Thursday, 31 August 1854

A normal day during a cholera epidemic in metropolitan London. Figures from the seventh week of the epidemic, ending the previous Saturday, were available: 847 fatalities from Asiatic cholera in a population greater than two and a quarter million. Worrisome, but not as severe as the number carried off by cholera during the seventh week of the 1849 epidemic. In fact, there were hopeful signs in the current epidemic’s weekly progress: just 5 dead during the first week, followed by 26, 133, 399, 644, 729, and 847 in subsequent weeks (MTG 1854, 249). Was this visitation on the cusp of peaking?

No such luck. To have the individuals I feature in the historical narratives engage in an exchange of theoretical views after all hell broke loose the following day would have been artificial and stilted. So I chose to begin the story on a day of hope in order to discuss theories of cholera causation and propagation that, while tacit knowledge amongst the participants, need to be unpacked if we are to be just in our assessments of actions taken during the point-source outbreak centered in Broad Street. Readers of the daily newspapers and major medical journals would have found many more theories than those I selected. It was a hot and contentious topic. Since the General Register Office regularly surveyed and assessed this literature, I decided to narrow the field to the GRO’s report on the 1848-49 English cholera epidemic, prepared under the direction of William Farr, the examiner and compiler of abstracts and head of the statistical department.

Farr (1852) highlighted eight “theories and analogies,” from which I chose three frequently cited by subsequent commentators who eventually sought to explain the 1854 outbreak in St. James, Westminster parish (v). Farr’s multicausal zymotic theory and John Snow’s alimentary-contagion theory are presented in one extended scenario. The third theory, atmospheric infection, involved assumptions and reasoning common to those contingent contagionists who believed that cholera could be transmitted person-to-person in extremely filthy “fever nests.” As an exponent I chose the experiential-minded clinician, Alexander Stewart, M.D., assistant physician to the Middlesex Hospital, where Florence Nightingale tended cholera victims from Soho during the outbreak.

Of course, first I had to confirm that she actually was there and done that.
Was Florence Nightingale at the Middlesex Hospital in 1854?

My misadventure with Edward Cook.

I’ll begin with a cautionary tale about how I initially deviated from my normal research methodology and wasted a lot of time.

The misadventure began with an impulsive response to a reasonable thought. It occurred to me that my historical narrative should include descriptions of what was happening at the Middlesex Hospital, where many cholera victims were either carried or dragged themselves during the early days of the outbreak in St. James, Westminster. I had recently re-read two medical journal pieces by Alexander Stewart in which he described what happened at the Middlesex Hospital during the “Soho outbreak,” as he called it (the mind boggles at the variety of place-names for the same event). Then I vaguely recalled a biographer’s mention that Florence Nightingale had served as a caregiver at that hospital shortly before departing for the Crimea. Worth looking into, I said to myself. Nightingale’s experiences might permit me to add a perspective from the era prior to formal training of nurses.

These musings occurred as I was walking toward a parking garage on the Emory University campus. I didn’t have time that afternoon to retrace my steps to the main library and undertake a systematic literature search. My choice was to wait a few weeks for my next library trip and make a proper job of it, or (fatal error—the temporary measure I chose) to pop into the Health Sciences library close by the parking garage and check their holdings. The only scholarly work on Nightingale in that library was a biography by Sir Edward Cook (1913), longer in the tooth than the one I had consulted while researching CC&SoM (Vinten-Johansen, et al. 2003), but frequently mentioned as a reliable work of scholarship. I located Middlesex Hospital in Sir Edward’s index, found the page, and read that in August 1854 Nightingale was vacationing at Lea Hurst, the family summer house in Devonshire.

Miss Nightingale cut short her holiday on hearing that an epidemic of cholera had broken out in London. She volunteered to give help with cholera patients in the Middlesex Hospital. She was up day and night receiving the women patients — chiefly, it seems, outcasts in the district of Soho — undressing them, and ministering to them. The epidemic, however, subsided, and she returned to her normal work in Harley Street (I:140).

Bingo! I copied the relevant pages with a hand scanner and headed home.

The next day I transferred the pages from Cook’s biography to my computer and considered my options. “Do nothing until you confirm Cook’s account in recent biographies,” whispered my empirical angel; “put Flo on the back burner until you make it back to a research library.”

“Nah,” countered my impatient angel.
Don’t be such a brick and mortar Luddite; follow Cook’s leads with internet searches whilst the idea is fresh.” Point taken, there might be electronic versions of scholarly works on the internet.

It would be two weeks before I made another research trip, and although I knew that it would be much faster to pull more recent biographies of Nightingale off library shelves than Google my way to confirmation, impatience won out. I spent far too much time the next two weeks pondering my reactions to the paragraph in Cook’s biography and following premature or inchoate lines of research. For example, I checked to see if Nightingale had mentioned her experiences as a caregiver at the Middlesex in one of her letters. Nothing came up in Hugh Small’s online version of Sue Goldie’s calendar (short summations) of Nightingale correspondence.

But Cook had had unrestricted access to Nightingale’s papers when writing the biography, so I assumed he had come across something about her ministrations during a Soho cholera outbreak in August 1854. Cook’s time-frame didn’t jibe with the major outbreak for which the Broad Street pump turned out to be the culprit, but I nonetheless revisited a well-worn path through London medical journals for August and early September 1854, hoping to find something I had missed earlier that would substantiate Cook’s comments. The only suspect, an unlikely one at that, was a minor uptick in deaths among residents well to the west of Broad Street during the second half of August.

On the other hand, perhaps Cook had meant the big one that began Friday 1 September and simply gotten the date wrong. If so, how would Nightingale have learned about it in a rural part of Devonshire? I looked for telling articles or notices in the Times via an online data-base; the first report of a major cholera outbreak underway in Soho appeared on Monday 3 September. I checked railroad routes, connections, and timetables between London and Whatstandwell, the nearest railway station from Lea Hurst. The worst of the epidemic was over by Tuesday afternoon, so even if she took the train back to London on Monday and went straight away to the Middlesex Hospital, she would have still have missed the massive influx of cholera patients that appeared during the previous weekend. Cook’s account wasn’t panning out. We can all make mistakes, and perhaps I had stumbled upon one by Sir Edward Cook.

But mine was a mistake as needless as it was dumb. I know better than to engage in premature and stab-in-the-dark speculation like a spinning top, bouncing helter-skelter from one topic to another. I had permitted my fascination with the wondrous opportunities of online research to override a method I’ve used, and taught others, for many decades: 1, Formulate an historical problem that sets up a line of research; 2, undertake a focused review of primary and scholarly
literature; 3, draft a preliminary thesis statement to guide me as I begin “writing up” the evidence selected from this literature; and 4, revise the thesis statement, if necessary, as my interpretation of the evidence evolves. Then repeat this process, over and over again, for each segment of the entire argument (see “The Research Essay” in Appendix A).

The next section demonstrates the first three steps in this method as applied to Nightingale’s potential involvement in the 1854 cholera outbreak in St. James, Westminster. Jump to the next section if you find my method at odds with what works quite well for you. There are many ways to do history.

1. Historical problem

Was Florence Nightingale at the Middlesex Hospital during the Broad Street cholera outbreak; if so, in what capacity?

2. Literature review

At my next research-library opportunity, I undertook a review of the available literature, guided by my two-part historical problem.

First I had to decide whether to begin by examining primary or secondary sources. Since eventually I would be looking at both, a blind search of Nightingale’s published works and correspondence seemed silly if other researchers had already found an answer that I could confirm later with a targeted search in primary sources. My historical problem was a narrow one, so it made sense to begin with secondary sources.

A catalogue search yielded nine titles that seemed to be interpretive studies of Nightingale. I pulled them all, found a free table in the stacks, organized the books in reverse order by publication date (most recent on top, Cook on the bottom), and set up my laptop computer and portable scanner. I decided to approach the topic from ignorance and allow the Secondary Way to inform me about the lay of this land and guide me to supporting primary sources — in short, I hoped for a clinching research-confirmation loop.

Ideally, scholarship builds on and corrects, as necessary, the work of our predecessors. I opened Mark Bostridge’s biography of Florence Nightingale (2008). A skim of the preface, table of contents, notes and bibliography suggested a comprehensive and balanced study. Chapter 8 dealt with Nightingale’s tenure as superintendent at the Establishment for Gentlewomen during Illness on Upper Harley Street, London, which I read quickly along with the relevant endnotes. Suggestion confirmed; this is a serious, scholarly study. Bostridge devoted one paragraph to her “temporary leave of absence from Upper Harley Street” as a volunteer at the Middlesex Hospital from 31 August until “the intensity of the epidemic receded” (199). I made electronic copies of chapter and endnotes, jotted down bibliographical information on the two sources he cited on her Middlesex Hospital so-
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journ, and then turned my attention to the other books I had pulled from the shelves.

I searched index and table of contents in every book for Middlesex Hospital, cholera, the Upper Harley Street institution for ailing gentlewomen, and the years 1853-54. I scan-copied everything I found, even seemingly trivial or insignificant comments, on these subjects; and then preliminarily sorted my findings into four categories:

- No mention of Nightingale at the Middlesex Hospital — 3 (Cope 1958; Hobbs 1997; Small 1999).
- Documented evidence that Nightingale nursed patients at the Middlesex Hospital during the St. James/Soho cholera outbreak — 4 (Bostridge 2008, 199; Haldane 1931, 98; Huxley 1975, 53-55; Woodham-Smith 1950, 79-80). All four cited one or two letters by Elizabeth Cleghorn Gaskell from October 1854.
- Undocumented accounts that she was at the Middlesex Hospital during a London cholera epidemic — Cook (1913); Bishop & Goldie (1962).
- Some documented evidence that Nightingale was at the Middlesex Hospital, but she deliberately misled Mrs. Gaskell about what she did there — 1 (Smith 1982, 16-17).

All five secondary sources offering supporting documentation relied on Mrs. Gaskell’s letters; a quick search of the library catalogue identified re-prints in two scholarly collections. I then searched Florence Nightingale and identified five volumes containing selections from Nightingale’s works and letters during the time period that interested me. I checked them out to study at home.

Eventually I read and re-read everything I had found at the library and updated my chart with information gleaned from the five scholarly collections of Nightingale writings: three more “no mentions” (Harthill 1996; McDonald 2004; Vicinus & Nergaard 1990) for a total of six; and two more undocumented assertions that she was at the hospital nursing cholera victims (Bishop & Goldie 1962, 132; McDonald 2001, 27) for a total of four.

I decided that the half-dozen no-mentions among the secondary sources did not in themselves constitute grounds to answer my historical problem negatively. Two of the three were monographs on specialized subjects, and the third was a brief biography. The fact that none of these scholars chose to mention the 1854 London cholera epidemic did not seem decisive; it could simply mean that it wasn’t significant for their interpretations. Shifting to the Primary Way, I was disappointed that two collections of selected Nightingale letters made no reference to this epidemic or the Middlesex Hospital, but, as before, I could not expect these scholars to share my research interests when they made their selections. The absence of relevant keywords in the index of the volume on public health from Night-
ingale’s *Collected Works* was particularly perplexing since the editor had stated, without citation, in the introductory volume that she had “nursed patients at the Middlesex Hospital, notably ‘Soho outcasts’” (McDonald 2001, 27). Were the latter words Nightingale’s, and if so, where did they appear in the *Collected Works*? Were they the words of an editor who did not wish to seem politically incorrect? Or had the editor also taken Cook at his word, without the follow-up I was undertaking?

Since five “she-was-there” commentators cited Mrs. Gaskell, I shifted my attention to her letters. In October 1854, Elizabeth Gaskell’s first visit to Lea Hurst partially overlapped with the annual holiday to which Florence Nightingale was entitled as the Superintendent at the Establishment for Gentlewomen during Illness. Mrs. Gaskell wrote two letters during her stay that included extracts from descriptions Florence Nightingale ostensibly gave her parents, sister, and house guest about her experiences at the Middlesex Hospital. In the first, a letter started on Wednesday evening, 11 October 1854, Mrs. Gaskell wrote that Nightingale began her “supervision” at the hospital on 31 August, that a flood of cholera patients from “the Soho district, Broad Street especially” overwhelmed her and the rest of the staff for the ensuing forty-eight hours, and suggests she remained at the hospital at least a week (Chapple & Pollard 1966, 305). In a second letter, Mrs. Gaskell elaborated on Nightingale’s “speeches” to the family about nursing prostitutes admitted with pronounced cholera symptoms to the Middlesex Hospital (Chapple & Shelston 2000, 115).

I then reviewed the use of these letters in the secondary sources I had consulted on Nightingale. Haldane (1931) appears to have been the first to print Mrs. Gaskell’s two letters nearly in full, including the descriptions of Nightingale at the Middlesex Hospital (93, 98), although O’Malley (1931) was close behind (207-08); either could have been the source for Woodham-Smith’s (1950) uncited snippets in her biography of Nightingale (79-80). It turns out that Cook (1913) was aware of the first letter Mrs. Gaskell wrote from Lea Hurst, and it was the likely basis for his undocumented assertion about Nightingale at the Middlesex Hospital; for he quoted other passages from this letter elsewhere in the biography. Bostridge (2008) based “the story of FN’s work at the Middlesex” on Mrs. Gaskell’s letters (583). He dismisses Smith’s contention that her version is flawed on the grounds that Nightingale’s sister, Parthenope, confirms it in an unpublished memoir, circa 1857 (583).

Whoops! This could be a she said, he said situation. Smith’s claim required closer scrutiny. Francis Smith’s (1982) thesis is that Nightingale could not have “supervise[d] the admission of female cholera victims” at the Middlesex Hospital, as recorded by Elizabeth Gaskell “later in August while both were on holi-
day at . . . Lea Hurst”; this claim is the invention of a “titillating fabulist” (16-17). Smith selects a passage from Mrs. Gaskell’s letter of 27 October 1854, where she summarized Nightingale’s description of a constant stream of prostitutes suffering from cholera who “‘staggered off their beat’” to the hospital for treatment (16, citing Chapple and Pollard 1966, 318).

