Historiographical Unpacking — Prologue

Prologue

The full sentence of the epigraph is: "And beginnings were always built on endings, a lama once told Shan" (Pattison 2001, 430).

I’ve wanted to try my hand at a Prologue featuring Benjamin Ward Richardson ever since I established that he was very probably the first person to tell the tale, “the pump-handle was removed, and the plague was stayed.” The first time that assertion appears is 1858, in the biographical memorial Richardson composed and then appended to a posthumous publication of the monograph that John Snow was writing when he died. Snow’s executors gave Richardson access to his friend’s personal papers, and much of this quirky, deeply personal and emotional essay provides a reasonably fair summation of Snow’s accomplishments. The cardinal exception is Snow’s epidemiological legacy, of which Richardson makes quick and inaccurate work.

The structure of the Prologue is based on an interpretation that Richardson deliberately doctored the Broad Street cholera outbreak to make Snow more heroic than actual circumstances warranted and to depict nearly everyone else as a fool. The intended purpose of framing the Prologue in this manner is a hope that historical justice may be furthered by a re-telling of the Broad Street cholera outbreak in London during September 1854 by those, among others, whom Richardson pilloried and Snow, himself.

This approach, however, required me to bump off Snow before creating a narrative in which Richardson is writing a biographical memoir about his friend. That is how the Prologue became two postludes: First, the death of John Snow, then his beatification by Benjamin Ward Richardson.

First Postlude

Richardson (1858) provided most of the information I employed in four pages of the original biographical memoir (xli-xliv; transcribed in Appendix E. The 1887 revision shortened this section to a single paragraph). It’s all second or third hand, however. Richardson was never present during an ordeal that began with an incapacitating stroke on 10 June 1858 and ended in a comatose death on the 16th. Jane Weatherburn, Snow’s housekeeper, was the most immediate witness. Yet Richardson seems to have based his account of Snow’s final illness solely on “several particulars” received from Dr. Charles Murchison, a consulting physician who had been asked to attend when the primary physician was dumbfounded by Snow’s symptoms (xli).
The opening scenario of the first postlude is based on documentary evidence in Richardson’s memoir, occasionally enhanced by historical probability and historical possibility. For example, Richardson (1858) wrote that Weatherburn was concerned about Snow from the moment “he came down stairs at 8 A.M.,” complaining “of slight giddiness”; she thought he seemed unsteady on his feet. But after a short rest on the couch, Snow “ate a very hearty breakfast” and resumed writing his manuscript “on anaesthetics.” Sometime afterwards, according to Richardson, Weatherburn, “who had scarcely left him, heard a great noise, as if some one had fallen. She ran up again . . .” (xlii). Although Richardson did not say where she was when the stroke occurred, he did say that “she ran up.” I think it possible that she was washing up after breakfast in the scullery, located in the basement. If she were elsewhere, no injustice is done by a narrative embellishment on Richardson since it informs the reader about the structure of this particular household in which this housekeeper did the work of several servants in other professional households.

All remarks and wishes but one that I attribute to Snow in the first scenario are based on what Richardson (1858) wrote in the memoir, including Snow’s use of “frequent inhalations of sulphuric ether” to relieve pain (xliii). The exception is the sentence that begins, “the flavor was more to his liking than chloroform . . .”; Snow (1858) had indicated these preferences in an already finished portion of the manuscript on anaesthetic agents he was writing when interrupted by the stroke on 10 June (357).

The subsequent scene, in which Weatherburn assembles an ether apparatus, is historical probability; Snow could not have done this himself since he only had use of one hand. The sequence of events is based on Snow’s description of his portable apparatus in the monograph (1847) on surgical use of inhalation ether (16). I used Photoshop to eliminate descriptive elements in the engraving that appears in that book (17).

The second scenario, covering Dr. George Budd’s initial examination and a follow-up visit with Dr. Charles Murchison in tow, is also constructed from information provided by Richardson (1858) in the memoir. Most comes from the last four pages, but mention of phthisis pulmonalis and re-
Historiographical Unpacking — Prologue

This scenario also contains direct suggestions of Snow’s viewpoint, which I felt comfortable offering since Richardson wrote that Snow gave him the very same medical history that I depict him as conveying to the attending physicians. Richardson, however, never said the physicians took a history; I justify the format, historiographically, on the grounds of historical probability. Murchison, chosen by Budd to make an independent diagnosis, is likely to have asked Snow these questions; if so, the responses I assume he would have received are identical to what Richardson remembers hearing from Snow on an earlier occasion.

