his priority in the theory that cholera is water-borne and taken into the system by the mouth, and also his pioneer work in anæsthesia. But Garrison is in error in stating that his essay on cholera was awarded a prize of 30,000 francs by the Institute of France. This mistake appears to have been copied from D'Arcy Power's article on Snow in the 1898 edition of the 'Dictionary of National Biography'. But Richardson (1858) in his memoir had made it clear that although Snow accompanied his uncle, Mr Empson, to Paris in April 1856, and during the visit deposited at the Institute of France a copy of his work, which he submitted as an entry for a prize of 10,000 francs offered for the discovery of a means for preventing or curing the disease, 'the decision of the judges has since been published but no note seems to have been made of Dr Snow's researches'. In a note in *Medical History* George Edwards (1959) published a letter from the Academy of Sciences which establishes that Snow did not receive the Institute's prize.

[1]After this address was completed I chanced upon William Budd's 'Memoranda on Asiatic Cholera' (1865). The first, written in 1861, concludes: 'Dr Snow was the first to announce publicly that Asiatic Cholera is disseminated by the rice-water discharges, and to substantiate the statement by evidence which, for my own part, I consider to be perfectly conclusive.'

Garrison makes a second error in attributing to Snow the pulmotor used for asphyxiated infants which Snow described in an early essay which he read to the Westminster Medical Society on October 16, 1841, entitled 'Asphyxia and the Resuscitation of New Born Children'. The object of this paper was indeed to introduce to the Society a pulmotor or double-air pump for supporting artificial respiration, but it had been invented by a Mr Read of Regent Circus. These two errors illustrate once again that though history may not repeat itself the writers of historical texts do.

In this Address, I have sought to convey something of the contribution which Snow made to the advance of medicine in the mid-nineteenth century. It was a time of change from the older authoritarian medicine to the growing appreciation of the role which science, and especially the application of physics and chemistry to the study of biological phenomena, could play in medicine. Amongst those in the front line of these advances was John Snow. To few men has it been given, as to him, to advance on two fronts. His pioneer work in anæsthesia made an impressive and lasting contribution to the relief of suffering of mankind; and his work on the mode of spread of cholera led to the saving of thousands of lives, not from cholera alone, but from all other water- and food-borne diseases. After reading Snow's original works, I share Carlyle's feelings when he wrote: 'One comfort is, that Great Men, taken up in any way, are profitable company. We cannot look however imperfectly upon a Great Man, without gaining something by him.'

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(1846*b*) *Lancet* i, 229
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Note: The reports of Society proceedings referred to in this Address are from the *London Medical Gazette* of 1846, 1847 and 1848, and the *Lancet* of 1846, 1849, 1850, 1854, 1855, 1856 and 1857.

Meeting May 15 1968

The following papers were read:

Nineteenth Century London Dispensaries as Teaching Centres
Dr W Hartston
(*Greater London Council*)

Some Medical Practitioners in the Pre-professional Era
Professor J Fisher
(*London School of Economics*)