Smith (1982) then marshals evidence from relevant Admission Books in the hospital archives that disprove “this story” (16). First, Smith argues that Nightingale’s name is conspicuous by its absence from a report on how the hospital coped with an unexpected volume of cholera patients from late August through the third week of September, 1854. Second, none of the female patients named in the hospital’s Admissions Book were listed as prostitutes; according to Smith, Middlesex Hospital would not have jeopardized its reputation among its primary clientele — male patients, “mostly ‘respectable artisans’ from a neighbouring piano factory” — by admitting female prostitutes. Third, the first female admission with “‘undoubted [Asian] cholera’” did not occur until 5 September, well after the epidemic’s peak (presumably quoting from an Admission Book). In short, Mrs. Gaskell “story is not supported by the Middlesex Hospital archives” (16); the novelist was the unwitting source of an enduring fable that Florence Nightingale created for herself.

Francis Smith, however, is hoisted by his own petard. First, negative evidence from the records he consulted at the hospital archives only show that Nightingale was neither a regular staff member nor amongst those who responded to the hospital’s call for volunteers on 2 September. As to Smith’s second point, prostitution was not a registered vocation in Victorian England and it is unimaginable that any person admitting female patients to the Middlesex Hospital would have assigned such an appellation. Although this hospital did normally limit admissions to patients bearing a letter of recommendation from a subscriber, these were not normal times for any hospital whose catchment area included St. James, Westminster and St. Anne’s, Soho. In this instance, a simple newspaper-search would have shown that the General Board of Health issued a directive that all hospitals should admit anyone presenting symptoms of cholera and choleraic diarrhea during this horrific cholera outbreak. Third, Smith was misled by a hair-splitting nicety in contemporary medical terminology. There was much disagreement about the symptoms of Asiatic cholera. Whoever authored the report Smith cited had a different notion of what constituted “undoubted cholera” than Septimus Sibley, Middlesex Hospital registrar, who listed four or five females (depending on the gender of a waistcoat maker) dying from “cholera maligna” at the hospital on 1 and 2 September (UK, GRO 1854, 309). In short, Smith’s questionable evidence left me unconvinced that Nightingale was a
“titilating fabulist” or that Mrs. Gaskell’s account is unreliable.

Had Sir Edward Cook inadvertently misled Smith, as he did me? Smith (1982) has high esteem for Nightingale’s early biographer; “he is accurate,” amongst a host of other accomplishments noted in the preface (xii). In essence, Smith considers every subsequent commentary on Nightingale, including his own, essentially derived from Cook. Smith follows Cook’s chronology, stating that Nightingale “moved” to the Middlesex Hospital in August 1854 and met Mrs. Gaskell “later in August” at the family’s summer house (16). By this reckoning, Nightingale would have been on holiday in Derbyshire, not at the Middlesex, when cholera burst forth in St. James, Westminster. Best I can tell, that’s why Smith thought the evidence he selected from hospital archives was so telling; they reveal Nightingale had been at the hospital in some undisclosed capacity but had left before this major outbreak.

Unlike Smith, I had found no reason why Cook’s chronology should trump Mrs. Gaskell’s. She wrote the letter Smith cites in October, shortly after arriving at Lea Hurst for a visit that only overlapped a few days with Nightingale’s “fortnightly” holiday that ended 10 October (Chapple & Pollard 1966, 305-07; Chapple and Shelston 2000, 115). That meant Nightingale could have been at the Middlesex Hospital during the height of the cholera outbreak and its aftermath in September. But I was still uncomfortable with Bostridge’s endorsement of Mrs. Gaskell’s account simply because it parallels what he found in a manuscript by Parthenope Nightingale. After all, Florence Nightingale’s sister seems to have been at Lea Hurst throughout September 1854, so whatever she wrote in her memoir about Flo’s doings in London then would have been second-hand knowledge, at best. It was time to see if the Primary Way could sort this matter.

I was aware of a document that Bostridge does not cite, written by someone closer to the action than either Mrs. Gaskell or Parthenope Nightingale, which could be used to assess the reliability of Mrs. Gaskell’s account. The Medical Times and Gazette of 7 October 1854 contained an article by Alexander P. Stewart, M.D., assistant physician to the Middlesex Hospital. Stewart had prepared a report with assistance from his colleagues containing “the complete statistics of the late fearful outbreak, so far as we have had to do with it” (364).

I compared a critical passage in Mrs. Gaskell’s letter to Catherine Winkworth with four passages from Dr. Stewart’s article on how the Middlesex Hospital coped with cholera victims coming from “the Soho district of St. James’s parish” during the first week of September (reproduced on the next page). Mrs. Gaskell took several evenings to write this letter. She states that Nightingale went “on the 31st of August to take superintendance of the Cholera patients in
all the people who write about poor George Duckworth’s death say that Cholera is not infectious i.e. does not pass from one person to another. Mr Sam Gaskell says so too; and last authority Miss Florence Nightingale, who went on the 31st of August to take superintendence of the Cholera patients in the Middlesex Hospital (where they were obliged to send out their usual patients to take in the patients brought every half hour from the Soho district, Broad St especially,) says that only two nurses had it, one of whom died, the other recovered; that none of the porters &c had it, she herself was up day & night from Friday evening to Sunday afternoon, receiving the poor prostitutes, as they came in, (they had it the worst, & were brought in from the ‘beat’ along Oxford St—all through that Friday night,) undressing them—and awfully filthy they were, & putting on turpentine suppes &c all herself to as many as she could manage—never had a touch even of diarrhea. She says moreover that one week the chances of recovery seemed as 1 to 10, but that since the chances of recovery are as 20 to 1.

Elizabeth Gaskell to Catherine Winkworth, Wednesday evening, 11 October 1854; Lea Hurst (Chapple and Pollard 1966, 305).

the Middlesex Hospital has been the receptacle of a very unusual number of the victims of the late epidemic in the Soho district of St. James's parish.

On the morning of Friday, the 1st inst., however, the numbers suddenly increased. Above a dozen were admitted in the course of a few hours; and, as it quickly became evident, from the fresh applications that came continually pouring in, that the demand for beds was only beginning, our first care was to discharge as many patients, both Surgical and Medical, as could with any propriety be sent home. Their places were soon filled by patients in all degrees of collapse, who were admitted, to the number of 57, before mid-day of Saturday, the 2nd instant, a very large proportion of these being, on admission, far beyond the reach of remedial skill. Such being the case, it is not wonderful, that of nearly 90 cases admitted during the fifty hours ending at one p.m., on Sunday, the 3rd of September, forty were at that hour already dead. That life, however, was prolonged for two, four, or six hours, in many even of the worst cases, by the warm bath, the mustard emetic, and the counter-irritants used in almost every case, does not admit of reasonable doubt.

The whole duties, therefore, overwhelming as they were, from the morning of the 1st till midday of the 2nd of September, fell to be and were discharged by the ordinary staff of the Hospital. To lessen this extraordinary pressure, which, if long continued, must have been attended with disastrous consequences, a large temporary addition was made as quickly as possible to the staff of attendants;

only two of the inmates have contracted the disease. One of these, after disregarding the presonitory symptoms, which were present for 12 hours before the fatal seizure, was allowed to pass into a state of hopeless collapse before advice was applied for. The other, who had had severe diarrhœa for eleven days before she made any complaint, is now completely convalescent.

Alexander P. Stewart, “Cholera in the Middlesex Hospital” (1854a, 364-65).
the Middlesex Hospital,” that an unexpected influx of patients began arriving at the hospital the following day, that Nightingale took part in nursing them, and that she remained at the hospital long enough to see a reduction in virulence as the epidemic abated.

Mrs. Gaskell composed this passage on Wednesday, 11 October, the day after Nightingale had cut short her holiday and returned to London. The context leads me to think that she had heard Nightingale describe her experiences the previous week, that is, before Stewart’s article appeared. However, someone who wishes to compound Nightingale’s character flaws as a plagiarizing fabulist could claim that she cribbed from Stewart’s article if it could be shown that the 7 October issue of *MTG* had been sent to Lea Hurst prior to the account documented by Mrs. Gaskell. I have found no evidence to that effect.

There are sufficient parallels between Mrs. Gaskell’s account of Nightingale’s first week at the Middlesex Hospital and the selected passages from Stewart’s article to convince me that Nightingale must have worked at that hospital during the point-source cholera outbreak in St. James, Westminster. Both authors are discussing the same local outbreak: Mrs. Gaskell refers to “the Soho district, Broad St especially,” which lies within the parish of St. James, Westminster mentioned by Stewart. Only an “insider” in a position of some authority would have known (1) that no more than two nurses came down with cholera, one of whom died and the other survived; (2) that the hospital discharged non-critical patients on Friday 1 September to free up beds for the mass of new cholera patients who required admission; (3) that the rush began mid-day on Friday 1 September and continued for forty-eight hours; (4) that the therapeutic policy of this particular hospital called for the employment of counter-irritants such as *stupes* — cloths moistened in hot turpentine; and (5) that very few of the cholera patients admitted to a hospital ward the first week of the epidemic survived the disease. In short, I now felt comfortable in accepting Mrs. Gaskell’s account that Florence Nightingale had been at the Middlesex Hospital during this cholera outbreak. But in what capacity?

Mrs. Gaskell wrote that Nightingale was at the hospital as a superintendent of patients, which meant that her intended task was to manage workers involved in patient care. Stewart is explicit that “the ordinary staff of the Hospital” handled the unexpected influx of cholera patients that began late Friday morning, 1 September, and lasted for twenty-four hours, at which point they were so knackered that “a large temporary addition was made” for an unspecified period. Recall that Smith could not locate Nightingale’s name among those of regular staff members and volunteers listed in a report on the outbreak housed in the Middlesex Hospital Archives. Not surprising. Nightingale was not on staff; she was superintendent of the Establishment for Gentle-
women during Illness on Upper Harley Street until mid-October 1854. She wasn’t a volunteer, either; Mrs. Gaskell is very explicit that Florence Nightingale arrived at the hospital on Thursday 31 August, and, according to Dr. Stewart, the hospital did not send out an appeal for volunteers until mid-day on 2 September. What could Nightingale have been doing at the hospital since 31 August that Dr. Stewart would have considered “ordinary”?

Nightingale’s experience at the Upper Harley Street establishment for ill gentlewomen only qualified her for two staff positions at a major London hospital: as the Matron, who managed domestic matters, and as a Sister, sometimes termed a Head Ward Nurse, who superintended salaried nurses. Domesticity had consumed so much of Nightingale’s time in her first year at the Upper Harley Street establishment that she had recently given its steering committee notice that she would soon depart. So it seems highly unlikely that she would have considered replacing the hospital Matron, even for a short period, and equally unlikely that the medical and surgical staff at Middlesex Hospital would have sanctioned such a temporary appointment without a formal interview process (which chatty Flo would surely have mentioned to someone in her family, and Bostridge duly noted).

Instead, I thought it very probable that Nightingale took the place of one of the regular ward Sisters who was scheduled for leave or a holiday in September, normally a slow period since the medical students were still on summer recess and the hospital deferred all but emergency surgeries until their return by the beginning of October. The Matron could make such an appointment on her own. Subbing for one of the regular Sisters at the hospital falls within Mrs. Gaskell’s description that Nightingale went there to superintend the care of patients. Nightingale’s experience as supervisor of salaried nurses for a year in what amounted to a small, private infirmary matches what the matron of a large metropolitan hospital would expect from a locum Head Ward Nurse.

Although I had already come up empty-handed during the literature review from a search of Nightingale’s Collected Works about being at the Middlesex Hospital during the 1854 cholera epidemic, I did find two suggestive summations by Sue Goldie (1983) in the “Calendar of the Letters of Florence Nightingale” (now available online, thanks to Hugh Small — URL in the bibliography for Goldie 1983; I searched for “Middlesex”). In 1859 Nightingale recommended the Middlesex Hospital as the best in London (FN to her mother, 20/3/59), and in 1862 she described it as a great London hospital (FN to Douglas Galton, 9/11/62). Were these recommendations just based on a previous on-site investigation, part of the survey of metropolitan hospitals she undertook in the winter and spring of 1854? Perhaps, but they could just as well re-
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reflect an intensive, personal experience during the 1854 cholera epidemic, as described to her family in Mrs. Gaskell’s presence.

3. Preliminary thesis statement

Florence Nightingale was probably substituting for one of the regular head ward nurses at the Middlesex Hospital when the Soho cholera outbreak began, and she remained in that role until the outbreak was essentially over.

I employ a preliminary thesis as the starting point for writing narratives and a lodestone for choosing lines of research required to answer questions that emerge as I write. The assumption is that the preliminary thesis will surely be tweaked, possibly discarded, depending on what happens as I write and research. If I end up with something satisfactory, I revise the thesis statement to reflect the argument imbedded in the narrative.

Crafting the Nightingale narratives

The opening narrative episode is in two parts, both about Florence Nightingale. It begins at the Establishment for Gentlewomen during Illness on Upper Harley Street. I settled on a meeting with Mrs. Clarke, the establishment’s Matron, for this imaginative scenario. No written evidence documents such a happening on this day. But it seems historically probable that these two women would have met at some point to discuss how the establishment should function while Nightingale put in some time at the Middlesex Hospital.

The tone of the narrative reflects my sense that there was nothing untoward or underhanded in Nightingale’s decision to double her superintendence responsibilities (as Smith, 16, suggests there was). Although two rooms at the establishment were set aside for her, she was under no contractual obligation to live there, or even be on premises every day. The establishment’s general council as well as the Ladies’ Committee, with whom she communicated directly, knew that she had a refuge in St. James Square, Pall Mall, and used it regularly; she shared the rent with an aunt who lived outside London but used the rooms when she came to the city (Bostridge 2008, 193).

Nightingale was superintendent from late April 1853 until mid-October 1854, although she did not take up residence in London until the establishment, formerly at 8 Chandos Street, formally moved to 1 Upper Harley Street in early August 1853. She wanted to select her own matron; the committee agree on the condition that Nightingale paid the matron’s salary. She chose Mrs. Clarke and gave her responsibility for daily domestic matters, maintaining housekeeping accounts, and control of the petty cash account (the following letters in Goldie 1983: FN to Lady Canning, 29/4/53; Add.MSS.45796.f.17, 3_681, 29/4/53). Mrs. Clarke worked tirelessly beside her boss at first but apparently became less reli-
able thereafter (Bostridge 2008, 194); she seems to have become homesick. Nightingale wrote her mother at the end of July 1854 that Mrs. Clarke wanted to leave the establishment and return to Sheffield after completing their joint contractual obligation of serving twelve months at Upper Harley Street (Goldie, 3_789 26/7/54).

