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

Looked at more closely, it becomes increasingly clear that Snow, the epidemiologist, was in a very different position from Snow, the anesthetist. His early training had equipped him remarkably well for the opportunities presented by ether and chloroform, whereas his theory of the transmission of cholera, arrived at almost intuitively, caught him unprepared and showed up his weaknesses.

It is clear also that the inclinations of many of his modern admirers have prevented them from accepting the fact that the theory might be a very good theory, yet the book which contained it might still be a very bad book.

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

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

"In the summer of 1837, I injected another body and dissected it, with five of my fellow students, during the very hot weather of. I think, August. Decomposition was retarded considerably, but there was only one of us who did not suffer more or less indisposition, principally bowels complaints; and the subject gave out a peculiar odour, which I suspected arose from the arsenic rising in combination with the volatile products of decomposition."
Five or six weeks later he examined parts of the body which had been saturated in the solution and found no trace of arsenic. He assumed that it had all passed into the atmosphere. He continued:

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

THE MEDICAL SCIENTIST

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

There were two dominant themes — toxicology and respiratory physiology.

The first came, it seems, from his interest in temperature. In the spring of 1838, when he was still a very new member, the Society discussed the dangers of the sudden withdrawal of alcohol in those who were accustomed to it. Snow, on evidence from a penitentiary in Georgia and from temperance societies in Leeds and York, showed that drunkards became healthier, if anything, when they left off drinking.

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

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

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

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

It was at this time, in 1842, that Snow published his well-known paper, "On Asphyxia, and on the Resuscitation of Still-born Children.12" — his "first attempt at authorship," as Richardson tells us.8
This paper was first presented to the Westminster Medical Society. He proposed that the traditional treatment of asphyxia by heat should be replaced by artificial respiration. He observed that Magnus, Edwards, and others had shown that external heat aggravated the condition by increasing the utilization of oxygen. He then demonstrated to the Society the operation of the resuscitation machine he had constructed.11

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

Another feature of these discussions is his lack of respect for great men when he felt they were being foolish. Orfila and Christison, he said, had overlooked the possibility of oxygen deficiency in deaths from carbonic acid poisoning. Adson was wrong about the cause of edema in scarlet fever. Even the mighty Liebig was not safe from attack. "Mr. Adson," he said, "had constructed a paper of the Royal Medical and Chirurgical Society for April 11, 1845," said that the recent work by Liebig contained many errors, and was by no means calculated to sustain the reputation its author had previously earned.12

On this occasion, and on many others, he was justified in his criticisms. Liebig claimed that cold air, because it was denser than warm air, enabled the body to generate more heat on account of its greater oxygen content. All air, said Snow, was warmed to the same temperature before it reached the lungs. But he was not always right. His early investigation into arsenic and dead bodies is by no means a model of experimental design. On another occasion he asserted that transudation from an inflamed part increased its heat and that if transudation were prevented by a covering of oiled skin the part kept cooler.13

He was particularly weak on the circulation. On January 21, 1845 he read a paper entitled "Circulation in the Capillary Blood-vessels." He said that the action of the heart was of itself, insufficient to effect the circulation of the blood. This was most evident from the phenomenon of asphyxia, in which the capillary blood flow ceased before the heart stopped beating. There must, he thought, be some other power of importance engaged in the circulation, an attraction and repulsion between the blood and tissues to which allusion had been made by Dr. Alison and some other authors. In the discussion, Mr. H. J. Johnson said he was able to comprehend neither Snow nor Alison, and Dr. Reid could not conceive a cause of motion without some mechanical impulse. For once, apparently, the ususal roles were reversed.14

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

**SNOW AND HIS CRITICS**

When London, in 1848, was under the threat of a new epidemic of cholera, the Westminster Medical Society devoted several meetings to the subject. Snow's thoughts, however, were still on ether and chloroform and he took no part in the preliminary discussions, other than to remark on the similarity between cholera and other asphyxias.15

He began to tackle the problem seriously towards the end of 1848, and it is evident that he once again adopted the approach of a toxicologist. Did the poison act locally and, if so, on what organ? Or did it first enter the blood stream? He concluded that it acted locally on the alimentary mucosa because, in his experience, the initial symptoms were always referred to the abdomen. And because
the effects of the poison could be counteracted in the early stages by agents such as chalk and opium, which were known to act locally, he concluded that it did not enter the alimentary canal by passing into the blood stream, but must be discharged in the vomit and feces. The next step was obvious. To reach other victims, all it had to do was enter the sewage system and pass thence to the drinking water—a circulatory process which had been well recognized by fasting Londoners for a number of years.51

The new theory was virtually complete. He now had to work out some method of proving it. The most direct way was to prove the poison itself in patients and drinking water. As Budd had said, "the detection of the actual cause of the disease, and the determination of its nature were all that was wanting to convert his views into a real discovery." But here he was in difficulty. Too close an association with Budd, Swaye, and Britton, who had independently suggested a water-borne fungus infection, would deprive him of the sole credit of being the first to publish the theory. And since, in any case, the idea of a fungus was so absurdly novel, the he felt it unwise to look any further in that direction.4

It would have been impracticable, too, to follow up his pathologic arguments. He must have seized their inherent weakness and gave three different accounts of how he made his discovery.1, 2, 31 Besides, he was almost wholly ignorant of the contemporary pathologic literature relating to cholera. The idea that it was a local affection of the intestine, which he first insinuated was entirely his own, was an old-standing theme of his fellow anatomist, Proctor Smith, and had long been discounted because of the numerous cases of persons who had died without showing any symptoms. Attempts had also been made to produce the disease in men and animals through the ingestion of cholera discharges.22, 27 All these had been unsuccessful.

