JOHN SNOW

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THE SECOND EDITION of John Snow's *On the Mode of Communication of Cholera*[1] is now regarded as "a classic that must always fascinate and inspire the student of epidemiology and preventive medicine."[2] Yet when it was published in 1855 it was not a success, and, in view of its present reputation, it is understandable that historians have been reluctant to accept the obvious explanation—that the book failed because it was not worth buying.

Macintosh[3] has listed all the reasons put forward for laying the blame on Snow's contemporaries. They were too much preoccupied with theories of foul air ever to think of cholera being spread by drinking water; there were men of ill will acting on behalf of the water companies; Snow was personally out of favor with his medical colleagues; he was in conflict with the scientific attitude of the age. "Snow," we read, "was a man out of his time. . . . In this dark period of British history it is surprising that any advance at all was made towards the removal of causes of disease or the improvement of health."

None of these explanations is altogether convincing. All discoveries meet with opposition of some kind. Snow, at least, was honored with the presidency of the Westminster Medical Society and was the attendant of Queen Victoria. His work on anesthetics, as, indeed, the novel concept of anesthesia itself, was received with a readiness which does not suggest any inherent contemporary prejudice against scientific invention.

Looked at more closely, it becomes increasingly clear that Snow, the epidemiologist, was in a very different position from Snow, the anesthetist. His early training had equipped him remarkably well for the opportunities presented by ether and chloroform, whereas his theory of the transmission of cholera, arrived at almost intuitively,[4] caught him unprepared and showed up his weaknesses. It is clear also that the inclinations of many of his modern admirers have prevented them from accepting the fact that the theory might be a very good theory, yet the book which contained it might still be a very bad book.

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CASE HISTORY

The John Snow who is familiar to most readers was largely concocted in the imagination of his friend and biographer, Benjamin Ward Richardson.[5] "All who knew him," wrote Richardson, "said he was a quiet man, very reserved, a clever man but not easy to be understood, and very peculiar." And although all present-day writers are necessarily indebted to this prejudiced witness for much of their material, wherever possible an effort will be made to break away from his tyranny.

John Snow was born in York in 1813, the eldest son of a farmer. From early childhood he showed his love of industry and, we are told, left school at fourteen, having learned all that there was to learn. He was next articled to a surgeon in Newcastle-on-Tyne and during his apprenticeship, according to Richardson,[5] "when he was seventeen years old, he formed an idea that the vegetarian system of feeding was the true and the old; and with a consistency which throughout life attended him, tried the system rigidly for more than eight years. He was a noted swimmer at this time, and could make head against the tide longer than any of his omnivorous friends." About this time, also, he joined the total abstinence reformers and became a powerful advocate of their principles; although in later life he occasionally drank a little wine, his views on the subject remained unchanged.

When his apprenticeship was over he worked as an assistant in the North of England, sometimes visiting York to help in the formation of temperance societies. "In leisure days during this period," wrote Richardson, "it was his grand amusement to make long walking explorations into the country, collecting all kinds of information — geological, social, sanitary and architectural."

In October 1836, he arrived in London, having walked through Wales and the West-country on the way. He enrolled as a student at the Hunterian School of Medicine and a year later began his hospital practice in the Westminster Hospital. He was soon in print. His first paper, "Arsenic as a Preservative of Dead Bodies," appeared in the *Lancet* in 1838.[6] At the suggestion of a lecturer, he had injected a body with arsenite of potash, a recently recommended preservative. During the subsequent dissection, one of the students became ill with diarrhea and vomiting. He wrote:

"In the summer of 1837, I injected another body and dissected it, with five of my fellow students, during the very hot weather of, I think, August. Decomposition was retarded considerably, but there was only one of us who did not suffer more or less indisposition, principally bowel complaints: and the subject gave out a peculiar odour, which I suspected arose from the arsenic rising in combination with the volatile products of decomposition."

About the Author

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Five or six weeks later he examined parts of the body which had been saturated in the solution and found no trace of arsenic. He assumed that it had all passed into the atmosphere. He continued:

"As an *experimentum crucis,* I some time afterwards placed some animal substances in a state of decomposition, on a dish along with the solution of arsenious acid, and placed over them a bell-glass receiver to collect the gases given off, and, at the end of two or three weeks, I added the air contained in the glass to a sufficient quantity of pure hydrogen to make an inflammable mixture, and burnt this as it proceeded from a small jet, holding a piece of glass in the flame, and I procured a small quantity of metallic arsenic. I expressed my conviction that this mode of injection was dangerous, and it was discontinued in that school."