Three nurses, one for each floor, handled routine care of between a dozen and a score of temporarily invalided patients. Stays were supposedly limited to two months, but they could be extended at the discretion of the Ladies’ Committee, and often were. Two medical men gave gratis service to the establishment: Mr. William Bowman, assistant-surgeon at King’s College Hospital with a residence in Golden Square; and Dr. Henry Bence Jones, physician at St. George’s Hospital with a residence in Lower Grosvenor Street. Either medical man could be called in an emergency (Bostridge 2008, 194). I confirmed their affiliations by checking listings for “Metropolitan Hospitals & Medical Schools,” Lancet (17 September 1853):265-66 and (16 September 1854):234-35, and I found their residences in the London Medical Directory 1846, 17, 85. If an emergency did occur at the establishment while Nightingale was at the Middlesex Hospital, she could have come at the end of a shift since less than a kilometer separated hospital from establishment.

Other details in the imagined scenario between Nightingale and Mrs. Clarke were taken from Bostridge 2008, 188-98; Vicinus & Nergaard 1990, 65-68; and FN to Lady Canning, which contains Nightingale’s specifications for rehabilitating the private residence at Upper Harley Street into a bona fide care-taking establishment (Add.MSS.45796.f.39, in Goldie 1983, 3_690 6/1853).

Besides Mrs. Clarke, three nurses, a cook, a male servant, and at least one female servant assisted Nightingale at the establishment. Of these, only Mrs. Clarke and the male servant would have been involved in Nightingale’s departure for the Middlesex Hospital. I thought the male servant deserved to be mentioned by his full name, but I did not have access to the establishment’s archive. However, Bostridge (2008) quotes from an 1853 letter in which Nightingale mentioned retaining “John, the Cook & Nurse Smith” from the Chandos Street staff (194), which I confirmed by consulting Goldie; FN to father and mother, (Add.MSS.45796.f.50, in Goldie 1983, 3_713 8/53). If this “John” had been employed at the establishment in March 1851 and present when the enumerator appeared at 8 Chandos Street, then his full name could be in the 1851 England Census.

I used the online Ancestry Library Edition via Michigan State University Libraries. If you are unfamiliar with this resource, here are the steps I took to navigate it to a successful outcome:

- On the home page, I selected U.K. Census Collection, then 1851 England Census.
- Since I did not know the surname, I had to find the particular enumerator’s page contain-
ing 8 Chandos Street, which meant that I had to “Browse this collection.”

- In the drop-down menu for county, I selected Middlesex.
- Next one must select a civil parish. The simplest way to narrow the list is to locate the address on a contemporary map. I consulted London Sheet 61 of the Ordnance Survey (Alan Godfrey, 1986); Chandos Street was in the parish of St. Marylebone, situated just north of Cavendish Square.
- The drop-down menu offered six possible sub-registration districts, one of which was Cavendish Square, which I selected.
- Twelve Enumeration Districts appeared. From here on, luck determines how long you have to slog. I began with District 1, and viewed the description page for successive districts until I found Chandos Street listed near the end of streets covered in District 7.
- There are 92 enumerator pages for this district. I worked backwards and found 8 Chandos Street on Ancestry’s page 71 ( Enumerator’s page 69). Barely legible on the bottom line was John Something-blurred, which I initially deciphered as John Strachim, 40, servant, born in Kent Deal.
- I needed to clarify and confirm the enumerator’s cursive script. I returned to the home page for the 1851 England Census and typed in my interpretation of the enumerator’s spelling. Under Residence, I selected Middlesex for county and typed St. Marylebone as the civil parish.

Under Personal, I selected male, Servant as the relationship to the head of household, and entered 1811 for birth year (+/- 5). For Birthplace, I entered England, county Kent, and Deal as the parish or place.
- I clicked Search, and the first entry was for a John Strachin who, other than the spelling, matched what I had already located:

As a final check, I clicked to view the record for John Strachin, which brought me to the enumerator’s page I had previously found. I now had a surname for John, the male servant at the Establishment for Gentlemen during Illness. He deserves to be remembered as much as his famous employer.
Another purpose of the opening narrative scenario is to suggest Nightingale’s probable state of mind on 31 August 1854 and pose an answer to the question: Why would she have wanted to do something at the Middlesex Hospital while still officially superintendent of the ladies’ infirmary in Upper Harley Street?

We know Nightingale was a short-timer at the ladies’ infirmary. In early August she completed her commitment to serve as superintendent for a year, and in a quarterly report she gave notice of her intent to leave, probably within three to six months (“To Committee of the Institution,” in Goldie 1983, 1854). She did not tell her bosses on the Ladies’ Committee that she had already interviewed for the post of Matron/Nursing Superintendent at King’s College Hospital (FN to her mother, in Goldie, 26/7/54). However, after completing her investigation of London hospitals early in the summer of 1854, she had informed selected individuals that she would consider leaving the institution for a post as superintendent of nursing at a major hospital willing to set up a training school under her administration.

But relations with King’s staff during the interview process had turned dicey at best, nasty at worst. It occurred to me that Nightingale might have considered Middlesex Hospital an alternative if negotiations at King’s fell through. Subbing as superintendent of a nursing ward at the Middlesex would have permitted hospital staff and prospective candidate alike many chances to assess each other prior to making any commitments. A win-win opportunity.

4. Revised thesis statement

Florence Nightingale had agreed to substitute as a head ward nurse at Middlesex Hospital in order to suss the set-up and staff there in the event that she decided not to pursue the proposed nursing superintendent position at King’s College Hospital; shortly after she arrived, cholera broke out in St. James, Westminster, and she remained at the Middlesex, which admitted many cholera patients, until after the local epidemic had peaked.

The revised thesis statement covers three elements: what I think happened, when, plus how/why it came about. I’ll add a fourth, why I think the experience was significant for Nightingale, when unpacking her responses during the outbreak itself.

The second imagined scenario in the opening episode, where the Matron at Middlesex Hospital looks through a personnel folder, conveys information about Florence Nightingale from a contemporary’s perspective. This approach seems preferable to third-person narration, which some readers could interpret as authoritative “objective history.” Of course, the scenario is written by me, a commentator, distant in time and place, borrowing a technique used by the Swedish mystery writer, Stieg Larsson (2005). Early on, the reader
receives information about the fictional journalist, Mikael Blomqvist, via a report prepared by another character, Lisbeth Salander (52-59). When Betty read the English translation, we discussed the book and she singled out this section (48-57) as a particularly interesting way to convey information.

At the time I had just begun drafting the episodes about Nightingale. I entered a square bracketed note [to differentiate between my views and the evidence] to consider informing the reader about Nightingale’s background via something being read by the Matron at Middlesex Hospital. At the time I was also experimenting with different ways to adapt James Wood’s notion of “free indirect speech” to historical narratives (Wood 2008, 9; see my Historiographical Unpacking of the Prologue and Appendix D). During one writing session, I reviewed my notes about Nightingale’s informal survey of nursing at metropolitan London hospitals during the spring of 1854. It suddenly occurred to me that an administrator such as the Matron of a major hospital would have expected a letter of reference before letting someone loose on the wards. My initial drafts of this scenario gave Sidney Herbert as the author of such a letter. For he and his wife, Liz, had been very interested in Nightingale’s findings for several reasons, not the least being that as Secretary at War, Sidney Herbert was responsible for what treatment rendered to British troops (Bostridge 2008, 197). When I could not find documentary evidence that Sidney Herbert actually wrote something in her behalf, however, the absence of evidence required me to fabricate an anonymous letter of endorsement or drop the notion entirely. Since there is, in my view, sufficient historical probability that Nightingale would have presented such a letter, I decided that fabrication was justifiable as long as I did not attribute it to Herbert or any other person.

In the imagined letter of reference, information about Nightingale’s superintendence of the Upper Harley Street institute is taken from Bostridge (2008, 188-98), supplemented by Nightingale’s letter of 5 June 1853 to Lady Canning (Vicinus and Nergaard 1990, 66-68). Additional background information came from Nightingale’s letter to her father and mother, in which she says she’s visited most hospitals in Paris (Goldie 1983, 3_691 6/1853; copy at the Wellcome Institute).

I did not have remote access to the archives of Middlesex Hospital that would have permitted me to locate the name of its Matron in August 1854. Instead I located the name of the Matron at the time of the 1851 English Census: county Middlesex>St. Marylebone parish>sub-registration district of All Souls>Middlesex Hospital>2 (H.O. 107, 1486, 2). Mary Jarrow is listed as Matron, and I decided to use her, even though I’m unsure if she was still matron when Nightingale was at the hospital. I did not find an entry for Mary Jarrow in the 1861 English census.
The narrative form:

The first sentence in the opening scenario — John would soon be knocking to say the cab was waiting — is not standard fare in historical narration. Normally, one would encounter a preliminary clause indicating who was doing the thinking. For example, if there existed verbatim documentary evidence of what transpired at a meeting on that day between Florence Nightingale and Mary Clarke, it’s possible I could have employed direct, or quoted, speech such as — Florence Nightingale said, “John will soon be knocking to say the cab is waiting.” If the extant evidence was sufficiently detailed but short of verbatim, it would have been historiographically valid to use indirect speech reported by myself as author, based on that documented account (sometimes referred to as authorial style) — Florence Nightingale looked at the clock and thought, John will soon be knocking to say the cab was waiting. If the evidence was only suggestive on this point, I could have made the indirect speech conditional — Perhaps Nightingale glanced at the clock and wondered if John would soon be knocking. Each of these examples flags an author as the source of the speech: Nightingale in the first instance, myself as the author/narrator in the next two (see Appendix D for Wood’s explanation of these styles, which I have borrowed).

My first sentence, however, is free indirect speech or thought. It’s neither direct speech by Nightingale (there are no quotation marks) nor regular, unquoted, indirect speech where I would state who was doing the thinking either by name or pronoun. Instead, it is indirect style free of flags to the source. The reader is immediately transported into the mind of someone yet to be identified. My purpose in the opening scenario is to convey the gist of Nightingale’s persona as the administrator of a small nursing home who lacked practical hospital and nursing experience. So I left the superintendent nameless since the mere mention of Florence Nightingale might conjure up “lady with the lamp” images from her later work at the British army hospital in the Crimea. The remainder of the opening paragraph is in authorial indirect style to indicate that the opening sentence is from a female’s perspective as she listens to the matron’s complaints.

I begin the second paragraph in authorial style to set up what the superintendent will say thereafter via free indirect speech:

After a few moments the Superintendent began speaking calmly to the Matron. Everything is sorted. . . .

The superintendent’s instructions are presented without recurring indicators of indirect speech such as “she said” or “she continued” because the opening sentence is sufficient to identify the speaker and clarity isn’t advanced by flagging myself as the author.

The rest of the opening scenario and most of the narrative in the second scenario where the
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Matron of Middlesex Hospital, Mary Jarrow, reviews Nightingale’s file is presented in third-person, indirect style reported by myself as author. I shift between such authorial indirect narration and free indirect thought to suggest Nightingale’s state of mind whilst Jarrow sits opposite her.

Both of the opening imaginary scenarios featuring Nightingale are historiographical extensions of documented primary evidence confirmed in my review of the literature. The evidence decides. Follow it carefully, explicate it transparently, and you have the makings of historical interpretation. Get ahead of it and you have the makings, at best, of historical fiction. Once Smith (1982) challenged Mrs. Gaskell’s account of Nightingale’s doings at Middlesex Hospital, subsequent historians are encumbered to justify using it. Bostridge (2008) found confirmation in an unpublished memoir by Parthenope Nightingale, which was unavailable to me. Stewart’s report (1854a) contains details about happenings at the Middlesex Hospital during the cholera outbreak that are similar to what Mrs. Gaskell wrote she had heard from Florence Nightingale for me to believe she was definitely on one of the hospital’s wards early on 1 September 1854, which made it reasonable for me to portray her as arriving the day before, as Mrs. Gaskell also stated. I then simply followed that evidence to reasonable conclusions about what had probably happened in the run-up to her first day at Middlesex Hospital. Everything in the two narrative scenarios I constructed is taken from documented evidence which could also be presented as indirect authorial style alone, without narration.

Additional documentation

Sometime in 1853-54, Nightingale sat for a photographic portrait by Kilburn of Regent Street (Bostridge 2008, xii, plate 21). I scanned a copy of Bostridge’s illustration (at right) and used Photoshop to crop the portrait, eliminate the shadow under her mouth, and simplify the woodwork of the chair for the setting — a hospital matron’s office.

The map insert showing the corner of Upper Harley and Weymouth Streets comes from Reynold’s Map of Modern London (1859), uploaded by Ralph Frerichs to the website he created and manages (<http://www.ph.ucla.edu/epi/snow/1859map/map1859.html>). Part A shows the area lying between the northeastern end of Hyde Park and the southern part of Regent’s Park; I selected Rows G-I, Columns 10-12 for the insert. Marylebone Road is the major thoroughfare just south of Regent’s Park. Although this map was created after the date of the narrative, it precedes the unification of Upper and Lower Harley Street into Harley Street in 1866. The Upper Harley Street one finds
today is Brunswick Place on Reynold’s map.

The new premises for the Establishment for Gentlewomen during Illness was a three-story house, plus attic and basement, at 1 Upper Harley Street; a stable was in the rear. Today, this property is numbered 90 Harley Street (Bostridge 2008, 191).

**Cholera Theories**

While roughing out a draft of Snow’s oral-fecal version of the doctrine of contagion, I couldn’t get William Farr and Alexander Stewart out of my mind. Why those two chaps? Neither was a contagionist? And then it struck me how historically unfair and inaccurate it would be to ignore two contingent contagionists who disagreed with the bulk of Snow’s views on the cause and transmission of cholera during the 1854 epidemic. I decided to present Farr’s ideas as an explanatory interlude during a meeting with Snow on August 24, and Stewart’s in a hypothetical meeting with Matron Jarrow and Nightingale later the same day. The narratives and explanatory commentaries about all three historical figures incorporated several predominant theories of epidemic diseases in England during the middle third of the nineteenth century.

**The epidemic constitution**

The most commonly held view of cholera as a disease amongst English medical men and sanitarians was anticontagionism, associated with the writings of Thomas Sydenham (1624-89). He coined an expression, the “epidemic constitution,” for a congeries of environmental changes, chiefly seasonal atmospheric conditions, variable soil and vegetation conditions, and temperature changes, believed to interact with stagnant bodily humors to produce diseases not endemic to a locality. In other words, Sydenham used constitution to mean two complementary things: the peculiar and ultimately unknowable combination of environmental factors that generated specific epidemic diseases; and the degree of physical vitality of the individuals in affected localities, interpreted essentially within the Hippocratic humoral framework. Everyone in a given locality with an “epidemic constitution” was exposed to its morbid influence, but predisposition determined which individuals came down with physiologically disruptive fevers and the extent of their infirmity.