The differential diagnosis undertaken by the two physicians after interviewing Snow is an example of historical possibility — something that might have happened under these circumstances. Snow’s condition was perplexing, given the diagnostic parameters for stroke at the time, particularly that he presented undiminished sensation on the affected side. Although such medical uncertainty may not have warranted the exact conversation I suggest, it was an opportunity to inform the reader about contemporary definitions of stroke, frequently termed apoplexy in the UK and US at the time. I used in Dunglison (1860) because it’s readily available in the US (80, excerpted in the column to the right).

After the differential diagnosis, the narrative depicts the two physicians speaking with alone with Weatherburn. This interview is derived

"**Apo'plexy, Apoplex'ia, (Sc.) Pop'lexy, from apoplexy, 'to strike with violence.' At the present day, the term apoplexy is employed by many writers to signify interstitial hemorrhage, (F.) Hémorrha'gie intersitialle, or every effusion of blood, which occurs suddenly into the substance of an organ or tissue. Hence, we speak of cerebral apoplexy, pulmonary apoplexy, &c. &c. Formerly it was always — and still is by many — used in a restricted sense, to signify, in other words, the train of phenomena, which characterize cerebral apoplexy. This disease, Hæmorrha'gia Cer'ebri, Apher'onia, Carus Aporplex'ia, Coma Aporplex'ia, Aporplex'ia cer'ebri sanguin'ea seu cerebra'lis, Encephalorrhag'ia, Son'quinis ietus, Haematenceph'alam, Pulpe'zia, Sidera'tio, Apileps'ia, Morbus atton'itus, Catta, Theopha'sia, Theoplex'tia, (F.) Apoplexie, A. cérébrale, Hématoencephahlag'ia, Coup de sang, is characterized by diminution, or loss of sensation and mental manifestation; by the cessation, more or less complete, of motion; and by a comatose state,—circulation and respiration continuing. It generally consists in pressure upon the brain; either from turgescence of vessels, or from extravasation of blood; hence the terms Hæmenceph'alam, Hémorrha'gie cérébrale, and Hématoencephalarrhag'ia, applied to it by some. The general prognosis is unfavourable; especially when it occurs after the age of 35. When Apoplexy is accompanied with a hard, full pulse, and flushed countenance, it is called Apoplex'ia sanguin'ea, Catap'hora Coma; when with a feeble pulse and pale countenance, and evidences of serous effusion, Apoplex'ia se'ro'sa, A. pituito'sa, Serous Apoplexy, Catap'hora hydrocepha'lica, Encephalo'hysis sent'itis, Hydro-ceph'alaus aequus senum, Hydroencephalarrh'ea,"

*(Poirry)*, *Hydropisie cérébrale saignant, Hydroen* 

**Apo'plexy, Renal, Apoplex'ia ren'al'is. A condition of the kidney, characterized by knotty, irregular, tuberculated eminences, some of a deep black colour. Effusion of blood into the substance of the kidney.*
from Richardson’s account of Snow’s final illness.

Most details about Snow’s condition and mood in subsequent days is based on documentary evidence recorded by Richardson (1858). I did take the liberty of elaborating on Richardson’s statement that, “throughout his illness, he had been sanguine of recovery, and expressed his belief frequently that he should soon be at his professional work again” (xlv) in having Snow propose that James Ball jerry-build a writing desk near the couch in the morning room. The notion seemed in line with Snow’s alleged optimism about recovery until the last. I selected James Ball’s name from a trades directory because of his shop’s proximity to Snow’s house (Kelly’s 1856 Directory, 1603).

I based comments about the weather on daily reports from the Royal Humane Society’s Receiving Station in Hyde Park, quite close to Sackville Street; they were printed in the Times, as were articles on the run-up to the Great Stink of London in June 1858.

The scenario in which Weatherburn begins writing Reverend Snow a telegram is historical probability, based on a death certificate that he was present when his brother died. Information about messenger boys and sending a telegram is taken from a contemporary guide to London (Cunningham 1850). Information about train service between Yorkshire and London is taken from Simmons (1978; 1986; 1991). It does not appear in Richardson; it’s my interpretation of what would have been required to contact him in Yorkshire on Tuesday 15 June in time for him to have reached London by mid-day on the 16th. Betty Vinten-Johansen located John Snow’s death certificate during one of her visits to the Family Research Center, London.

