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## A case study in explanatory power: John Snow's conclusions about the pathology and transmission of cholera

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### ABSTRACT

In the mid-1800s, there was much debate about the origin or 'exciting cause' of cholera. Despite much confusion surrounding the disease, the so-called miasma theory emerged as the prevalent account about cholera's cause. Going against this mainstream view, the British physician John Snow inferred several things about cholera's origin and pathology that no one else inferred. Without observing the *vibrio cholerae*, however,—data unavailable to Snow and his colleagues—, there was no way of settling the question of what exactly was causing cholera and how, or if, it was passed on. The question then arises as to how Snow arrived at conclusions so systematically different from those of his opponents. In this paper, I want to look at Snow's reasoning in some detail, and show that there were certain principles, explanatory power in particular, that were epistemologically important to Snow in their own right. I will show that Snow himself takes explanatory power to be an *epistemic* property, and makes explicit links between explanatory power and confirmation. Systematically juxtaposing Snow's claims and his opponents', I will show that Snow was right to tout the explanatory power of his theory, and that his conclusions about the epistemic superiority of his theory over that of the miasmatisms<sup>1</sup> were justified.

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### 1. Introduction

In the mid-1800s, there was much debate about the origin or 'exciting cause' of cholera. Despite much confusion surrounding various aspects of the disease, the so-called miasma theory emerged as the prevalent account about cholera's cause. Going against this mainstream view, the British physician John Snow (1813–1858), during this time, inferred several things about the origin and pathology of cholera that no one else inferred, and that, at the time, were unobservable. Among his claims were the hypothesis that there is a biological cause for cholera, that cholera was a local, not a general disease, that a small quantity of the cholera

era poison is sufficient to bring about cholera, that the cholera agent multiplies in the intestine, that cholera is communicable, that it is communicated, not by effluvia, but by evacuations containing the cholera poison, and that it could be communicated through contaminated water (such as through the Broad Street pump, in Snow's most famous example).

Without observing the *vibrio cholerae*, however, none of these claims could be verified, and there was simply no way of settling for good the question of what exactly was causing the disease and how, or indeed if, it was passed on. In order to do this, it was necessary to observe the poison at work, and all this was, of course, data unavailable to Snow and his colleagues.<sup>1</sup> The question

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<sup>1</sup> Robert Koch (1843–1910) is often credited with the discovery of the *vibrio cholerae*; however, the bacterium was actually discovered much earlier by Filippo Pacini (1812–1883) during the Asiatic Cholera Pandemic of 1846–1863. In 1854, the year cholera came to Florence, Pacini published a paper with slides of the cholera bacteria (see Pacini, 1854). However, there is no evidence that this data was available to Snow and his colleagues (see also Vinten-Johansen, Brody, Paneth, Rachman, & Rip (2003, 224)). For more detail, see Bentivoglio & Pacini (1995) and Howard-Jones (1984); for an account of Koch's 1883/1884 cholera expedition, see Gradmann (2005, chapter V.2).

then arises as to how exactly it was that Snow, in each of the above cases, arrived at conclusions so systematically and substantially different from those of his opponents.

While there have been many discussions of Snow, and, in particular, many discussions of the by now famous Broad Street pump episode,<sup>2</sup> relatively little attention has been given to the kinds of considerations, especially the kinds of reasoning, that Snow used in arriving at his sometimes complex conclusions about the various aspects of cholera. In fact, the only discussions that explicitly pay attention to Snow's reasoning are by Vinten-Johansen et al. (2003) and, although they mention Snow using principles such as reasoning by analogy, and the fact that Snow thought miasmatic explanations unsatisfactory, they do so only in passing and with the further aim of supporting their claim that Snow's various strategies provide an illustration of the hypothetico-deductive method, and that this method is useful for epidemiology in general.<sup>3</sup>

In this paper, I want to look at Snow's reasoning in more detail, and show that there were certain principles that were epistemologically important to Snow in their own right. In particular, I want to focus in some detail on explanatory power, and show that its role for Snow was substantially bigger than previous analyses suggest. As we will see, explanatory considerations played a crucial role for Snow at almost every juncture. What's more, Snow himself is clear about the fact that he takes the explanatory power of his theory to be an *epistemic* property,—a property connected to the theory's truth(-likeness)—, repeatedly suggesting that what puts his own conclusions on a more secure epistemic footing than those of the miasma and effluvia theorists' is the fact that his theory can explain a number of phenomena for which the extant rival hypotheses fail to provide an explanation. In fact, Snow is explicit about the fact that he thinks that there is a connection between explanatory power and confirmation: it is not just the case that “[t]he belief in the communication of cholera is a much less dreary one than the reverse” (MCC1, 30), and that “[t]hese opinions respecting the cause of cholera are brought forward . . . as containing a greater amount of probability in their favour than any other” (MCC1, 29), but also that “there are certain circumstances connected with the

history of cholera which admit of a satisfactory explanation according to these principles explained above [i.e. Snow's views about the mode and transmission of cholera], and *consequently tend to confirm those principles*” (MCC 2, 115, my emphasis).<sup>4</sup> Thus, Snow believes there is a direct connection between explanatory power and his theory's confirmation-theoretic status: the fact that his theory could explain phenomena that the various miasmatic and effluvial explanations failed to account for makes it more likely, in Snow's view, for his theory to be *true*.<sup>5</sup> Lastly, I want to show—through a systematic juxtaposition of Snow's various conclusions and those of the miasma/effluvia theorists—that Snow was correct in claiming that his theory could explain the phenomena he claimed it could explain, and that the extant rivals failed to do so. Thus, Snow was right to tout the explanatory power of his theory. Explanatory considerations did support Snow's theory over others, and Snow was justified in making the claims he did.<sup>6</sup>

## 2. The miasma theory

In the mid-1800s, there was much debate about the origin or 'exciting cause' of various diseases.<sup>7</sup> While it was generally accepted that some diseases were contagious—smallpox, syphilis, and measles, for example—, there was much discussion surrounding the transmissibility of other diseases, such as cholera, typhus, typhoid fever, and tuberculosis.<sup>8</sup> In particular, there was much debate about cholera, not just its origin, but also about its pathology and treatment.<sup>9</sup> During the first big cholera outbreak in Britain, the 1832 epidemic, many different treatments had been tried, but doctors varied widely as to what the right medicine to prescribe was, with prescriptions differing from opium, bleeding, laxatives, and peppermint to brandy. This confusion is reflected in an editorial in *The Lancet* from 1853 in which Thomas Wakley, the journal's founder and editor, wrote: “The question, What is cholera? is left unsolved. Concerning this, the fundamental point, all is darkness and confusion, vague theory, and a vain speculation. Is it a fungus, an insect, a miasm, an electrical disturbance, a deficiency of ozone, a morbid off-scouring from the intestinal canal? We know nothing; we are

<sup>2</sup> Despite its popularity, I won't discuss the Broad Street pump episode here, since its complexity would make a full discussion too lengthy for the purposes of this paper. Snow himself discusses the pump in a number of places, but of particular interest are his 1849a, 1849b, and 1855d. Besides Snow's own accounts, the single best treatment is Vinten-Johansen et al. (2003, especially chapter 11).

<sup>3</sup> In fact, Vinten-Johansen et al. discuss Snow's reasoning in more than one place. There are a number of joint publications, in which they link Snow and the hypothetico-deductive method (see, for example, Brody, Vinten-Johansen, Paneth, & Rip (1999), Brody, Rip, Vinten-Johansen, Paneth, & Rachman (2000), and Paneth, Vinten-Johansen, Brody, & Rip (1998)). In their book, they discuss Snow as a thinker in more detail (see especially chapter 8); in particular, on pp. 219–223, they focus on “Snow as a systems thinker” (220), outlining, in the form of a table, various inductive and deductive instances of reasoning they attribute to Snow, and once again, they claim that Snow “used a hypothetico-deductive [sic] model of science” (222), omitting any discussion of abductive aspects of Snow's reasoning, such as those tied to explanation and considered in this paper.

