

Extracts from Richardson, Benjamin W. "The Life of John Snow, M.D." In John Snow, *On Chloroform and Other Anæsthetics: Their Action and Administration*, i-xliv. Edited by B. W. Richardson. London: Churchill, 1858.

[xix] In the year 1848, Dr. Snow, in the midst of his other occupations, turned his thoughts to the questions of the cause and propagation of cholera. He argued in his own mind that the poison of cholera must be a poison acting on the alimentary canal by being brought into direct contact with the alimentary mucous surface, and not by the inhalation of any effluvia. In all known diseases, so he reasoned, in which the blood is poisoned in the first instance, there are developed certain general symptoms, such as rigors, headache, and quickened pulse; and these symptoms all precede any local demonstration of disease. But in cholera this rule is broken; the symptoms are primarily seated in the alimentary canal, and all the after symptoms of a general kind are the results of the flux from the canal. His inference from this was, that the poison of cholera is taken direct into the canal by the mouth. This view led him to consider the mediums through which the poison is conveyed, and the nature of the poison itself. Several circumstances lent their aid in referring him to water as the chief, though not the only, medium, and to the excreted matters from the patient already stricken with cholera, as the poison. He first broached these ideas to Drs. Garrod and Parkes, early in 1848; but feeling that his data were not sufficiently clear, he waited for several months, and having in 1849 obtained more reliable data, he published his views *in extenso* in a pamphlet entitled "The Mode of Communication of Cholera". During subsequent years, but specially during the great epidemic outbreak of the disease in London in 1854, intent to follow out his grand ideal he went systematically to his work. He laboured [xix/xx] personally with untiring zeal. No one but those who knew him intimately can conceive how he laboured, at what cost, and at what risk. Wherever cholera was visitant, there was he in the midst. For the time, he laid aside as much as possible the emoluments of practice; and when even, by early rising and late taking rest, he found that all that might be learned was not, from the physical labour implied, within the grasp of one man, he paid for qualified labour. The result of his 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 the south districts of the metropolis, where one water company, the Southwark and Vauxhall, supplied water charged with the London faecal 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.

There was, however, another fact during this epidemic, which more than the rest drew attention to Dr. Snow's labours and deductions. 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 [xx/xxi] 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 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. There arose hereupon much discussion amongst the learned, much sneering and jeering even; for the pump-handle removal was a fact too great for the abstruse science men who wanted to discover the cause of a great natural phenomenon in some overwhelming scientific problem. But it matters little. Men with great thoughts in their heads, think of little things which little men cover with their wide-spread feet. It matters little, for the plague was stayed; and whoever will now read dispassionately the report of a committee, afterwards published by the vestry, and the demonstrative evidence of the Rev. Mr. Whitehead, will find that the labours and suggestion of Dr. Snow, in reference to the Broad-street epidemic of cholera, must become each day better and better appreciated, as time, which never yet told a lie, tells the tale and points the moral of the event which is here so imperfectly described. Some who, at first, were amongst those who held up the labours of our friend to ridicule, or passed them over in contemptuous silence, have, indeed, since modified their opinions; and have either tacitly accepted his facts, or have done far worse by attempting to put them forward as though they were the work of no single man, or of some one unknown, or as though their connection [xxi/xxii] with a theory destroyed the originality of the facts themselves. It was my privilege, during the life of Dr. Snow, to stand on his side. It is now my duty, in his death, as a biographer who feels that his work will not be lost, to claim for him not only the entire originality of the theory of the communication of cholera by the direct introduction of the excreted cholera poison into the alimentary system; but, independently of that theory, the entire originality of the discovery of a connection between impure water supply and choleraic disease. The whole of his inquiries in regard to cholera were published in 1855, in the second edition of his work on the “Mode of Communication of Cholera” — a work in the preparation and publication of which he spent more than £200 in hard cash, and realized in return scarcely so many shillings.

In 1856, he made a visit to Paris in company with his uncle, Mr. Empson, who having personally known the present Emperor many years, had on this occasion special imperial favours shown to him, in which the nephew participated. During the visit, Dr. Snow lodged a copy of his work on Cholera at the “Institute”, in competition for the prize of £1,200 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.

The Medical Society of London, reformed under that name in 1849-50, by amalgamation with the Westminster Medical, was at this time the principal scene of Dr. Snow’s scientific exertions. In 1852, the Society elected him as Orator for the ensuing year; and at the eightieth anniversary of the Society, held on March the 8th, at the Thatched House Tavern, he delivered an admirable oration on “Continuous Molecular Changes, more particularly in their Relation to Epidemic Diseases.” He made no claim to the ora- [xxii/xxiii] tor’s gown; but the address was too forcible and first class not to call forth the enthusiasm of the audience. It was admirably received; and few of us who were present on that interesting occasion will forget the simple and genuine earnestness of our beloved associate, as in the twinkling twilight he carried us along with the smooth current of his thoughts. He spent nearly twelve months in the preparation of this oration. It was intended to convey, in the most pleasing manner at his command, a broad view of his observations on the communication of certain spreading diseases. He advanced, on this occasion,

the idea that intermittent fever, and perhaps yellow fever, are, like cholera, carried by their poisons direct into the alimentary system.

