

JOHN SNOW—THE AUTUMN LOITERER

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

Browning.

John Snow's book on the mode of communication of cholera has attracted a good deal of attention in recent years and has come to be regarded as the prototype of much modern epidemiological research. One aspect of the work which has so far escaped notice and is not unrelated to this question is his attitude to the microbic and fungus theories.

"The writer," he says, "does not wish to be misunderstood as making this comparison (with intestinal worms) so closely as to imply that cholera depends on veritable animals or even animalcules, but rather to appeal to that general tendency to the continuity of molecular changes, by which combustion, putrefaction, fermentation and the various processes in organised beings are kept up."¹

This neutral outlook enabled him to escape the fate which overtook his rivals, Budd, Swayne, and Brittan, when their fungus theory was discredited. It indicates also the frame of mind in which he approached his problem. So when we examine Snow's connection with the fungus theory we can begin to understand the processes of his discovery and the direction taken by his later research.

Snow and the Fungus Theory.

John Snow's theory that cholera was communicated through drinking water was first published on August 29th, 1849. His idea was that a contagious principle attacked the intestinal mucosa without passing into the blood-stream. Such an agent could only enter the body by being swallowed and leave it in the vomit or stools—the chain of infection being completed by the faecal contamination of food and water. Some form of poison which could "multiply itself by a kind of growth, changing surrounding materials to its own nature"² was clearly essential to this theory, since the disease was able to spread without limit from one or

¹J. Snow: *On the Mode of Communication of Cholera*. London: Churchill, 1849, p. 9.

²J. Snow: "On the pathology and mode of communication of cholera." *London M. Gas.*, n. s. 9: 745.

two affected individuals. As it happened, a fungus which appeared to have just these properties was already under investigation at the time when Snow put forward his views.

In the second week of July, 1849, a few weeks before Snow's publication, Brittan and Swayne, at a meeting of the Bristol Medico-Chirurgical Society, demonstrated "peculiar microscopic objects in the characteristic rice-water discharges of persons affected with malignant cholera."³ These objects were believed to be living organisms of the nature of yeasts. Shortly afterwards William Budd detected the same objects in the drinking-water of cholera districts in Bristol. His conclusions, published on September 27th, were:

1. That the cause of malignant cholera is a living organism of distinct species.
2. That this organism . . . is taken by the act of swallowing into the intestinal canal, and there becomes infinitely multiplied by the self-propagation which is characteristic of living beings.
3. That the presence and propagation of these organisms in the intestinal canal, and the action they there exert, are the cause of the peculiar flux which is characteristic of malignant cholera; and which, taken with its consequences, immediate and remote, constitute the disease.
4. That these organisms are developed only in the human intestine.
5. That these organisms are disseminated through society (1) in the air, in the form of impalpable particles; (2) in contact with articles of food; and (3) and principally, in the drinking water of infected places.⁴

Snow reacted at once to this announcement. On October 4th, in a lecture to the Western Literary Institution, he "made allusion to the recent discoveries of Messrs. Swayne, Brittan, and Budd, without, however, laying absolute stress on the circumstance that the fungi recently discovered were really the *fons et origo mali*."⁵ There was obviously a great deal in common between the two theories, and on October 13th Snow and Swayne jointly addressed the Westminster Medical Society. Snow was again guarded in his remarks, and though he thought that the recent discovery of peculiar microscopic cells tended to confirm his view of the nature of cholera he was not sure that they were the real cause.⁶

This was as far as he ever went in accepting the fungus theory, and he took no further part in its subsequent development and collapse. The situation on November 2nd was described by the *London Medical Gazette*:⁷

³ W. Budd: *Malignant Cholera, Its Mode of Propagation and Its Prevention*. London: Churchill, 1849, p. 5.

⁴ *Ibid.*

⁵ *Lancet*, 1849, 2: 413.

⁶ *Ibid.*, pp. 431-433.

⁷ *London M. Gaz.*, 1849, n. s. 9: 759.