<sup>4</sup> The references to Snow's works are as follows: ‘MCC1’ refers to *On the Mode of Communication of Cholera*, first edition, London: John Churchill, August 1849; ‘MCC2’ refers to *On the Mode of Communication of Cholera* (1855), second and much enlarged edition; ‘PMCC’ refers to ‘On the Pathology and Mode of Communication of Cholera’, *London Medical Gazette*, vol. 44, Nov. 2, 1849, 745–752/Nov. 30, 1849, 923–929; ‘MPC’ refers to ‘On the Mode of Propagation of Cholera’, *Medical Times* 3, Nov. 29, 1851, 559–562/Dec. 6, 1851, 610–612 (Snow, 1851a, 1851b), and ‘OPC’ refers to ‘On the Prevention of Cholera’, *Medical Times and Gazette* 7, 1853, 367–369 (Snow, 1853b).

<sup>5</sup> In this respect, the case-study in this paper supports a view held by many scientific realists: that a theory's non-empirical virtues (such as explanatory power, but also others, such as consilience, unifying power, etc.) can be epistemic properties. This is in direct contrast to the scientific anti-realist view, according to which such virtues might be pragmatically useful but are explicitly denied any sort of epistemic power or link with truth (see, for example, van Fraassen, 1980, p. 87). I develop a philosophical case for the potential epistemic significance of such theoretical virtues in Tulodziecki (2011). For an overview and some discussion of this debate, see Psillos (1999), Churchland (1985), and Kukla (1998).

<sup>6</sup> Morens (2004), in a book review of Vinten-Johansen et al., raises “[t]he problem of missing context”, “such as the distancing of Snow as a person and a “Snow-centric” emphasis on what he alone thought and did, omitting much of the context within which his ideas evolved” (605). Let one put forth the same objection to my project, I should stress that, in this article, my main concern is with the potentially *epistemic* properties of Snow's reasoning. Thus, this project is not a historical one (in which missing context might be problematic), but one which is better viewed as making a contribution to philosophical projects involving historical records and patterns, and whose proponents think we can learn important philosophical lessons from examining historical case-studies from an epistemic perspective. Stanford, for example, has argued (2006) that historical patterns suggest that, typically, there are unconceived alternatives to our scientific theories that are also scientifically credible, and might even be equally well-confirmed by the existing evidence. However, Stanford thinks there are still important questions about what theories are particularly vulnerable, and, thus, this case-study might contribute to the important project of figuring out “when we should and should not expect to be able to confirm theoretical hypotheses against a knowably exhaustive space of alternatives” (2009, p. 256). See also Tulodziecki (2007, this journal), for trying to link theories' methodological properties with their overall epistemic success. Defending any such philosophical conclusions adequately, however, would require a separate paper, and, thus, more argument than is possible here.

<sup>7</sup> For some general accounts of medicine in the nineteenth century, see, for example, Bynum (1994), Porter (1997, especially chapters XI and XII), and Rosenberg (1977). For more detail on the confusion concerning the various cholera theories, see Baldwin (1999, especially chapter 3), Hamlin (2009, especially chapter 4), Pelling (1978), and Vinten-Johansen et al. (2003, especially chapter 7).

<sup>8</sup> For different views of and responses to some of these different diseases (cholera, smallpox, and syphilis, in particular) by different states, see Baldwin (1999).

<sup>9</sup> For more detail on cholera treatment, see, for example, Hamlin (2009, pp. 28–34) and Howard-Jones (1972).

at sea in a whirlpool of conjecture” (The Lancet II, 393, 1853, also quoted in Vinten-Johansen et al., 166).

Despite this confusion, however, one competitor emerged as the prevalent theory about the cause of cholera: the so-called miasma theory of disease. It was this theory that was the main and most widely accepted theory about the origin of cholera at the time at which Snow proposed his own theory of cholera, according to which cholera was caused by ingestion of infectious matter and according to which it could also be transmitted this way.<sup>10</sup>

According to the miasma theory, organic matter decays and decomposes (decaying animal matter was supposed to be particularly poisonous), and in this process gives rise to a new compound that is given off into the surrounding air and atmosphere—the miasma. This was thought to be poisonous to people, and when they inhaled the miasma given off into the air, the poisonous compound was thought to act on the blood and disturb the body’s balance. People with certain predispositions then became sick from having inhaled the toxic odours, got fever, and, depending on the specific local conditions, contracted one of a variety of diseases. While it was thought that all diseases of this kind were variations on one and the same basic theme, fever, whether one contracted a disease at all, and, if so, what disease was contracted in a certain area, depended on various local factors and circumstances, including geographical location, season, humidity, barometric pressure, and the weather. Factors such as these thus determined, among other things, whether, for example, one contracted cholera or typhoid fever. In addition to this, people also thought that when there already was an epidemic, “other diseases that happened to occur there (such as diarrhoea) would be modified by the prevailing epidemic influence and bear the ‘impress’ of the epidemic disease” (Vinten-Johansen et al., 173). However, the specifics of each case also depended on who inhaled the miasmas. So, while the general diseases were determined by the specific local circumstances, individual constitution determined whether a specific individual contracted the disease; this was supposed to explain why not everyone was affected by a given disease. People were thought to have different predispositions and these different predispositions were thought to affect their susceptibility to the disease.<sup>11</sup> Thomas Southwood Smith, author of the then authoritative *A Treatise of Fever* (1831), captures this with the following remark:

“Fever is a genus consisting of several species, and each species presents many varieties. The external characters of these varieties and the internal states upon which they depend, are so opposite, that no two diseases in any two parts of the catalogue of nosology present a more diversified appearance, or require a more varied treatment, than may be the case with two different types of fever. The fever of one country is not the same as the fever of any other country; in the same country, the fever of one season is not the same as the fever of any other season; and even the fever of the same season is not the same in any two individuals” (24).

As with everything else, there was considerable variation among people as to what exactly the predisposing factors for diseases were, but the general view was that there were two kinds. The first kind consisted of certain environmental factors, and the second of people’s personal habits and characteristics:

For example, W. Lauder Lindsay of Edinburgh argued in the 1850s that strong emotions, especially fear, rendered one sus-

ceptible to epidemic diseases like cholera... Immorality (specifically, overindulgence in alcohol and sex) also predisposed people to contracting cholera, especially when the ‘epidemic constitution’ was most conducive: “The probability of an outbreak or increase during [calm, mild] weather, is believed to be heightened on holidays, Saturdays, Sundays, and any other occasions where opportunities were afforded the lower classes for dissipation and debauchery.” (Vinten-Johansen et al., 174, see also n. 42 on 191).

There were, in turn, two kinds of environmental/external factors: those affecting everyone (climate, weather, season, wind, barometric pressure, geography, elevation above sea-level, humidity) and those not affecting everyone (bad hygiene, living in overcrowded quarters, bad ventilation). These factors determined what disease prevailed in a certain area and who contracted it, in addition to determining the degree of the toxicity of the miasmas. The individual and personal factors included poor nutrition, sickly constitution, poor morals, consumption of alcohol, and similar factors. So, in short, the picture is this: decomposing organic matter gives rise to miasmas. Atmospheric influences might or might not play a role in determining the degree of toxicity of the miasmas, and the same goes for other external factors. People then inhale the toxic substances, and people with certain predispositions contract the disease. Their internal balance is disrupted and they fall ill.

