John Snow
An aesthetist to a Queen
&
Epidemiologist to a Nation
A Biography

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CHAPTER 6

Snow On Cholera: Formulation Of His Ideas, 1848–1853

Snow began his ground-breaking research on volatile anesthetic agents in 1848, and its scope was so extensive that it would not have been surprising if he had confined his research to anesthesia. In that same year, however, Snow began to focus his intellectual and physical energy on a second subject that was equally extensive and equally little known. This was cholera. Specifically, the problem was a major public health issue: how cholera was transmitted, sometimes across vast distances and sometimes within small communities. Remarkably, from 1848 onwards Snow dedicated the rest of his career not only to practice and research in anaesthesia but also to finding answers to questions concerning the complex problem of cholera.

Snow's interest in cholera evolved over the years. First concerned with this serious disease in 1848, his thinking on the way it spread may be considered to have developed in two phases. The first extended from 1848 to 1853; the second, from 1854 to 1858. The first was initiated by Snow's study of cholera in two areas of London, was marked by his publication of the first edition of his monograph on cholera in 1849, and was consolidated by the Oration he delivered to the Medical Society of London in 1853. The second was initiated with his investigation of the cholera epidemic in 1854 and with the resulting second edition of his cholera monograph. Following that, Snow's ideas began to gain more attention, and the final years in this phase were given over to a consolidation of these ideas. Snow's work in cholera is therefore discussed in two separate chapters, according to these two phases.

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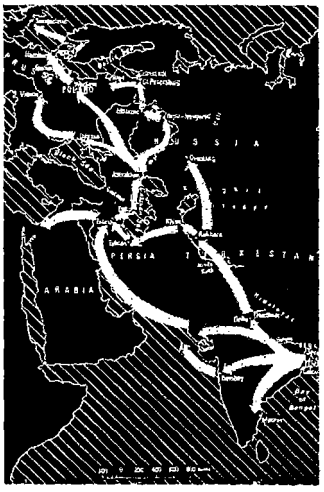
His work on cholera, like his work on anesthesia, tells us much about Snow. His investigation of the disease and his hypothesis concerning its transmission, while clarifying much that had been obscure about one of the great pestilences of the 19th century, reflects the nature of the man and how he worked. But, especially, a review of his work on cholera indicates why it was Snow, rather than any of his equally intelligent and competent contemporaries who arrived at the correct answer as to how cholera spread, and so could be prevented. Moreover, he did so in the prebacteriologic era when ignorance of the nature of infectious diseases and wild speculation about their causation and vested interests of some of the water companies made it extremely difficult to see, and convince other people of, the truth concerning the spread of cholera. Why did Snow succeed when others failed? To answer this question, and to evaluate Snow's stature as an epidemiologist, it is necessary to go in some detail into the early history of cholera and his research on cholera.

Overview: The Significance of Snow's Work on Cholera

Although the Asiatic form of cholera may have existed in India as long ago as 400 A.D.,[1] it began to concern Europeans only after an apparently new and devastating form became recognized in India from about 1817 onwards. This mutant form of the disease[2] caused concern and even panic in Europe as it moved steadily westward.



Westward progress of cholera, 1817 onwards

These feelings are understandable. Its progress seemed relentless. Cholera reached Europe in 1820, killing hundreds of thousands of people in its wake.[3] Physicians had a particular concern, for it was not easy to differentiate the deadly Asiatic form from the common and much less severe *cholera nostra* that they were accustomed to treat each autumn. (In England it was known as the English cholera.) Now, when a patient complained of fever and diarrhea, which type of cholera would a doctor have to keep in mind as a possibility? This was only one of the many aspects of the complex problem of cholera.

Another aspect of the problem was the lack of understanding of the cause and spread of cholera. The pervasive ignorance about cholera bred numerous fanciful ideas about it. Ideas on the prevention and the treatment of cholera were irrational and ineffective. In such an ideological maelstrom confusion and controversy inevitably flourished. As well, the general fear and dread of cholera, combined with the frustration resulting from the failure to stop the advance of the disease, induced a hopeless acceptance that the disease was a form of punishment by God. (The same reasoning has typified reactions to all major epidemics over the centuries, down to the current pandemic of AIDS.) In 1831 and 1832, when the first pandemic of cholera reached Great Britain, the outlook was indeed bleak, for the disease did seem to be a killer; this first pandemic killed 801 of the more than 1330 persons who became ill with cholera in England and Wales.[4]

Fortunately the situation was not entirely negative, for many physicians took the oppportunity to study the disease, and, though most were unable to draw valid conclusions about cholera, useful knowledge did accumulate. For example, it was during this pandemic that the idea of giving fluids intravenously was conceived [5] – the form of treatment that, when applied more vigorously, became the mainstay of treatment some 80 years later, [6] and that has remained the cornerstone of treatment of cholera today. As far as Snow was concerned, it was also during this pandemic that he, as a doctor's apprentice, first experienced the ravages of cholera – and began to think about the manner of its spread. In general, however, before Snow began his research in 1849, cholera seemed a complex subject, even an impenetrable mystery.

Part of the reason why cholera was so complex a problem lay in the controversy about infectious diseases in general – diseases like smallpox, yellow fever, typhoid and typhus, as well as cholera. Ideas about infection polarized around two schools of thought. One school believed that these diseases were contagious in the sense that the common cold or influenza can be "caught" by means of contact between a previously healthy person and an infected person. This contagionist school of thought could trace the lineage of its ideas back over many centuries. As Ackerknecht has pointed out, "the notion of contagion... had become firmly entrenched in Western culture after the acceptance of the (contagionist) Jewish Old Testament as a holy book in Christianity."[7] In contrast, the other school held that diseases like cholera were spread throught the agency of the atmosphere or the environment. The anticontagionists also traced their ideas through an ancient lineage. Classic Hippocratic teaching, for example, stressed the importance of the atmosphere and the climate, and the obvious existence of stifling filth, abominable sanitation and poor and even absent hygiene in the 19th century seemed to support anticontagionist views.

The debate between these two schools of thought, though quiescent at times, flared up from time to time, often generating strong feelings. Snow himself noticed this, pointing out that "the question of contagion in various diseases [had] often been discussed with a degree of acrimony that is unusual in medical or other scientific inquiries."[8] In Snow's day the general opinion favored the anticontagionists. The prevailing view was that disease was caused primarily by the presence of airborne poisons, or miasmata, which arose from the unhealthy and unsanitary environment in which the majority of urban citizens lived. This seemed quite reasonable in view of the lack of hygiene in the 19th century, and any sensitive person would naturally attribute a disease such as cholera to the lack of running water and water closets in most homes and the accumulation of disgusting piles of night soil by the sides of houses. Neither side, however, could claim victory. However much the protagonists of each school might quote from the steadily growing body of literature, conclusive evidence was lacking.

It is in this context that Snow's proof that cholera resulted from the contamination of water (and sometimes food) by a microor-

ganism must be evaluated. There are several reasons why Snow's work is significant. Not least important was that it put an end to the contagionist-anticontagionist debate. The apparent complexity of the issues in the debate vanished, for Snow produced evidence that could not be ignored, even before the work of Louis Pasteur confirmed the validity of the "germ" theory of infectious disease and long before the studies of Koch finally convinced people that cholera was indeed a microbial disease. It was Snow's publications on cholera, beginning with his classic monograph of 1849, [9] that, in Ackerknecht's words, "were probably among the most effective gravediggers of anticontagionism."[10]

Snow's work on cholera thus began to influence people's thinking about the nature, causation and transmission of cholera. Attitudes to cholera as a disease that was frequently fatal began to change, albeit slowly, after he began to publish the results of his investigations, particularly those of 1854 that he described in the second edition of his monograph, published in 1855.[11] He forced people to seriously consider polluted water as the principal vehicle by which cholera was spread, particularly to distant locations, and to understand the part played by lack of hygiene and contamination of food as additional factors. As well, his studies of the epidemiology of cholera laid the basis of the concept of the fecal-oral transmission of the cholera vibrio.

In Snow's day few people believed in disease-producing germs (i.e., bacteria and viruses), though this is not surprising, for when Snow first stated his ideas the existence of these microorganisms was just beginning to be demonstrated.[12] Therefore Snow was far ahead of his time when, in 1849, he suggested that the "poison" of cholera was a living agent and not inanimate and that it "might attach itself to the mucous membrane of the small intestines, and there multiply itself...."[13] The efficacious treatment of cholera still lay in the future – it was not until 1910 that Leonard Rogers introduced intravenous fluid therapy[14] – but Snow's work provided a rationale for the *prevention* of the disease. Although it took time for Snow's ideas to take effect, it is significant that no further epidemics of cholera occurred in England after 1866. This was partly related to the ascendancy of his hypothesis concerning the mode of the spread of cholera [15] and to the gradual, but inevitable, rejection of contrary views.[16]

Snow was one of the few in the middle of the 19th century who discerned the issues concerning the epidemiology and pathology of cholera. His work has stood the test of time. In 1936 Frost praised Snow's investigation of cholera in London in 1854 as "a nearly perfect model" of epidemiological analysis,[17] and as recently as 1982 Feachem noted that "little progress" had been made in understanding cholera transmission since the work of Snow and Koch.[18]

Snow's work, then, continues to be of interest. As does his work on anesthesia, it has scientific and biographic importance, being intimately related to medical thought in the 19th century on cholera and other communicable diseases and it highlights many of his personal qualities.