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

Even here circumstances were against him. He had no previous training for the work and soon found that it was impossible to trace the route of infection from case to case while an epidemic was raging in a crowded city. (William Budd, independently of Snow, by studying the connection between small groups of cases in a sparsely populated area, had relatively little difficulty in coming to the conclusion that cholera was spread by drinking water.) Snow, therefore, had to limit the scope of his inquiry to demonstrating that cholera spread most extensively in those areas where there were the greatest facilities for swallowing excreta.4 Such an investigation, however clearly it established the relationship between polluted water and cholera, was not directed at the main point of the controversy; it could never decide between his own theory, that water was the vehicle of transmission, and that of his opponents, that polluted water predisposed to infection from another source.

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

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

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

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

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

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

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

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

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

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

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

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

A year later Snow repaired his obse-
vation by publishing details for every sub-
district and amply confirmed his original
statement. This step, as Bradford Hill
has pointed out, was fundamental to the
argument. It is not a little unfair to Snow's contemporaries to forget that it
was Parkes, the critic, and not Snow
who first called attention to its impor-
tance?

Parkes, although excelling in his criti-
cism, seems to have shown a remarkable understanding of the problems of an
author who was clearly less familiar with epidemiologic methods than he was him-
self. In conclusion he wrote:

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

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

Why, then, has the fate of the book aroused so much resentment in our own
generation? Partly because the book it-
self has been overvalued. It has given
many modern readers their sole contact
with the period in which it was written.
They are not likely to be worried with
page upon page of quotation from Simp-
son's "Asiatic Cholera" in support of the
already accepted principle of contagion.25
They are able to fill in the gaps in his
reasoning with the comforting knowl-
edge that Snow was, after all, right.

In part, also, we have been misled by the
powerful imagination of Benjamin
Warid Richardson. Richardson had two
missions in life. One was to justify his
own eccentric opinions. The other was
to create romantic images out of rela-
tively unromantic medical prototypes.
Snow served both purposes. He could be
presented to the public as an unrecog-
nized genius; a genius, in fact, who was
so peculiar that only Richardson had the
discreetness to realize his worth. "It was
my privilege," he wrote, "during the life
of Dr. Snow to stand on his idle. It is
now my duty as a biographer... to
record his... the entire originality of the
discovery of a connection between im-
proper water supply and choleric dis-
ease."26

On the other hand, he could become
the hero of any number of Richardsonian
characters. He was the victim of "the
fortunate" who was eager for the few
scraps of other practice which could be
spared by the bustling druggist. He was
the Victorian Fled Piper who confronted
the vestry of St. James with his book
for ridding them of the plague which
menaced their door. These most un-
Snow-like figures do nothing but confuse
the serious historian.

Perhaps the most significant factor,
however, is the importance that Snow
has for many epidemiologists. His work
on cholera has been cited as one of the
crucial contributions of the epidemiolog-
ic method to medical knowledge.27 It has
been repeatedly claimed that the dem-
stration that typhoid fever and cholera
could be water-borne
rested on the establishment of significant associations. For example, “It was this significant association of cholera incidence with water supply that led, before the organism of cholera had been discovered, to his thesis that cholera was water-borne.” Or, “This control (of cholera) was suggested by ensuring that people did not drink water that had been contaminated by sewage, and this action was suggested by the painstaking collection of social data which showed a frequent association of contaminated water supplies with epidemics. In short, social action based on social observation proved to be as effective means of prevention as the cause of the illness were understood in technical detail.”

It may be remembered that in Snow’s case the social data were collected between 1849 and 1855, the theory was formulated in 1854, while the Lambeth Water Company placed its proposals for obtaining a purer source of supply before Parliament in 1847. These dates are not unimportant.

One last quotation deserves notice. Bradford Hill, the medical statistician, has written, “For close upon 100 years we have been free in this country from epidemic cholera, and it is a freedom which, basically, we owe to the logical thinking, acute observations and simple sagacity of Dr. John Snow.” In fact, by forestalling William Budd, Snow advanced our understanding of the spread of cholera by exactly 29 days. To insist on historical accuracy in these matters is not to discredit John Snow. Snow postulated an undoubted fact, as Macaulay might have said, “a real merit, and a merit of a very rare, though not of a very high kind.” And if we replace the man named by some elegant cipher, however well-meaning our intentions, we are denying ourselves acquaintance with one of the most fascinating human characters in medical history.

REFERENCES
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TRAGEDY’S COINCIDENCES

Going the rounds is a compilation of astounding coincidences between two assassinated Presidents of the United States—Abraham Lincoln and John F. Kennedy.

Both President Lincoln and President Kennedy were concerned with the issue of Civil Rights.

Lincoln was shot in 1865 and Kennedy in 1963.

Both Presidents were slain on a Friday and in the presence of their wives.

Their assassins, both named Johnson, were Southerners, Democrats, and had previously served in the U.S. Senate.

Andrew Johnson was born in 1808 and Lyndon B. Johnson in 1908.

John Wilkes Booth and Lee Harvey Oswald were both murdered before trials could be arranged.

Booth and Oswald were Southerners favoring unpopular ideas.

Both Presidents’ wives lost children through death while residing in the White House.

Lincoln’s secretary, whose name was Kennedy, advised him not to go to the theater.

President Kennedy’s secretary, whose name was Lincoln, advised him not to go to Dallas.

President Kennedy was shot to death in a Lincoln made by the Ford Co.

Lincoln was killed in Ford’s Theater.

John Wilkes Booth shot Lincoln in a theater box and afterward ran to a warehouse.

Oswald shot Kennedy from a warehouse and ran to a theater.