The “epidemic constitution” was a congeries of unknown causal factors, beyond full human understanding. Therefore, Sydenham believed that medical men only had one option when epidemic diseases appeared: employ experience at the bedside and pharmacological empiricism in search of remedies effective for each unique disease. Sydenham’s prototype for disease specificity was ague (later renamed benign tertian malaria) since judicious administration of ground cinchona bark seemed to cure ague but have no beneficial effects in other epidemic
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diseases (actually, it only suppresses symptoms). Historiographically, it was not necessary for me to analyze Sydenham’s seventeenth-century notion, “epidemic constitutions of the season,” in order to introduce the three major theories in play during the mid-nineteenth century. But it was necessary to introduce Sydenham. I located sufficient background information on this esteemed clinician in two readily accessible books: Porter (1997, 230) and Hamlin (2009, 156-57). Hamlin, 2002 was especially valuable in helping me understand how Sydenham’s dual usage of constitution (epidemic and predisposing) remained central to the thinking of various non-contagionists in the mid-nineteenth century.

I did need to investigate what mid-nineteenth century medical men thought was still useful in Sydenham’s writings on epidemic fevers and the treatments he recommended. I consider Gavin Milroy’s review (1847) especially helpful in this respect. Rothstein (1972, 28-29) explains why Sydenham and his followers considered cinchona bark a valid therapy for intermittent fevers.

**Asiatic cholera**

Cholera, long endemic in parts of India, began a pandemic progress in November 1817 near Calcutta that would eventually spread westward to Bombay, south to Ceylon, and as far east as China. Many English surgeons and physicians spied signature characteristics of Sydenham’s “epidemic constitution” model in reports sent from India. They pointed to descriptions by army surgeons of instantaneous outbreaks, with the greatest number of victims occurring in the first few days; cholera suddenly appearing in areas where it had never been endemic, including localities infamous for deleterious marshes; outbreaks that could not be traced to human contact but could be associated with the movement of prevailing winds; and most tellingly, medical personnel treating cholera victims rarely, if ever, came down with the disease themselves. Such evidence strongly suggested that cholera was a typical epidemic disease; the fever that occurred in people susceptible to its peculiar “epidemic constitution” never eventuated in a morbid, infectious *virus* that could produce cholera when inhaled by healthy people (note the different meaning of virus from that currently used in biomedicine).

A few observers in India, however, weren’t convinced that cholera was a common epidemic on the Sydenham model. There were no seasonal differences in virulence; cholera appeared whether it was hot or cool, wet or dry. Critics of atmospherically induced epidemics also cited positive evidence that cholera was a contagion. They offered evidence of outbreaks that had progressed from village to village, often in directions contrary to wind movements, and never appearing in two distinct parts of a province at the same time (as one would expect if cholera were spread by something general in the atmosphere).
Cholera only occurred in villages after the arrival of persons from areas where the disease already prevailed, whereas isolated villages escaped the scourge entirely. Some hospitals reported that most, sometimes all, medical attendants and caregivers became ill with cholera. That could only happen if pathological processes followed the contagion pattern in which a morbid virus produced in victims finds fresh material for its reproduction in healthy people who are in close contact with the ill.

But how does it pass from person to person? So far, there was no evidence of the strict, contact-based contagion that occurred with smallpox. Too many caregivers had touched cholera victims with impunity to invoke direct contact as a mechanism of communication, and the evidence for indirect contact via fomites (clothing, bedding and other objects that had been in contact with diseased victims) was inconclusive, at best. Some wondered if the comparison with smallpox was misguided. The mortality from cholera was significant, but hardly comparable to smallpox. Perhaps cholera was a minimally contagious disease, like typhus; that might explain why it was so deucedly difficult to trace its progress via specific individuals in many instances.

*CC&SoM* (165-72), Hamlin (2009, for detailed context), and Durey (1979) offer secondary overviews of early British responses to the first cholera pandemic that began in India in 1817, as well as the second which reached England in the autumn of 1831. Among primary sources, I recommend Corbyn and Blaine’s entry in *Medico-Chirurgical Transactions* available on JSA&RC. The *Times*, the *Lancet*, and the relatively newer *London Medical Gazette*, if accessible, are especially helpful in assessing the general mood in England as the pandemic of Asiatic cholera came ever closer.

**Contingent contagion**

In 1831 another pandemic originating in India brought widespread fear that Asiatic cholera would reach England for the first time. Contagionists saw an ineluctable, north-westward progress of outbreaks which they interpreted as proof that any place where humans travelled was susceptible; why should England be an exception? Anticontagionists saw telltale geographical gaps among the outbreaks, undeniable proof in their minds that Asiatic cholera was an epidemic disease spread by prevailing winds, not human contact; its epidemic constitution would determine if England was affected, and if so, its virulence would likely be limited to those already predisposed to epidemics of Sydenham’s *cholera morbus* (summer diarrhea).

James Johnson, however, an Irish born London physician, thought the two warring camps had ground in common. In June 1831, he sought to stiffen the British spine as rumors of an inevitable calamity of contagion roiled the body public. He argued in a letter to the *Times* that Asiatic
cholera did have some signature characteristics of a classic epidemic disease. Like other epidemics on the Sydenham model, Asiatic cholera probably sprang from unknown chemical reactions in the bowels of the earth; the disease seemed counter-intuitively pervasive when conditions were optimal, whereas actual outbreaks were very localized; and normally, the disease was non-contagious and limited to predisposed individuals. However, Johnson spied credible evidence of person-to-person transmission where people lived in especially filthy conditions at low elevations. In such circumstances, the bodies of cholera victims produced a morbid material that was infectious; effluvial vapors from the exhalations and skin of the ill suffused the room where they lay and anyone in close proximity for an extended period of time was at risk of contracting cholera. A worrisome, but very manageable problem, according to Johnson. With “proper attention to cleanliness, ventilation, and separation,” contagious infection can be halted and Asiatic cholera returned to its typical, non-contagious nature (June 1831).

Johnson repeated his views in October 1831 that cholera is not contagious unless certain environmental contingencies existed. The following month, a critic in the Lancet coined the term, contingent contagion, which stuck. It did so, perhaps, due to an ironic incongruity between Johnson’s erroneous predictions about the course of the disease and the actual outcome: “I venture to prophesy that it [cholera] will never prove formidable in an English climate” (June 1831).

Many medical men considered Johnson’s notion of contingent contagion nonsensical. For them, Asiatic cholera was either contagious or it wasn’t. Anything else simply gave a fancy name to medical uncertainty. But for this historian, contingent contagion is a viable interpretive category for analyzing views on cholera transmission in England at least until the 1866 epidemic. For Johnson’s notion of contingent infectious effluvia jarred the neat, oversimplified opposition between contagionists and anticontagionists, explains the rift that occurred in the anticontagionist camp during the 1832 epidemic, and cleared the way for a detente with contagionists that evolved into the sanitary movement.

I have posted two of James Johnson’s letters to the Times on JSA&RC and a letter he sent to the editor of the Lancet; see the Bibliography for details and URLs. The Westminster Medical Society debates (1832, two listings) mentioned in the Bibliography offer insights into the disagreements among contagionists and anticontagionists, including contingent contagion, in the midst of the first English epidemic. I’ve posted samples of contingent contagionists on JSA&RC who believed that the infectious nature of Asiatic cholera depended, at least in part, on environmental circumstances and/or individual predisposition: Watson (1842); Fourcault (1849); and Frettenbacher (1849).
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Infection as medium of contagion

Contagionists who were troubled by the absence of evidence that cholera was directly transmissible on the smallpox model, by touch or fomites, could embrace the intermediate notion of contingent contagion because infection assumed disease progress by person-to-person transmission of morbid material. Infection, the notion that contagious matter entered the body via the lungs, was the oldest proposed medium of contagion, overshadowed in recent decades by the skin because of inoculation and vaccination. Although cholera infection and smallpox differed in the mode of communication, they had a common contagious signature: a morbid virus in both diseases was produced in the victims’ bodies and transmitted to others. Infection in Asiatic cholera occurred when the virus emanating from a victim’s skin and breath was inhaled by healthy persons, in whom the virus found fresh material for its reproduction and propagation.

Indirect transmission by atmospheric infection had the added advantage of explaining cholera’s progressive mode of communication when direct contact could not be established, as well as contradictory evidence about the fate of health care workers. If cholera was not an extremely contagious disease, the threshold for infection would be higher than most medical men encountered during routine treatment of victims.

Contagionists could find common ground with local miasmatists simply by making infection the rule rather than the exception. Environmental contingent contagionism gave a huge boost to an incipient sanitarian movement that, in alignment with Benthamite reformers, sought to improve the condition of the laboring classes, especially in rapidly industrializing towns and cities, via massive social engineering projects. Cholera, the new modern plague, was more intractable than smallpox; it could not be tempered by inoculation. But sanitarians believed there was a socio-economic vaccination available for the scourge of cholera; reduce, perhaps even eliminate, its capacity to infect by preventively improving the living conditions and changing the habits of the humans essential to its progress.

Writings by strict contagionists, who believed that Asiatic cholera was always contagious and only transmitted by human contact, posted to the JSA&RC include: the Editors of the Lancet (1842) on three modes of contagion; Copland and the Editors of the Lancet (1846) on infectious nature of pestilential cholera; James (1848) on communicability by infection; and the Editors of LMG (1849) on diffusion of cholera by fomites.

Local miasmatists

During the 1831-32 English cholera epidemic, an increasing number of anticontagionists found the general atmospheric explanation at odds with their experience in local outbreaks, just as Johnson had in India. The usual suspects for morbid sources quickly shifted from vegeta-
tive miasmata carried by prevailing winds to local concentrations of miasma and, especially, effluvia — the invisible, fetid vapors generated by decomposing animal products and by-products, including excrement. Whether or not a healthy person contracted cholera depended on the miasmatic or effluvial virulence of an offending place and constitutional susceptibility to any epidemic fever. Typical Asiatic cholera was non-contagious; those who contracted it did so from something morbid about a particular place, not a fellow human.

A rare exception proved that rule. Experience suggested that the cholera poison could transmogrify into an infectious virus in some victims who inhabited confined spaces. Certain environmental circumstances seemed especially conducive to initiate this pathological process in bodily constitutions. Again, the evidence suggested to this group of medical men and sanitarian-minded lay persons that poverty, overcrowding, poor ventilation, low-lying habitations, and filth set in motion inscrutable changes in the bodily economy that heightened susceptibility to cholera. Moreover, adults inhabiting such fever nests frequently compounded their situation by engaging in debilitating habits (especially excessive drinking of distilled liquor) and consuming an unwholesome diet. The most ardent of non-contagionists had to admit that in very enclosed places, only a pathological explanation could account for the massive morbidity and mortality from Asiatic cholera. Infection had occurred.

Formerly healthy persons come down with cholera after inhaling morbid matter produced in the bodies of cholera victims, a classic medium of contagion.

In short, local miamatists accepted the argument for contingent contagion (whether or not they would salute Johnson’s flag), partly because it considered infection a rare exception, partly because it preserved the theory of “epidemic constitution.” An “epidemic constitution” could be retained as an unknown first cause of epidemic cholera. Thereafter, environmental circumstances determined the particular manifestations of the disease — usually a non-contagious fever, exceptionally an infectious fever. For Sydenham loyalists, Johnson’s compromise accounted for local cholera outbreaks in areas contrary to prevailing winds, as well as exceptional mortality in confined, overcrowded spaces with no immediate source of organic decomposition. Contingent contagion would explain why occasionally medical attendants did succumb after treating victims in fever nests or poorly ventilated dispensaries over long stretches of time. Infection was fortunately the rarest contingency.

The following examples of local miamatists who believed that Asiatic cholera is one disease with local manifestations, perhaps including rare instances of infection, are posted on the JSA&RC: Laycock (1846) and Clark (1846). Also posted are examples of the increasingly outmoded general miasmatic notion among some anti-
contagionists and strict interpreters of Sydenham who believed that the “epidemic constitution” did apply to Asiatic cholera: A pithy formulation by the pseudonymous Ero (1837); the editors of the *Medical Times* in 1848; and Hingeston (1855) on the collateral science of meteorology and epidemic cholera.

**Pragmatic contingent contagion**

The 31 August narratives seek to validate the theoretical integrity of two dominant epidemic disease theories in mid-nineteenth century England. The first of these is Farr’s zymotic theory, straddling local non-contagion, contingent contagion, and infectious contagion. The second, and more mainstream at the time than Farr’s theory, is Alexander Stewart’s localized non-contagionism with a rare contingent-contagious exception for improperly ventilated and unsanitary spaces.

Farr’s *zymotic* and elevation theories in the 1840s are elaborations of Johnson’s original notion. Farr believed that the “epidemic constitution” conducive to the generation of *cholerine* varied from place to place. Temperature, humidity, wind direction and speed, barometric pressure, and precipitation were foreseeable contributors. In the London metropolis, Farr proposed that human sewage deposited into the River Thames was associated with the production of poisonous *zymotic* material which formed a bond with harmless miasmata, became volatile, and rose into the atmosphere when the meteorological conditions were opportune. In Farr’s theory, elevation determined the maximum dosage of poisonous London fog one was likely to inhale; low-lying areas (one of Johnson’s prime contingencies) environmentally magnified individual susceptibility. Once inhaled, the *zymotic* material could (in susceptible persons) alter the blood, reproduce itself, and be passed to others as an infectious contagion.

William Farr, employing a fascinating blend of Parisian medical and Benthamite positivistic influences, developed a sophisticated statistical nosology of diseases in the early 1840s. Later, he used mortality data from the 1849 London epidemic to develop an elevation theory of causation based on distribution of cholera-laden vapor from the surface of the Thames, the very same data Snow had used to implicate impure water supplied by private water companies. In August 1854, Farr’s *zymotic* and elevation theories posed arguably the most significant challenge to Snow’s interpretation of increasing cholera mortality in the low-lying South London districts. Consequently, it deserved inclusion in my cholera narratives. But how detailed should I make it?