The Epidemic of 1831-1832: Snow's Introduction to Cholera

In 1831, when the first pandemic of cholera reached England, the 18-year-old Snow was working as an apprentice-apothecary to Dr. William Hardcastle in Newcastle. Like the majority of informed people, he became concerned about the virulence of the disease. Nothing had seemed to be able to stop the steady advance of cholera westwards from India in the 1820s towards Russia and continental Europe. Terror, panic and social unrest grew as the numbers of deaths in different countries had mounted.[19] In Hungary alone, 100,000 of the quarter of a million persons who were affected succumbed. Riots broke out as the poor became convinced that the rich, swayed by the ideas of Malthus, were deliberately spreading the disease in order to counteract the steady growth of the world's population. By the summer of 1831 cholera had spread to much of continental Europe; only some parts of central Europe, the Balkans and Scandinavia escaped. The English realized, with great dismay, that the channel, even with strict measures such as quarantine, would not keep cholera away.

The first hint of the arrival of cholera in England was the occurrence of some cases in ships lying in the river Medway, not far from London, in the summer of 1831. The epidemic of 1831 and 1832 did not generally become apparent, however, until October 1831, when cases broke out in Sunderland, a town in the northeast[20] – not far from where Snow was working. He saw

it spread to Newcastle-upon-Tyne at Christmas of that year and to nearby villages in the first days of 1832. One of these villages was Killingworth, and it was there that Snow came to confront Asiatic cholera at first hand.[21] The main livelihood of the village was coal mining, and cholera did not spare the miners. This impressed Snow, for he pointed out, years later, that miners were particularly susceptible to cholera: below ground there were no washing and toilet facilities and the potential for contamination of the miners' hands, and thus their food, was considerable.[22] He made this observation in 1849, when he developed his hypothesis concerning the spread of cholera and published the first edition of his now celebrated monograph on the spread of this disease.

In 1832, though he had never had to deal with the deadly disease before, Snow was no worse off than other doctors in Britain, or anywhere else. Everyone had their own beliefs about cholera – but no one understood the cause of the disease, how it spread, how to treat it, or how to prevent it. It continued to kill hundreds of people daily during this epidemic, and when it had dissipated at the end of 1832 some 21, 800 persons in England and Wales had died from cholera, regardless of the treatment offered. In Ireland another 20, 000 died, and in Scotland nearly 10,000.[23]

Beliefs concerning the cause of cholera were as invalid as they were varied. Most doctors treated their patients depending on whether they believed in a contagious or a non-contagious cause, but what they believed made little difference to the outcome. Theories of the cause of cholera abounded. A good example of the diversity of views was given by Dr.C.F. Favell. He wrote a tract *On the Nature, Causes and Treatment of Spasmodic Cholera* in 1832, from which the following passage is taken:

> *A great diversity of opinion has existed, and in fact does exist at the present time, as to the exciting cause of the disease in question ... . Some we find so wedded to the doctrine of contagion, that they imagine that it is propagated in different placed solely in consequence of a communication being established with the afflicted – others perceive in the state of the atmosphere the seeds of this malignant melody; whilst others again regard the earth and the waters by which it is surrounded, as the source of its existence.*[24]

Favell was evidently one of the more perceptive and open-minded physicians of his day. He was honest enough to say that "the partial views which have thus been entertained, and the partial observations which have been made in order to support them, have tended very materially to keep the subject overcast by clouds of doubt and mystery," and he warned that "if we attribute the origin of the disease to anyone of these causes singly, we shall generally be wrong...." Favell recognized the weaknesses in the different ideas about the causation of cholera. An atmospheric cause he dismissed since he found it "impossible" to regard "the common atmospheric vicissitudes" as the cause of the disease. He rejected the common belief in an electrical cause, since no electrical experiments proved the existence of an altered electrical condition. He also disregarded the spread of "some subtle poison which is ultimately mixed with the air, or ... some alteration in the chemical condition of the atmosphere, which is too minute to be detected by the senses" because chemical experiments would have detected it. Favell was admirably sceptical. He concluded that "the evidence of its aerial origin is not... sufficiently strong to induce us to form the hypothesis" of an atmospheric poison, and he added that "the idea of its terraqueous origin ... is quite as unsatisfactory as the hypotheses which we have just examined." The belief in a miasma arising from "putrid exhalations" was just as illogical; "how," he asked, "can its origin be accounted for in those situations where there are no vegetables to decay and no water to evaporate?" Favell was prescient, too. His observation that the first appearance of cholera in India was attributed to "an intemperate use of rank fish and bad rice" predated by over a century modern observations that cholera vibrio could indeed survive and be transmitted by these common foodstuffs [25] – and Snow's own observations that contaminated food might convey the cholera agent. [26]

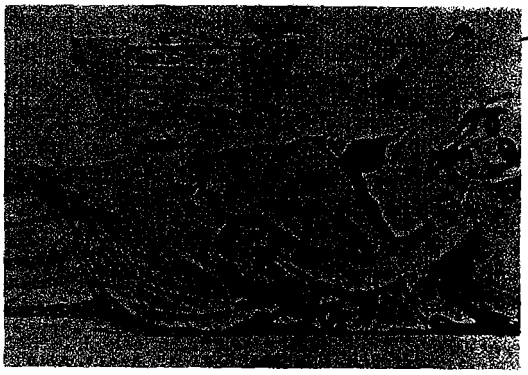
While Favell appears to have agreed with the contagionists, he identified the nature of a problem that they would have to solve before their views could be accepted – the problem that was eventually solved by Snow. Favell understood that "many diseases which are most undoubtedly of a contagious nature, are not capable of being communicated at their origin; and many others, whose contagious nature is admitted, arise independently of any communication with those who had been previ-

ously affected." The contagious nature of cholera was often not apparent; as Snow was to show 17 years later, it could indeed appear at points quite distant from its origin, and it could, again, indeed "arise independently of any communication with those who had been previously affected."[27]

Remedies and therapies for cholera were debated endlessly. The choice of treatment, of course, depended on what a particular physician thought about the nature of the disease. The variety of treatments was therefore "bewildering." As Morris has noted, "each had its rationale, some in tradition, others in old and new systems of science, whilst experience produced new empirically tested treatments."[28] Bleeding, for example, was based on the belief that disease resulted from an imbalance in the body "humours," or fluids. If one thought that any of these fluids were at fault one should remove them; so one bled the patient. Purging and sweating achieved similar objectives. In the early part of the 19th century modern concepts were grafted onto old ones. So, for cholera, in which the tarry-thick blood of the dehydrated patient was characteristic, it seemed logical to bleed a patient in order to overcome the slowing and blockage of the circulation to the vital tissues. Yet it was equally logical to argue that, as cholera dried up the bodies of its victims, there would be very little left to bleed. As J.G. French, who worked with Snow in 1854, put it more bluntly, "the violent means frequently resorted to for the recovery of the patient from the stage of collapse ... often assisted in hurrying him to the grave."[29]

Laxatives were frequently used, in an attempt to expel the poison from the body. Opponents of this therapy held that it was too demanding on the weakened body, and so they preferred purgatives like calomel or vegetable derivatives like castor oil and rhubarb, or magnesia. These stimulated the body, which was thought to be "depressed" in cholera. Peppermint and brandy served much the same purpose. Cholera made the body cold, and so various forms of external heating were appropriate. Doctors prescribed an astonishing variety of domestic remedies – hot bricks, linseed, mustard and bran poultice, plasters of hayseeds and warm sand or salt, for example.[30] These remedies were largely domestic ones. As one commentator remarked, "in choosing ingredients for the poultices, as in selecting medicines for emetics, doctors drew as much on the cookery book as on the pharmacopoiea."[31]

Only one form of treatment was rational, in the sense that it was potentially curative. This was the attempted rehydration of the patient by injecting salty solutions of water intravenously. In Britain this technique was first used by Thomas Latta in 1832,[32] following a suggestion by William O'Shaughnessy the previous year.[33] The idea was sound, for it is dehydration that kills the patient with cholera. The picture of extreme dehydration is unforgettable. It is the picture of "the blue death."



Gaunt appearance of the "Blue Death"

The description of the condition of William Sproat, the first victim of cholera in England in 1831, is typical: "the pulse scarcely beating under the fingers, countenance quite shrunk, eyes sunk, lips dark blue, as well as the skin of the lower extremities; the nails...livid...."[34] At this stage of the disease, an intravenous infusion would likely do little good, but today we know that if this is instituted early enough, it becomes the single most important part of the therapy of severe cholera. O'Shaughnessy and Latta were on the right track in their recommendation, and they failed only in not being vigorous enough, for literally liters of fluid must be given to the dehydrated cholera victim.

The standby of treatment was opium (or its derivative, laudanum), which is probably what Snow used to treat his sick miners.[35] At least logic supported its use; it was effective against vomiting and diarrhea and so seemed rational in the treatment of the patient with cholera. It was readily available and portable, and people were familiar with it and trusted it.

Insofar as cholera forced physicians to think objectively and seriously about the nature of the disease and to test remedies by putting them to use, the epidemic of cholera in 1831 and 1832 was not an unmitigated disaster, even though it slaughtered thousands. The same could be said of its impact on public health, for this benefitted from the rise of the sanitary reform movement in England, which was stimulated in large part by this epidemic of cholera. The epidemic spawned numerous accounts of cholera, many of which were based on careful observation and on recording of data relating to large numbers of patients; and many of these accounts Snow would have read, at least in 1849 if not in 1832. Yet, while the authors of these accounts were confronted with the same facts as Snow was, and careful though their work might have been, they failed to reach the correct conclusion that, 17 years later, Snow reached.