Weatherburn’s musing about her years together with Snow is historical possibility, based on details provided in CC&SoM (82 and 234). Snow wrote a Last Will and Testament in August 1857 (Clover/Snow Collection, VIII.5.i), and I’m assuming that he would have told Weatherburn then that she would receive an annual annuity in the event of his death. The Will stipulates that the executors must purchase an annuity from the Government Annuity Office that will guarantee Weatherburn £20 for the rest of her life, and that such purchase should precede distributions to family members, including his mother. Snow and Weatherburn were sole companions at 18
Historiographical Unpacking — Prologue

Sackville Street, however one may parse that word, for at least six years. Before then she had been the housekeeper at 54 Frith Street, where Snow had rented a flat and surgery from 1838 until he purchased the Sackville Street attached house in 1852. In short, they had known each other for almost two decades when he died.

In addition to Snow’s Will, Lee Perry at the Woodward Biomedical Library sent me a copy of the receipt for Snow’s burial (Clover/Snow Collection, VIII.5.i). I located the address for the Economic Funeral Company (Kelly’s 1856 Directory, 1001). The firm had two locations; I chose the one in the West End, given that the receipt is for a burial plot in the West of London and Westminster Cemetery, Earl’s Court, Old Brompton (abbreviated by Richardson as the Brompton Cemetery). I located the registrar, George Keith, in a street directory (Kelly’s 1856 Directory, 59). Snow’s family wanted a private funeral and, according to Richardson (1858), rebuffed his attempts to invite any “medical friends,” including himself (xliiv). Eventually, William Snow, one of the designated executors of Snow’s estate, granted permission for Richardson to solicit funds for a commemorative gravestone.

Additional documentation & unpacking
Snow came down, Weatherburn ran up, Budd and Murchison come in. Richardson’s account assumes an understanding of the spatial layout at 18 Sackville Street. I needed illustrations that give a sense of place.

I chose The Survey of London, a multi-volume description of housing. Not all streets and houses are discussed. My luck was good, but not brilliant, for I found an extensive description of 16, not 18, Sackville Street and the following notation:

Nos. 17–20 (consec.) Sackville Street. These four houses are similar in many respects to No. 16, but the paired fronts of Nos. 18 and 19 are finished with a secondary cornice above the attic storey, and have a steep-faced garret storey hung with slates (Sheppard 1963, 358).

In short, I would have to adapt the Survey’s illustrations of 16 Sackville Street (356, Fig. 63) for Snow’s house two doors farther north.

I decided to begin with the front view of 18 Sackville Street since a photograph in CC&SoM (Fig. 9-1) manifests Sheppard’s description. I scanned and printed a high resolution image of
Fig. 63, separated the two views, and cut off the roof and chimney. Next, I placed a semi-transparent sheet of paper over the truncated front view and drew a garret storey plus chimney to match the description provided in the Survey to the photograph. After scanning this drawing, I photo-pasted it to the truncated portion. I then used the same procedure to adapt the side view in Fig. 63 of the Survey to match the description for 18 Sackville Street:

The caption to the resulting illustration ascribes names to various rooms and parts of Snow’s house. It’s my guess of how Snow would have used his house, based partly on occasional references in his writings, partly on Richardson’s description in the biographical sketch, and partly on inductive reasoning. For example, I knew from comments Snow made in several articles that he used the garret at 18 Sackville Street as a home laboratory and to house animals for experimentation purposes. The photograph suggests that the front portion of the garret would have been well lighted by three windows. I assumed that he would have wanted natural light in his laboratory, so I marked the garret accordingly.

Snow was primarily a peripatetic anesthetist by the time he purchased 18 Sackville Street. Nonetheless, he maintained a small general practice which necessitated an at-home surgery — a room for seeing and treating patients — and a parlor, both on the ground floor. I assumed that Snow would have made do with a morning room for seeing other visitors and personal relaxation, for which natural light would also be desirable. Since Richardson (1858) noted that Snow “came down” from his bedroom to a floor where he could lie on a couch and also eat breakfast (xlii), I placed the dining room next to the morning room on the first floor (second floor in American terminology) and Snow’s bedroom on the floor above next to a study with natural light at the front of
the house. I assumed the two rooms below the garret would have been at Weatherburn’s disposal, as well as the kitchen and scullery in the basement. The small room behind the dining room seemed the most likely location for a water closet (toilet).