Farr was tangential to my main event since he considered the local outbreak that occurred in the higher-elevated residential areas of St. James, Westminster and St. Anne’s, Soho an exception to his elevation theory; being a multi-causalist, he was quite willing to accept Snow’s explanation for this particular event. On cholera
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mortality in low-lying South London, however, Farr still held the interpretive advantage in 1854, and he seemed to think Snow’s SoLo inquiries would ultimately strengthen the elevation theory. Since I was limiting my analysis of the SoLo project to explanatory context for Snow’s involvement in the Broad Street pump outbreak, explanation of Farr’s theory should also be limited.

A narrow search of scholarly literature on Farr yielded a book and several articles by John Eyler. I began with the article (1973) that seemed most likely to give me the overview I was hoping for. It was almost forty years old, but that wouldn’t matter for my purposes if the research was solid. I tested Eyler’s trustworthiness by comparing the article with a few primary source references he cited. All was well.

I was lucky. A twenty-two page article contained most everything I would need to summarize Farr’s views of relevance to my topic. After roughing out this portion of the narrative scenario, I compared what I had written with Eyler’s book (1979) published after the article, made a few revisions that reflected new thinking, used two primary sources (Farr 1842, 119-22 for adaptation of Liebig to *zymosis*; and Farr 1852, lxi-lxv for the elevation theory) for additional details, checked each index entry for Farr in Hamlin (2009), and was done. Mind you, I only needed enough information to establish Farr’s *zymotic* theory and his take on the natural experiment in South London that he was facilitating for Snow.

It isn’t important for my purposes, for example, whether Farr’s interpretation of Liebig was accurate or even fair. I just had to present what Farr thought about a very specific issue.

The inserts with two elevation tables and the equation are based on Eyler (1973, 89); Eyler’s formulation summarizes and very much simplifies Farr (1852, lxii-lxiv [sequence 78-80]). The quotation that cholera mortality varies inversely with elevation, is from Farr (1852, lxix [sequence 85]). The quotation in which Farr traces the notion of *zymotic* diseases to Sydenham appears in Farr (1842, 121; reprinted in Farr 1852, lxxxii-lxxxiii [sequence 98-99]). And the quotation where Farr asks questions about the production and propagation of *cholerine* is taken from Farr (1852, lxxx [sequence 96]).

Alexander Stewart, Assistant Physician to the Middlesex Hospital, was also a local miasmatist who believed in the possibility that cholera could be infectious under certain circumstances. Two of his publications — a letter to the editor of *MTG* and the follow-up report about the hospital’s doings during the outbreak (extracts already presented in my analysis of Mrs. Gaskell’s account) — contain signature characteristics of contingent contagionism. In his mind the localized outbreak was caused by something “inscrutable” (1854) that first appeared in a very contained part of the parish of St. James, Westminster in the latter part of August. When the full eruption occurred, it
was near-instantaneous and pervasive in the localized area. More than 250 patients with symptoms of cholera and choleraic diarrhea were admitted to well-ventilated wards in which the beds were widely separated from each other. Stewart believed that this layout and a generous food allowance for hospital staff during the height of the epidemic largely prevented infectious transmission; only two staff members contracted cholera during the entire month of September. One nurse ignored premonitory symptoms for half a day and became too ill to save. The other recovered fully from severe diarrhea (Stewart 1854a, 365). Stewart does not explain why, given such policies, a disease he considered typically non-contagious felled either staff member. He emphasizes that the hospital had institutional guidelines for ward cleanliness, ventilation, and spatial separation of patients (Johnson’s preventatives for avoiding contingent contagion). By implication, perhaps he believed that the chaos engendered by the overwhelming number of admissions during the horrific Soho outbreak resulted in an unusual situation where two nurses were in close contact for too long, with too many patients, given their individual predispositions.

**Snow on cholera**

As far as his contemporaries were concerned in the summer of 1854, Snow had yet to make a compelling case in support of what many considered an interesting but very peculiar theory.

During the winter of 1848-49, he had made a knight’s move in his thinking about the mode of communication of Asiatic cholera. Experience at Killingworth in 1832 had convinced him that cholera was an infectious contagion. Men came out of the pits, retching and with the trots; it seemed more reasonable to assume they had inhaled effluvia given off by fellow miners than miasmata carried by prevailing winds. Snow’s teetotal address from 1836 and his last known comments about cholera (21 October 1848) indicate that, during his early years in London as student and general practitioner, he considered cholera a febrile disease, regardless of subsequent symptoms. That notion, as well as the ostensible parallel with asphyxia that Snow articulates at the meeting of the WMS mentioned previously, fit well with Copland’s definition and description that cholera is “infectious . . . not by contact, but from the inhalation into the lungs, along with the air, of the morbid effluvium given out from the body or bodies of the affected” (1846, 517).

Nine months later, the opening pages of *MCC* show how Snow advanced the contagionist argument with respect to cholera by rejecting infection and moving sideways to an alternative contagionist mode of communication. He hopped over contact (the acclaimed smallpox model) and made a case for the alimentary canal via pathological analogies with other diseases; yet he still gave contact an indirect role if cholera victims or caregivers handle food without taking sanitary precautions. It was, by anyone’s measure, a bril-
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But the confirmation process since 1849 had been less than brilliant. Many of Snow’s examples floundered on probabilistic shoals in an era when most medical men and medical commentators used absolutist charts. If caregivers treating cholera victims also prepared food without first washing their hands, why didn’t every partaker come down with cholera if the morbid matter was in the food? Snow offered several reasons, but individual constitutional predisposition, the chestnut for exceptions at the time, was not among them. Snow’s carefully documented examples of water-borne oral-fecal communication were often easily trumped by an alternative suggestion that his data could be explained by clouds of infectious effluvia coming from filthy locations. The natural experiment he conceived in the autumn of 1853 to study cholera mortality in the intermixed subdistricts of south London was his best opportunity to turn some minds his way.

I chose the Primary Way (basing my reasoning mainly on the documentary evidence) in developing my interpretation of Snow’s work on cholera because I wanted to establish something we did not feature in the biography (CC&SoM) and is not fully developed in other interpretive studies – that Snow had used natural experiments as confirming evidence since 1849, four years before the Lambeth/S&V natural experiment came to light.

For someone without this particular interest, however, the Secondary Way (reliance on existing interpretive studies) would certainly be a place to start. I’m still comfortable recommending CC&SoM as a starting point for such a tack since it provides the gist of Snow’s ideas on cholera from initial hypothesis (1849 [MCC]) to full-fledged and partially supported theory (1849a [PMCC]) to expanded analysis of the 1854 epidemic (1855 [MCC2]; and 1856a [SoLo56]). Of course, one should always double-check specifics in secondary works against the relevant primary documents, now accessible online at JSA&RC. The ones I used are listed with URLs in the Bibliography. I also strongly recommend Hamlin (2009); a “biography” of a disease rather than a single individual, Hamlin’s interpretation of Snow is able to be contextually more expansive than CC&SoM (2003).

*  *  *

My discussion of cholera theories often melds the argument in CC&SoM (165-198) with Hamlin (2009, 152-62). Snow’s notion of disease specificity and oral-fecal transmission is presented as a contagionist position, pathologically and epidemiologically. Farr’s position was more complicated and changeable: his Parisian training, statistical bent, and initial assumption that epidemic cholera emerged from a multiplicity of unknown causal factors (an “epidemic constitution”) first aligned him with an orientation Hamlin terms positivist
Anticontagionism. In the 1840s, Farr amended his views to include a mode of transmission, the *zymotic* hypothesis. The change was amenable to a group Hamlin calls miasmatic anti-contagionists. By the early 1850s Farr’s hypothesis allowed for the possibility of aerial contagious infection, so I interpret him as a contingent contagionist in his interactions with Snow. I interpret Stewart as a classic contingent contagionist, epidemiologically, although he would probably fit the bill of a positivist anticontagionist in Hamlin’s typology; Stewart’s two reports (1854, 1854a) suggest near-deification of numbers and statistics, drawn from careful, inductive analysis of experiential knowledge gained at the bedside. Hamlin and I differ, however, on the use of contingent contagionism; he focuses on the convergence of the three positions after 1860, whereas I use the term as I read it to have been employed by contemporaries in London medical journals from the concept’s inception in 1831 through 1854.

**Farr and Snow at the GRO**

Snow (1855) wrote that he and Farr met sometime during the week ending Saturday, 26 August, when Snow presented preliminary results of door-to-door inquiries in two Kennington sub-districts he had begun “about the middle of August” (77). During this meeting Farr offered to have all registrars in South London make inquiries about the water supply at each address where someone would die from cholera in the current epidemic, beginning 27 August (Snow 1854a). Why, I asked myself, would Farr make such an offer? Had Snow encountered an unexpected difficulty?

A bit of reverse literary-archaeological reasoning about Snow’s writing method makes me think something unexpected and catastrophic had occurred, necessitating a change in the scope of the natural experiment. I began with the fact that Victorian publishers often asked writers to submit fair copy on the installment plan to facilitate typesetting of long works, and John Churchill was no exception. He seems to have struck an agreement with Snow early in 1854 for an expanded edition of *MCC* (Snow 1849). Long before the second wave of the 1853-54 epidemic began in July 1854, Snow replicated the structure of argument in the first edition and began submitting installments for *MCC2* (1855), which contained the fruits of additional research on cholera outbreaks he considered suggestive of oral-fecal transmission.

The installment scaffolding is especially evident in the section on point-source cholera outbreaks caused by polluted neighborhood water supplies. Snow essentially copied the *MCC* discussion of Horsleydown and Albion Terrace from 1849, then writes up additional cases as he becomes aware of them via correspondence, personal communication, government reports, medical journals and newspapers. At least I can detect no argumentative reason why, for example, in this section his discussion of cholera at Ilford in
1849 is followed by the 1832 epidemic in Newburn and an 1814 outbreak in India. It appears to me that he sent them seriatim to the publisher for typesetting (32-36).

Whereas an archaeologist exposes successive layers in a midden back in time, I follow Snow’s reasoning forward in time by examining installment segments in his published remains. Virtually no significant revision was possible once type had been set; only an infrequent, visibly squeezed footnote was reluctantly permitted. The resulting chronology of creation is often self-evident, sometimes embarrassingly so as in the first line of Snow’s (1855) introduction to the Broad Street pump episode:

The most terrible outbreak of cholera which ever occurred in this kingdom, is probably that which took place in Broad Street, Golden Square, and the adjoining streets, a few weeks ago (38).

MCC2 was published in January 1855 whereas the outbreak in Golden Square happened during the first week of September 1854. “A few weeks ago” suggests that Snow had given John Churchill the initial portion of his discussion of the Broad Street outbreak in late September. The five paragraphs following the introductory paragraph containing the sentence quoted above suggest that what would turn out to be an erroneous chronological reference was an inadvertent outcome of Snow’s economical writing method. For he essentially re-used text from a letter to the editor, published in a medical journal the third week of September, detailing his analysis of 83 deaths during the first week of the epidemic (1854b; see the parallel-column word analysis in Appendix B). Subsequent text about the Broad Street outbreak was written later that autumn, also in installments, as he completed each phase of a more extensive investigation than the limited inquiries conducted in three days during the first week in September.

**Impure water and the propagation of cholera in South London**

Several weeks into the 1853 cholera epidemic in metropolitan London, William Farr detected a possible pattern in South London. There appeared to be fewer deaths in districts supplied with piped water by the Lambeth Company compared to the same stage during the 1849 epidemic. His own elevation theory could not account for this difference in cholera mortality. Farr wondered if purer water was a significant contributing factor in rendering predisposed individuals less susceptible to cholera. He knew that the Lambeth Company had shut its pumping station on the Thames at Lambeth in January 1852 when new works were completed at Thames Ditton, three miles beyond the tidal reach. But there were two other companies serving South London, which, like other private water companies supplying the metropolis, were forbidden by an 1852 Act of Parliament to draw water anywhere below the Teddington...
Lock on the Thames or from any of its tributaries below the highest tidal point after August 1855 (unless special extensions were granted). What progress had they made in making this transition? The Registrar General approved a questionnaire that was sent to ten water company directors in mid-October (UK. GRO 1853, 402).

Farr had answers from eight companies in hand by mid-November, so he prepared a supplement to the Weekly Return of 19 November entitled, “Cholera and the London water supply” (UK. GRO 1853, 401-06). The information on company watersheds (areas in which they had laid pipes) showed that the districts supplied by both the Lambeth Company and the Southwark & Vauxhall (S&V) Company had two-thirds of the cholera mortality through week twelve of the epidemic than districts where S&V was the sole provider (406). S&V was still distributing Thames water from its old Battersea works (a half mile above the Vauxhall Bridge), where tidal inflows brought metropolitan sewage released from outlet pipes near London Bridge. In 1849, when the Lambeth Company drew impure Thames water from a pumping station located near the Hungerford Suspension Bridge (closer to the main sewage outlets), cholera mortality in areas served by it had surpassed that in districts only receiving S&V water.

This data suggested that another natural experiment on the Exeter model was underway, where an improvement in municipal water supply brought a reduction in cholera mortality. But Farr did not think it qualified as a defining study, an experimentum crucis, which would require identical factors except for water quality:

> to measure the effect of good or bad water supply, it is requisite to find two classes of inhabitants living on the same level, moving in equal space, enjoying an equal share of the means of subsistence, engaged in the same pursuits, but differing in this respect, that one drinks water from Battersea, the other from Kew [far from sewage outlets and the Thames’ tidal reach].

In his mind, “the circumstances of London” did not present the requisite conditions since “generally . . . the poorest and lowest, if not densest districts, use the worst water,” whereas those in more fortunate social and economic circumstances inhabit higher terrain where the water is often the best available in the metropolis (401; CC&SoM, 260). John Snow, however, transformed Farr’s notion of crucial experiment into a workable concept when he discovered that S&V and Lambeth were still active competitors in sixteen sub-districts of South London.

Snow’s installment approach in writing MCC2 (1855) reveals how this idea came to fruition. The germ was not the paragraph on the ideal experimentum crucis mentioned above but a table that appeared in the following number of the Weekly Return: specifically, a statement that Lambeth and S&V supplied the same districts in
parts of South London (68-69). Then occurred another example of Snow’s phenomenal memory for salient facts: he located verbatim testimony provided by Mr. Joseph Quick, an engineer with S&V, to the Commission that produced the first *Health of Towns Report* (a short-hand title) in 1844, to the effect that some districts in South London had two or three sets of pipes in the same streets (UK. Parliament 1844, 2:133; Snow 1855, 61, 68; Snow 1856a, 241. See also UK. Parliament 1845, 1:192-94).