THE MEDICAL SCIENTIST

In 1838, Snow joined the Westminster Medical Society and began a phase of his life well documented in the *Lancet's* reports of its meetings. During the next 5 years, he took the floor on almost every possible occasion, speaking on such subjects as contagion in typhus, alcoholism, poisoning with carbonic acid, the mechanism of respiration, deformities of the chest and spine in children, edema in scarlet fever, salivation due to mercury and lead, the mode of action of alcohol and opium, the effects of anemia, and paracentesis of the thorax.

There were two dominant themes — toxicology and respiratory physiology. The first came, it seems, from his interest in temperance. In the spring of 1838, when he was still a very new member, the Society discussed the dangers of the sudden withdrawal of alcohol in those who were accustomed to it. Snow, on evidence from a penitentiary in Geneva and from temperance societies in Leeds and York, showed that drunkards became healthier, if anything, when they left off drinking.[7]

From alcohol he turned, naturally enough, to the study of other poisons, opium, arsenic, and mercury. His remarks indicate that he was familiar with the standard toxicologic textbooks, Orfila[8] and Christison.[9] The latter especially, in its systematic treatment of the physiologic effects of poisons — their local action, their absorption and distribution in the body — displays a method which is reflected time and again in Snow's later research and which was no doubt useful to him in his appointment as lecturer in Forensic Medicine at the Aldersgate medical school.

It was his interest in poisons, too, which in turn led him to respiratory physiology. In November 1838, a man died while gazing into one of Joyce's Jerusalem Coffee-house stoves. The *Lancet,*[10] under the heading "Poisoning from Carbonic Acid," had described this contraption as "the Jerusalem Coffee-house bubble, Joyce's charcoal arcanum, and patent chafing dish for heating apartments economically and poisoning their inmates gratis with the products of combustion."

Snow entered the controversy with the argument that, contrary to the beliefs of Christison and Orfila, it was oxygen deficiency rather than excess of carbonic acid gas which was responsible for the man's death (carbon monoxide was not under suspicion at that time). He tested his theory by placing small animals in different mixtures of these gases, after which he had to admit that carbonic acid was lethal even when the oxygen level was normal.[11]

His next concern was the mechanism of respiration. This arose out of a debate at the Westminster Medical Society on whether the heart was solely responsible for the circulation of the blood. It was said that changes in pressure within the thorax during respiration might make a significant contribution. One person had calculated that the forces of respiration must be sufficient to overcome an atmospheric pressure, over the whole thorax, of more than 2 tons. Snow pointed out the absurdity of these calculations. The atmosphere, he said, exerted its pressure on the inside of the thorax as well as the outside, and he showed, by leaving one nostril open and connecting the other to a mercury manometer, that the pressure changes during respiration were really quite small.[12]

It was at this time, in 1842, that Snow published his well-known paper, "On Asphyxia, and on the Resuscitation of Still-born Children"[13]—his "first attempt at authorship," as Richardson tells us.[5]

This paper was first presented to the Westminster Medical Society. He proposed that the traditional treatment of asphyxia by heat should be replaced by artificial respiration, on the grounds that Magnus, Edwards, and others had shown that external heat aggravated the condition by increasing the utilization of oxygen. He then demonstrated to the meeting a small artificial respiration machine he had constructed.[14]

His technic in these discussions is almost always the same: the subjects he chooses are of topical interest to members; he challenges some flaw in their arguments and then goes away to think up an experiment to prove his point. His experiments are of two kinds: trying out the effects of different concentrations of potentially toxic gases on small animals and birds; constructing a piece of apparatus to demonstrate some aerodynamic or hydrodynamic principle. Examples of the latter are his manometer, his artificial respiration machine, and his two-way syringe for aspirating the chest. These experiments have their exact counterparts in his subsequent investigations into the effects of ether and the best ways of administering it.

Another feature of these discussions is his lack of respect for great men when he felt they were being foolish. Orfila and Christison, he said, had overlooked the possibility of oxygen deficiency in deaths from carbonic acid poisoning. Addison was wrong about the cause of edema in scarlet fever. Even the mighty Liebig was not safe from attack. "Mr. Snow," runs the report of the Royal Medical and Chirurgical Society for April 11, 1843,[15] "said that the recent work by Liebig contained many errors, and was by no means calculated to sustain the reputation its author had previously earned."