The floor plans are exact images of Fig. 64 in the *Survey* (Sheppard 1963, 357), with the exception of a few lines I added to indicate the presence of an operational fireplace. The illustration on the title page for the Prologue that depicts house numbers in Sackville Street is an adaptation of Fig. 59 in the *Survey* (345).

The narrative has Snow informing Budd and Murchison that he first began drinking an occasional glass of port or wine in the mid-1840s after experiencing acute kidney distress (Richardson 1858, xiii). In other words, Snow had substituted temperance for teetotalism (“A moral and physical thermometer; or, a scale of the progress of temperance and intemperance” published in the *London Medical Gazette* [11 October 1844]: 52 <http://kora.matrix.msu.edu/files/21/120/15-78-155-22-1844-TemperanceTherm.pdf>).

I located addresses for Budd and Murchison (*Kelly’s 1856 Directory*, 900 and 1245). David Zuck sent me information about contemporary testing for albuminuria (Bright 1983, 134).

**The narrative form**

It was the scary time again. One of those periods when one stares at a blank page or is flummoxed by a draft that isn’t working. At this stage of my life, I at least had enough experience to be confident that I would eventually get through it. But it was no less unsettling at the moment.

This time I was stuck. I had followed my usual writing procedure to begin a session by revising what I had done previously, in this instance the makings of the first postlude. I felt that everything I’d written so far either came off as falsely authoritative (as if I had access to CCTV footage from the morning room at 18 Sackville Street) or historical fiction. I kept saying to myself that it shouldn’t be so difficult to transform Richardson’s account of Snow’s final illness into a readable historical narrative.

Over the years I’ve tried various ways to open my mind to randomness, usually a sure-fire strategy for getting myself unstuck. One technique I’d found helpful when I was at Michigan State University was to jot down every word or phrase that popped into consciousness and eventually cluster or connect them in search of patterns. If that failed to sort the problem, I would pick something else to work on for the remainder of that day’s writing session and hope that a few keywords of clarity would emerge during a meditative run or walk to campus. Falling arches and retirement changed that strategy; now, walking a forest road or an everyday task like brushing my teeth often does the trick.

I was still stuck on the first postlude after
Two of the three styles, or forms of speech, identified by James Wood as central to modern fiction have direct counterparts in modern historiography. The first, **direct speech**, is analogous to **quoted documentary evidence**. The following example is my riff on an illustration Wood used in a radio interview (Wachtel 2009):

> Cinderella looked at the clock, saw that it was nearly midnight, and said to herself, “It’s time to go.”

The fictional character’s internal thought is directly expressed within the quotation marks. The narrator’s mediating presence lies in reporting what Cinderella did and saw prior to her reaction. Wood argues that in fiction, direct (quoted) speech by characters, whether expressed audibly or kept internal, limits the author’s omniscience. It’s an illusion, of course. The author wrote the characters’ speech. But when done well, it’s among the grandest of illusions.

Historians have a different task than writers of fiction, but disciplinary distinctions should not obscure the stylistic forms they do have in common. Historians use quotation marks to set off verbatim documentary evidence from narration and interpretation. The content within them belongs to someone else; it’s borrowed and the rightful owners should be transparently acknowledged in some fashion. Unlike writers of fiction, historians may not create or make up what
appears within quotation marks. Nonetheless, the manner in which historians treat quotations is similar to a fiction author’s virtual distance from the direct speech of characters. Quotation marks are indicators of autonomy, whether such “speech” belongs to an actual person or an imagined character.

I did not quote directly from documentary evidence in the first postlude of the Prologue. Although Richardson (1858) wrote that “Dr. Murchison . . . has kindly given me several particulars in regard to the fatal illness, which I embody in the text . . .” (xli), he does not distinguish Murchison’s views from those of Dr. George Budd. Hence, I could not use direct quotes when either physician appears in the narrative. Moreover, since there is no evidence that Richardson spoke directly with Jane Weatherburn, I had to assume that all information and observations Richardson attributed to Snow’s housekeeper came from information he received from Dr. Murchison, which prevented me from employing quoted speech by Jane Weatherburn.