One reason why the early workers on cholera failed in this respect is that anticontagionist ideas still exerted a powerful and apparently logical attraction for careful and intelligent observers. A good example of one such physician is David Craigie. His "Account of the Epidemic Cholera of Newburn in January and February 1832" was published in the widely read *Edinburgh Medical and Surgical Journal* later that year.[36] Newburn was one of the places hardest hit by cholera in 1832. As Snow wrote in 1849, Snow said that "there is no spot... [England] in which the cholera was more fatal during the epidemic of 1832 than the village of Newburn, near Newcastle-upon-Tyne."[37] There, 320 of the 550 inhabitants were affected and 55 of them died. Craigie, in detailing the effects of cholera in 65 of the villagers, was constrained to reject contagion as an explanation for the outbreak. He did so because in his experience "the disease appears in several persons insulated and unconnected with each other, either all or at once in the same night, or within an incredibly short space of time." Although he also observed that cholera sometimes did run in families, "two, three, or four of which were usually attacked ... [which] perhaps might be regarded as the strangest possible proof of contagion," he set against this observation two others. The first was "the qualification, that two or even three of a family were often attacked at once, or within too short a time of each other, that it is difficult to see how the disorder could have been communicated from the one to the

other"; the second, the finding of "numberless instances in which the closest contact and the freest intercourse for some time was not followed by any attack." To Craigie the operation of a miasmatic agent seemed more plausible than a contagious one. In arguing for the former he recalled a story that Lancisi had told more than a century earlier. Thirty ladies and gentlemen "of the first rank in Rome," who had set out "on an excursion of pleasure" north of the river Tiber, were exposed to "a sudden change of wind blowing from the south over the marshes," after which "29 of whom were immediately seized with tertian fever."[38]

Craigie's account of cholera highlights the problem that he was unable to solve but which Snow, 17 years later, did begin to solve. To what degree was cholera "contagious"? Craigie could not see how the disease could appear, at much the same time, in persons who were "insulated and unconnected with each other." In contrast, Snow could. Moreover, he did so within the contagionist framework rather than, as Craigie had tried to do, within the framework of the popular belief in the miasmatic theory. To learn how Snow formulated his own ultimately correct hypothesis, and to contrast his ideas with some of Craigie's successors in the anticontagionist school, it is necessary to examine the lessons of the second pandemic of cholera, which produced the epidemic in England that started in September 1848.

The Epidemic of 1848-1849: Snow's Hypothesis of Cholera

The epidemic of 1831-1832 is remembered for the fear and panic it produced and for its social distress. The epidemic of 1848-1849 is remembered for its greater severity, some 54, 000 persons in England and Wales dying of cholera. Of these, more than 14, 000 were from London,[39] where Snow now lived. In relation to Snow's work, the epidemic of 1831 and 1832 is relevant because it was then that he first had to deal with cholera; the epidemic of 1848 and 1849 is memorable because it led him to formulate the hypothesis that, when eventually accepted, was followed by the disappearance of cholera from his country. To assess the validity of Snow's hypothesis and to compare Snow's ideas with those of some of his contemporaries, it is necessary to examine the epidemic of 1848-1849 and Snow's reaction to it.

Cholera returned to London in the last few months of 1848, and it was then that Snow began to think seriously about the disease. Several events in 1848 and 1849 impressed him –and sufficiently so to induce him to write the monograph *On the Mode of Communication of Cholera*, which bears the publication date of August 29, 1849.[40]

The timing of the arrival of cholera in London and of the first case of the disease there were of great significance for Snow. Cholera arrived in London on September 22, 1848, when the steamer *Elbe* docked there from Hamburg. A seaman from the *Elbe*, John Harnold, after leaving the ship took lodgings in Horsleydown, a part of London, and became fatally ill with cholera. To Snow the significance of this first case of the 1848 epidemic was enormous. Three facts seemed particularly important. First, "a man named Blenkinsopp ... came, after the death of [John Harnold], to lodge and sleep in the same room, and had the cholera eight days after him." [41] Next, the doctor who had treated these two men (a Dr.Russell), reported that some subsequent cases in Horsleydown, which commenced three or four days later, broke out "a little way removed from that of the two preceding...." [42] This intrigued Snow. Although there was no "apparent connection" between the cases, closer study did reveal a connection. As Snow emphasized, *"an open sewer, up which the tide flows, runs past both places, and the sewage from the houses in the first neighbourhood is, when the tide rises, carried past those in the second."*[43] Here Snow was stating, for the first time, his views on the means by which cholera seemed to spread so quickly. It was communicated from one person to another, principally by water but also by fomites.

Snow had come down firmly on the side of the contagionists – with some relief.. "Who can doubt," he argued, "that the case of John Harnold ... was the true cause of the malady in Blenkinsopp, who came, and lodged, and slept, in the only room in all London in which there had been a case of true Asiatic cholera for a number of years?"[44] So he was led to ask, rhetorically, "if cholera be communicated in some instances, is there not the strongest probability that it is so in the others – in short, that similar effects depend on similar causes?"[45] For Snow, always the rational physician, it was most satisfying to explain outbreaks of cholera in terms of contagion in a specific cause and

effect relationship. It was a relief to find evidence for the contagionist rather than the noncontagionist viewpoint. So he asked, again rhetorically, "what is so dismal as the idea of some invisible agent pervading the atmosphere, and spreading over the world?"[46] Snow's concept seemed sound. Especially reassuring was the likelihood that "cholera might be checked and keep at bay by simple measures that would not interfere with social or commercial intercourse; and the enemy would be shorn of its chief terrors"[47] – terrors that manifested themselves in the deaths of 988 Londoners between September 1848 and March 1849.[48]

There was another reason why the Harnold-Blenkinsopp route of infection was significant to Snow: although the relationship of the initial two cases of cholera to those that followed in Horsleydown could be explained on the basis of pollution of water by the agent of cholera, personal contact was a factor also. Snow noted that Harnold became ill and died on September 22, 1848, and that Dr. Russell attended Blenkinsopp "in the *same room*" (Snow's italics) on September 30.[49] Since the two men had stayed in the same room, another, more direct, route of infection in addition to the indirect one of water pollution by sewage seemed likely. As Snow well knew from his Killingworth days, cholera could be transmitted on physical objects, particularly the hands and food, both of which could be contaminated by the cholera organism in the absence of hygienic conditions, as in mine pits – and no doubt in a room of a sleazy lodging house in a seamy part of London. So cholera could be spread by person-to-person contact, either directly or by means of fomites like bedlinen (which, when moist, can support the life of the cholera vibrio for up to 5 weeks, and even when dry, for a few days[50]). Water was important in the transmission of cholera, but so were contaminated food and fomites. This understanding would facilitate the prevention of cholera. Snow was certain that "the sanitary measure most required ... is a supply of water ... from some source quite removed from the sewers," but he also stressed the need *"for all persons attending or waiting on the patient to wash their hands carefully and frequently, never omitting to do so before touching food...."*[51] This would be one of the "simple measures" that would prevent outbreaks of cholera, particularly where hygiene was lacking, as so often it was in London in the first three decades of the 19th century.

One such outbreak particularly interested Snow. This occurred in December 1848, in a poor children's institution in Tooting. There, among 1,000 children, 180 of the 300 who contracted cholera died of cholera within the space of a few weeks.[52] This did much to bring home to Londoners the gravity of the 1848 outbreak – though none had Snow's insight into its spread.

The evolution of Snow's thinking in this period can be traced by examining the first edition of his monograph on cholera. He published it after becoming aware of further outbreaks of the disease and collecting enough information about its natural history to warrant stating his conclusions concerning the spread of cholera. As well as presenting a valid hypothesis on cholera that was based on his clinical as well as his epidemiological understanding of the disease, this short pamphlet manifests the qualities that Snow will be remembered for. One cannot read it without being impressed by his wide knowledge of the subject, his acute powers of observation, his painstaking diligence, his logical argument and his conviction and authority. In the context of cholera Snow is usually regarded as an epidemiologist – and indeed the clarity of his understanding of cholera in this respect fully merits his reputation as an epidemiologist – but he should also be regarded as an astute clinician whose understanding of the disease was derived also from his ability to make deductions that were primarily clinical and pathological. The early part of the monograph illustrates some of the qualities that enabled Snow to find the correct answer to the riddle of cholera, and it bears emphasis that as a clinician he had acute insight into the pathological aspects of the disease, and as a medical scientist he had clear epidemiological understanding. This is illustrated by the following passage:

> *Reasoning by analogy from what is known of other diseases, we ought not to conclude that cholera is propagated by an effluvium. In all known diseases in which the blood is poisoned in the first instance, general symptoms ... precede the local symptoms; but it has always appeared, from what the writer could observe, that in cholera the alimentary canal is first affected, and that all the symptoms not referable to that part are consecutive, and apparently the result of the*

local affection. ... The quantity of fluid lost by purging and vomiting ... the suddenness of the attack, and the circumstance that the loss is not replaced by absorption, has seemed sufficient, in all the cases witnessed by the writer, to account, by the change it must occasion in the quantity and composition of the blood, for the collapse, difficulty of breathing, and, in short, for all the symptoms, without assuming that the blood is poisoned ...

It is generally assumed that the blood becomes so altered by the cholera poison, that its watery and saline parts begin to exude by the mucous membrane of the alimentary canal; but it is more consonant with experience, both therapeutical and pathological, to attribute the exudation to some local irritant of the mucous membrane; no instance suggesting itself to the writer in which a poison in the blood causes irritation of, and exudation from, a single surface, as in cholera ....

Having rejected effluvia and poisoning of the blood in the first instance, and being led to the conclusion that the disease is communicated by something that acts directly on the alimentary canal, the excretions of the sick at once suggest themselves as containing some material which, being accidentally swallowed, might attach itself to the mucous membrane of the small intestines, and there multiply itself ....[53]

While the germ of Snow's hypothesis may have been planted as early as 1832, as a result of his Killingworth experience, and while he had certainly begun to think seriously about cholera in the "latter" part of 1848, it was closer studies of cholera outbreaks in the summer of 1849 that led him to develop his hypothesis more fully. But what is particularly interesting is the belief that he stated at this time that cholera was associated with "something" that can *multiply* itself – that is, with a form of life, rather than with an inanimate "poison" or "miasma." Two other episodes in London besides the occurrence of cholera in a sailor from the *Elbe* led Snow to this firm, contagionist belief.