Minute fungi or organisms as they are called, have been discovered by the aid of powerful microscopes in the excretions etc. of cholera patients, and in their intestines after death. The excretions and bodies of persons dead of other malignant diseases, after careful and minute enquiry, have, it is said, failed to disclose similar organisms or any traces of them. The original discoverers of the fungi have stopped here: they contented themselves with merely recording what they observed; but the subject was instantly taken up in the public journals and a theory founded upon it, which has gone the round of the daily and Sunday newspapers. It ran thus:—The fungi or organisms are peculiar to cholera; *ergo* they are the cause of the disease; *ergo* their immediate destruction is a sure preventive against the spread of the malady, and that object is effected in the easiest manner possible, by simply throwing the excretions in which they are found, when expelled from the body, into a chemical solution!

This mode of dealing with medical researches, i. e. of adopting hasty conclusions and giving them spurious newspaper currency is derogatory to the true character of science.

The end of the theory was already on its way. On October 29th James Paget wrote in a letter "I believe, however, the whole hypothesis will be shortly exploded; for it appears certain that many of the things seen are not fungi, but remnants of food taken."⁸ This, indeed, was the substance of the report published in early November by Drs. Baly and Gull, for the Sub-committee of the College of Physicians on the Cholera Fungi. They concluded: "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."⁹

Snow had only narrowly escaped involvement in a failure which might well have put an end to his research. And yet his reasons for refusing to accept the theory are not apparent at first sight. If he sensed its defects he never called attention to them. And he rejected its sound parts also, since he at no time recommended the chemical treatment of cholera evacuations, although his own hypothesis seemed to demand it. The explanation for his attitude must be looked for outside the realm of his scientific judgment.

The Claim to Priority

Snow's earliest views on the nature of contagion were derived from Liebig, whose influence he acknowledged on a number of occasions. The concept of molecular changes had the stamp of this great scientific authority, and it was natural that Snow should prefer it to vague and

⁸ S. Paget: *Memoirs and Letters of Sir James Paget*. 3rd ed. London: Longmans Green and Co., 1903, p. 169.

⁹ *Lancet*, 1849, 2: 493.

popular notions on animalcules and fungi. The choice to begin with was of no particular significance. But after Budd's announcement, Snow was faced with a more serious conflict over the renunciation of the fungus theory. The problem declares itself in his remarks to the Western Literary Institution and the Westminster Medical Society. He had to have the publicity which the new theory gave to his views, but he was not prepared to forego his sole right to the discovery, which he was in danger of losing by throwing in his lot with Budd and his colleagues. The weakness of his position is brought out by this footnote in Budd's paper: ¹⁰

Dr. Snow, whose ingenious pamphlet on cholera fell into my hands while these materials were preparing for publication, has been led, by the consideration of particular instances of some of the facts above alluded to, to the same conclusion as to the part which water plays in the diffusion of the disease. Of being the first to develop and to publish this very important conclusion he must, therefore, have the whole merit. To no part of this merit do I lay the slightest claim. In Dr. Snow's illustration of the entire subject of the propagation and prevention of cholera there is, besides, much that is so apt and in such entire accordance with the truth, that the detection of the actual cause of the disease, and the determination of its nature were all that was wanting to convert his views into a real discovery.

The artfulness of this manoeuvre is quite apparent. Budd conceded the claim to priority of publication without admitting that his conclusions owed anything to Snow. He implied that Snow was welcome to the merit of being the first to make the discovery, if he thought it mattered, but for his part he would rather be judged on the intrinsic superiority of his own theory. Snow's response was not to defend his discovery, by explaining the processes which had led him to make it, but to take further steps to establish its priority. On October 13th he told the Westminster Medical Society that "since he (Dr. Snow) first published his views on this point, Dr. William Budd had found the microscopic bodies before alluded to in such drinking-water of cholera districts as received the contents of sewers."¹¹ In November, 1849, when giving his reasons for believing that cholera was a local affection of the alimentary canal, he added "an opinion which I thought almost peculiar to myself when I was first led to adopt it, but which, as I have since been informed, others were beginning to entertain."¹²

These remarks, and a number of other instances in which Snow revealed

¹⁰ Budd, *op. cit.*, p. 19.

¹¹ *Lancet*, 1849, 2: 432.

¹² Snow: "On the pathology and mode of communication of cholera," *loc. cit.*, p. 745.

his preoccupation with the whole question of prior publication,¹³ make it reasonable to assume that the claim to priority was one of the chief factors in his rejection of the fungus theory and the ideas connected with it. Bound up with this assumption there are two new problems. The first is to discover why he preferred not to defend his theory on rational grounds, and the second is to study the effect on his later research of a decision to ignore "the detection of the actual cause of the disease, and the determination of its nature."