This, then, is the basic miasma theory. However, despite the fact that it explained some of the things that needed explaining, it was also problematic in many ways. The miasma theory, as presented, was a theory that held that diseases that were caused by miasmas were not, on the whole, contagious. However, it was also the case that everyone agreed that there were *some* diseases that clearly were contagious. Smallpox was the standard example here, since anyone coming into contact with someone sick from smallpox would also get sick. Other diseases, however, were thought to be classic miasma and non-contagious diseases, such as influenza. But while there were some clear-cut cases, the majority of diseases could not be neatly categorised as either, as was the case for cholera (but also including typhus and yellow fever). The problem with these diseases was that they exhibited some properties of contagious and some properties of non-contagious diseases. To account for this, some people adopted a modified version of the miasma theory, according to which, in some rare cases, certain diseases could be transmitted from one person to another. On this view, diseases were not contagious on the whole. However, when a disease exhibited certain traits characteristic of contagious diseases, people were, in some cases, willing to grant that, perhaps, under certain circumstances, diseases that were, on the whole, non-contagious, might become contagious. That is, they believed that under certain (rare and special) circumstances, diseases that were usually non-contagious, could be transmitted from one person to another. In particular, it was thought that extremely bad hygienic conditions and overcrowding would exacerbate this. Thus, it was thought that diseases that were usually non-contagious could transform themselves into contagious diseases if people lived in particular dirty and crowded quarters.

There were thought to be three main ways for diseases to be transmitted (and there was much disagreement about this): through simple touch (such as smallpox), through fomites (linen, clothing, and other material used by the sick that had the ‘virus’ in

<sup>10</sup> In fact, the situation was considerably more complicated than this brief section suggests. In this sense, it is misleading to speak of *the* miasma theory, since there were a whole host of related, but slightly different theories involving miasmas (and a variety of non-miasmatic theories). However, since these differences do not affect my argument, I will not pursue them here. For a nice overview of how some of the different theories answered different questions about cholera, see Hamlin (2009, pp. 154–155). For more detail on some of the different germ theories, see Worboys (2000); for more on the variety of cholera theories see the references from note 7 above.

<sup>11</sup> In fact, Hamlin argues that “[t]he importance of predisposing causes as distinct from miasmatic explanations has been insufficiently appreciated” (1990, p. 123, n. 26). See also Hamlin (1992).

them and could be contagious for some time), and ‘infection’, which at the time meant inhalation by the healthy of so-called effluvia given off by the sick through breathing or skin pores in sweating. In the case of infection, it was believed that a sufferer from the disease gave off effluvia through sweating and exhaling and these effluvia were believed to be noxious. Other people would then inhale the noxious stuff and, if they were so predisposed, they would contract the disease too. (That is, usually things would depend on people’s predisposition; there were thought to be some effluvia that were so toxic that they affected and eventually killed everybody.)

In the case of cholera, it was clear that it wasn’t as contagious as some other diseases, such as the plague. It was noticed, for example, that doctors hardly ever became sick after having treated cholera victims:

As it became increasingly clear that cholera was not as directly contagious as the plague, as experience showed that the medical personnel in closest contact were not necessarily more afflicted than others, that its incidence varied by class, season, region, neighborhood and person, the evidence seemed to mount that something other than a contagion was at work . . . (Baldwin, 1999, p. 123, quoted in Vinten-Johansen et al., 2003, p. 178, cf. also n. 62 on 194).

Thus, the consensus with respect to cholera was that it was non-contagious, and those few who thought that it was, under certain special and rare circumstances, contagious, thought that it was transmitted by effluvia only.

### 3. The first cholera outbreak

Snow encountered his first cholera outbreak at the age of eighteen, in the summer of 1831. That year, London was hit by an epidemic, and from there it went to the North, among other places to Newcastle where Snow was an apprentice to one Dr. Hardcastle at the time. Because Dr. Hardcastle couldn’t treat the many victims all by himself, Snow had to treat patients as well. Among others, he treated the miners of Killingworth Colliery, many of whom had cholera. However, there wasn’t much Snow could do, since the standard treatments at the time (opium, bleeding, laxatives, peppermint, brandy) didn’t work. After cholera suddenly disappeared again in February 1832, there was no cholera in England for the next 16 years, and Snow directed his efforts towards anaesthesiology. When the next outbreak struck London (where Snow lived by now) in September 1848, his interest in cholera surfaced again. Snow was unhappy with the miasma theory for several reasons. In particular, he thought it failed to explain how cholera spread, and how exactly it was that the miasmas were responsible for making people sick.

He had noticed, for example, during the 1831 epidemic, that large numbers of miners contracted cholera while working underground, although there are no sewers or swamps underground. According to the miasma theory, however, these played a crucial role in causing people to suffer from cholera. The miasma theory could not explain why the mining population suffered worse than other groups; even worse, this was in fact problematic for the miasma theory, since the miasmas were not supposed to exist in the pits underground. There also seemed to be no other reason on the miasma theory as to why the miners should be affected so severely. Snow’s theory, however, can explain this. In fact, Snow believes that the fact that “[t]he colliers of this country suffered much more from cholera than persons in any other occupation

whatever, both in 1832 and 1849 [is] a circumstance which can only be explained by the mode of communication of the malady above pointed out” (OPC, 368; also see Snow (1855a)). According to Snow, part of the reason is that the miners are underground for long periods of time—eight hours—and do everything down there, in particular eat, without adequate opportunities for hygiene: “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” (MCP, 560a). Compare also Snow in PMCC: “[A]s soon as one pitman gets the cholera, there must be great liability to others working the gloomy subterranean passages to get their hands contaminated, and to acquire the malady; and the crowded state in which they often live affords every opportunity for it to spread to other members of their families” (PMCC, 747).

### 4. The 1848 London cholera outbreak

Thus, when the 1848 epidemic struck London, Snow set his mind to figuring out what was going on. He wanted to see exactly how cholera was spread and began collecting data. In particular, he decided to track various cases in order to get a clearer idea of the propagation of the disease. As pointed out before, the official position was that cholera was not transmissible from person to person (except for those few who thought that it could sometimes, under special circumstances, be transmitted through effluvia). Snow, however, first suspected, and later was clear about the fact that he thought cholera was definitely transmissible from person to person. Thus, early on in MCC1, he discusses cases which, he thinks, make this unequivocal. He begins MCC1 by pointing out that there have been several cases “in which cholera dates its commencement in a town or village previously free from it to the arrival of the illness of a person coming from a place in which the disease was prevalent . . .” (MCC1, 5).

Snow explains that the first definite case of cholera came to London in the autumn of 1848 when a merchant seaman of the name of John Harnold came from Hamburg, where there was known to be cholera, on an Elbe steamer. He left the ship, went to lodgings in Horsleydown, got symptoms of cholera on 22 September, and died a few hours later. Snow went to talk to the doctor who treated Harnold, and learned from him that he had gone back for another case shortly after Harnold’s death. It turned out that the second man was the next person to occupy the room Harnold had stayed and died in. The new victim was a man called Blenkinsopp, and he first exhibited signs of cholera on 30 September. Right after having rented the room, he got the first symptoms and died a week later. Snow suspected that the room hadn’t been cleaned properly and that some germs had stayed in it after Harnold’s death (MCC1, 27–29). Snow thought that this was a good example of the human to human transmission of cholera and, thus, evidence for the contagiousness of the disease.<sup>12</sup>

However, as an isolated incident, this might not have had much force (although it would have been a remarkable coincidence), but Snow examined more cases, and found his suspicions confirmed.<sup>13</sup> Taken together, all these cases would have been very curious indeed if cholera were not contagious, and Snow thought that this showed, at the very least, that cholera was transmitted in these cases, and, he argued, that “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?” (MCC1, 30).