As the first wave of the new epidemic of cholera struck London from September 1848 to May 1849 and the second dashed over the metropolis beginning in June 1849 and November 1849, Snow had become as concerned as any one about the ravages of

the disease. Although, in addition to looking after his own patients, he was busily engaged in conducting a series of investigations into anesthesia, he continued to ponder the problem of the spread of cholera throughout the first half of 1849. His ideas on cholera were crystallizing when, in July 1849, two outbreaks of cholera stimulated him to publish his monograph in August 1849 because they "seem[ed] to offer more direct proof" of the hypothesis he had been formulating since the latter part of the previous year. One of these outbreaks occurred among the residents of Thomas Street, a poor area in Horsleydown; the other, among a more "genteel" group of persons who resided in Albion Terrace, on the Wandsworth Road.

The overcrowded houses in Thomas Street were too intimately connected with each other to be considered healthy. This is Snow's description of them:

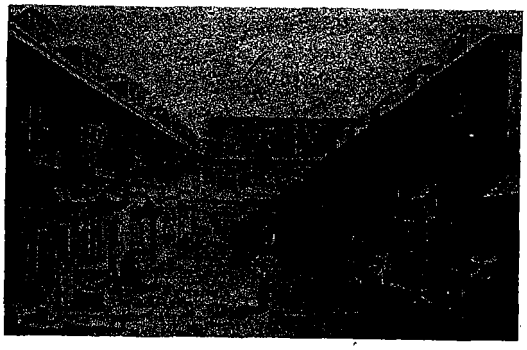
> *In Thomas Street, Horsleydown, there are two courts close together, consisting of a number of small houses or cottages, inhabited by poor people. The houses occupy one side of each court or alley – the south side of Truscott's Court, and the north side of the other which is called Surrey Buildings, being placed back to back, with an intervening space, divided into small back areas, in which are situated the privies of both the courts, communicating with the same drain, and there is an open sewer which passes the further end of both courts.*

Snow found that "in Surrey Buildings the cholera ... committed fearful devastation, whilst in the adjoining court there [was] but one fatal case, and another that ended in recovery." The only way the two groups of houses differed physically was that "in the former court the slops of dirty water poured down by the inhabitants into a channel in front of the houses got into the well from which they obtained their water ... ." The drainage channel was quite close to the well, and was "covered in on a level with the adjoining ground;" the drinking water therefore became polluted. As Snow reported:

> *owing to something being out of order, the water for some time past occasionally burst out on top of the well, and overflowed into the gutter or channel, afterwards flowing back again*

mixed with the impurities; and crevices were left in the ground or pavement, allowing part of the contents of the gutter to flow at all times into the well... and when [the well] was afterwards emptied a large quantity of black and highly offensive deposit was found it.

This was not a healthy situation – but it was typical of the unhealthy environment of these residents and many others in other parts of London.



Typical residential court (Southwark, London)

In such an environment cholera could break out at any moment. The "fearful devastation" in Thomas Street actually began on July 20, 1849. A girl contracted cholera, though she recovered. An elderly woman fell ill with cholera the next day but, after appearing to rally, she died on August 1. The damage was done while these two were ill, for Snow found out from the local doctor "that the evacuations were passed into the beds, and that the water in which the foul linen would be washed would inevitably be emptied into the channel" that polluted the well-water. Four others became ill on July 28, "seven or eight" on July 29, and "several" on July 30. Eleven of the inhabitants living in the houses backed onto the court died; they had lived in 7 of the 14 houses situated in the court. This mortality greatly exceeded the average for the neighbourhood then; indeed, just the first two cases would have constituted the average. Snow postulated that "in a few days, when the dejections of these [two] patients must have become mixed with the water the people drank, a number of additional cases commenced nearly together." The victims

were women or children; men, at work much of the time, and who may well have preferred beer to water, escaped.

In Albion Terrace cholera appeared to have broken out after "water got contaminated by the contents of the house-drains and cesspools." Snow discovered that it affected nearly all the houses in which the water was thus "tainted," but no others. Nor did any other cases occur in the neighbourhood.

A thorough medical detective and a natural epidemiologist, Snow traced the events in detail. The terrace comprised 17 houses. Their water came from a "copious" spring in the road onto which the houses fronted. The water ran along a brick barrel drain between Nos. 7 and 8 to behind them, and was distributed behind each house to water tanks made of brick and cement and covered with a flat stone. The tanks were connected with each other by stoneware pipes, and behind the kitchen at the back of each house a lead pipe conveyed the water to a pump situated in each kitchen. An obvious hazard was the proximity of the water supply to the cesspool behind each house under the privy. Since the cesspool was only 4 feet from the water tank the drinking water readily became polluted. This is what Snow had to say about the situation:

> *...the top of the overflow-drain from the cesspool was found to be fifteen inches above the top of the tank, and the interviewing ground was very wet. The overflow-drain ... had no bottom, or one so soft that it could be penetrated with a stick; and it crossed at right angles above the earthenware pipe of the water tank, the joints of which were leaky, and allowed the water to escape. This arrangement provided a foul breeding place for disease.*

Another problem was that the 17 houses were all part of a unit. The tanks were all on the same level, so that pumping from one would draw water from the others, any impurity seeping into one tank consequently polluting the rest. This problem was compounded by a second. Behind No. 7 a drain, which lay over the brick barrel drain that took drinking water to the houses, received not only the water from drains from the cesspools and the kitchens but also surface water from the road. This drain proved inadequate during a storm on July 26, for it burst near No. 8, flooding that house and No. 9 with "foetid" water. The same thing happened during another storm on August 2.

Snow realized that the inhabitants of Albion Terrace, and those of Thomas Street, were at serious risk of pollution of their drinking water by the cholera "poison." In Albion Terrace the first case of cholera occurred on July 28, at No. 13, after which "more than half the inhabitants of the part of the terrace in which the cholera prevailed were attacked with it, and upwards half of these cases were fatal." The 20 fatal cases, together with four or five among those who were attacked "after flying from the place," occurred in 10 of the 17 houses. Snow was unable to determine precisely how the disease developed in the first patient, but he found that "it was two or three days afterwards, when the evacuations from this patient must have entered the drains, having a connected with the water supplied to all the houses, that other persons were attacked." The outbreak continued for about 10 days.

What impressed Snow about these two outbreaks was "the circumstance of the cholera evacuations getting into the water which caused the disease to spread so much beyond its ordinary extent." Careful study of these outbreaks led him to conclude that it was "evident" that the only "special and peculiar cause" of this outbreak was the state of the water, "which was followed by the cholera in almost every house to which it extended, whilst all the surrounding houses were quite free from it." Cholera, it seemed to Snow, was communicated by quite specific means – means that were too specific and too localized to be consistent with an atmospheric cause. He was convinced that his conclusion that cholera was spread primarily by water, and secondarily by lack of hygiene, was sound. For Snow "it was the circumstance of the cholera evacuations getting into the water which caused the disease to spread so much beyond its ordinary extent." The purpose of his monograph *On the Mode of Communication of Cholera* was to bring his hypothesis to the attention of his colleagues.

An examination of cholera elsewhere in London was the next logical step, and Snow extrapolated his reasoning to the wider area of the metropolis of London and its water supply. He reasoned that "as soon as the cholera began to prevail in London, part of the water which had been contained in the evacuations of patients would begin to enter the mains of the Water Works." The inevitable consequence would be the spread of cholera by the polluted water.

In investigating cholera in the summer of 1849 Snow was one of the first to use the epidemiological approach to solving medical problems.[54] He examined the statistics for deaths from cholera in different districts of London, analyzing the reports of the Registrar-General over a period of a year, from September 23, 1848, to August 25, 1849. What he found strengthened his hypothesis. The death rate from cholera was much higher in districts south of the Thames – in those districts where the water was supplied by companies that drew their water from the Thames where it was "much polluted by the sewers." The difference between the death rates for these districts and the rest of London was quite striking: "out of the 7, 466 deaths in the metropolis, 4, 001 ... occurred on the South side of the Thames, being nearly eight to each thousand of the inhabitants." The precise rates were as follows:

London District	No. of deaths from cholera	Population in 1841	Deaths per 1000 inhabs.
West	533	300, 711	1.77
North	415	375, 971	1.10
Central	920	373,605	2.48
East	1, 597	392, 444	4.06
South	4, 001	502, 548	7.95

The western district was supplied by the West Middlesex Company, which obtained its water from a reach of the Thames above Hammersmith, where the river was relatively free of sewage. Parts of London were also supplied by the Chelsea Company, which obtained its supply from Chelsea, which was close to Vauxhall, but this company filtered its water. The northern and central districts were supplied partly by the New River Company, which drew water from the New River, in the rural county of Hertfordshire, north of London. The eastern district obtained water from the East London Water Company, which took water from the River Lea; though this was somewhat polluted by sewage and though the company had recently switched to water from above the tidal reach of the river it was not clear whether some of the water was still taken from the original part of the river. Therefore, once again, it looked as

though the quality of water supplied to people's homes was a factor in the spread of cholera, and in this respect the inhabitants of the region south of the Thames were at particular risk, for the South London Water Works and other water works south of the Thames obtained their supplies no higher up the Thames than Vauxhall Bridge, where it was quite certainly polluted by sewage.