On this occasion, and on many others, he was justified in his criticisms. Liebig claimed that cold air, because it was denser than warm air, enabled the body to generate more heat on account of its greater oxygen content. All air, said Snow, was warmed to the same temperature before it reached the lungs.

But he was not always right. His early investigation into arsenic and dead bodies is by no means a model of experimental design. On another occasion he asserted that transudation from an inflamed part increased its heat and that if transudation were prevented by a covering of oilskin the part kept colder.[16]

He was particularly weak on the circulation. On January 21, 1843, he read a paper entitled "Circulation in the Capillary Blood-vessels." He said that the action of the heart was of itself, insufficient to effect the circulation of the blood. This "was most evident from the phenomenon of asphyxia," in which the capillary blood flow ceased before the heart stopped beating. There must, he thought, be some other power of importance engaged in the circulation, an attraction and repulsion between the blood and tissues to which allusion had been made by Dr. Alison and some other authors. In the discussion, Mr. H. J. Johnson said he was able to comprehend neither Snow nor Alison, and Dr. Reid could not conceive a cause of motion without some mechanical impulse. For once, apparently, the usual roles were reversed.[17]

Snow, it seems, excelled at seeing his way through an argument when he had the necessary facts at his disposal. But he was incapable of telling when he was out of his depth. His was the fallacy of attempting to explain complicated phenomena by a simpler theory than their nature admits of, the characteristic failing, according to Mill,[18] of medical men. With anesthesia he was on his own ground. The problem of cholera was a very different matter.

SNOW AND HIS CRITICS

When London, in 1848, was under the threat of a new epidemic of cholera, the Westminster Medical Society devoted several meetings to the subject. Snow's thoughts, however, were still on ether and chloroform and he took no part in the preliminary discussions, other than to remark on the similarity between cholera and ether asphyxia.[19]

He began to tackle the problem seriously towards the end of 1848, and it is evident that he once again adopted the approach of a toxicologist. Did the poison act locally and, if so, on what organ? Or did it first enter the blood stream? He concluded that it acted locally on the alimentary mucosa because, in his experience, the initial symptoms were always referred to the abdomen. And because the effects of the poison could be counteracted in the early stages by agents such as chalk and opium, which were known to act locally, he concluded that it did not enter the blood stream. To explain the general symptoms he adopted the theory which had recently been advanced[20] that these all followed the loss of fluid through the intestinal wall.

Having established that the poison acted primarily on the stomach, he deduced that it must get there by being swallowed; and since it did not leave the alimentary canal by passing into the blood stream, it must be discharged in the vomit and feces. The next step was obvious. To reach other victims, all it had to do was enter the sewage system and pass thence to the drinking water—a circulatory process which had been well recognized by fastidious Londoners for a number of years.[21]

The new theory was virtually complete. He now had to work out some method of proving it. The most direct way was to demonstrate the poison itself in patients and drinking water. As Budd had said, "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."[22] But here he was in difficulty. Too close an association with Budd, Swayne, and Brittan, who had independently suggested a water-borne fungus infection, would deprive him of the sole credit of being the first to publish the theory. And since, in any case, the idea of a fungus was soon discredited, he felt it unwise to look any further in that direction.[4]

It would have been impracticable, too, to follow up his pathologic arguments. He must have sensed their inherent weakness and gave three different accounts of how he made his discovery.[1, 23, 24] Besides, he was almost wholly ignorant of the contemporary pathologic literature relating to cholera. The idea that it was a local affection of the intestines, which he first imagined was entirely his own, was an old-standing foible of his fellow anesthetist, Prothero Smith,[25] and had long been discounted because of the numerous cases of persons who had died without showing alimentary symptoms. Attempts had also been made to produce the disease in men and animals through the ingestion of cholera discharges.[26, 27] All these had been unsuccessful.

Although Snow must have very soon become aware of these objections he seems to have chosen to disregard them. Instead, he decided to establish his theory by means of a series of epidemiologic investigations.

Even here circumstances were against him. He had no previous training for the work and soon found that it was impossible to trace the route of infection from case to case while an epidemic was raging in a crowded city. (William Budd, independently of Snow, by studying the connection between small groups of cases in a sparsely-populated area, had relatively little difficulty in coming to the conclusion that cholera was spread by drinking water.[21]) Snow, therefore, had to limit the scope of his inquiry to demonstrating that cholera spread most extensively in those areas where there were the greatest facilities for swallowing excretions.[28] Such an investigation, however clearly it established the relationship between polluted water and cholera, was not directed at the main point of the controversy; it could never decide between his own theory, that water was the vehicle of *transmission*, and that of his opponents, that polluted water *predisposed* to infection from another source.