Pathological Considerations

Quite clearly the theory of cholera which Snow was called upon to defend was, in the first place, a pathological theory, since he said himself "it was from considerations of its pathology that the mode of communication was first explained."¹⁴ What at once awakens our curiosity is the obscurity in which he envelops its origins and the shift of ground from one account of it to another. In his first pamphlet he wrote:

It is generally assumed that the blood becomes so altered by the choleric 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 poison in the blood causes irritation of and exudation from a single surface as in cholera.¹⁵

This is the analogy between cholera and the irritant poisons which occurs in Christison's *Treatise on Poisons*,¹⁶ a well-known work, and which does not seem to lead by itself to the conclusion that cholera is water-borne, since irritant poisons are rare in drinking water. Snow gave a more useful clue in his paper of November 1849,¹⁷ when he said that it was because cholera began with a premonitory diarrhoea and because the general symptoms could all be explained by loss of fluid into the bowels, that he came to believe that cholera was a local disease. The essential part of the argument came from the observation that the disease could be cured in the early stages by the ordinary remedies for diarrhoea:

"Now this circumstance," he said, "is a strong reason for concluding

¹³ See remarks on Dupuytren, *Lancet*, 1847, 1: 461.

¹⁴ Snow: *On the Mode of Communication of Cholera*. 2nd ed., London: Churchill, 1855, p. 16.

¹⁵ Snow: *On the Mode of Communication of Cholera*, 1849, p. 8.

¹⁶ R. Christison: *Treatise on Poisons*. 3rd ed., Edinburgh: Black, 1835, p. 112.

¹⁷ Snow: "On the pathology and mode of communication of cholera," *loc. cit.*, p. 745.

that the mischief in cholera is at first confined to the mucous membrane; for it is not easy to conceive that chalk, and opium, and catechu could neutralize or suspend the action of a poison in the blood."¹⁸

Once he had the idea of a poison which was neutralised in the intestine and could not enter the blood-stream, the conclusion that it was water-borne followed almost automatically. The poison must gain access to the mucosa by being swallowed and leave it in the evacuations, whose disposal had excited a good deal of public interest throughout 1848. R. D. Thomson and William Farr had expressed their concern over the discovery of urine in the wells of Glasgow.¹⁹ A review of a book by C. F. Ellerman had condemned as unhealthy and wasteful the plan "which consigns the excreta of the population to rivers or water-courses."²⁰ In the summer of the same year R. A. Smith informed the British Association that "all wells near houses and all wells in towns contain nitrates which may be easily traced to sewers and accumulations and outlets of refuse."²¹

The actual date on which he first arrived at his conclusions on the mode of communication of cholera was in the latter part of 1848, as Snow himself said.²² Now, the first time he joined in any of the Westminster Medical Society's many discussions on cholera was on October 21st 1848. At that meeting Dr. Peregrine mentioned the drainage of serum from the system as the cause of the general symptoms and recommended compound chalk powder. Dr. Skiers stressed the need to arrest the first symptoms of diarrhoea if suppression of urine was to be avoided. Snow, whose mind was evidently not then on the central problem, called attention to the resemblance between cholera and ether asphyxia.²³ However, all the elements of his theory were already in the air, and one is tempted to think of his brilliant and original concept as a kind of intellectual doodle which he sketched out on that day while his more informed colleagues were expounding their views in his presence. Certainly he behaved as if this were so and never acknowledged the voluminous literature in which many of his supposedly novel ideas had been repeatedly discussed, and he never attempted to answer the criticisms directed at the pathological parts of his argument.

¹⁸ *Ibid.*

¹⁹ *Lancet*, 1848, 2: 223.

²⁰ *Ibid.*, 1848, 1: 261.

²¹ R. A. Smith: "On the air and water of towns." *Rep. Brit. A. Adv. Sc.*, 1848, p. 30.

²² Snow: *On the Mode of Communication of Cholera*, 1855, p. 125.

²³ *Lancet*, 1848, 2: 507.