Instead, I frequently used the historical equivalent of “reported or indirect speech” (bold font mine). Indirect speech/narration (1) lacks the quotation marks of direct speech, and (2), is flagged by a third-person point of view, particularly the use of names and pronouns indicating the presence of the narrator who reports what the characters think, say, and do. According to Wood (2008), reported or indirect speech is “the most recognizable, the most habitual, of all the codes of standard realist narrative” (8-9).

The previous Cinderella example may be reworded so that her thought is reported indirectly:

Cinderella looked at the clock, saw that it was nearly midnight, and said to herself that it was time to go.

By removing the quotation marks from the first example and reporting what Cinderella is thinking, the narrator’s omniscience has been enhanced at the expense of Cinderella’s autonomy.

Such indirect style is standard historical narration. I used it to begin the first postlude:

She was in the scullery finishing the breakfast dishes when it happened. There was a heavy thud upstairs and she assumed someone was at the front door.

The use of pronouns and the absence of quoted material in this passage parallels indirect style in realist fiction. But this is historical narration because it’s based on extant documentary evidence (that Snow’s “housekeeper, who had scarcely left him, heard a great noise, as if someone had fallen . . .” [Richardson 1858, xlii].). Codes of historiography limit my imagination to a simple transformation of this documentary evidence into an historical narrative using indirect style. What Weatherburn may have heard and thought is
conveyed without quotation marks. In short, the
stylistic parallels in fiction and historiography are
as follows:

direct speech or style // quoted evidence
indirect speech or style // unquoted evidence +
pronouns and names

**Free indirect speech**, the third style or form
of speech in Wood’s typology, does not have a
common counterpart in modern historiography.
In fiction, this style is free of pronouns or names
provided by the narrator (hallmarks of autho-
rial flagging), and indirect because the speech,
whether internal or audible, is reported rather
than quoted.

Wood gave the following example when
interviewed by Eleanor Wachtel in 2009:

Cinderella looked at the clock. Almost
twelve. Time to go.

The passage begins with a narrator reporting
what Cinderella is doing. This sentence, in indi-
rect style, sets up a shift to Cinderella’s mind-set.
Subsequently, a phrase and a short sentence
present her internal thoughts, free of quotation
marks and third-person pronouns. A stylistic
change has emancipated Cinderella from the nar-
rator. Whereas regular indirect style features the
person telling the story (the narrator or author
who knows what is happening), free indirect style
features the characters — their mental deliber-
tions, feelings, and assumptions.

I used a similar combination of reported
and free indirect styles in the opening paragraph
of the first postlude. To the two sentences al-
ready discussed in the previous section, I added
another using indirect style to set up the final
sentence:

She was in the scullery finishing the break-
fast dishes when it happened. There was
a heavy thud upstairs and she assumed
someone was at the front door. Suddenly, a
panic feeling roiled her innards. Shouldn’t
have left him.

The unnamed woman’s internal thought is writ-
ten in free indirect style — no quotation marks,
no use of pronouns or proper names. The change
in style shifts the reader’s attention from the
historian’s narration to Weatherburn’s reaction.
“Shouldn’t have left him,” the sentence in free
indirect style, isn’t a figment. It’s just a stylistic
way of narratively interpreting Richardson’s state-
ment that Snow’s “housekeeper . . . had scarcely
left him.”

* * *

Free indirect style is not conventional fare among
professional western historians, at least since
the late nineteenth century. On occasion, Garrett
Mattingly (1959) comes close, as in this passage
when he interprets what Elizabeth I of England
may have been feeling as she anticipated foreign
reaction to the beheading of Mary, Queen of the
Scots, in February 1587:

But if we can be sure of anything about Elizabeth it is that she hated war. Because it was the one point of the arts of a ruler at which a woman could not pretend to be as good as a man? Because its uncouth violence offended her complicated sense of order? (27-28).

The second sentence is almost an internal monologue, free of quotes and pronouns. But the author’s presence hovers in the question mark, only to reassert himself with a pronoun in the third. Such indirect reporting, as well as the use of direct quotations from documentary evidence, characterize Mattingly’s historical narratives.

Simon Schama (1992), however, begins *The Many Deaths of General Wolfe* in free indirect style.

‘Twas the darkness that did the trick, black as tar, that and the silence, though how the men contriv’d to clamber their way up the cliff with their musket and seventy rounds on their backs, I’m sure I don’t know even though I saw it with my own eyes and did it myself before very long (3).