Snow began by collecting particulars of a number of small incidents in various parts of London and finally undertook a more elaborate analysis of the Broad-street outbreak centered round a pump in the Soho district. In the course of these inquiries he had the idea of relating the mortality from cholera to the water supply in South London. There were two water companies in this area. The one was supplied from an adjacent part of the Thames, while the other had recently gone to a purer source further up river. Snow described the advantages of this situation:

"the intermixing of the water supply of the Southwark and Vauxhall Company with that of the Lambeth Company over an extensive part of London, admitted of the subject being sifted in such a way as to yield the most incontrovertible proof on one side or the other. In the sub-districts enumerated . . . as being supplied by both companies, the mixing of the supply is of the most intimate kind. The pipes of each company go down all the streets, and nearly all the courts and alleys. . . . Each company supplies both rich and poor, both large houses and small. There is no difference either in the condition or occupation of the persons receiving the water of the different companies."[1]

He had only to compare the mortality in those houses receiving the improved supply with those whose supply was unchanged. "No experiment," he wrote, "could have been devised which would more thoroughly test the effect of water supply on the progress of cholera than this, which circumstances placed ready made before the observer."[1]

The comparison which he then made, together with a description of the Broad-street outbreak and several smaller incidents, make up the bulk of the 1855 edition of *On the Mode of Communication of Cholera.*

The book was duly received by the medical press. One of the most thoughtful reviews was provided by Parkes in the *British and Foreign Medical Review.*[29] After pointing out that none of his readers could be ignorant of Snow's theory, nor of the perseverance and energy with which he had sought for facts to corroborate his view, he felt that it was the duty of the reviewer to look for "anything hollow or unsound in the facts brought forward, or in the arguments founded upon them."

Parkes then referred to the instances quoted by Snow to show that cholera tended to select houses whose water supply was known to be impure. He considered that Snow had neglected the most elementary epidemiologic principles. Of one of these instances he wrote:

"In this example, as in almost all the other cases adduced by Dr. Snow, we miss the very necessary information as to the number of persons resident in each house; . . . In six houses there were altogether twenty-four cases of cholera, in the seventh house (one with its own pump) only one case. For anything we are told to the contrary, however, there may have been only a single case in one of the six houses, and a greater number than the average in some of the others. If this were so, the power and force of the argument at once disappears."

Parkes next criticized Snow's conclusions from the Broad-street outbreak. Although Snow had shown that the greatest mortality occurred in the area supplied by a particular pump, he had not proved that this pump was actually contaminated, nor had he eliminated other sources for what was obviously a locally diffused poison. He had not explained why the disease reached its peak and then declined without any change in the water supply. Finally, as Parkes said, "There are, indeed, so many pumps in this district, that wherever the outbreak had taken place, it would most probably have had one pump or other in its vicinity."

The main difficulty, however, was with the inquiry into the South London water supplies. Parkes' first impression, and that of most other readers of the book, was that Snow had actually compared the subdistricts which he had described, those in which the supplies of the two companies were intimately intermingled. But this was not so.

What happened was this: Snow obtained, from the Registrar General, the addresses of those dying from cholera in these subdistricts and then ascertained the source of water supply for the houses where the deaths had occurred. He then had to find the total number of houses supplied by each company, but here he ran into trouble:

"A return had been made to Parliament of the entire number of houses supplied with water by each of the Water Companies, but as the number of houses which they supplied in particular districts was not stated, I found that it would be necessary to carry my inquiry into all the districts to which the supply of either Company extends, in order to show the full bearing of the facts brought out in those districts where the supply is intermingled."[1]

On reperusing the relevant passages, Parkes realized what Snow had done and that the experiment, which fortune had presented to the observer and which appeared so conclusive, had never been carried out. It was true that the mortality rate of 5 per 10,000 houses supplied by the Lambeth Company was strikingly less than the 71 per 10,000 of the Southwark and Vauxhall Company in the figures which Snow actually presented. This was certainly impressive, but, as Parkes said, "We doubt if the comparison can safely be made, for the Lambeth Company supplies, to a considerable extent, a good neighbourhood on elevated ground . . . while the Southwark and Vauxhall Company supplies the greater part of the poorest, lowest and marshiest district in London."