The Period of Epidemiological Proof

The rejection of the fungus theory had saved Snow's reputation and left him as the undisputed author of the new concept of cholera. But it had deprived him of another method of proving his hypothesis—the method of detecting and demonstrating the cause, by which Koch eventually settled the whole controversy. His choice of research was now limited to one field only, and that was to establish the channel of communication between one case and another. As early as 1849 he had investigated outbreaks in which contaminated drinking water seemed to be the most probable vehicle of infection. In Albion Terrace the drinking water was contaminated by the contents of the house-drains and cesspools, and "the cholera extended to nearly all the houses in which the water was thus tainted, and to no others."²⁴ Critics at that time objected that there was "entire failure of proof that the occurrence of any one case could be clearly and unambiguously assigned to the use of water. . . . Any local cause operating in one house," they said, "might fairly be considered to extend its influence to those immediately adjoining it; and whatever produced one case of cholera, might suffice to produce twenty in succession."²⁵ They further argued:

We do not say that this disease may not thus be communicated to the healthy, but we consider that the facts here mentioned only raise a probability, and furnish no proof whatever of the correctness of the author's views. 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.²⁶

The crucial experiment was actually performed in 1854, when a bottle of water from the Broad Street pump was taken to a widow in Hampstead and infected her and her daughter, although there were no other cases in the area. The scale of the experiment was far too small to make any impact on those who observed it, and even Snow does not seem to have appreciated its full significance. Snow's interests lay in other directions, and he paid little attention to the advice proffered by the *Medical Gazette*. By 1850 his plan of action had become clearly defined. This was to trace the route of transmission wherever this was possible, and where it was not possible to show that the disease spread most extensively where there were the greatest facilities for the swallowing of excretions.²⁷

The limitations of this comparatively unambitious programme have not always been understood. At the most it could establish the plausibility

²⁴ Snow: *On the Mode of Communication of Cholera*, 1849, p. 15.

²⁵ *London M. Gaz.*, 1849, n. s. 9: 466.

²⁶ *Ibid.*

²⁷ *Lancet*, 1850, 1: 154.

of Snow's theory without ever being able to exclude the principal rival hypotheses. In particular it could do nothing to decide the question of whether the association between polluted drinking water and cholera was due to the distribution of the actual cause, or due to a predisposition which made its consumers more susceptible to an aerial poison. Snow hardly seemed interested in designing an experiment to that end. He was a convinced unifactorialist and dismissed the idea of multifactorial causation as "a metaphysical abstraction assumed to account for the facts."²⁸

So the well-known comparison of the cholera mortality in the customers of the Lambeth and the Southwark and Vauxhall water companies had, as far as Snow was concerned, the simple object of showing that cholera spread most extensively where the greatest facilities for swallowing excretions existed. In other words, it was to supplement defects in tracing the actual route of infection. If it had been intended to demonstrate a causal connection, even Snow would have realised that his method of identifying the source of water, from its chloride content, would invalidate an inference of that kind.

The results of this investigation which has occupied such a prominent place in 19th century sanitary mythology may be fairly stated as follows: It disposed of the evidence of the Newcastle observations which showed no difference in the cholera mortality between those who drank company water and those who drank pure spring water. It added further weight to that part of Snow's argument which was no longer in need of proof. It had no influence on the schemes for the improvement of South London's water supplies which were originally promoted in 1848.²⁹ Indeed it was these improvements which made the investigation possible. It played no part in leading Snow to propose his theory that cholera was a water-borne disease, which, as has been shown, came into being six years earlier.

Some confusion on this last point has centred round Snow's belief that the mode of communication could have been arrived at on the epidemiological evidence, independently of the pathology of the disease.³⁰ He had every reason for wanting this view accepted, since it would have distracted attention from the actual means of discovery. However, as he believed on similar evidence that plague, dysentery, intermittent fevers, yellow fever, and typhoid were transmitted in the same way, and as Ross was beguiled into wasting a good deal of valuable time as a result of the belief, it is clear that his methods are not based on principles worthy of uncritical and universal imitation.

²⁸ *Ibid.*, 1854, I: 109.

²⁹ *Ibid.*, 1848, I: 103.

³⁰ Snow: *On the Mode of Communication of Cholera*, 1855, p. 16.