<sup>12</sup> For further discussions of Harnold’s case, see Snow (MCC1, 27–30, MCC2, 3; MPC, 611; 1857, p. 419; 1853a, p. 169, and 1854a, p. 170).

<sup>13</sup> For example, the case of John Barnes that Snow discusses in MCC2, 4–5. Barnes was an agricultural labourer in a cholera-free region who contracted cholera and died. Barnes’s illness was a mystery, until a connection was established linking Barnes to a box of unclean clothes that had belonged to his sister, who had just recently died of cholera. For more cases along these lines, see, for example, MCC2, 5–9.

Snow goes on to claim that cholera is

a disease ... evidently spreading by communication in very numerous instances [and that] we ought not to conclude that part of the cases must depend on some other occult cause, but rather, first to examine the one sufficient cause we have found, to ascertain whether it will not explain more and more of the facts the further they are inquired into; and to search whether the localities which are favourable to cholera do not promote it through physical conditions which favour its communication. (PMCC, 929).

Moreover, Snow believes that his more general conclusions are supported by the following observations:

[i]n extending itself to a fresh island or continent, the cholera has always made its appearance first at sea-port, and not till ships had arrived from some infected place. Crews of ships approaching a country in which the disease was prevailing, have never been attacked until they have had communication with the shore. The cholera, moreover, in progressing from one place to another has never travelled faster than the means of human transit, and usually much slower (PMCC, 746).

Once again, this does not make sense, except on the hypothesis that cholera is communicable, since there is no reason for miasmas to wait for the arrival of sailors in order to infect people. Moreover, even if some of these cases could be explained by the miasma theory, for example by claiming that the arrival of the ships coincided with a time in which areas were particularly prone to miasmas, what cannot be explained is the systematic correlation between the arrival of infected sailors and the advent of cholera in precisely those places.<sup>14</sup>

If there had only been some isolated occurrences, there would have been no need to explain anything, but, since these observations are both systematic and ubiquitous, Snow himself points out that they cannot be dismissed as coincidence:

Such are the general considerations which show that cholera is communicated by human intercourse; and there are besides instances so numerous of persons being attacked with the disease within a day or two after immediate proximity to the sick, that it seems impossible to attribute the circumstance to mere coincidence. (PMCC, 746)

## 5. The pathology, transmission, and mode of communication of cholera

However, while this might establish that cholera was transmitted, it didn't say anything about *how* it was transmitted. Snow begins by mentioning some objections to the hypothesis that cholera is communicable, but points out that these objections hold only on the assumptions that cholera is communicated through effluvia given off by the sick. Thus, although people have argued against the communicability of cholera,

besides the objection that negative evidence ought not to overthrow that of a positive kind, the instances that are believed to oppose the proofs of communication are reasoned upon in the opinion that cholera, if conveyed by human intercourse, must be contagious in the same way that the eruptive fevers are considered to be, viz., by emanations from the sick person onto the surrounding air, which enter the system of others by being

inhaled, and absorbed by the blood passing through the lungs. There is, however, no reason to conclude *a priori* that this must be the mode of communication of cholera; and it must be confessed that it is difficult to imagine that there can be such a difference in predisposition to be affected or not by an inhaled poison, as would enable a great number to breathe it without injury in a pretty concentrated form (the immunity not having been earned by a previous attack, as in the case of measles, &c.), whilst others should be killed by it when millions of times diluted ... we ought not to conclude that cholera is propagated by an effluvia. (MCC1, 6)

Rather, the fact that

[n]umbers of persons come into near proximity with the sick without contracting the malady ... only shows that it is not communicated either by contact with the patient or by effluvia given off into the surrounding air; and as there was no right to assume that these two modes, usually denominated contagion and infection, are the only ways in which disease can be transmitted from one person to another, the evidence of the communication of cholera remains unshaken to afford a full explanation of the progress of the disease; indeed, the only explanation of it that can be offered. (OPC, 367)

These considerations lead Snow to his second hypothesis—that cholera is propagated through patients' evacuations and spread by either direct contact with cholera victims or else through items contaminated by someone afflicted with cholera. As we have seen, the rival hypothesis to this claim is a modified version of the classic miasma theory that allows for the communicability of cholera. While the main cause of cholera is still through miasmas, cholera, on this modified version, can also be transmitted from person to person through effluvia given off from the sick, either through skin pores (e.g. through sweating) or through breathing. If someone susceptible to cholera inhales these emanations, they also contract the disease. So, according to this theory, cholera might be transmissible under certain special circumstances.

So how is Snow led to reject the miasma/effluvia theory and, instead, propose his own views about the mode of communication of cholera? On examining the evidence, Snow realised that the miasma theory, including its modified contagion version, could not account for it in a satisfactory manner. According to the miasma theory, cholera was a blood-borne and general disease. However, Snow reasoned that if cholera were a general disease that is caused by inhaling poisons that then act on the blood, sufferers of the disease ought to exhibit some of the general symptoms generally associated with such diseases, such as headaches, chills, rigours, and quickened pulse (MCC1, 6–7). Moreover, if the cause of cholera was to be found in poisoned air (i.e. miasmas), then the disease should have started in those parts of the body that are exposed to air, viz. the nose or the lungs. Thus, Snow thought that "consideration of the pathology of cholera is capable of indicating to us the manner in which the disease is communicated" (MCC2, 10), and ends up concluding that cholera is a local, not a general disease.<sup>15</sup>

As cholera spread, Snow made more enquiries. He went to see many people exhibiting the symptoms and found that in all the cases he examined, the victims told him that their symptoms had started with digestive problems. After Snow had collected many observations regarding the pathology of cholera, he concluded

that cholera invariably commences with the affection of the alimentary canal. The disease often proceeds with so little feeling

<sup>14</sup> Snow tells a similar story about troops, pointing out that, in India, the villages that became infected were close to the main travel routes, and villages through which infected troops had passed. In addition, cholera tended to stay with infected troops through different climates and geographical conditions, all facts for which the miasma theory lacked an explanation (see, for example, PMCC, 746).

<sup>15</sup> For "the consideration of the influence which this view of its pathology ought to have on the treatment of the disease", see Snow (1854b, p. 180).

of general illness, that the patient does not consider himself in danger, or even apply for advice, till the malady is far advanced. In a few cases, indeed, there are dizziness, faintness, and a feeling of sinking, before discharges from the stomach or bowels actually take place; but there can be no doubt that these symptoms depend on the exudation from the mucous membrane, which is soon afterwards copiously evacuated. This is only what occurs in certain cases of haemorrhage into the alimentary canal, where all the symptoms of loss of blood are present before that fluid shows itself in the evacuations. In those rare cases, called “cholera sicca”, in which no purging takes place, the intestines have been found distended with the excretion peculiar to the disease, whenever an examination of the body has taken place after death. In all the cases of cholera that I have attended, the loss of fluid from the stomach and bowels has been sufficient to account for the collapse, when the previous condition of the patient was taken into account, together with the suddenness of the loss, and the circumstance that the process of absorption appears to be suspended. (MCC2, 10)

Since cholera begins with diarrhoea and vomiting, Snow thought that the cholera ‘poison’ acted on the alimentary system rather than the whole body. If cholera were caused by miasmas, it should start in the nose or lungs, then pass into the blood, and finally enter the alimentary system—but in that case general symptoms ought to have preceded the local symptoms which was found not to be the case. In cholera, on the other hand, the symptoms *begin* in the alimentary canal, and all general symptoms can be attributed to and explained by “the flux from the canal” (Richardson, 1952, pp. 276–277). Any general symptoms that occur during the progress of cholera occur only *after* other symptoms that are restricted to the alimentary canal.<sup>16</sup> As Vinten-Johansen et al. put it:

In cholera, however, all the constitutional symptoms occur later and can be better accounted for by the amount of fluid lost from the gut as the result of the massive vomiting and diarrhea and the consequent state of dehydration (MCC[1], 7–8). He reasoned, moreover, that the fluid losses were most likely due to “some local irritant of the mucous membrane”, of the gut, rather than to some generally circulating poison, no instance suggesting itself to the writer in which a [blood-borne] poison causes irritation of, and exudation from, a single surface (MCC[1], 8) (202).