A year later Snow repaired his omission by publishing details for every subdistrict and amply confirmed his original statement. This step, as Bradford Hill[2] has pointed out, was fundamental to the argument. Is it not a little unfair to Snow's contemporaries to forget that it was Parkes, the critic, and not Snow who first called attention to its importance?

Parkes, although exacting in his criticism, seems to have shown a remarkable understanding of the problems of an author who was clearly less familiar with epidemiologic methods than he was himself. In conclusion he wrote:

"We have already said, that from the positive evidence adduced by Dr. Snow, we were unable to do more than conclude that he had rendered the transmission of cholera by water an hypothesis worthy of inquiry; we cannot draw any other conclusion from his researches on water supply, than that the predisposing effects of impurity of water are also rendered highly probable. We may be mistaken in this, and the evidence which seems weak to us may not be so to others. If so, when additional evidence shall be given, we shall receive it with the greatest pleasure; for though we think Dr. Snow's hypothesis, if proved, cannot explain all the phenomena of the spread of cholera, it would yet clear up some of the mysterious phenomena of its diffusion. Its establishment would therefore be an immense gain to science, and, we need not add, an important service to the State."

THE GROWTH OF A LEGEND

The answer seems plain enough. The book did not sell because it contained very little that Snow had not already said many times, because its arguments were inconclusive, and because it was difficult to read. The theory which it contained had always been treated with respect by responsible authorities,[30, 31] and if they withheld their wholehearted approval it was because the time had not come for it to be incontrovertibly established. There were many other theories with apparently equal claims on their attention.

Why, then, has the fate of the book aroused so much resentment in our own generation? Partly because the book itself has been overvalued. It has given many modern readers their sole contact with the period in which it was written. They are not likely to be wearied with page upon page of quotation from Simpson's "Asiatic Cholera" in support of the already accepted principle of contagion.[32] They are able to fill in the gaps in his reasoning with the comforting knowledge that Snow was, after all, right.

In part, also, we have been misled by the powerful imagination of Benjamin Ward Richardson. Richardson had two missions in life. One was to justify his own eccentric opinions. The other was to create romantic images out of relatively unpromising medical prototypes. Snow served both purposes. He could be presented to the public as an unrecognized genius; a genius, in fact, who was so peculiar that only Richardson had the discernment to realize his worth. "It was my privilege," he wrote, "during the life of Dr. Snow to stand on his side. It is now my duty as a biographer . . . to claim for him . . . the entire originality of the discovery of a connection between impure water supply and choleraic disease."[5]

On the other hand, he could become the hero of any number of Richardsonian charades. He was the "scientific unfortunate" who was eager for the few crumbs of ether practice which could be spared by the bustling druggist. He was the Victorian Pied Piper who confronted the vestry of St. James with his scheme for ridding them of the plague which menaced their doors. These most un-Snow-like figures do nothing but confuse the serious historian.

Perhaps the most significant factor, however, is the importance that Snow now has for many epidemiologists. His work on cholera has been cited as one of the crucial contributions of the epidemiologic method to medical knowledge.[33] It has been repeatedly claimed that the demonstration that typhoid fever and cholera could be water-borne rested on the establishment of significant associations. For example, "It was this significant association of cholera incidence with water supply that led, before the organism of cholera had been discovered, to his thesis that cholera was water-borne."[34] Or, "This control (of cholera) was suggested by ensuring that people did not drink water that had been contaminated by sewage, and this action was suggested by the painstaking collection of social data which showed a frequent association of contaminated water supplies with epidemics. In short, social action based on social observation proved to be an effective means of prevention before the causes of the illness were understood in technical detail."[35]

It may be remembered that in Snow's case the social data were collected between 1849 and 1855, the theory was formulated in 1848, while the Lambeth Water Company placed its proposals for obtaining a purer source of supply before Parliament in 1847.[36] These dates are not unimportant.

One last quotation deserves notice. Bradford Hill, the medical statistician, has written, "For close upon 100 years we have been free in this country from epidemic cholera, and it is a freedom which, basically, we owe to the logical thinking, acute observations and simple sums of Dr. John Snow."[2] In fact, by forestalling William Budd, Snow advanced our understanding of the spread of cholera by exactly 29 days.[4]

To insist on historic accuracy in these matters is not to discredit John Snow. Snow possessed undoubted merit, as Macaulay might have said, "a real merit, and a merit of a very rare, though not of a very high kind."[37] And if we replace the man himself by some elegant cipher, however well-meaning our intentions, we are denying ourselves acquaintanceship with one of the most fascinating and human characters in medical history.

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