By August 1849 Snow had collected enough evidence to warrant his publishing his monograph. In bringing his ideas before the medical profession, he hoped that his colleagues would "decide what there may be of value in his opinions." A perfectionist, he would have preferred "to have given the subject a much more extensive examination, and only to have published his opinions if he were able to could bring toward such a mass of evidence in their support as would have commanded ready and almost universal assent," but he was busy with his anesthesia research and thought it expedient to present his data in their present form for his colleagues' critiques.

However, the monograph had relatively little effect. As a pamphlet, a vehicle that was often used in the 18th and 19th centuries for the dissemination of ideas, it became one more of the many tracts on various subjects that appeared in this form rather than as articles in medical journals. The anonymous reviewer for the *London Medical Journal* of September 14, 1849, however, did go so far as to write that "Dr. Snow ... deserves the thanks of the profession for endeavouring to solve the mystery of the communication of cholera." He appreciated Snow's endeavor: "it is only by a close analysis of facts, and the publication of new views, that we can hope to arrive at the truth."[55] The truth was always Snow's primary goal, but his case was not yet complete. Its weakness was that his explanation was at best only a probability, and his hypothesis was not a certainty. As the reviewer argued in connection with some of the later cases in the outbreak in Surrey Buildings, "other causes, irrespective of the water, may have been in operation, especially as the persons were living in close proximity to the affected. It might," the reviewer continued, "turn out on inquiry that the men living in [Truscott's] court, who were not attacked, used the same water." The facts that Snow had presented could "furnish no proof whatever of the correctness of [his] views."

This reviewer, however, made a valuable suggestion: "the *experimentum crucis* would be, that the water conveyed to a distant locality, where cholera had been hitherto unknown, produced

the disease in all who used it, while those who did not use it escaped." Snow acted on this suggestion, though he had to wait for 5 years before he was able to do so.

Meanwhile, Snow continued, as he wrote on September 15, 1849, to bring forward "a variety of details" that would "show the connection between tainted water and the extension of cholera."[56] Whenever he could he publicized his views. Thus in October 1849 he spoke to both the Western Literary Institution (on October 4)[57] and to the Westminster Medical Society (on October 13).[58] On the latter occasion he presented his views at length and referred to the occurrence of cholera in other places in order to strengthen his argument. He also took part in a discussion on cholera at the Westminster Medical on October 20.[59] His fellow members commented on his ideas, but few agreed with him. Dr. Francis Sibson, another pioneer anesthesiologist, stated that "he did not agree with Dr. Snow that the primary seat of the disease was in the mucous membrane of the intestines, for often [Dr. Sibson argued] the complaint set in with the greatest intensity without any intermediate stage of diarrhea; and, on the contrary, diarrhoea of a very depressive character might exist, and yet no cholera supervene." Dr. James Bird agreed that the disease might be communicated from person to person "under favourable conditions," and, while he did not reject Snow's views, he could not accept that "such medium of communication had more than partial effect on spreading cholera." Like many others, Dr. Bird was still convinced that "the atmosphere is the principal channel by which cholera is disseminated." Dr. Lancaster said that, while "Dr. Snow's theory of the progress and development of cholera involved the necessity of its being something generated in the mucous membrane, and capable of being diffused by handling, and especially through drinking-water," if there was some sort of poison generated, "no such poison had, however, yet been demonstrated to exist." (The age of microscopic demonstration of bacteria, of course, had not yet arrived.)

Concerning the priority of his hypothesis vis-a-vis the ideas of others on both the communication of cholera and his credibility as a scientist, Snow's discussion of cholera on October 4, 1849, is particularly important. Wisely he distanced himself from a group in Bristol who had arrived at a plausible, but erroneous, conclusion.

In his October 4 lecture to the Western Literary Institution he referred to the Bristolians' theory of the propagation of cholera. This theory had originated, to quote one of its proponents, William Budd, a noted Bristol physician, in "the very important discovery, that peculiar microscopic objects exist constantly in the characteristic 'rice-water' discharges of persons affected with Malignant Cholera, and in the atmosphere of infected places."[60] Budd and some fellow members of the Bristol Medico-Chirurgical Society were concerned about the arrival of cholera in Bristol in June 1849.[61] On July 7 the Society agreed to study the pathological nature of the disease. Budd was appointed to the microscopical subcommittee, his fellow members being Drs. J.F. Bernard, J.C. Neild, J.C. Swayne, J.G. Swayne, A. Prichard, F. Brittan, and J.A. Symonds. Budd, who is best known for his sterling work on typhoid and, like Snow, was interested in epidemic diseases, then organized a meeting of the subcommittee on July 9 in order to examine the chemical and microscopical characteristic of rice-water evacuations. Drs. Brittan and J.G. Swayne, the microscopists, appeared to have detected abundant and unusual structures in the evacuations that seemed so remarkable that the subcommittee later reported that "they were characteristic of the evacuations of cholera, if not the very agents causing the disease."[62] Brittan also studied liquid condensed from the atmosphere of a house where five persons had just contracted cholera. His finding of ring-like bodies that were similar to those he had discovered in the rice-water evacuations seemed to open a new chapter in the understanding of the transmission of cholera that might reconcile the contagionist and the miasmatist. Budd conducted his own studies and reported finding "the same organism in almost every specimen of drinking water" obtained from districts where there had been cholera." He added that he had found no such organisms in a "great number of specimens of water from healthy quarters."[63]

Budd, therefore, believed that he and his colleagues had discovered the causative agent of cholera, and firmly said so in his own monograph on cholera. This monograph was published only 29 days after Snow's. The two monographs have often been compared, [64] though the content and the methodological approach of Budd's were quite different from those aspects of Snow's monograph. It is true that, on a superficial level, Budd's

deductions were not dissimilar to Snow's, as the following statements suggest: "the cause of malignant cholera [wrote Budd] is a living organism of distinct species"; the organism, after being swallowed and thus taken into the intestines, "there becomes infinitely multiplied by the self-propagation, which is characteristic of living beings"; these organisms "are the cause of the peculiar flux which is characteristic of malignant cholera ... and ... constitutes the disease"; and the dissemination of the organisms occurs not only in the air and the food but also "principally in the drinking-water of infected places." Budd, however, considered the structures that the Bristolians had seen to be "bodies ... most nearly related to simpler orders of fungi" and that those based in the human intestinal canal are sufficient to account for the propagation of the disease."

By going out so far on a fragile limb Budd ultimately forfeited his claims to credibility. Drs. W. Baly and W.W. Gull, members of the Subcommittee of the Royal College of Physicians on the Cholera Fungi, concluded in November 1849 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."[65] The importance of what Snow said on October 4, 1849, lies in his taking some trouble to avoid being identified with the cholera-fungi theory. While Snow said of cholera that "the disease was set up by agents morbidly influencing the alimentary canal," he wished not to be seen as "laying absolute stress on the circumstance that the fungi recently discovered were really the *fons et origo mali.*"[66] This is fortunate. As Brown has observed, "Snow had only narrowly escaped involvement in a failure which might well have put an end to his research."[67] In fact, Snow's research into cholera now intensified and his thinking then steadily evolved.

Snow next stated his concept of the nature and spread of cholera in a two-part article "On the Pathology and Mode of Communication of Cholera." Published in the *London Medical Gazette* on November 23 and 30, 1849, [68] this and his monograph of August 1849 incorporate the essence of Snow's earliest ideas on cholera. In 1849, however, the article would have been read more widely than the monograph.

In the first part of these two articles, Snow presented much the same information as he had in his monograph but he did go into

greater detail and he wrote with greater conviction. He had by now progressed to the belief that the cholera "poison" was self-replicating and a form of life rather than an inanimate miasma. An early paragraph confirms this:

> *... we have arrived at two conclusions – first, that cholera is a local affection of the alimentary canal; and secondly, that it is communicated from one person to another. The indication from these data is that the disease must be caused by something which passes from the mucous membranes of the alimentary canal of one patient to that of the other, which it can only do by being swallowed; and as the disease grows in the community by what it feed upon, attacking a few people in a town first, and then becoming more prevalent, it is clear that the cholera poison must multiply itself by a kind of growth, changing surrounding materials to its own nature like any other morbid poison; this increase in the case of the* materies morbi *of cholera taking place in the alimentary canal.*

Snow again stressed the role of lack of hygiene, indicating the spread of the disease through soiling of the hands and then the food that was thereby contaminated. Here he brought the high incidence of cholera in miners to bear on the argument. The absence of washing and toilet facilities down the mines, he believed, favored the spread of cholera by the fecal-oral route, while outside the mines overcrowding in the miners' homes facilitated spread of the disease to their families.

Poverty, overcrowding and lack of hygiene, Snow noted, were thus important factors in the spread of cholera, though they would tend to confine outbreaks of cholera to "poor and crowded dwellings." "But," he added, "there is often a way for it to extend itself more widely, and that is by the mixture of the cholera evacuations with the water used for drinking and culinary purposes, either by permeating the ground and getting into wells, or by running along channels and sewers into the rivers." He thus reemphasized his concept of a bipolar fecal-oral model – spread by socially determined factors and spread by water-supply factors. His hypothesis was beginning to become both specific and comprehensive.

As well as describing the outbreaks on Thomas Street and in Albion Terrace, Snow built up his case by relating the circumstances elsewhere in London and in England. He rejected William Farr's view that the incidence of cholera was universally proportional to the altitude of the area in which a cholera victim lived.[69] Snow believed, rather, that "the reason is not one simply of level, or of the state of the air in connection with it, but ... it depends altogether on the water used by the people." He then documented the relation of water quality to cholera in different areas of England and Scotland, concluding in the second article that "the ... evidence of the communication of cholera through the drinking-water, confirms the view of the disease being propagated by the swallowing of the materies morbi in the cases resulting from personal intercourse ...."