From this, Snow concludes that “the morbid material producing cholera must be introduced into the alimentary canal and not through some other system, e.g. the lungs” (Hill, 1955, p. 1008). The fact that cholera patients showed symptoms of the alimentary system meant, he thought, that the cholera poison had to be ingested, not inhaled. Snow thought that the process by which cholera spread was this:

the excretions of the sick... [contain] some material which, being accidentally swallowed, might attach itself to the mucous membrane of the small intestines, and there multiply itself by the appropriation of surrounding matter, in virtue of molecular changes going on within it, or capable of going on, as soon as it is placed in congenial circumstances (MCC1, 8–9).

Thus, considering the pathology of the disease, Snow reasons that cholera enters the body through ingestion and exits it in some form(s) of bodily discharge. Combining this with his beliefs about the communicability of the disease, he concludes:

In the meantime 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 induction from these data is that the disease must be caused by something which passes from the mucous membrane of the alimentary canal of the patient to that of the other, which it can only do by being swallowed; and as the disease grows in a community by what it feeds 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 is the case of the *materies morbi* of cholera taking place in the alimentary canal. (PMCC, 746)

Compare also Snow in MCP:

The proofs that the cholera poison is contained in these discharges [evacuations from the stomach and bowels] and that the disease is communicated by their being accidentally swallowed, are of a general as well as a particular kind. (MCP, 560a)

What is this evidence that Snow has in mind and how does it convince him that cholera is not transmitted through effluvia but instead through evacuations? Once again, there are several phenomena that Snow thinks support his view but lack any sort of explanation on the rival hypothesis.

Snow begins by talking about the connection between bad hygiene and cholera, pointing out that “the want of personal cleanliness aided very much the propagation of cholera, although no explanation could be given of the circumstance” (MCP, 560a). The classic miasma theory cannot explain this, and even the modified version is at a loss here, since, if cholera were contagious, there would be no reason to expect that patients are more likely to give off emanations when their surroundings are dirty than when they are clean. Perhaps one could have tried to postulate a connection between cholera-causing miasmas and lack of hygiene, arguing that, because of the higher presence of cholera in the first place there are more sick people to begin with, and, hence, more emanations are given off to infect others. However, this connection seemed unsupported, since there didn’t seem to be any relationship between initial incidents of cholera and bad hygiene. However, while miasmatic and effluvial explanations fail here, Snow’s theory about the mode of communication of cholera can explain the observed connection between want of cleanliness and the propagation of cholera perfectly well. Indeed, Snow claims that “[n]othing has been found to favour the extension of cholera more than want of personal cleanliness, whether arising from habit or scarcity of water, although the circumstances till lately remained unexplained” (MCC2, 16–17). His hypothesis is that the patients’ discharges and evacuations are contaminated with the cholera poison and that cholera is passed on in this way. Cleanliness, or rather lack thereof, bears on this in the following way:

[W]ithout habits of strict cleanliness persons waiting on the sick must get their hands soiled with the cholera discharges, and must unknowingly contaminate the provisions they handle, in eating their own food or preparing that of others. The sudden discharge of the evacuations, which often soil the clothing or bed linen, and the little colour or odour they possess, very much increase the liability to their being swallowed in this way, and under some circumstances render it almost certain. (MCP, 560a)

Accordingly, it ought also be the case, according to Snow’s theory, that crowding exacerbates the propagation of cholera and that, since this is most common among the poor, there ought to be more propagation of cholera among the poor than the wealthy (see also 1855a). Moreover, what ought to aggravate the situation among the poor is that in poor households usually the same person looks after the sick and prepares the food. This is not the case in wealthy

<sup>16</sup> For Snow’s account of other “affections of the alimentary canal”, see, for example, Snow (1855b, pp. 457–458).

households where food is prepared in a room other than the sick-room and where it is possible to follow the basic precepts of hygiene. It turns out that this is exactly what is the case:

For instance, when a large family, or more than one family are crowded into a single room, and when the same persons have to attend to the patient, and also to prepare and serve the meals for the rest of the inmates, without the materials for washing the hands, even if the inclination should exist, it is next to impossible that the provisions should be eaten without being contaminated with the peculiar discharges of the patient; and these are the circumstances under which the disease is found most frequently to spread among the inmates of a room. Mr. Baker, of Staines, who attended 260 cases of cholera and diarrhoea in the late epidemic, chiefly among the poor, informed me in a letter, with which he favoured me on December 1849, that, "where the patients passed their stools involuntarily the disease evidently spread". Deficiency of light is a great obstacle to cleanliness, as it prevents dirt from being seen, and it must aid very much the contamination of the food with the cholera evacuations". (MCP, 560a)

Snow then considers how the miasma theory fares on these counts. He acknowledges that

[t]he assistance which crowding lends to the spread of cholera could be explained on the hypothesis of effluvia or miasmata given off from the patient in the surrounding air, but points out that the extension of the disease from want of cleanliness, deficiency of water, and deficiency of light, cannot be explained on such a hypothesis (MCP, 560a).

However, there are more phenomena that the miasma/effluvia theory cannot account for. One phenomenon that was to be expected on the miasma theory was that medical men ought to be affected by the disease more than others. If the disease were indeed communicated through effluvia, they should have been particularly prone to catching cholera, since they, as a class, had more contact with the sick than others. However, it turned out that, in fact, medical men were hardly ever affected by cholera; thus, this was the exact opposite of what was to be expected on the miasma theory. What could have been the reason for this? Snow's explanation is that medical men hardly ever contract the disease, because they wash their hands whenever possible and, moreover, do not take food on their visits to the sick. Hand in hand with this, Snow can also explain some corollary facts that lack an explanation on the miasma/effluvia theory. According to Snow, what explains the absence of cholera in doctors, also accounts for the absence of cholera in those performing post mortems: post mortem examinations are always accompanied by careful hand-washing and not occasions on which food is taken. On the miasma/effluvia theory, however, people performing post mortems, just as doctors, ought to have been particularly susceptible to cholera, since they dealt with large numbers of victims and, through performing autopsies, ought to have been more exposed to the effluvia than others. Another related fact is that, in contrast to the coroners and doctors, the (working class) women preparing dead bodies for funerals very often did contract cholera. While the miasma theory could explain this by claiming that they had higher exposure to effluvia, the problem was that, according to the theory, this should also have been the case for the doctors and coroners. Snow's explanation of the high incidence of cholera among women preparing dead bodies, on the other hand, is that they are at a higher risk of contracting cholera because they eat and drink on these occa-

sions, handling the victims' bodies at the same time without having much of an opportunity to wash their hands. Lastly, it was a problem for the miasma/effluvia theory to explain why the number of funeral attendees contracting cholera was comparatively high. There was no reason at all for this on the miasma/effluvia theory, since the funeral attendees usually had no direct contact with the body at all. Once again, however, Snow can explain this. As is the case with the women preparing the bodies, funerals are occasions on which food and drink is consumed and this food is usually prepared (in the working class, at least) by people who had contact with the victims and their (most likely infected) bed linen and clothes.