Quick’s testimony made Snow wonder if there was a way to establish a clear demarcation of the intermixed watersheds (which in 1854 had been reduced via amalgamation to two companies, S&V and Lambeth). The second *Health of Towns Report* (UK. Parliament 1845) contained a map depicting the distribution boundaries of all private water companies in the metropolis (facing page 1:137). It seems very likely, given the layout of Map 2 in *MCC2* (Snow 1855, facing page 74) that Snow used the 1845 map, updated to reflect changes in supplied areas reported to the GRO in 1853, as well as reported property rates showing values of pipes owned by the two companies, to determine that there were sixteen intermixed sub-districts (Snow 1855, 72-74). When the Registrar-General’s office published a final list of fatalities from the cholera epidemic that extended from August 1853 until January 1854, Snow recalculated these numbers by sub-districts and entered them into a table: there were 60 deaths/100,000 in the intermixed area, compared to 114/100,000 in twelve sub-districts in which S&V was the only supplier and 0/100,000 in three sub-districts where the Lambeth Company had no competition (73). Compelling evidence for the credibility of his theory, but he knew it was insufficient to seal the deal for many of his contemporaries.

One senses Snow’s excitement (1855) as he describes the making of a study that would meet Farr’s criteria for an *experimentum crucis*:

Although the facts shown in the above table afford very strong evidence of the powerful influence which the drinking of water containing the sewage of a town exerts over the spread of cholera, when that disease is present, yet the question does not end here; for the intermixing of the water supply of the Southwark and Vauxhall Company with that of the Lambeth Company, over an extensive part of London, admitted of the subject being sifted in such a way as to yield the most incontrovertible proof on one side or the other (74).

It was too late in the epidemic and there was too little daylight at that point in the astronomical year to undertake anything systematic, but Snow did make a few inquiries. He was encouraged to find that in the sub-districts enumerated in the above table as being supplied by both Companies, the mixing of the supply is of the most intimate kind. The pipes of each Company go down all the streets, and into nearly all
the courts and alleys. . . . In many cases a single house has a supply different from that on either side. Each company supplies both rich and poor, both large houses and small; there is no difference either in the condition or occupation of the persons receiving the water of the different Companies (74-75).

Snow realized that this was a natural experiment unlike any he had come across before:

As there is no difference whatever, either in the houses or the people receiving the supply of the two Water Companies, or in any of the physical conditions with which they are surrounded, it is obvious that no experiment could have been devised which would more thoroughly test the effect of water supply on the progress of cholera than this, which circumstances placed ready made before the observer (75).

He had used population data from the 1851 census to calculate cholera fatality ratios for table 6 of MCC2 (73), according to which 301,149 people were registered in the sixteen intermixed sub-districts. That meant that this experiment . . . was on the grandest scale. No fewer than three hundred thousand people of both sexes, of every age and occupation, and of every rank and station, from gentlefolks down to the very poor, were divided into two groups without their choice, and, in most cases, without their knowledge; one group being supplied with water containing the sewage of London, and, amongst it, whatever might have come from the cholera patients, the other group having water quite free from such impurity (75).

If cholera returned to London while S&V still used the pumping station at Battersea to send piped water to its customers, Snow believed the next task was simple enough: “To turn this grand experiment to account, all that was required was to learn the supply of water to each individual house where a fatal attack of cholera might occur” (75).

Note the conditional, “might” in the previous sentence. All three hundred thousand residents would not contract cholera, of course, even fewer would actually die from the disease. Hence, Snow was confident when he wrote this passage that he’d be able to complete such an investigation entirely on his own: “I was desirous of making the investigation myself, in order that I might have the most satisfactory proof of the truth or fallacy of the doctrine which I had been advocating for five years” (76).

The SoLo denominator problem

Another “might” was sorted when epidemic cholera returned to London in the early July 1854. By the fourth week of the epidemic, it was evident that the 1853 pattern in the South London intermixed sub-districts was re-occurring. But where should Snow begin inquiries? He waited another week. The Return for the week ending 12 August, which was publicly available the middle of the following week, showed that cholera mortality was particularly extensive in the
two Kennington sub-districts of Lambeth.

I commenced my inquiry about the middle of August . . . and I found that thirty-eight of the houses in which these deaths occurred were supplied with water by the Southwark and Vauxhall Company, four houses were supplied by the Lambeth Company, and two had pump-wells on the premises and no supply from either of the Companies.

As soon as I had ascertained these particulars I communicated them to Dr. Farr . . . (Snow 1855, 76-77).

The task turned out to be considerably more complicated than he had first imagined. Snow often found it difficult to locate the precise addresses reported to the GRO due to absent or duplicated house numbers. When he did find proper addresses, the residents were frequently unaware which water company supplied their houses. It took some time to devise a near fool-proof chemical test, the outcome of a stroke of luck. During the weeks he undertook inquiries, there was nearly forty times the amount of salt per gallon in water supplied by S&V compared to Lambeth water. Determining correct enumerators for his study “was necessarily attended with a good deal of trouble” (77) and more time-consuming than he had anticipated, but he persevered. If only he had been equally assiduous in locating proper denominators for his study.

An intermixed water supply in similar neighborhoods within sixteen sub-districts was the defining feature in Snow’s South London (SoLo) “grand experiment” (Snow 1855, 75). Within the intermixed area, however, the numbers of houses each company supplied could vary considerably. That meant Snow must calculate very specific exposure ratios — numbers of houses with cholera mortalities in each sub-district (the enumerator) divided by the total number of houses served in that sub-district (the enumerator) — for each company. Otherwise, the results could be badly skewed.

Take the two Kennington sub-districts as an example. During the first five weeks of the 1854 epidemic, Snow found 9.5 times greater cholera mortality in S&V-supplied houses in Kennington than those served by Lambeth; looking good for Snow’s theory, eh? Although the two companies had roughly equal market share at the time in the two sub-districts, what if S&V were sending water to ten times more houses in those two sub-districts than Lambeth was? In that case, the exposure ratios would have been roughly identical.

The manner in which Snow (1854a) described his initial investigation in the two Kennington sub-districts suggests that he initially flirted with exposure ratios based entirely on houses. He showed Farr figures on the number of houses in which a cholera mortality had occurred during the first five weeks of the current epidemic, organized by the source of water consumed in
those houses (247). As to denominators, Snow interpreted the information on estimated value of pipes and other materials that S&V and Lambeth had supplied Parliament to suggest that the companies supplied nearly the same numbers of houses in each of these four sub-districts (247). In other words, he arrived at the GRO on or about Thursday, the 24th August 1854, with the results of initial inquiries in the “grand experiment” he had envisioned during the winter of 1853/54:

- A study limited to the 16 sub-districts where S&V and Lambeth had laid pipes in the same streets, alleys, and mews.
- Snow anticipated that he would be able, on his own, to investigate the water supply at every house in this intermixed area where one or more fatal attacks of cholera was recorded for the full duration of the epidemic.
- Exposure ratios calculated for each company as the number of houses with one or more fatalities in each intermixed sub-district / total number of houses supplied in that sub-district.

Farr’s offer to involve GRO registers in Snow’s SoLo study was the first indication that something was awry. There is no need to speculate about when a passage in MCC2 was composed; we have real-time documentary evidence from Snow (1854a) in the form of a letter to the editor of the Medical Times and Gazette (MTG) dated “Aug., 1854”:

Sir,--I have been engaged, during the last ten days, in an inquiry which promises to yield very conclusive evidence respecting the mode of propagation of cholera. . . .

. . . . . . .

I intend to continue the inquiry, extending it to the other sub-districts in which the two water companies are intermixed, and to bring it down to the 26th inst [August]. After this date, I am informed by Mr. Farr that the supply of water at the house in which every fatal attack of cholera may occur, will be returned by the Registrars in all the Districts on the South of the Thames (247).

This letter was published on 2 September and included the results of inquiries Snow (1855) had made in two Waterloo sub-districts; that is, more data than he had shown Farr previously when the offer was actually made (76-77). The passage quoted above shows that within ten days of initiating house-to-house inquiries in Kennington, Snow had abandoned the notion that he, personally, would make inquiries for the duration of the current epidemic at every house in the intermixed area where someone had died of cholera. Few at that time would have known his original intention. The first public disclosure (Snow 1854a) of the SoLo study outlined it as a joint venture: Part 1, the first seven weeks, with inquiries undertaken by Snow in the intermixed sub-districts; part
from the eighth week onward, all registrars in South London making inquiries about water sources when assembling their regular weekly reports.

It strikes me as unlikely that Farr would involve all South London registrars in part 2 unless he and Snow had already broached the possibility of expanding part 1 beyond the intermixed area mentioned in the letter. The narrative flashback to 24 August suggests that this discussion occurred at the same meeting when Snow presented his Kennington findings. Snow could state, without equivocation, that he would conduct all inquiries about deaths in the intermixed sub-districts during the first seven weeks of the epidemic, and still have known that the absence of data required for calculating exposure ratios for the intermixed area necessitated eventual expansion of part 1. Perhaps, at the time, he hoped Farr was as adept at magic as he was at statistics, and the housing data would be divulged in rapid fashion.

The letter to the editor Snow (1854a) wrote at the very end of August side-stepped the denominator problem he would describe five weeks later. Although I believe he had just found out the problem existed, I’m not suggesting that Snow misled anyone with preliminary, unadjusted exposure ratios for S&V and Lambeth. District and parish rate books (containing the value of pipes and other property) indicated roughly equal market share by the two companies in the four sub-districts he had investigated. Consequently, he felt comfortable in drawing preliminary conclusions:

both Companies supply alike all kinds of houses, — those of the rich and the poor indiscriminately. It is evident, therefore, that, in the sub-districts to which the inquiry has extended, the people having the improved water supply enjoy as much immunity from cholera as if they were living at a higher level, on the north side of the Thames (247).

Snow (1854c) wrote the Medical Times and Gazette on Monday 2 October that he had just completed inquiries into 642 cholera deaths in the intermixed sub-districts of South London registered during the first seven weeks of the epidemic. Only 93 fatal attacks took place in houses supplied by the Lambeth Company, whereas a whopping 509 instances occurred in houses supplied by S&V; the water source in the remaining 40 cases came from pumps, ditches, directly from the Thames, or could not be determined.

But he could not seal the deal, as Snow (1854c) admitted publicly for the first time, until he knew “the number of houses in each sub-district supplied by each of the Water Companies respectively” (365). That is, gross totals for the intermixed area were only suggestive; they did not show what he believed a sub-district analysis would — “the effect of the impure water [from S&V] in propagating cholera . . . in a very striking manner, and with
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great detail” (365). He attached a table depicting what he had discovered about water sources and fatal cholera attacks at the sub-district level, which meant he had the enumerators for calculating detailed exposure ratios for both companies. He only lacked the proper denominator data to complete his part of the “grand experiment,” although he “hope[d] shortly” to receive this information (365). Without it Snow could not complete his analysis of the natural experiment underway in the intermixed area of South London.

Snow then described the back-up plan that had been underway throughout the month of September. “In the mean time” — by which he meant, since shortly after learning that the necessary denominator data was unavailable:

in order to be able to compare the mortality from cholera among the customers of each Company, with the entire number of houses supplied by each of them respectively, I thought it desirable to extend the inquiry to Rotherhithe, Bermondsey, Camberwell, and certain parts of Southwark, which are supplied by the Southwark and Vauxhall Company alone. I was unable by myself to execute this part of the inquiry . . . (365).

The quote doesn’t mention four outlying sub-districts with little cholera supplied only by Lambeth, which Snow had put on his own to-do list and completed in September. Snow and Farr had decided that the only way to take advantage of the natural experiment currently underway in the sixteen intermixed sub-districts was to extend inquiries into the twelve sub-districts served solely by the S&V water company, as well as the four Lambeth-only sub-districts.

Snow had farmed out inquiries in the twelve S&V-only sub-districts to a newly minted apothecary Licentiate, John Joseph Whiting. Since there was no competition in this area with the Lambeth Company, Whiting’s task was to determine whether S&V ran pipes to houses where someone had died of cholera during the first seven weeks of the epidemic, or the residents fetched water from alternative sources. Unfortunately, Whiting was only able to complete inquiries on cholera fatalities during the first four weeks before he departed the metropolis for a country practice, forcing Snow (1854c) to “calculate” (high-falutin for guess) that the water sources for all cases in these twelve sub-districts for weeks five through seven were “in the same proportions as those occurring previously” (365). Snow’s letter contained a table depicting his and Whiting’s findings by district, with asterisks next to the S&V-only sub-districts that involved estimated results.

Had Snow ended the letter (1854c) at this juncture, he risked giving the impression that the SoLo study was just another, suggestive-at-best, example of comparing cholera fatalities after the introduction of pure water in some areas to other areas with unchanged supply. He decided to give his audience a prefigurement of the analysis to
come when he received the sub-district supply data from S&V and Lambeth that would permit him to plot exposure ratios for the early part of the epidemic within the intermixed area, before normal propagation really set in and skewed the causative effect of impure water. Pending that ideal scenario, he used the only denominator data available to him at the time and calculated exposure ratios during the first seven weeks of the epidemic for each company’s entire watershed:

The entire number of houses supplied by the Southwark and Vauxhall Company, according to a return made to the General Board of Health in 1850, was 34,217, and the number supplied by the Lambeth Company, according to the same return, was 23,396. The number of houses supplied by both Companies has increased with the extension of the Metropolis, but it is pretty certain that the proportion continues nearly the same, and for the sake of comparison, the number of houses may be supposed to remain the same also (366).

Snow then divided these figures on the total number of houses each company supplied in South London by the total of actual plus “calculated” cholera fatalities in houses traced to each company, then proposed that while a death from cholera had occurred in 1 house in every 28 supplied by the Southwark and Vauxhall Company, a fatal attack of cholera had occurred in only 1 out of 251 of the houses supplied by the Lambeth Company. The mortality, in short, to August 26, was just nine times as great in the houses supplied by the former Company as in those supplied by the latter (366).