Snow's second article concluded with a summary of preventive measures. One was to impress the need for "the most scrupulous attention to cleanliness ... on those waiting on the sick." Another was to soak soiled bedclothes in water as soon as they were removed, "as the evacuations might fly about as a fine dust whenever linen should be disturbed," which, Snow proposed, was one of the few means of spreading cholera through the air. Avoiding unclean food (another vehicle of infection) and water also was important. Water for drinking and cooking should be filtered and boiled.

Preventive measures were developed further in Snow's next two articles, published in 1851 and 1853. By now Snow had accumulated a great deal of information on cholera, which gave him authority when speaking about the disease. One of the forums in which he spoke was the Epidemiological Society. This Society was founded in 1850, as a consequence of the concern that a number of physicians felt about cholera.[70] Snow, one of its founding members,[71] was one of those who "first gave expression to the notion, that that the members of [the] profession ought to combine for the purpose of accumulating experience with respect to the treatment of that disease." [72] He also served as one of the Society's councillors,[73] and, in 1857 and 1858, as president – positions that reflected his standing as an epidemiologist.

In his article of 1851,[74] which was based on a paper he read before the Epidemiological Society, he reiterated his belief that cholera was propagated by "human intercourse." He also re-

peated his observation that, clinically, cholera "always commences with the affection of the alimentary canal," so that "it is pretty evident, that the material of the disease must have been applied to the part first affected." Not everyone recognized that cholera was primarily a gastrointestinal disorder, the "poison" being "contained in the evacuations from the stomach and bowels." This would explain the spread of cholera in a way that an hypothesis based on the existence of "effluvia or miasmata given off from the patient into the surrounding air" could never do. This latter, invalid hypothesis could not explain the spread of cholera "from want of cleanliness, deficiency of water, and deficiency of light," each of which "aided very much the propagation of cholera." Here the basic route by which the disease was transmitted comprised the "evacuations" of an infected person and the mouth of one to become infected – the so-called fecal-oral model. In his paper of 1851 Snow provided evidence that supported his belief in this means of communication of cholera. He cited one group of cases that could be traced to the consumption by several people of "stale cow-heels, which had been the property of a man who died... after a short and violent attack of cholera," and another case in a man who had handled furniture, including the bedding, belonging to a woman who had just died of cholera. Fecal-oral communication of this type was reminiscent of that which had occurred in the cases of cholera that he had seen in miners in 1832; as he had observed in Killingworth, "there are neither privies, hand-basins, nor towels in the mines; and when a case of cholera occurs in a pit, the hands of the workmen, in the dark subterranean passages, can hardly fail to become soiled with the discharges."

Snow is best known for the proof he provided for the transmission of cholera by means of cholera-contaminated water, but, as Snow emphasized in this article of 1851 even more strongly than in his article of 1849, cholera is not always or only water-borne. The importance of water as a vehicle is, of course, quite evident, and Snow must be credited with convincing the miasmatists of its importance; but Snow saw water as only one of several vehicles for the transmission of cholera. As Feachem has noted, "throughout his writing Snow maintained clearly a concept of faecal-oral cholera transmission, of which waterborne transmission is but one special case."[75] Although Snow is the

individual who is remembered for emphasizing the importance of clean water, he also stressed the importance of other aspects of hygiene. His importance as a historical figure can only be fully appreciated if these aspects of his hypothesis are remembered. Even today it is emphasized that the use of clean drinking water alone is not sufficient to afford complete protection against cholera in places like Bangladesh; the handling of food and hygiene among dense populations is also most important.[76]

In these two articles of 1851 and 1853, Snow provided much information about the nature and transmission of cholera. In them he went far to confirm his hypothesis that the disease spread as he had postulated in 1849 and to suggest that its spread could be prevented if quite simple measures were taken. Thus he wrote in his article of 1853,[77] "we cannot hope to prevent any disease unless we have a correct knowledge of its causes" – and in 1853 the causation of cholera was still not generally understood. "The true preventive measures," wrote Snow, "have not been applied" – a dig at public health authorities like the Board of Health, which, Snow claimed, directed its attention "more particularly to offensive effluvia, as a supposed cause of cholera." Clearly, Snow had to keep on making the same points about the causation and the prevention of cholera –though it would not be until two further epidemics had occurred before public health authorities would begin to be convinced.

Priority of Ideas on Cholera: Snow Versus Others

When he began to formulate his hypothesis in the latter part of 1848, Snow, like many other physicians, had had experience of cholera in 1831 and 1832. Likewise, in the epidemics of 1848 and 1849 he was confronted with much the same body of data as were other physicians and public health authorities. Yet it was only Snow who was able to perceive the truth as to how cholera was spread, both in communities and among individuals. It is therefore a question of both biographic and scientific significance as to why it was Snow, rather than any of his equally intelligent and competent contemporaries, who succeeded.

In considering the answer to this question, several aspects of Snow as a thinker, an individual and a physician must be taken into account.

Snow's ideas may be compared with those of four of other physicians – David Craigie in 1832, and William Budd, Edmund Parkes and William Farr in 1849.

Craigie, in 1832, had studied the outbreak of cholera in Newburn,[78] not too far from Killingworth, where Snow, as a very young man, was treating the miners who came down with the disease in this same epidemic. Although Craigie made careful observations, he could not, in the framework of a contagionist theory, reconcile the apparently incompatible findings of simultaneous sickness in some people who were physically close with its surprising absence in others who also lived in close proximity. So to Craigie a miasmatic agency seemed more plausible than a contagious one – while to Snow, in practice at the same time just a few miles away, the crowded and insanitary conditions down the mines impressed him just as strongly, though he would not make them part of his argument for another 15 years.[79] Snow, and not Craigie, *was* able to explain both the occurrence of cholera among persons who lived close to each other[80] and among those who lived far apart and its absence among those who lived near to each other and even with each other.

In 1849 it was William Budd, Edmund Parkes and William Farr who, like Snow, were prominent among investigators on cholera. Only Budd came close to Snow in formulating an influential hypothesis.[81] Budd justly achieved fame for his work on typhoid, but his ideas on the cause of cholera were flawed by his erroneous belief that the disease was caused by infection with fungi. He and his fellow Bristolians had read too much in what they had seen under the microscope when examining feculent matter. As well, Budd's conclusions lacked the firm epidemiological base that characterized Snow's monograph.

More useful to set against Snow's views are those of Edmund A. Parkes, a London physician. Parkes, like Craigie before him, was a close and careful student of cholera, and he too published his findings in a monograph (*An Inquiry into the Bearing of the Earliest Cases of Cholera, Which Occurred in London during the Present Epidemic, on the Strict Theory of Contagion*[82]) a few weeks before Snow published his. Parkes endeavored to elucidate the causation of cholera by reviewing the case details of a series of patients whom he saw from September 28 to October 9, 1848. A diligent and objective observer, he wished to determine

whether the cases of cholera he dealt with had arisen from contact or proximity of healthy with diseased persons. But, like Craigie earlier, Parkes concluded that the theory of strict contagion could not explain the occurrence between certain individuals – partly on the basis that contact or proximity had been impossible:

> *These cases are, then, to be accounted for on other grounds ... [and] it appears to me more probable that the cholera poison should have entered the localities first infected by virtue of some peculiar force acting irregularly and partially, so as to drive the poison into one rather than another locality; or that the poison should have been actually generated in those localities under the influence of a general atmospheric condition cooperating with the local conditions proper to each place, than that it should have reached those localities by direct emission from diseased individuals in the vicinity or at a distance, or from clothes which had been in contact with infected persons.*

Parkes, like Craigie, based his conclusions on negative evidence. Craigie had not been able to explain the observation that "the closest contact and the freest intercourse" between healthy and diseased persons was not necessarily followed by the onset of cholera in the former; Parkes could not explain the development of cholera in persons who had not been together. Their preference for an atmospheric means of spread contrasts with Snow's positive statement concerning cholera that "it is propagated by human intercourse" and that "its progress along the great channels of that intercourse ... seem to leave no room for doubting its communicability."[83]

Snow's view of the manner in which cholera spread in a community was thought to be "entirely novel"[84] – even though Snow was confronted by the same body of facts as were his colleagues. To further emphasize Snow's originality, it is useful to examine the beliefs of William Farr, whom Snow also knew well.

Farr, who was medically trained, was the Compiler of Abstracts in the office of the Registrar General.[85] Particularly interested in statistics (as was Snow), Farr lacked experience of

the actual clinical practice of medicine (which was one of Snow's great strengths). Farr's beliefs reflect some of the scientific opinions that were popular in the 1840s and 1850s – opinions that Snow was familiar with and even influenced by to some extent, though they did not prevent *him* from reaching the correct answer. Farr believed that diseases such as smallpox, typhus and plague, as well as cholera, resulted from the presence in the body of a characteristic *inanimate* substance that was specific for each communicable disease. Farr, like others, including Snow, was influenced by the emphasis that the German chemist, Justus Liebig, placed on the role of chemistry in body processes, including disease,[86] and he believed that the disease process, or "zymosis," resembled fermentation. The particular agent that caused cholera Farr termed "cholerine." Although Snow accepted many of Liebig's ideas, he more clearly enunciated the belief that the agent of cholera (like those of other communicable diseases) was not an inanimate one but a living one that reproduced itself.[87] Snow, as a clinician, was as impressed by the clinical and pathological features of cholera as by the statistical.