Two years after this event, having, meantime, passed the office of vice-president, the Society elected him to the highest honour it can confer,— to the presidential chair. He took his place as President, in his unassuming manner, on March 10th, 1855, delivering a short but pleasing address. Throughout the year he carried out the duties of his office with great success. One of his presidential acts was peculiarly graceful. One evening, while presiding, Dr. Clutterbuck (then the father, or oldest member of the Society) came into the meeting. The venerable and distinguished old man, then long past his eightieth year, had lately been a stranger to the assembly, and was known but to few of the members. The President, as Dr. Clutterbuck entered the room, himself rose, and in a way that was irresistible in its simple courtesy resigned his chair to the veteran Esculapian. “It is near fifty years,” said Dr. Clutterbuck with emotion, as he took the proffered seat, “since I last occupied this honourable position.” At the next anniversary meeting, held on March the 8th, 1856, Dr. Clutterbuck came to his last meeting, and to see (so the fates willed it) his friend the President play also [xxiii/xxiv] his last part in presidential duties. At the anniversary dinner on that same day, the President reviewed, in feeling terms, his own career in the professional strife, and expressed that his success in life had originated in his acquaintance with the Society over which he then governed by the general will.

In addition to the fellowship of the Medical Society, Dr. Snow belonged to the Royal Medical and Chirurgical, Pathological, and Epidemiological societies. He was also a member of the British Medical Association. The Medical Society, from its old associations, was, however, that in which he took the most active part. Next to this, the Epidemiological Society claimed his regard. When Mr. Tucker first contemplated the formation of the Epidemiological Society, Dr. Snow was one of the first with whom he held consultation, and from whom he received that able support which enabled him to found that excellent institution. From the first of the Society, Dr. Snow was an active member. He was on many of its committees; he was a member of council, and a frequent contributor to its *Transactions*. He used often to meet with opponents to his peculiar opinions at the meetings of this Society, but he always retained friendships.

The position which he took as an epidemiologist was original, and in opposition to the views of many eminent men who had in the matters relating to public health considerable influence, scientific and political. He could not consequently, and did not, expect to go on his way unopposed. But he did sometimes expect a more deliberate and considerate attention to his hard wrought labours than he received or deserved. He used constantly, though no great professor of Shakespearian lore, to deplore the long admitted fact, that nothing so inevitably tends to transform an earnest inquiring and enthusiastic man, into a supercilious, superficial, and cold-hearted egotist, as translation from the stool of self-reliance [xxiv/xxv] and independence, into the gilded chair of office and brief authority.

It must be admitted that Dr. Snow's views on the spread of epidemics were extreme in character; but from the slight which they too hastily received, they were not, I believe, properly understood. It has often been said that he encouraged by his arguments the perpetuation of certain offensive arts and occupations which are injurious to the public health; and in 1855, several journalists commented on him severely for this supposed error. But the fact is, he never presumed that any man could breathe with impunity other gaseous mixture than oxygen and nitrogen in atmospheric proportion. He knew too well the effect of inhaling chemical substances to allow of such supposition to enter his mind. But he contended, in regard to pure epidemic disorders,

distinguished by specific symptoms, that these have a specific poison, which is propagated by certain fixed laws, which attains its progression and increase in and through animal bodies; which is communicate from one animal body to another, and which is the same in its essence from first to last. This was his position, and he adhered to it. No mere emanation arising from evolution of foul smelling gas can, *per se*, according to his views, originate a specific disease, such as small-pox or scarlet-fever; as well expect that the evolution of such gas should plant a plain with oaks or a garden with crocuses. True, small-pox may occur over a cesspool as an oak may spring up from a manure heap; but the small-pox would never appear over the cesspool in the absence of its specific poison; nor the oak rise from the manure heap in the absence of the acorn which seeded it.

In 1855, Dr. Snow gave evidence before the select committee on the “Public Health and Nuisances Removal Bill,” in which evidence he strove to convey the impressions which are condensed above. Feeling [xxv/xxvi] that he had not been correctly understood, he afterwards wrote a letter to Sir Benjamin Hall, in which he set forth the whole of his argument very distinctly and sensibly. He indicated in this letter that he was no defender of nuisances, but that whereas a bad smell cannot simply because it is a bad smell give rise to specific disease, so an offensive business conducted in a place where it ought not to be should be proceeded against by ordinary law as a nuisance, without using in regard to it the word pestiferous, or otherwise dragging in and distorting the science of medicine. As time rolls on, it will probably be elicited that the groundwork of Dr. Snow’s theory is sound. That if he committed error, it was in adhering too closely to the abstract fact, and in not allowing sufficient importance to the favouring influence of impure conditions in the propagation and distribution of the specific poisons of the specific diseases.

At all events, the view he had maintained originally, he maintained to the end, and throughout conscientiously; and the aspersions that the object of his argument was to support his special theory regarding the communication of cholera, are utterly unfounded. In the present year, 1858, he read at the Epidemiological Society, and published in the *Medical Times and Gazette*, a repetition of his previous opinions, strengthening them by a statistical record, showing that the mortality of persons working at so-called offensive occupations is at certain ages lower, and at certain ages slightly higher, than in the general population. When the paper was read at the Epidemiological Society, Mr. Edwin Chadwick made a long series of objections to the paper, and complained that the argument was illogical. It was so, doubtless, on Mr. Chadwick’s premises; but on the premises advanced by Dr. Snow, as to the specific propagation of specific diseases by specific poisons, physiological problems on which, from his experimental researches and knowledge, he was far the best authority, his arguments were perfectly logical, and perfectly consistent.

In relation to public health, Dr. Snow contributed many other observations. In the first number of the *Sanitary Review*, he communicated a valuable paper, previously read at the Epidemiological Society, on the “Comparative Mortality of Town and Rural Districts”; and, previous to his decease, he was busily occupied in investigating the question of adulteration of bread with alum. He made several analyses of different specimens of bread, but his papers merely leave a brief record of the fact, without any comments or results.

We return for a few moments to some further points connected with his researches on inhalation. . . .