This is a first-person account by a British soldier who scales the cliff face and then fights the French and their Indian allies on the Heights of Abraham outside Quebec on 13 September 1759. It’s a riveting read that Schama described in a note as “pure imagined fiction” (327). Yet Schama also considered *Many Deaths of General Wolfe* an “experiment with historical narrative” (Acknowledgements). The soldier’s account cannot be both fiction and history, and it isn’t.

Schama’s soldier turns out to be a real, unnamed possibility. Wolfe was mortally wounded during the battle for Quebec. Some reports give names of those present when the general died, and many state that an unknown junior officer was there as well. Schama (1992) stated that he used these documents, including first-hand accounts by actual witnesses, to construct an account of what this unnamed soldier might have done and seen (329). At a critical stage of the battle, Schama’s use of indirect free style renders himself virtually invisible as the soldier he constructed explains how he came to witness General Wolfe’s death rattle:

Up comes the Captain and tells me to take a message to the General to say our line had held and the enemy was put to flight. . . . I suddenly saw him, lying on a mound beside a sorry little bush attended by just two men, one leaning over and supporting Wolfe with his arm. Mr. Browne, for that was his name [a lieutenant whose account Schama cites] . . . shouted at me to come fast and help. . . . I told him our news and in a groaning, gurgling sort of way
I could hear him praise God for it (68-69).

Hold the phone! This scene of the dying general seems very familiar. I page backwards in Dead Certainties until I come to a figure showing a detail from a 1763 painting by Edward Penny (29).

The soldier’s account is a composite of contemporary documentary evidence; Simon Schama made it up. It’s a work of the imagination, but so is the historical enterprise, with certain limitations. One of them involves the use of direct style; all quoted material must be cited, verifiable documentary evidence. There are no quotation marks in Schama’s construction of the soldier’s account to mislead the reader into thinking it’s documentary evidence.

Instead, Schama expanded the documentary record to include a contemporary painting in order to actualize the unnamed officer reported to have been with James Wolfe when he succumbed to battle wounds. That soldier left no report of his own, so Schama filled that gap in the historical record by constructing one for him, based on a composite of first-hand accounts by those who had scaled a cliff to fight on the Heights of Abraham and were present at the scene depicted by Edward Penny. Since Schama used verifiable evidence, broadly construed, and wrote the soldier’s imaginary account in first-person indirect and free indirect styles, his experiment in this instance is an example of how to introduce historical possibility into historical narratives. It includes neither quotations nor third-person authorial reporting to suggest anything else.

I did something similar in the prologue to an unpublished manuscript on Edvard Munch, but the nature of the evidence available to me raised the bar to the level of historical probability. That is, whereas the story told by Schama’s soldier is an imaginary construct, drawing on contemporary accounts and a painting, the narrative I composed used verbatim quotations from notes, sketchbooks, and journal entries that Munch made in France during the winter and spring of 1890.

News of his father’s death at home in Kristiania (Oslo) profoundly affected Munch. Depression ensued, and memories from his childhood and youth, especially bouts of consumption that had killed his mother and a sister, and nearly felled himself, consumed him, day and night. A friend, the Danish Symbolist poet Emmanuel Goldstein, made a suggestion he thought would be cathartic: notate and sketch whatever came to mind. Munch, already an episodic diarist, decided to follow Goldstein’s suggestion, which set in motion a process of self-reflection that, over several years, eventuated in a transition from representational to expressionistic painting and a coherent Life-Force philosophy.

The point of my prologue was to set up this thesis of the origins and resolution of Edvard Munch’s creative illness. It began as a dream sequence in direct (quoted) and indirect (reported)
speech, drawn from journal entries dated 5 February and 5 March 1890 (Munch-museet, T2771):

“It’s blood, Papa.”

Papa stroked my head. He said soothingly, “Don’t be frightened, my boy.”

But my heart was pounding. “I’m dying of consumption, aren’t I, Papa?”

Thereafter, the prologue depicted Munch reflecting on this and additional dreams and memories of his early life, based on the assumption that he was the brooding figure in a preliminary sketch (T126, 28) and the final oil painting, entitled *Natt* [Night in St. Cloud, 1890].