To Farr, like Parkes and Craigie before them, an atmospheric or miasmatic agency seemed likely as the explanation of the spread of cholera, and Farr's zymotic theory relied "heavily" on such an explanation.[88] Here again Snow, holding his own views, differed from Farr, as he did from Parkes and Craigie. Farr, at any rate early in his career, held that the zymotic substances and miasmata were non-living organic particles that spread through the atmosphere. In contrast, Snow was quite definite in stating that cholera was propagated by "human intercourse," and he had no doubt about its "communicability" by means of human agencies[89]– primarily by means of water, and secondarily by means of other media such as the hands, food and flies.[90]

Snow was a well-educated and well-read physician who was, unlike Farr, at home in clinical practice. He was, in particular, able to focus on the clinical and pathological features of cholera and then to apply epidemiological insight to an understanding of its natural history. Snow's clarity of perception and logical reasoning, combined with open-mindedness, also served him well. He never allowed himself to be confined, in his thinking, too rigidly within any framework of ideas, whether it was that of anticontagionists or of contagionists. This is what set him apart from Craigie, Parkes and Farr in terms of contagion versus

anticontagion. As for his difference from Budd, who shared with Snow a concept in an animate as opposed to a miasmic agency, it was Snow's open mind that enabled him to maintain credibility and intellectual distance from Budd by carefully avoiding "laying absolute stress" on the claim that fungi caused cholera.[91]

In this anticontagionist and prebacteriologic period, Snow's thinking was, therefore, original and certainly "novel." Snow was not only in advance of his contemporaries in what each of them concluded from similar data but also ahead of his time with respect to thought in the middle of the 19th century. Indeed, his concept of the disease, its causation and and its pathology have not been invalidated by more recent experience. Two excerpts from his writing on cholera testify to Snow's clarity of understanding of cholera in 1849:

> *...the disease is communicated by something that acts directly on the alimentary canal, the excretions of the sick at once suggest[ing] themselves as containing some material which, being accidentally swallowed, might attach themselves to the mucous membrane of the small intestine, and there multiply itself by the appropriation of surrounding matter, in virtue of molecular changes going on within it....*[92]
>
> *In those diseases in which there is reason to conclude that a morbid poison has entered the blood, there are symptoms of general illness...before any local affection manifests itself; but...it is not so in cholera. On the contrary, the disease begins with the affection of the bowels, which often proceeds with so little feeling of general illness, that the patient does not consider himself in danger....*[93]

In basing his hypothesis on clinical as well as epidemiological common sense, Snow had no need to resort to any other hypothesis. In contrast, Farr did, because his zymotic theory, being entrenched in the existence of a miasmatic agency, ultimately became invalid – as did his theory that the prevalence of cholera in a particular area was inversely proportional to its elevation above sea level.[94] Snow, an astute observer whose mind remained open, formulated an hypothesis that, because it was soundly based on facts and not speculation, required no modification but only corroboration, which came readily from his later epidemiological observations.

Another reason why Snow, rather than his colleagues, was able to discern the truth about the spread of cholera relates to the nature of Snow himself. The task of finding out how cholera was spread was a laborious one, and Snow's powers of perseverance and diligence, combined with his ability to work for hours on his own, meant that he was admirably suited to so tedious a task. Those traits that made him so persevering and perceptive an investigator are quite evident in his writings on cholera.

A good example of this is afforded by the details that are to be found in the "small print" of a footnote to Snow's description of the two initial cases of the London epidemic of September 1848. These give a clue as to how it was that his personal characteristics enabled Snow, rather than anyone else, to reach a valid conclusion about the nature and the means of the spread of cholera. Snow, ever particular about accuracy, noted that "some serious mistakes respecting these cases ... crept into the documents furnished to Dr. Parkes by the General Board of Health, as subject matter for his inquiry into the bearing of the earliest cases of cholera on the question of contagion."[95] Parkes understood that Harnold became ill on September 28, 1848, but this was incorrect. The more careful Snow, after he had talked to Dr. Russell while investigating another outbreak of cholera in London a few months later, was able to correct Parkes in this respect. Snow seized on this point because it seemed to him that "the mistake in the date alone of which the first case occurred, alters the bearing of all the facts submitted to Dr. Parkes," particularly in relation to the cases that developed after the initial two. What we learn from this footnote is the extent to which Snow would go in order to get at the facts. Parkes, an excellent physician and as interested in cholera as he was, appears to have been satisfied to accept information from the rather impersonal body of the General Board of Health. In contrast, Snow took the time and trouble to unearth facts by looking into the circumstances for himself.

Snow's attributes and his experience as a physician are keys to the answer to the question as to why it was Snow who could cut through the mass of anecdote and prejudice related to cholera to arrive at the truth. In 1848, when he had just reached the prime of his career at the age of 35, Snow was as well read and trained as any of his contemporaries (see Chapter 3). Attending, and

often participating as a speaker in, the weekly meetings of the Westminster Medical Society (later the Medical Society of London) also enabled him to be informed about medical progress. By the mid-1840s, therefore, Snow was an experienced and astute and perceptive clinician who was prepared to confront any medical problem. As far as cholera was concerned, his experience of the disease in Killingworth in 1832 meant that he knew more than most of his colleagues did about the disease, and by familiarizing himself with the medical literature he kept himself informed about cholera, in both its past history and, currently, its present behaviour. Then, in 1848, the return of cholera to England rekindled in this physician-scientist a fresh interest and curiosity in the disease.

Snow had one most potent advantage over his contemporaries – and this he brought to bear on the vexing problem of cholera, as he did in tackling the problems of anesthesia. His personal and his professional attributes were melded admirably. As his work on cholera (and on anesthesia) showed, he had the facility for making astute and painstaking observations, for formulating an appropriate hypothesis, and then conducting a carefully designed and exhaustive investigation in order to investigate the problem at hand. Snow, indeed, was exceptionally able as an investigator, in addition to being a knowledgeable and experienced practitioner, and in this respect he was in a class by himself.

Altering the Ground of Debate: Snow on Communicable Diseases

From 1849 to 1853, convinced that his ideas were sound, Snow endeavored to build up a strong and detailed point-by-point case that would give convincing evidence in support of the largely original concept of cholera that he had delineated in his monograph of 1849. Thus armed, he would be enabled, with speed and purpose, and when the time was right, to carry out the necessary *experimentum crucis* that would finally convince his sceptical colleagues.

Although it was not until 1854 that Snow would be able in that way to begin to convince the medical profession and the public of the validity of his ideas, he was soon given the opportunity to

think at length and in depth about cholera and infectious diseases in general. This occurred in 1852, when he was elected the Medical Society of London's Orator for 1853. He delivered his oration on March 26, 1853. Then vice-president of the Society, he used the forum of this oration to amplify his ideas not only on cholera but on communicable diseases in general. He spent the better part of a year in preparing for his presentation. His oration, which was published later under the title *On Continuous Molecular Changes, More Particularly in Their Relation to Epidemic Diseases,* [96] therefore reflects his carefully considered views. The scope of his lecture was broad and its approach was shaped very much by his understanding of the biology and the chemistry of this scientifically fertile era. His stated intention was "to make a few remarks on some of the chief phenomena of living beings," though his real objective appears to have been to contribute to the study of "the prevention of epidemic or communicable diseases."

The prevention of communicable diseases, Snow emphasized, deserved "increased investigation." The concepts that physicians held of the nature and communication of these diseases, and therefore their treatment, varied as widely as ever. Discussions focused chiefly on the questions of whether diseases like cholera were communicated from person to person, and how. Although the debate at this time was conducted chiefly between those who held a contagionist view and those who held an anticontagionist view, there were shades of opinion on each side. Even among the contagionists, for example, there were differences about the specificity of the means of communication, the operation of predisposing factors and the strictness with which "contagion" was defined. Snow therefore focused attention on the need to look at the communication of a disease like cholera from a fresh viewpoint:

> *The chief arguments against the communication of disease have been shaped according to the belief that, if communicated at all, it must be by contact with the patient, or by effluvia from his person, clothing, or bedding. The communication of disease by accidentally swallowing morbid excretions to which attention has more particularly called of late considerably alters the ground of debate ... .*

In 1853 two facts made it difficult to believe firmly in the contagionist concept of cholera. First, contact between the sick and the sick-to-be was not always obvious: and, second, the existence of the causative agent had not yet been demonstrated. Snow understood this. Perhaps as a result, in his Oration Snow expressed himself much less forthrightly than he did in his other presentations. He was, however, speaking on a formal occasion and as a future President of an august medical audience, half of whom rigidly believed in a non-contagionist view of communicable diseases. The noncontagionist viewpoint was still held strongly, and Snow had yet to perform that crucial experiment that would finally prove his case. Although Snow was reasonably convinced in his own mind of the validity of his hypothesis, the occasion of the Oration did not call for a polemic, which was, in any case, not Snow's natural approach. On this occasion he seems, at first sight, to have been prepared to hedge his bets. Thus, in addressing the question as to whether communicable disease might arise spontaneously – a belief that some non-contagionists still held in this pre-Pasteurian era – Snow spoke, with uncharacteristic circumlocution, as follows:

> *It may very fairly be asked whether communicable diseases do not sometimes arise spontaneously – that is, from other causes than their communication, just as ordinary combustion, putrefaction, and some other continuous molecular changes, very often commence anew, from various causes, without any continuity with previous changes of the same kind, and it is not unthinkable that some communicable diseases may arise, so to say, spontaneously. The erysipelatous inflammation, for instance, which attacks the neighbourhood of wounds, probably arises now and then without being communicated; otherwise we must suppose the material which causes it to be almost widely diffused as the spores of some of the fungi.*

Snow may perhaps have been simply avoiding too extreme a stand, though he did go on to say that "there is, however, great reason to believe that the larger number of communicable diseases never arise from any other cause than the communication of the specific virus from a previous patient." This in fact Snow firmly believed, for he also emphasized, in quite striking terms, that "to be of the human species, and to receive the morbid poison in a suitable manner, is most likely all that is required."