The possible effects of pure and impure water on cholera mortality were even more striking in the first four weeks, when other forms of propagation weren’t as pronounced:

During the first four weeks of the present epidemic, 563 persons died of cholera in London. Of these it has been ascertained, by a personal inquiry at every one of the houses in which the attack took place, that no less than 268 of the fatal attacks took place in houses supplied with water by the Southwark and Vauxhall Company. . . . During these four weeks there were but ten deaths from cholera in houses supplied with water by the Lambeth Water Company; although it has been shown above that they supply fully two-thirds as many houses as the other Company. The cholera was consequently eighteen times as fatal among the population supplied with the water from Battersea Fields as among that with the purer water from Thames Ditton, during these four weeks, although this latter population is intimately mixed with the former.

A lot of guesswork, two dodgy numbers, and a slight over-reach in the last sentence; only 62% of the population in the combined watersheds had an intermixed supply (Snow 1855, 84). But it was the best he could do with the information available. However, it was not the analysis of the “grand experiment” Snow had already described for the forthcoming publication of MCC2.
Snow’s SoLo analysis in MCC2

October ended without Farr receiving the desired denominator data. Snow had a deadline to submit the remaining sections of MCC2. He had delayed writing up the South London study as long as he could.

We can imagine Snow’s chagrin. He had in hand enumerator data on cholera fatalities in the intermixed sub-districts during the first seven weeks of the epidemic. He had personally collected it, as promised in the letter to MTG published on 2 September. Granted, his findings did not cover the entire epidemic, but they contained neither guesswork nor “calculations.” Part of the planned “grand experiment” was ready for analysis. But he could proceed no further without sub-district level information on how many houses each of the private water companies supplied in the intermixed area. What was to be done?

Snow really had no option except to revise and expand the two letters he had sent to MTG, an approach he had already used for the opening sections of MCC2 with the eponymous 1849 pamphlet (the title page states, “Second Edition, much Enlarged”). The South London portion of MCC2 runs just over sixteen pages of text and tables (1855, 76-92). Snow transformed the letter published in September (1854a) into a brief discussion of his initial inquiries in Kennington, Farr’s offer to involve GRO registrars beginning 27 August, and Snow’s “resolve” to investigate cholera fatalities in the entire intermixed area until the 26th on his own (76-77). The October letter to MTG (1854c) contained a long description of difficulties he encountered when he asked residents which company supplied their houses; “It would, therefore, have been impossible for me to complete the inquiry if I had not found that I could distinguish the water of the two Companies with perfect certainty by a chemical test” (365). Snow reduced the entire passage in MTG when writing MCC2, but some wording is nearly verbatim: “It would, indeed, have been almost impossible for me to complete the inquiry, if I had not found that I could distinguish the water of the two companies with perfect certainty by a chemical test” (77-78).

The structure of Snow’s argument in MCC2 often differs from that in the second letter to MTG, but snippets of varying length frequently reappear. He condensed a long paragraph on the absence of sub-district housing supply and hiring Whiting, deleting the phrase, “I hope shortly to learn . . .” but recopying verbatim that “I was fortunate enough to obtain the assistance of a medical man,” and additional phrases about Whiting’s inquiries on fatalities during the first four weeks of the epidemic in S&V’s unmixed watershed (1855, 78-79). An entire paragraph in MCC2, with the exception of one word, comes from the MTG letter (1854c):

It may, indeed, be confidently asserted, that if the Southwark and Vauxhall Water Company had been able to use the same expedition as the Lambeth Company in complet-
ing their new works, and obtaining water free from sewage, the present epidemic of cholera would have been confined in a great measure to persons employed among the shipping, and to poor people, who get water by pailsful direct from the Thames or tidal ditches (366).

Snow changed “present” to “late” since the cholera epidemic had ended when he wrote this passage (1855, 81). There are many other incidents where he re-used, with minimal alteration, parts of sentences from MTG when writing parallel sections in MCC2.

Perhaps the most notable differences between the October letter to MTG and MCC2 are corrections Snow made to his discussion of the number of fatal attacks he and Whiting had investigated and an accompanying table; a change in the data he used to calculate tentative exposure ratios; and addition of data gathered by GRO registrars from 27 August through 14 October. For the intermixed area during the first seven weeks, Snow changed the numbers for S&V from 509 to 525, Lambeth less drastically from 93 to 94 (1854c, 365 and 1855, 85). For entire watersheds during the first four weeks, 268 deaths in S&V-supplied houses became 286 and Lambeth went from 10 to 14 (1854c, 366 and 1855, 79); the likelihood that the early S&V number was a typographical error at the press is lessened by the fact that Snow changed the fatality proportion between the two companies from 18:1 to 14:1 in MCC2. He also corrected figures in the single MTG table for the first seven weeks, added a new table covering the first four weeks, and organized both tables to show sub-district fatalities in three categories: solely S&V, intermixed, and solely Lambeth (1855, 84-85). Denominator data in MTG is from 1850, whereas in MCC2 it’s from 1853 (1855, 80); accordingly, the figures Snow used for Lambeth’s percentage of market share compared to S&V dropped from 68% to 65%. Snow used total watershed housing figures and population data to discuss fatalities in South London for the fourteen weeks of the full 1854 epidemic (1855, 86-91). In addition, in MCC2 Snow toned down his criticism of “ill-directed efforts of benevolent individuals among the non-medical part of the community” that he believed had caused the sewage concentration of the Thames to increase since the 1830s (1854c, 366).

I’ve carried the discussion of MCC2 well beyond what transpires in the 31 August 1854 narrative because of a common misperception that it contains Snow’s analysis of the “grand experiment” during the 1854 cholera epidemic where the watersheds of the Lambeth and S&V companies intermixed in South London. The revised Lancet obituary (2013) during the 200th anniversary year of Snow’s birth is an example:

It was his “Grand Experiment” that same year [1854] that secured his huge reputation in epidemiology. . . . During 1848–49, the death rates for the two companies were the same, but by 1854, after Lambeth’s
move, Southwark and Vauxhall’s rate was between eight and nine times higher, and in the first 4 weeks of the epidemic, Southwark and Vauxhall customers had a 14-fold higher risk. In 1855, Snow published a much-expanded second edition of *On the Mode of Communication of Cholera* that included these results.

The 14:1 risk disparity (60% of S&V inquiries during this period were conducted by Whiting) refers to what Snow described in *MCC2* (1855, 80) as total cholera fatalities during those weeks in the entire watersheds of the two companies, not the intermixed area that constituted Snow’s intended “grand experiment.” He was not able to undertake those calculations until most of the sub-district level housing data he required was published in a retrospective study lead by John Simon (1856).

In short, *MCC2* does comprise a description of why Snow believed this natural experiment (in which happenstance determined if one’s house received impure or purer Thames water) had the makings of an *experimentum crucis*. *MCC2* does describe what had to be done to try and determine the water supply at each house where someone had contracted a fatal case of cholera, why it was necessary to farm out some investigations, and the totals he, Whiting, and the registrars were able to amass in their respective assignments. *MCC2* does contain Snow’s risk-exposure calculations per company for their entire watersheds during various phases of the epidemic. But one must consult Snow’s article (1856a) for his analysis of the “grand experiment,” despite Wade Hampton Frost’s dismissal of it as “not altogether essential to Snow’s original argument, which was already well established . . .” (1965, xvi) — yep, you guessed it, in *MCC2* reprinted after Frost’s introduction.

**Historical continuity vs. historical change**

One of my goals when teaching historiography has always been to make transparent the tacit knowledge of a professional historian. This often necessitated a my-way-or-take-another-class approach to counter default notions of doing history. I gave students worksheets containing intellectual rubrics they must follow when researching and writing until they mastered my methodology. Appendix A is an example where I asked them to decide, before drafting a thesis statement, if their interpretations involved historical continuity or historical change.

Nightingale’s sojourn on a Middlesex Hospital ward was an example of historical continuity. It was the actualization of a decision she had already made to leave the Gentlewoman’s Establishment for something else, some new post or experience yet to be determined. Hence, my thesis statement for the opening narratives in which she is featured reflected basic elements of historical continuity: what she did, when she did it, and how/why it came about.
My interpretation that a circumstance beyond John Snow’s control forced him to alter his intended focus on just the intermixed watersheds in South London is an example of historical change. It requires a different thesis statement, historiographically, than the one that underpins the opening narratives featuring Florence Nightingale, a thesis statement based on a diagram similar to what I would assign students to construct with the following four c-words in mind:

First, depict a change in chronology as “From > To”; for example, from Snow’s initial vision of the SoLo project as limited to intermixed watersheds > to the expansion to include all subdistricts where either S&V or Lambeth was the sole provider of piped water.

Second, propose a causal explanation for this change, such as the denominator problem Snow encountered — the unavailability of data on the exact numbers of houses the two water companies served in each of the intermixed subdistricts.

Third, present evidence of a contingent event or factor that substantiates the proposed “From > To” chronology and the hypothesized causal explanation. The contingent event for Snow in the 31 August narrative occurs when his anticipated SoLo project changed on a dime: a documented meeting in August 1854 where Snow (1854a) showed Farr preliminary findings from two intermixed subdistricts and Farr’s response was to offer the services of his registrars. This meeting has the characteristic feature of a contingent historical event as set forth by Stephen Jay Gould (1989, 283). The causal explanation and/or contingent event constitute the “Because” component in the thesis statement.

Fourth, identify the context necessary for a reader to understand your narration of this historical change. For the 31 August narrative, I chose to summarize Snow and Farr’s respective cholera theories and the serendipitous appearance of a natural experiment during the London cholera epidemic of 1854.

My thesis statement for Snow and Farr at the GRO is an example of historical change: The absence of denominator data essential to analyzing just the intermixed Lambeth/S&V watersheds forced Snow to suspend his “grand experiment,” then expand and share inquiries into fatal cholera attacks occurring in every South London district served by the two companies. This unexpected change in plans affected everything he did during the 1854 epidemic, including his approach to investigating the local outbreak in Golden Square.

The narrative form

Farr and Snow’s ideas and actions are developed in a series of dated flashback-narratives, some containing sub-titled explanatory sections, as well as a final narrative on Thursday 31 August.

Although I have found no documentary evidence
that Snow and William Farr actually met at the GRO on that day, it is very possible that they did so. It was the eighth week of the London cholera epidemic. The *Weekly Return* no longer contained details of each death; the numbers were too enormous. So Snow would have had to visit the GRO to secure a list of everyone who had died of cholera during the week ending 26 August in the South London sub-districts covered by the natural experiment.

Farr had already instructed GRO staff to provide Snow (1854a) each week with mortality lists containing information not available in daily newspapers or printed *Weekly Returns*. Sub-district registrars were supposed to complete returns of births and deaths each Sunday and pass them on to their district registrar for review. District registrars then forwarded their reports to the head GRO office in Somerset House, where Farr’s team attempted to prepare complete copy for each return by Monday evening for type-setting into pamphlet form; copy was also sent to major newspapers. When all went smoothly, *Weekly Returns* were available at District Registrars’ offices on Wednesday and extracts from it appeared in Wednesday editions of the dailies; the *Times*, for example, published overviews, tables, and meteorological summations for the week ending 19 August 1854 on Wednesday the 23rd (“The Health of London,” 8c-d), and on Wednesday 30 August for the week ending the 26th (“The Public Health,” 9c-d).

Snow could have found the gist of recent cholera mortality in the metropolis from reading extracts in the *Times* or procuring a complete copy of the return; but to suss out specifics, he had to walk to Somerset House. He had no anesthetic administrations scheduled between 25 August and 1 September, so he could have made the visit on Wednesday or Thursday. Absent evidence to the contrary, I chose Thursday. Whichever day he did visit the GRO that week, it is very likely that he would have popped into Farr’s office for a chat and to give him an update on the progress of the South London investigation. This was sufficient probability for me to compose an historical narrative with a setting at the GRO on the same day that Florence Nightingale began her sojourn at the Middlesex Hospital.

As in the Nightingale episode at the institute on Upper Harley Street, the narrative style I employ for meetings between Farr and Snow is predominantly free indirect speech. When the narrative contains conversational speech, it is always in free indirect style, not quoted speech, since the latter in an historical work denotes quotations from documentary evidence. Take the opening lines of the 24 August flashback as an example:

> So far, the *experimentum crucis* is yielding anticipated results!
>  
> Farr looked perplexed. What was Snow talking about?
This is free indirect style — Snow’s spoken statement (without attribution) and Farr’s reaction (his internal speech) as free of authorial flags (in this instance, an historian’s seeming omniscient knowledge) as I could make it. Had I used reported or simple indirect speech in this instance, I would have added, “said Snow” to the first sentence and “thought Farr” to the third. My authorial presence is limited to providing context: how Farr looked.

Left-justified dates in bold font designate encounters between Snow and Farr, beginning with the flashback to 24 August 1854. This narrative is interrupted by explanatory sections (set off by centered and bolded sub-titles in a smaller font) that provide historical context and Snow’s doings during the early stages of the SoLo investigation (also designated by left-justified dates in bold font), before resuming at the point where Snow shows Farr the results of preliminary inquiries in the Kennington sub-districts. When narrating Snow’s activities in the run-up to the SoLo project and the initial inquiries in Kennington, I often employ indirect speech. That is, I explicitly report what I believe Snow was thinking and doing.

In my view, Snow became aware of the denominator problem and decided to expand the SoLo study during the same (documented) meeting — which in this narrative occurs on the 24th — at which Farr offered to have sub-districts registrars begin collecting data on water sources starting 27 August. The Snow/Farr section ends on 31 August, the narrative present so to speak, with an hypothesized meeting that updates Snow’s findings with two Waterloo sub-districts and shows his resolve to begin inquiries in the other sixteen sub-districts on his expanded to-do list in South London. Narratives about Snow’s hypothesized meetings with Farr on 24 and 31 August are based on Snow (1855, 76-79; 1856a, 242-43; and 1854a.

Additional documentation & unpacking

The page preceding the Farr-Snow scenario contains four figures scanned from the 19 November Weekly Return (UK. GRO 1853, 389 [title page]; 391 [sample of individual deaths]; 401 [supplement title and extract, marginal check marks mine]).

Three figures on the opening page of the Farr-Snow scenario:
- Detail from portrait of John Snow, painted by Thomas Jones Barker in 1847. Photo by David Zuck of the original owned by Geoffrey Snow.
- Detail from a photograph of William Farr, circa 1870, courtesy of Ralph Frerichs <http://www.ph.ucla.edu/epi/snow/farr/farr_fig1.html>.
The figure on the tidal reach of the Thames and Lea rivers is an amended version of a map in Luckin (1986 frontispiece). I scanned Luckin’s map into Photoshop, erased everything but the two rivers and the distance marker, and used the Horizontal Type Tool to add place names mentioned in the Snow-Farr narrative.