It was an experiment in writing historical narrative, undertaken twenty years ago, and one I have not repeated. I soon realized that the quotations were misleading in this particular context. Nothing is made up, but I selected and translated extracts from Munch’s journal in order to construct dream sequences that he never explicitly admitted to having. Were I to revise this manuscript now, I would eliminate the dream sequences, use indirect and free indirect styles instead of quoted speech, connect Munch’s memories explicitly to the solitary figure in *Natt*, and provide the original Norwegian for all translated extracts. That would be an historical narrative of what probably happened, unsullied by a conceit too clever by half.

* * *

Additional comments on the narrative styles used in the first postlude.

Whereas my uncertainty about the ultimate sources of Richardson’s account of Snow’s final illness prevented me from ascribing quoted speech to any of the figures who appear in the first postlude, the nature of that documentary evidence does allow for indirect and free indirect styles.

I use reported indirect style, the standard historical presentation, in every scenario. In such instances the gaze is mine; the description of what is happening is my interpretation of the evidence, often flagged by pronouns. For example:

He lay on the floor, beside his chair and partly under the writing table, attempting in vain to regain his seat. Right arm and leg moved jerkily. Left arm and leg were completely limp. He turned his face toward her as she entered the room. His mouth was strangely drawn to the side and drooping.

Twice in the above passage I approach free
Historiographical Unpacking — Prologue

indirect style by leaving out the pronouns. The intent is to share the narrator’s gaze with Snow’s housekeeper, who is presented as the observer of what’s happening.

George Budd’s initial bafflement at Snow’s symptoms and the subsequent consultations with Charles Murchison are also depicted in indirect and free indirect styles. The decision to involve Murchison offered me an opening to present Snow’s medical history in a narrative manner. For it seems likely, or certainly possible, that both physicians would have wanted Murchison to gather an independent perspective by interviewing both Snow and Weatherburn himself. These interviews are mainly in free indirect speech, which permit me to establish a conversational tone without quotation marks. These quasi-conversations are primarily based on Richardson’s memoir, secondarily on how the symptoms and diseases mentioned are described in contemporary medical dictionaries and journals.

I could have used quotations from Snow’s works on several occasions in the first postlude, but chose not to. For example, his stated preference for ether rather than chloroform as an analgesic is documented. But nothing would be gained by introducing direct evidence at that point in an historical narrative dominated by the other two styles. It seemed sufficient to clarify the authenticity of this remark when unpacking the sources and leave the narrative uncluttered.

The scenario where the attending and consulting physicians take Snow’s medical history contains vocabulary that would be unremarkable among three physicians but could be opaque to some readers. The conversation is presented in free indirect style, so it would have been artificial to depict the physicians using lay terminology and disruptive for me to interrupt with popularized reporting. Essential terms are clarified eventually in the postlude, either by the turn of events or what the physicians explain to Weatherburn as Snow’s illness progressed. I borrowed this technique from Eliot Pattison’s opening pages in Bone Rattler (2008), where the reader encounters eighteenth-century nautical and shipboard vocabulary, without explanation, until clarified later by the story line. Pattison’s novel is historically situated, and as such is an apt example of what I am trying to do in applying James Wood’s typology of styles in fiction to historical narration.

Second Postlude

My initial hunch was wrong. Sloppy and inadequate research did not explain why Benjamin Ward Richardson overstated the significance of Snow’s epidemiological investigations during the 1854 London cholera epidemic. The evidence, direct and indirect, suggested a different and very troubling possibility.

As it turned out, Richardson (1858) had done his homework and usually done it well.
Historiographical Unpacking — Prologue

The memoir contains many faithful and accurate comments about Snow's writings on cholera. For example, the beginning of Richardson's overview is spot-on: “In the year 1848, Dr. Snow, in the midst of his other occupations, turned his thoughts to the questions of the causes and propagation of cholera” (xix; transcribed in Appendix F). Then comes a precise summation of the patho-physiological and clinical inferences Snow used in both editions of On the Mode of Communication of Cholera (1848; 1855 [MCC2]), followed by a seamless segue to the 1854 London epidemic when Snow, “intent to follow out his grand idea, . . . went systematically to work” (xx).