Snow held an exclusive viewpoint: a communicable disease resulted only from the introduction into the body of a specific causative agent. Thus he stated categorically that "when the morbid matter of any disease is received into the system, in the way required in that particular disease, it is almost certain to produce its specific effects, except in the instances in which the patient has gained an immunity by a former attack." Predisposing factors, which some of his colleagues, such as Parkes, accepted in taking a stand midway between the contagionist and anticontagionist camps, he rejected, again in picturesque metaphorical language:

> *For want of knowing any/other cause, epidemics were attributed, by the ancients, to the atmosphere, without any evidence; just as political and social events were believed to be occasioned by the stars. Now as people are not only exposed to the atmosphere, as soldiers in battle are to bullets, but are actually immersed in it, as fishes are in the sea, it became necessary to explain why certain persons were attacked and others not attacked, and the word predisposition was used as affording an explanation. The alleged predisposition, however, was nothing visible or evident: like the elephant, which supports the world according to Hindoo mythology, it was merely invented to remove a difficulty.*

Snow's analogy of fishes immersed in the sea perhaps implies an ecological viewpoint, a connection between various forms of life, and one that was compatible with the idea of the *milieu interieur* of his great contemporary in France, Claude Bernard.[97] Certainly, he was interested in the idea of continuity of life from one generation to another. Evidently influenced by the ideas of Liebig, two of whose books Snow referred to,[98] Snow suggested that continuous molecular changes preserve the individual and the species. He also seems to have anticipated Darwinian ideas in suggesting that "procreation by sexes ... appears to have the effect of preventing deviations from the form and character of the species."

Snow was particularly interested in both the characteristics of "vital actions" and what was responsible for their continuity in time and space. It was in this sense that he used the word

molecular, seeking "a general term to include what is understood by the words physical, chemical, and vital, in order to avoid the disputes respecting these two latter words ... and the needless antagonism in which these words are sometimes placed towards each other." Snow held that one of the "most striking" phenomena in living beings at the molecular level is that "the changes or actions to which it gives rise are often a cause of their continuance and evidence." He added that "the most characteristic property, indeed, of vital actions probably is, that they are always caused by similar processes which have preceded them," and that "a species of plant or animal consists ... of a number or collection of continuous molecular actions." This continuity accounted for "the spinning of the thread of life." Equally, a disturbance of this continuity of molecular action might result in disease, or, as in "the deficiency of warmth, of moisture or of oxygen ... [in] a pause in the spinning of the thread of life without its being cut or broken."

Snow's interest in chemistry kept him aware of the advances in chemistry in the middle years of the 19th century and of ideas based on these advances. A fundamental advance was the synthesis by Wohler in 1828 of urea, a basic biochemical constituent of the body. Current ideas in chemistry were invoked, for example, by Liebig, who attempted to provide an understanding of health and disease in chemical terms. Liebig, who was well known in England, which he visited on three occasions in the 1840s, believed that many chemical compounds in inorganic nature and in animal and vegetable life produced peculiar changes or diseases in living beings. In the specific contagious diseases the increase in the morbid material amounted to a state of putrefaction, which was chemically capable of inducing change in the constituents of the blood and the body, and Liebig's principle of "contagionist molecular action" must have seemed to many in England, including Snow, a valuable idea.[99] According to Liebig, "every substance or matter, every chemical or mechanical agency, which changes or disturbs the restoration of equilibrium between the manifestations of the causes of waste and supply, in such a way as to add its actions to the causes of waste, is called a *cause of disease*." [100] As a chemist, however, Liebig's perspective was that of chemical processes, while as a physician, Snow's was that of pathological processes.

In formulating his ideas on communicable diseases, Snow seems to have taken hold of Liebig's general principle of the continuity, in chemical terms, of organic matter. This is not surprising. This was an era in which chemistry reigned supreme; its prospects seemed unlimited. Belief in the supremacy of chemistry influenced the thinking of numerous people on many aspects of life. The chemist's ability to synthesize urea and the apparent chemical basis for changes in disease were, therefore, factors that Snow was bound to take into account. As a result, the challenge for him was to incorporate the Liebigian viewpoint with his own.

The Oration of 1853 gave Snow the opportunity, therefore, to introduce his somewhat conservative audience to several new ideas concerning the understanding of communicable diseases. Aware that "the communication of diseases was not generally recognized till a recent period," Snow relished this opportunity. In this respect, the following passage is a key statement:

> *In addition to the series of continuous molecular changes having for their result the preservation of the individual and the species, there are others, occurring in living beings, which have the opposite tendency; they divert part of the substance of the individual from the actions which are natural to the species to another kind of action, in consequence of which this substance is employed in the multiplication and increases of the materies morbi of communicable diseases – an extensive group of maladies, each case of which is caused by some material that as a general rule, has been produced in the system of another individual.*

To many of Snow's contemporaries this must have seemed an extreme contagionist view – even a fantastic notion – but Snow must have intrigued some of his listeners by adding that "the material cause of every communicable disease resembles a species of living being in this, that both one and the other depend on, and in fact consist of, a series of continuous molecular changes, occurring in suitable materials." The symptoms of communicable disease, Snow continued, were induced by the presence of "organized matter," which "possesses one great characteristic of plants and animals – that of increasing and multiplying its own kind." Snow had said that he would make

"a few remarks on some of the chief phenomena of living beings"; few in his audience would have expected him to include as yet unseen agents of a disease like cholera as living beings.

Snow, as he evidenced in his writings on anesthesia as well as those on cholera, had a strong sense of history, and he noted that even to so great a physician as Thomas Sydenham the communicability of acute diseases was unknown, except for plague.[101] Sydenham, influenced by the ancient belief in the humoral theory of disease, believed that each specific disease arose from some specific "exaltation," or an individual quality of some humour existing in the body. For his part, Snow in his Oration endeavored to bring his audience up to date, steering them away from ideas such as Sydenham's and other non-contagionist ideas, like those held by physicians who believed in the atmospheric origin of diseases. Part of his purpose was to convince his listeners of the validity of his contagionist viewpoint, which was an advance on these old ideas. Aware that many physicians still believed in the atmospheric origin of epidemic diseases, he pointed out that "as the composition and physical properties of the air began to be better understood, it became evident that the atmospheric hypothesis of epidemics did not explain their phenomena, even with the assumption of a predisposition existing in some persons and not in others." The belief in an atmospheric origin of epidemic diseases was illogical: "it is not possible, for instance, that a disease caused by anything in the general atmosphere should progress in opposition to the wind, or should remain for weeks in a place before extending to the next parish on either side." Snow did admit, however, that "it is quite possible, and, indeed, almost certain, that the material cause of some communicable disease may be wafted a short distance through the air, like the seeds and spores of many plants."[102] Here he was thinking of the dried matter of smallpox postules, but he said more than once that the same might be true of cholera.

Of the communicable diseases, cholera was the one that was of the greatest interest to Snow, and his Oration on continuous molecular changes related to the epidemic diseases can be appreciated today in connection with his interest in cholera. But it can be appreciated even more for the evidence it provides of Snow's interest in topics of much broader interest – the other epidemic diseases, certainly, but also the current ideas in the medicine and science of his day.

The Oration of 1853 was a landmark in Snow's thinking on communicable diseases. Preparing it gave him the oppportunity to pull together the threads of the hypothesis he had proposed in 1849 and to firmly place in it the concept that the introduction of animate, "morbid" matter into the body produces specific effects characteristic of a particular disease. The contagionist viewpoint seemed to Snow a logical one, and all he read about cholera and all he himself saw of the disease served to confirm this. Studying for, and then writing, the Oration served him well with respect to his work on cholera, for he thereby became all the better prepared intellectually to conduct the *experimentum crucis* that had been recommended to him in 1849; his intellectual muscles were flexed so that, when the time came – as it would in 1854 – he need waste no time in further deliberation. He would simply be able to get on with the task of conducting the two-part epidemiological investigation into the transmission of cholera that would eventually bring him great fame.

The Oration was also a landmark in Snow's career. Apart from confirming him as a "deep thinker,"[103] it marked Snow's rise to the upper ranks of the medical hierarchy in London. He was acknowledged now as one of the leaders of his profession. Soon to be elected Vice-President, and then, in 1855, President, of the Medical Society of London, he had come a long way from his admission to the Westminster Medical Society in 1838, when, reticent and shy and speaking in his husky voice, he was reported to have "made some observations in a very low tone, and consequently his meaning could not be very well caught."[104] In those days, although he always spoke to the point, he never received favorable notice and nobody ever replied to what he said.[105] By 1853, however, he had earned credibility among his colleagues with respect to both the organizational sphere – the Medical Society of London – and the academic and intellectual – especially the causation and spread of cholera and the administration of anesthesia. (Two weeks after he delivered the Medical Society's Oration he was summoned to Buckingham Palace to anesthetize the Queen.) His success in organizational medicine was probably a surprise to him, but his success in the academic and intellectual world must have quietly given him intense satisfaction. Snow ended his Oration by reiterating the importance of investigating the mode of propagation and means of

prevention of the epidemic diseases. He added that if any inducement were necessary to stimulate his audience to think along these lines, it would only be necessary to the work of a former Fellow of the Medical Society of London. Snow was referring to Edward Jenner, who, in his work on vaccination against smallpox, had made "the greatest discovery that has ever been made in the practice of medicine, and to render the greatest benefit to his species which they have probably ever received." It is unlikely that Snow saw himself as another Jenner, though he was undoubtedly inspired by his work; nevertheless, as he delivered his Oration, Snow was poised to make his own great discovery and to render to his species a benefit that would one day be counted, like Jenner's, as one of the greatest.