The figure of the first published data documenting a reverse in cholera mortality in districts served by Lambeth and S&V between the 1853 and 1849 London epidemics is an extract from the 19 November Weekly Return (UK. GRO 1853, 406). This Return suggested that the 1853 London cholera epidemic contained the makings of before-and-after natural experiment like those occurring in previous epidemics in Exeter and Hull.

The 26 November Return contained the game-changing evidence that there were still intermixed watersheds in South London (UK. GRO 1853, 409).

Luckin (1986) also has an illustration of London registration districts as of 1850 (71). I imported it into Photoshop and cropped the section containing the South London districts served by the Lambeth and S&V water companies. Then I erased Luckin’s registration district numbers, altered the bridges to comply with the detail covered in the narration, and filled in the missing district boundaries on a printed copy. I used this enhanced printed copy as a base, along with the Brush and Horizontal Type Tools in Photoshop, to create the figure of the intermixed sub-districts, as well as the subsequent three figures depicting aspects of the SoLo natural experiment. An experienced
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graphic designer would, of course, produce more professional figures that I can manage. But one purpose of my narrative is to show what anyone with an elementary facility in editing images might have done.

Shortly after the first of the SoLo figures comes the prepared-mind reference to cholera in the Baltic Sea. The narrative is based on Snow’s letter to the editor of MTG (Snow 1854). The PDF at <http://johnsnow.matrix.msu.edu/work.php?id=15-78-41> includes the notice, ”Health in the Baltic Fleet,” that spurred him to write.

The individual cholera deaths in the South Districts for the week ending 5 August 1854 (UK. GRO 1854, 249-58) would have been available to Snow on Wednesday 9 August. The Times did not print the entire mortality list.

The article that Snow skimmed at breakfast on 17 August 1854 would have been “Health of London” (Times [London, England], 17 August 1854: 9) <Times Digital Archive>.

- The figure depicting the Teddington Lock is a detail scanned from “Teddington Lock, 1894” (Godfrey, London sheet 132).
- The figure depicting the S&V works at Battersea is a detail scanned from “Pimlico, Sloane Square & Nine Elms, 1869” (Godfrey, London sheet 88). Unlike the previous figure, in which any changes at the lock that occurred during the forty years since 1854 are immaterial to the narrative, the 1869 map depicts a functioning railroad bridge, rails for four different railways, and the Battersea Park Station — none of which appear on Map 2 of Snow (1855, after 74). Consequently, I used Photoshop to modify details in the 1869 Ordnance map so that I could approximate the likely situation in 1854.

Information about Snow’s chloroform administrations on Thursday, 17 August, 1854 is taken from Snow (1858, 341; see Appendix C).

The table showing cholera mortality in the two Kennington sub-districts through 12 August is adapted from the letter to the editor of MTG Snow submitted for publication the following week (1854a, 247). After meeting with Farr during the fourth week of the month, Snow was able to complete inquiries in the Waterloo Road sub-districts on deaths registered through 19 August, which are also included in the MTG letter. He did not update the Kennington sub-districts beyond what he had already shown Farr for this letter.
Stewart and Nightingale at the Middlesex Hospital
Alexander Stewart was a clinician who believed that cholera was not directly contagious, but could be circumstantially infectious. Huh? How can something be infectious but not contagious? Is this chap a total nutter?

Hardly. It’s all about a very common phenomenon, the changing meaning of words over time. For example, in a report detailing the sudden increase in ill residents who flocked to the Middlesex Hospital in early September, Stewart (1854a) remarked that “a very large proportion of these being, on admission, [were] far beyond the reach of remedial skill. Such being the case, it is not wonderful, that of nearly 90 cases admitted during the fifty hours ending at one p.m., on Sunday, the 3rd of September, forty were at that hour already dead” (364; italics mine). Of course, Stewart did not consider that many deaths either admirable or unusually good; Stewart chose a word that in his age meant astonishing, a usage infrequent now. Similarly, if we parse Stewart’s viewpoint about cholera in terms of the cholera theories in play in mid-nineteenth century England, it was quite reasonable for a London physician in 1854 to consider cholera a non-contagious infectious disease.

The narrative form
A cardinal rule in historiography is to respect chronology; never explain something that happened earlier by something that happened later. The narrative scenario in which Nightingale first meets Dr. Stewart was crafted to emphasize that on 31 August 1854, Nightingale was a medical novice. The gentlewoman’s institute she had founded in Upper Harley Street was a short-term nursing home, not a hospital. She would have known very little about many diseases that might eventuate in admission to a female ward at a major metropolitan hospital.

Consequently, it would be historically inaccurate to depict Nightingale on the afternoon of her first day as a ward sister on the basis of the hospital reforms she instituted after travelling to the Crimea in November 1854. In fact, the reforms for which Nightingale is justly renowned had much in common with what was already in place at the Middlesex Hospital during the cholera outbreak in September, nothing at all in common with how she had organized and managed the facilities in Upper Harley Street. This point is important when considering the historical significance of Nightingale’s brief sojourn at the Middlesex Hospital during the Golden Square cholera outbreak.

I chose to use an imagined (but likely) meeting with Dr. Stewart to have him explain how Middlesex Hospital was set up to facilitate good nursing practice and minimize contingencies that could promote infectious transmission of epidemic diseases such as cholera. In the mid-1850s, ac-
cording to Stewart’s October 1854 report, the entire medical staff considered typical Asiatic cholera a non-contagionist epidemic fever, although hospital infection was a rare, but very realistic, possibility. In short, Stewart and his colleagues were contingent contagionists.

Although Stewart had been concerned for several weeks about a dramatic increase in the number of dispensary outpatients who presented with diarrhea, he did not foresee the major outbreak of cholera that began the night after his hypothesized meeting with Nightingale. No one expected it, especially not a severe outbreak in any of the northern districts of metropolitan London because most sickness and mortality from the current epidemic, underway since early July, was occurring elsewhere, particularly south of the Thames.

Additional documentation & unpacking:

In the twenty-first century, historical narratives set in the mid-1850s require contemporary terms and concepts that are made accessible to present-day readers. In order to explain and validate Alexander Stewart’s contingent-contagionist perspective on cholera, I chose to begin with primary sources. It was the simplest approach for me because I had already compiled detailed notes when researching CC&SoM and entered them into various data bases (many available in the research schemas sub-menu of the Online Companion in JSA&RC <http://johnsnow.matrix.msu.edu/snowcontextual.php>). “Contextual Material,” a data base of medical journal articles and letters to the editor (in PDF format), was particularly relevant in preparing this narrative scenario. If you wish to locate other citations in the data base, please employ the “Find” function on your browser.

If you have access to a research library, be sure to run at least a few of my suggestions to ground in their respective issues. Just as one often finds interesting stuff around a desired item on a stack shelf, paging through an issue (whether electronically or getting your fingers dirty) may yield a fuller harvest than just reading what I posted on the web site. If available to you, James Copland’s Dictionary (an encyclopedia, really) is useful for a contemporary’s viewpoints, although keep in mind that he was a staunch contagionist. The OED often establishes earliest usage.

A faster, secondary-source route is available to the same end. CC&SoM (Vinten-Johansen 2003, 165-98) and Durey (1979, 101-21) cover much of this terrain. However, if one chooses this option I recommend at least using Durey’s endnotes (231-33) to supplement those given in CC&SoM above for targeted searches of the primary literature as checks on interpretations in the secondary literature.

At the beginning of this scenario, the figure depicting the location of Middlesex Hospital is a detail from Godfrey, “The West End, 1870.” I also wanted an illustration of a medical ward
at Middlesex Hospital, but the only one I have located is from early in the nineteenth century. Al-

though the sanitary advantages of hospital wards as described by Stewart are clearly evident in this lithograph, I decided not to place it in the narrative because the clothing styles depicted were outmoded by the 1850s.

Stewart’s overview of diseases represented on the female ward is based on currently prevalent zymotic diseases listed in each Weekly Return, including the one issued 2 September 1854 (UK. GRO 1854, 318).

The narrative assumes that Nightingale had at least as much knowledge of epidemic cholera and the current situation in metropolitan London as any educated person who read the Times. Even though I could find no mention of cholera prior to September 1854 in Goldie’s summations of Nightingale letters, during the literature-research phase I had come across the following statement attributed to her: “. . . every day this week . . . those who live ten feet above a pesti-
lential river will die, and those who live forty feet above will live” (Small 1999, 129). The source, however, was not cited, so I sent an e-mail mes-
sage to the author requesting clarification. Hugh Small kindly replied with the desired bibliographi-
cal information: Goldie, 3_794, 25/8/54 (a lesson to all researchers; nothing untoward in asking).

The information Small provided suggests to me that the source of Nightingale’s comment may have been an abstract from the Weekly Return for 19 August which appeared in the Times the following week. Nightingale’s mother sent her issues when they were done with them (FN to mother, 5/10/53, in Goldie 1983, 3_730 5/10/53). Consequently the narrative reflects my interpre-
tation that Nightingale had a basic awareness of the current cholera epidemic and Farr’s statistical analysis and zymotic theory that mortality was associated with elevation relative to the Thames.

Dr. Stewart’s general therapeutic orientation and treatment of cholera victims will be unpacked in the next chapter.

Stewart (1854) accused the Vestry of St. James, Westminster of having wilfully ignored a significant outbreak of diarrhea and cholera in the greater Soho district for a fortnight prior to 1 September. In the narrative I have Stewart mention this outbreak in Soho and St. Giles, as well as the death of a dispensary surgeon who had been treating victims. Charles Tilly’s death was
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listed in the *Times* (31 August 1854, 1a); he was 34. The Westminster General Dispensary, where Tilly worked, put an announcement in the *Times*, requesting public support to help it cover unexpected expenses during the second half of August (2 September 1854, 6d).

**Whitehead and the Hopkins St. Widow**

Henry Whitehead was the senior curate at St. Luke’s, Berwick Street, the official parish church for nine thousand persons. He was three weeks short of his twenty-ninth birthday when he made an evening call at 9 Hopkins Street. My reading of his published writings, especially the sermons he delivered at St. Luke’s, makes me certain that he had excellent rapport with his parishioners, which I wanted to demonstrate in this short narrative scenario. Another purpose was to set up the opening narrative for the following day.

Historical detection led me to the address and the names I use in this scenario. I began with Whitehead’s (1854) pamphlet, *The Cholera in Berwick Street*, written shortly after the conclusion of the point source outbreak in his parish district but while epidemic cholera was still plaguing the metropolis. Whitehead states that “the first intimation which the writer received” that an outbreak was underway “came in the form of a summons to the death-bed of one with whom he had cheerfully conversed at a late hour on the preceding evening” (2-3). He does not give the person’s name, just that she was a widow, with dependent children, who resided in a “small house” in which eight people had died by 4 September, including the widow and some of her children.

First I had to find the actual address of this small house. Three houses in Whitehead’s street-by-street accounting listed eight dead (1854, 4); but only Hopkins Street contains a suggestive description: “only 3 small houses; population, about 70; deaths, 13, of which 8 were in one house” (1854, 4). I then checked a “plan” containing mortality data produced in 1855 for the Board of Health, and found eight deaths recorded at 9 Hopkins Street (UK. GBH. Medical Council 1855). An enumerator for the 1851 census listed a Mary Ann Plynn, widow, with five children at 9 Hopkins Street (HO 107/1483/132. Online: Middlesex>Westminster, St. James>Berwick Street>District 3/21). I confirmed the spelling of her name by a search of the census data base, which brought me to the same page of the census. An extract from the GRO’s *Weekly Return* for the week ending 2 September, published in the *Morning Chronicle* on 6 September 1854, noted the death of three residents of 9 Hopkins Street – a mother and two of her sons. I had the name of the person Whitehead visited and where she lived. The census enumeration also gave me names and vital data on other occupants at this address. I made the assumption that all of them were still lived there in the summer of 1854, although that’s clearly a stretch and, at best, his-
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torical possibility.

The map detail is from the plan mentioned above; I erased mortality data in Photoshop.

The narrative form

Whitehead wrote that he spent part of the evening with "a patient, gentle widow"; my description of the room at 9 Hopkins Street for which the 1851 census listed her as head conveys what I imagine such a person would do to make it livable for herself, five children, and four lodgers. I decided to have mugwort hanging in the room to reflect the widow's interest in cleanliness, regardless of the family's poverty. Mugwort (from muggia, meaning midge) is an old Saxon word and common name for *Artemisia vulgaris*, a widespread plant attractive to some insects. Bunches of the plant were hung for a period of time, then taken down and placed in a bag, which was beaten, thereby killing the insects trapped inside (MacKinnon, et al. 1992, 105). Sprigs might be used again, or discarded, depending on their condition and one's financial circumstances.

I constructed the purpose of Whitehead's visit to be discussing arrangements for the parish Sunday School outing that was initially scheduled to occur two days hence (Whitehead 1856, 58).

The opening two paragraphs are authorial third-person narrative. The next two paragraphs are in indirect and free indirect styles; I introduce the speakers and immediately shift into their mind-sets without further flagging devices (he thought, she thought, etc.). After the editor's friends, it's one paragraph in third-person (indirect style) before ending the scenario in free indirect style.

Conclusion

We know what began to happen late in the evening of Thursday 31 August 1854. Farr and Snow, Nightingale and Stewart, Whitehead and the Hopkins Street widow did not. As explained earlier, this disjunction is the essence of dramatic irony, which I have incorporated into all the historical narratives for this day. This was no master plan from the outset, however.

My decision to apply dramatic irony was an unexpected outcome of writing up Florence Nightingale's doings at the Middlesex Hospital on 31 August. The two short narrative scenarios I composed reflected Mrs. Gaskell's perception that Nightingale and the hospital staff she met that day had no inkling an outbreak was brewing. I also knew from having read Whitehead (1854) that he was similarly inclined. So, I asked myself, why not establish the same feeling in a narrative about Snow and Farr on the 31st?

That decision also solved a vexing organizational problem. How, I had frequently wondered, could I present Snow's initial cholera hypothesis from 1849 and its theoretical updating in the years thereafter without disturbing the narrative integrity of the Broad Street cholera
outbreak? The same concern applied to Farr’s zymotic view of diseases and the multitude of cholera causation theories. The solution I devised was to interrupt narrative interchanges with extended explanatory sections and then incorporate the information presented there into subsequent narratives.