Overall, Snow concludes that

[t]he non-communication of cholera in cleanly families, where the hand-basin and the towel are in constant use and where the apartments for cooking and eating are distinct from the sick-room; and also its non-communication, as a general rule, to medical men and other visitors of the sick belonging to the educated classes of society, are fully explained on the doctrine here laid down, although these circumstances are inexplicable on the supposition of its spread by means of effluvia (MPC, 560)<sup>17</sup>

## 6. Statistics about average populations and durations of cholera epidemics

The next kind of evidence that Snow adduced in support of the communicability hypothesis is of a substantially different kind than the previous, but, like the previous, he thinks that this circumstance [is] greatly confirmatory of the communication of cholera (MPC, 559a). Here, Snow is particularly explicit about the fact that he takes explanatory power to be confirmatory. He writes,

there are certain circumstances connected with the history of cholera which admit of a satisfactory explanation according to these principles explained above [i.e. Snow's views about the mode and transmission of cholera], and consequently tend to confirm those principles. The first point I shall notice, viz., the period of duration of the epidemic in different places, refers merely to the communicability of the disease, without regard to the mode of communication. (MCC 2, 115).

Snow begins by explaining that "[t]he duration of cholera in a place is usually in a direct proportion to the number of the population. The disease remains but two or three weeks in a village, two or three months in a good-sized town, whilst in a great metropolis it often remain a whole year or longer" (MCC2, 115–116; also see Snow, 1855c). Snow includes a table to illustrate this point, showing the figures that he calculated (Fig. 1). The data in this table is that of the 1832 epidemic, but Snow goes on to point out that the numbers are roughly the same for the 1849 epidemic,

a relation which points clearly to the propagation of the disease from patient to patient; for if each case were not connected with a previous one, but depended on some unknown atmospheric or telluric condition, there is no reason why the twenty cases which occur in a village should not be distributed over as long a period as the twenty hundred cases which occur in a large town (MCC2, 116).

Moreover, Snow informs us that

[e]ven the duration of the cholera in a street, when compared to its duration in the individual houses, points to the same conclusion. A table has been published<sup>18</sup> in the report of the late

<sup>17</sup> In fact, "[i]ts fearful extension in certain pauper asylums for children and lunatics is also clearly accounted for, together with its non-liability to spread in more commodious and better regulated establishments" (MPC, 560).

<sup>18</sup> Medical Times and Gazette, Nov. 25th, 1854.

No. of Places.	Duration in Days.	Average Population.
52 ...	0 to 50 ...	6,624
43 ...	50 to 100000 ...	12,624
33 } 34 }	100 and upwards ...	{ 38,123 { 78,823

Fig. 1. The last line includes London in the number of places where cholera lasted for more than 100 days. The middle line is clearly supposed to indicate places where cholera lasted between 50 and 100 days; one can only assume that '100000' is a typographical error. The source of the table is <http://www.ph.ucla.edu/epi/snow/snowbook4.html> (last accessed May 2010).

discussion on cholera at Munich, which shows that whilst the epidemic remained three or four weeks in a street, it only remained six or seven days in houses where several people were attacked. Dr. Pettenkofer remarks, that “if the proximate cause of the disease had been generally diffused over a certain number of streets of a certain district, and its invasion had been opposed by individual disposition alone, one might have expected that both the cases of disease and the instances of death would have occurred in single houses, where many such appeared together, at similar periods of time throughout the whole street; but, supposing that the proximate cause of the disease was not general, but local, then it would act in such a manner that the period of time within which the disease would show itself in single houses would be very different from that which was applicable to the entire street.” The local cause in a house we know to be the illness of some individual, who, in many cases, has newly arrived from some place where the disease was prevailing. (MCC2, 117)

Snow reasons that if cholera were not communicable, then all instances of the disease would be completely independent of each other (except for the common cause), and we ought to expect the rate of infection to be independent of the number of people in the afflicted area. Thus, if different cases of cholera were independent of each other, then the rate at which they arose ought to be the same regardless of how many people there are. But this is not the case. Rather, it is found that, the larger the average population, the longer the duration of the epidemic. On the other hand, if the cases were connected, and cholera were communicable, then this result is to be expected since there are more people to be infected, and, as people pass on the disease to others who in turn pass it on to even more people, the disease as a whole goes through more generations than it would in a small population. The smaller the population, the faster the epidemic will be over, since there are fewer potential victims and since it will run out of people to infect faster.

Adherents of the non-communicability thesis might have tried to explain the fact that cholera stays for a shorter time in small villages by the fact that there are fewer sources of miasmas in rural than in urban areas, or by claiming that miasmas in the country are infectious for shorter periods of time. However, there is no reason at all as to why this should be so, since there don't appear to be any relevant conditions that could account for such miasmatic differences; all the usual conditions, such as weather or climate, were the same. An alternative miasmatic explanation might have involved pointing out that cities were more polluted than the country. To my knowledge, no one ever appealed to any explanations of these kinds, but note that even if some attempts of this sort had been made, this could have at most explained a limited subset of the numbers that Snow was looking at, but not the systematic and proportional correlations that the numbers suggest. These statistics, however, are exactly what is to be expected on Snow's assumption that cholera is transmissible from person to person and through fomites.

Snow himself concludes that

[t]he views propounded in this paper offer a more ready explanation of the decline of the disease for want of fresh victims, than the usual theory of contagion or infection; for all the members of the community are not liable to be reached by a poison which must be swallowed, as they would be by one in the form of an effluvium (PMCC, 928).<sup>19</sup>

## 7. Mortality rates: men vs. women/different occupations

Another fact that Snow can explain, but that the miasma theory cannot, is the difference in relative mortality rates between men and women at different times of an epidemic. Snow points out the following: early on in the cholera epidemics, there is a higher mortality rate for men than women; this slowly approaches an equilibrium, and when the overall mortality rate is highest, more women than men die of cholera. Snow believes his theory can account for this in the following way: on the whole, he says, women spend most of their time at home, in particular, they eat at home, whereas many men “move about in following their occupations” (MCC2, 119). Moreover, men often eat and drink out of the house, in many different places. Because of this, Snow explains, “in the early part of an epidemic, when the disease only exists in a few spots, the male part of the population is most liable to come within the operation of the morbid poison” (MCC2, 120). However, once cholera has already spread, Snow points out, “it may reach those who stay at home as readily as those who move about” (MCC2, 120), and, moreover, it is also the case that the women care for the sick, this making it more likely for them, at this stage, to contract cholera; hence the higher mortality rate for women as the epidemic goes on and reaches its climax. Once again, although the phenomenon is new and different, Snow does not need to introduce any ad hoc modifications in order to accommodate and explain the data, which is not the case for the miasma theory, which, as it stands, cannot explain the phenomena and would have to introduce several ad hoc modifications in order to even be able to accommodate, much less, explain these phenomena.

Snow's next point is a problem for both the non-communicability hypothesis and the modified miasma/effluvia theory. This is that there are different mortality rates depending on the victims' occupation. For data, he points to a table compiled by “Dr. Guy, physician to King's College Hospital, [who] made a table showing the occupations of 4,312 males, of fifteen years of age and upwards, who died of cholera in London in the epidemic of 1848–1849; together with the ratio which the deaths bear to the living” (MCC2, 121). Snow selects those occupations that were most affected and those that were least affected, and finds significant differences with respect to different occupations and relative cholera mortality rates (see Fig. 2).