John Snow and William Budd

The fact that Snow's methods of investigation did not necessarily lead to correct predictions, as was seen in their application to diseases other than cholera, provides an excuse for those of his contemporaries who were not over-enthusiastic in the acceptance of his ideas. Among the first to whom Snow mentioned his new theory, as early as 1848, were Drs. E. A. Parkes and A. Garrod. Many years later, in 1864, Parkes explained his lack of interest:⁸¹

There seemed at once an *a priori* argument adverse to this view, as, at that time, all evidence was against the idea of cholera evacuations being capable of causing the disease. They had been tasted and drunk (in 1832) by men, and been given to animals, without effect. Persons inoculated themselves in dissections constantly, and bathed their hands in the fluids of the intestines; in India, the pariahs who remove excreta, and everywhere the washerwomen who washed the clothes of the sick, did not especially suffer. And to these arguments must be added the undoubted fact, that there were serious deficiencies of evidence in Dr. Snow's early cases. Add to this the unfortunate circumstance, that Dr. Snow, with all the enthusiasm of a discoverer, adopted the view that cholera entered only by means of water, and not at all by air, an hypothesis which is quite irreconcilable with the history of cholera, and thereby created at once a prejudice against his view, and it is no wonder that this alleged mode of entry gained little credence.

In our present position we are able to see that the answers to nearly all these objections lie in our understanding of the nature of the cholera vibrio and the properties of micro-organisms in general. In this respect Budd was extraordinarily far-sighted in his original judgment of Snow's paper. That does not mean to say that Snow would have been well advised to devote his energies to the investigation of the actual cause of cholera, for he had none of the qualities or the opportunities which favoured Koch and Pasteur. Whichever way we look at it, we are forced to conclude that Snow had hit on an idea which he had not the means nor the abilities to put to the test.

The first of his difficulties came from the fact that his original inspiration was the result of a haphazard process of reasoning which no later rationalisation could ever turn into a convincing argument. The second came from the circumstances in which he attempted to demonstrate the route of transmission from one group of cases to another. A severe epidemic in a crowded city produces a situation in which all kinds of routes of transmission will intersect in many ways and presents an epidemiological problem which is complex enough, even when the basic

⁸¹ E. A. Parkes: *A Manual of Practical Hygiene*. London: Churchill, 1864, p. 57.

mechanisms are not under dispute. It was a great misfortune that he was not a country practitioner.

William Budd was in a far better position to make use of the opportunities available at that time. It is quite clear that he had arrived at his conclusion by the systematic observation of small epidemics in rural areas. In May 1849 he had traced the entrance of cholera into Bristol:

From the first outbreak of the disease at Keynsham, on the bank of a stream which feeds the Avon, about eleven miles above Bristol, to its subsequent appearance at Hanham, on the Avon itself, and then at Cruwys Hole, and at the Bristol Cotton Works a little lower down the river; from its transference thence to Redcross-street, and its spread there in the way already described, to its next frightful outbreak in the Rackhay, and the succession of similar outbreaks by which this one has since been followed, we see so many illustrations of the propagation of cholera by means in which it must be plain to the commonest observer that water took the leading part.⁸²

He had observed "the frightful fatality of the disease in particular parts of infected towns, in which the drinking-water of the inhabitants has been *known* to be contaminated by the contents of sewers."⁸³ But above all he had perceived the similarity between cholera and a disease capable of providing far more convincing proof of water-borne transmission than cholera, with its terrifying infectivity, could ever afford. In the late summer of 1847, typhoid had broken out in Richmond Terrace, Clifton—a middle-class district of Bristol. The pump, from which thirteen of the thirty-four houses obtained their water, became contaminated with sewage in September. A few weeks later cases occurred in these thirteen houses, and no others, although they were distributed at random in the terrace, and there was no special social communication between them.⁸⁴ A number of years later the transmission of typhoid between two groups of cottages in the Kingswood area and connected in no other way except by a stream which passed by both of them provided Budd with another example of the way in which water-borne infection can be legitimately inferred without calling on bacteriological evidence. If it had not been for Budd's premature adoption of the fungus theory, correct as we know in its basic principles, his more careful epidemiological studies might well have supplanted those of Snow in the history of preventive medicine. Detailed local studies, in contradistinction to the analysis of mass statistical data, have not proved unrewarding to those who, like Pickles,⁸⁵ have been wise enough to understand their advantages.

⁸² Budd, *op. cit.*, p. 19.

⁸³ *Ibid.*

⁸⁴ W. Budd: *Typhoid Fever*. London: Longmans, Green, and Co., 1873.

⁸⁵ W. N. Pickles: *Epidemiology in Country Practice*. Bristol: John Wright, 1939.