Snow first notes that “[i]n some of the occupations which show a high relative mortality, the number of living is too small to allow for any reliable statistical result, and the relative mortality is probably clue to accidental circumstances quite unconnected with the occupation” (MCC2, 121), so he disregards these cases. He goes on, however: “[i]n other cases, . . . , the numbers are so considerable as to indicate something more than an accident.” (MCC2, 121) and on these numbers he focuses. In particular, he points out that occupations which are particularly affected include sailors (1 in 24), ballast-heavers (1 in 24), coal-porters (1 in 32), and coal-heavers (1 in 32). Snow's explanation for this relatively high number is this:

Now all those persons lived or were employed on the river, where it is the habit to drink water drawn by pailfuls from

<sup>19</sup> For more detail on this, and, in particular, Snow's actual data on different towns and regions, see his 1855c.

TABLE XIV.

	No. of Deaths.	Ratio.
Agents	12	1 in 40
Bricklayers and builders	14	1 „ 39
Cowkeepers, dairymen, and milkmen	8	1 „ 20
Egg merchants	5	1 „ 6
Fishmongers	11	1 „ 20
Fruiterers and greengrocers	12	1 „ 28
Jobmasters, livery-stable keepers	5	1 „ 37
Oilmen	13	1 „ 46
Paper-makers	2	1 „ 15
Poulterers	3	1 „ 32
Sail-makers	2	1 „ 30
Turners	2	1 „ 50
Ballast-heavers	7	1 „ 24
Coal-porters and coal-heavers	53	1 „ 32
Dustmen and scavengers.	6	1 „ 39
Founders	10	1 „ 12
Hawkers, etc.	67	1 „ 22
Lithographers	3	1 „ 48
Modellers	3	1 „ 41
Polishers	4	1 „ 36
Sailors, including Greenwich pensioners	299	1 „ 24
Tanners	22	1 „ 39
Weavers	102	1 „ 36
Physicians, surgeons, & general practitioners	16	1 „ 265
Magistrates, barristers, conveyancers, and attorneys	13	1 „ 375
Merchants	11	1 „ 343
Auctioneers	1	1 „ 266
Saddlers	1	1 „ 250
Brass-finishers	3	1 „ 318
Coach-makers	16	1 „ 262
Cork-cutters	2	1 „ 279
Footmen and men-servants	25	1 „ 1572
Jewellers, goldsmiths, and silversmiths.	6	1 „ 583
Millwrights	2	1 „ 266
Tallow-chandlers	2	1 „ 430
Type-founders	1	1 „ 390
Undertakers	2	1 „ 325
Warehousemen	8	1 „ 472
Watchmakers	11	1 „ 364
Wheelwrights	8	1 „ 294

Fig. 2. Snow comments on this table in the following way: "I have not room for the whole table, but have selected the occupations which suffered most, and those which suffered least. The following abstract of Dr. Guy's table contains all the occupations where the deaths from cholera equaled one-fiftieth of the number living, and all those in which the deaths did not exceed one in two hundred and fifty living" (MCC2, 121). The source of the table is <http://www.ph.ucla.edu/epi/snow/table14a.html> (last accessed May 2010).

the side of the ship. The 67 hawkers are one in 22 of the whole number. These persons are constantly moving about, and are in the habit of living in crowded lodging-houses, and consequently must be extremely liable to contract any communicable disease. Tanners nearly all live in Bermondsey and Lambeth, supplied in 1849 with none but very impure water, as was previously explained. The weavers probably suffered the high rate of mortality from the crowding of their apartments in Spitalfields, and the uncleanness of their habits. (MCC2, 122)

Next, he noted those occupations in which people suffered an extremely low mortality rate from cholera. First among these were (by far) footmen and men-servants (1 in 1572). According to Snow and his hypothesis, this made sense since he thought it was "impossible to conceive a class less exposed to the disease. They

live in the best parts of London, and go from home much less than their masters" (MCC2, 122). Snow also points to the

low rate of mortality amongst medical men and undertakers... If cholera were propagated by effluvia given off from the patient, or the dead body, as used to be the opinion of those who believed in its communicability; or, if it depended on effluvia lurking about what are by others infected localities, in either case medical men and undertakers would be peculiarly liable to the disease; but, according to the principles explained in this treatise, there is no reason why these callings should particularly expose persons to the malady (MCC2, 122).

He also notes that only one master-brewer died (making the rate 1 in 160), but no other kind of brewery worker, although he

suspected that there must be several thousand of them in London. Snow attributes this to the fact that, as for example in the case of the Lion Brewery in Soho, “[t]he men are allowed a certain quantity of malt liquor” (MCC2, 42), and hardly ever drink water.

All of these facts did not admit of an explanation on the miasma theory, at least not the way it was. There was simply no reason why people in certain occupations should have been more susceptible to cholera than people in other occupations. And while, for each of the preceding pieces of evidence the miasmatisms could have perhaps introduced modifications or additional principles to their theory, it is hard to see how they could have possibly added principles that would have accounted for the diverse evidence in one go rather than ending up ‘adding epicycles to epicycles’.

## 8. Unexpected cases

Another class of phenomena that could be explained by Snow’s theory, but not the miasma theory, were the apparently isolated cases of cholera that surfaced every now and then. Although there are many other cases, the following two, connected to the famous Broad Street pump outbreak, are particularly striking. Snow talked to one of the inspectors appointed by the General Board of Health, Dr. Fraser, who gave him important evidence of the following cases that at first appeared completely unconnected to the cholera outbreak around Broad Street in Soho:

1. A man from Brighton had gone to visit his brother in Poland Street, near the Broad Street pump. When he arrived, his brother had already died and he had no contact with the body. He didn’t stay long, just long enough to for a quick lunch which he accompanied with a brandy mixed with water from the pump. He left again and the next day began to suffer from cholera only to die of it a day later on 3rd September.
2. More importantly, however, Dr. Fraser also informed him of “the following circumstances, which are perhaps the most conclusive of all in proving the connection between the Broad Street pump and the outbreak of cholera” (MCC2, 45). The *Weekly Return of Births and Deaths* of 09 September recorded the death of Susannah Eley, widow of the percussion-cap maker of Broad Street, as having died of cholera on 02 September. Mrs. Eley lived a long way from the Broad Street pump, in Hampstead, an area completely free from cholera. Snow talked to her son, and was told that she had not been to the Broad Street neighbourhood in several years. However, he also found out that she had always been extremely partial to the water from the Broad Street pump and it turned out that a “cart went from Broad Street to the West End every day, and it was the custom to take out a large bottle of the water from the pump in Broad Street, as she preferred it” (MCC2, 44). Moreover, the day she first exhibited cholera symptoms she had been visited by one of her nieces who also drank of the water and then died from cholera. As Snow points out, both Mrs. Eley and her niece, who was from Islington, lived in parts of London that were far away from the Broad Street pump, where there was no cholera at all, and in which the two of them constituted the only isolated incidents in otherwise healthy parts of London.

In addition to these cases, there were many others. Cases such as these simply did not make any sense on the miasma theory, since it was clear both that the area was not infested, because in that case other people would have also fallen ill, and also because the victims were known not to have been to a miasmatic region. Even the modified miasma theory with occasional contagion would have had a hard time explaining these cases, since the victims had

not been anywhere near a patient; thus, explaining them by means of effluvia would also have been unsuccessful.

## 9. Conclusion

Where does this leave us, then? It is clear from an examination of Snow’s various hypotheses about cholera that he used certain principles of reasoning, explanatory power in particular, that his opponents did not. What’s more, Snow took these considerations to be essentially confirmatory, and to provide not just pragmatic, but epistemic reasons for preferring his theory to those of others. In addition, as we have seen, Snow is quite explicit about this. Moreover, it is clear, from the account of Snow’s reasoning above, that explanatory considerations clearly played an overarching role for him. Time and again, Snow points out that his theory can explain what the rivals cannot, both with respect to general claims and also with respect to some of the details that he observed. Snow appeals to explanatory power at virtually every juncture and with respect to a variety of evidence types, for example, statistical evidence (such as the distribution of deaths during cholera outbreaks (related to gender and profession), and certain numbers about the average populations and the duration of epidemics), pathological evidence (such as Snow’s observations that, in cholera, local symptoms of the intestinal region precede general symptoms related to the whole body), and epidemiological evidence (such as links between mortality rates and location). A systematic comparison between Snow’s conclusions and a variety of rival hypotheses shows that Snow was correct in claiming that alternative theories failed to explain several of the phenomena that he could explain in a satisfactory way, and that, for this reason, his theory really was explanatorily preferable to other extant options—something that, in Snow’s eyes, put his theory on more solid epistemic grounds than those of his rivals.

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## References

- Baldwin, P. (1999). *Contagion and the state in Europe, 1830–1930*. Cambridge: Cambridge University Press.
- Bentivoglio, M., & Pacini, P. (1995). Filippo Pacini: A determined observer. *Brain Research Bulletin*, 38(2), 161–165.
- Brody, H., Vinten-Johansen, P., Paneth, N., & Rip, M. (1999). John Snow revisited: Getting a handle on the Broad Street pump. *Pharos Alpha Omega Alpha Honor Medical Society*, 62(1), 2–8.
- Brody, H., Rip, M., Vinten-Johansen, P., Paneth, N., & Rachman, S. (2000). Map-making and myth-making in Broad Street: The London cholera epidemic in 1854. *The Lancet*, 356, 64–68.
- Bynum, W. F. (1994). *Science and the practice of medicine in the nineteenth century*. Cambridge: Cambridge University Press.
- Churchland, P. (1985). The ontological status of observables: In praise of superempirical virtues. In P. Churchland & C. Hooker (Eds.), *Images of science* (pp. 35–47). Chicago: University of Chicago Press.
- Gradmann, C. (2005). *Krankheit im Labor—Robert Koch und die medizinische Bakteriologie*. Göttingen: Wallstein Verlag.
- Hamlin, C. (1990). *A science of impurity: Water analysis in nineteenth century Britain*. Berkeley: University of California Press.
- Hamlin, C. (1992). Predisposing causes and public health in early nineteenth-century medical thought. *Social History of Medicine*, 5(1), 43–70.
- Hamlin, C. (2009). *Cholera: The biography*. New York: Oxford University Press.
- Hill, A. (1955). Snow—An appreciation. *Proceedings of the Royal Society of Medicine*, 48, 1008–1012.
- Howard-Jones, N. (1972). Cholera therapy in the nineteenth century. *Journal of the History of Medicine and Allied Sciences*, 27(4), 373–395.

- Howard-Jones, N. (1984). Robert Koch and the cholera vibrio: A centenary. *British Medical Journal*, 288, 379–381.
- Kukla, A. (1998). *Studies in Scientific Realism*. New York: Oxford University Press.
- Morens, D. M. (2004). Review of cholera, chloroform, and the science of medicine: A life of John Snow. *American Journal of Epidemiology*, 160, 605–607.
- Pacini, F. (1854). Osservazioni microscopiche e deduzioni patologiche sul cholera asiatico. Original in *Gazzetta medica italiana federativa toscana*, Firenze, Vol. 4 (reprint in *Lo Sperimentale*, Firenze, 1924, 78, 277–282).
- Paneth, N., Vinten-Johansen, P., Brody, H., & Rip, M. (1998). A rivalry of foulness: Official, unofficial investigations of the London cholera epidemic of 1854. *American Journal of Public Health*, 88(10), 1545–1553.
- Pelling, M. (1978). *Cholera, fever and English medicine, 1825–1865*. New York: Oxford University Press.
- Porter, R. (1997). *The greatest benefit to mankind: A medical history of humanity*. New York: W.W. Norton and Company.
- Psillos, S. (1999). *Scientific realism: How science tracks truth*. London: Routledge.
- Richardson, B. (1952). John Snow, M.D., a representative of medical science and art of the Victorian era. *British Journal of Anaesthesia*, 24(4), 267–291 (reprinted from *The Asclepiad* of 1866).
- Rosenberg, C. E. (1977). The therapeutic revolution: Medicine, meaning, and social change in nineteenth-century America. *Perspectives in Biology and Medicine*, 20(4), 485–506.
- Smith, T. S. (1831). *A treatise on fever*. Philadelphia: Carey and Lea.
- Snow, J. (1849a). *On the mode of communication of cholera*. London: J. Churchill.
- Snow, J. (1849b). On the pathology and mode of communication of cholera. *London Medical Gazette*, 44, November 2, 745–752; November 30, 923–929.
- Snow, J. (1851a). On the mode of propagation of cholera (Part I). *Medical Times*, 3, 559–562.
- Snow, J. (1851b). On the mode of propagation of cholera (Part II). *Medical Times*, 3, 610–612.
- Snow, J. (1853a). *On continuous molecular changes, more particularly in their relation to epidemic diseases* (pp. 145–175). New York: Hafner (reprint, *Snow on Cholera*, edited by Wade Hampton Frost, 1965).
- Snow, J. (1853b). On the prevention of cholera. *Medical Times and Gazette*, 7, 367–369.
- Snow, J. (1854a). Cholera in the Baltic Fleet. *Medical Times and Gazette*, 170.
- Snow, J. (1854b). The principles on which the treatment of cholera should be based. *Medical Times and Gazette*, 180–182.
- Snow, J. (1855a). Further remarks on the mode of communication of cholera; including some comments on the recent reports on cholera by the General Board of Health. *Medical Times and Gazette*, 31–35. 84–88.
- Snow, J. (1855b). On the chief cause of the recent sickness and mortality in the Crimea. *Medical Times and Gazette*, 457–458.
- Snow, J. (1855c). On the comparative mortality of large towns and rural districts, and the causes by which it is influenced. *Journal of Public Health, and Sanitary Review*, 16–24.
- Snow, J. (1855d). *On the mode of communication of cholera* (second much enlarged ed.). London: J. Churchill.
- Snow, J. (1857). On the outbreak of cholera at Abbey-Row, West Ham. *Medical Times and Gazette*, 417–419.
- Stanford, K. (2006). *Exceeding our grasp: Science, history, and the problem of unconceived alternatives*. New York: Oxford University Press.
- Stanford, K. (2009). Scientific realism, the atomic theory, and the catch-all hypothesis: Can we test fundamental theories against all serious alternatives? *British Journal for the Philosophy of Science*, 60, 253–269.
- Tulodziecki, D. (2007). Breaking the ties: Epistemic significance, bacilli, and underdetermination. *Studies in History and Philosophy of Biological and Biomedical Sciences*, 38, 627–641.
- Tulodziecki, D. (2011). *Underdetermination, methodological practices, and realism*. Unpublished manuscript.
- van Fraassen, B. (1980). *The scientific image*. Oxford: Clarendon Press.
- Vinten-Johansen, P., Brody, H., Paneth, N., Rachman, S., & Rip, M. (2003). *Cholera, chloroform, and the science of medicine: A life of John Snow*. Oxford: Oxford University Press.
- Worboys, M. (2000). *Spreading germs: Diseases, theories, and medical practice in Britain, 1865–1900*. Cambridge: Cambridge University Press.