Suddenly, however, Richardson’s tone (1858) shifts from measured to eulogistic and even a tad self-serving. “No one but those who knew him intimately can conceive how he laboured, at what cost, and at what risk,” beginning with Snow’s realization that a natural experiment existed south of the River Thames where two water companies, one (Southwark & Vauxhall) providing sewage-contaminated river water, the other (Lambeth) supplying relatively purer river water, were in active competition (xx). Richardson knew Snow had “paid for qualified labour” (xx), hiring John J. Whiting, a recently qualified surgeon, to make inquiries about the water consumption at addresses where fatal attacks of cholera had occurred during the first four weeks of the epidemic in all sub-districts served solely by the Southwark & Vauxhall Company. Snow did the same for Lambeth-only and inter-mixed sub-districts. In MCC2 (1855), Snow was generous and complimentary about Whiting’s contribution to their findings that:

as 286 fatal attacks of cholera took place, in the first four weeks of the epidemic, in houses supplied by the former Company [S&V], and only 14 in houses supplied by the latter [Lambeth], the proportion of fatal attacks to each 10,000 houses was as follows. Southwark and Vauxhall 71. Lambeth 5 (80).

Richardson (1858), however, transformed what he knew to have been a close collaboration into an investigation due solely to “his [Snow’s] endeavours”:

. . . of 286 fatal attacks of cholera, in 1854, occurring in the south districts of the metropolis . . . the proportion of fatal cases to each 10,000 houses supplied by these waters, was to the Southwark and Vauxhall Company’s water 71, to the Lambeth 5 (xx).

In a review essay three years earlier, Richardson (1855) had used similar language (again, obviously cribbed from MCC2): “In the first four weeks of the epidemic the proportion of fatal attacks to each 10,000 houses supplied by these different waters was as follows: Southwark and Vauxhall 71; Lambeth 5” (140).

But there’s a devil lurking in this passage of the memoir. The substitution of “1854”
for “the first four weeks” is a deliberate distortion of Snow’s carefully qualified remarks. The use of “286” indicates that Richardson had consulted MCC2 (or any of three subsequent writings in which Snow repeated this information) when composing the passage in the memoir. Richardson didn’t just copy from the review essay since the figure doesn’t appear in it.

**Why** did Richardson conflate the results of the South London study of a seventeen-week epidemic into just what Snow and Whiting had discovered during its first four weeks? Perhaps because the immense 14:1 fatal-attacks disparity between the two water companies during those weeks enhanced Snow’s theory of the role of cholera-contaminated drinking water in the emergence of metropolitan epidemics. In weeks five through seven, the disparity decreased to 7.5:1 as other means of propagation increased the number of fatal attacks in Lambeth-served sub-districts. During the final ten weeks of the epidemic, when registrars from the General Register Office undertook all inquiries, the disparity fell to almost 5:1. The average proportion for the entire epidemic was 6:1, but Snow repeatedly emphasized that the influence of impure water in South London was only evident if one disaggregated the epidemic by periods; the 14:1 disparity during the first four weeks expressed that influence (eg., 26 June 1856). Richardson could have said as much and showcased the prescience of Snow’s theory and his epidemiological acumen in substantiating it. Instead he chose subterfuge.

Richardson (1855) did not address the local cholera outbreak in Golden Square and Soho in the review essay; he may have considered it one of the “less significant examples” not worth mentioning in detail (140). When writing the biographical memoir of Snow three years later, however, Richardson (1858) stated that this episode, “more than the rest drew attention to Dr. Snow’s labours and deductions” regarding the communication of cholera (xx). Richardson may also have had MCC2 open when he wrote this part of the memoir. Snow’s version (1855) is on the left, Richardson’s redaction (1858) on the right:

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“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. Within two hundred and fifty yards of the spot where Cambridge Street joins Broad Street, there were upwards of five hundred fatal attacks of cholera in ten days” (38).

“In the latter part of August 1854, a terrific outbreak of cholera commenced in and about the neighbourhood of Broad-street, Golden-square. Within two hundred and fifty yards of the spot where Cambridge-street joins Broad-street, there were upwards of five hundred fatal attacks of cholera in ten days” (xx).

After the opening sentence in the left box, Snow (1855) launched into an account of the drinking-water habits of 83 residents he had investigated on the 6th and 7th of September. Thereafter, “I
Historiographical Unpacking — Prologue

had an interview with the Board of Guardians of St. James’s parish, on the evening of Thursday, 7th September, and represented the above circumstances to them. In consequence of what I said, the handle of the pump was removed on the following day” (40).

Whereas Snow suggests that the local authorities were rational men who decided to disable the Broad Street pump because they found the results of his house-to-house investigations alarming, Richardson (1858) chose to depict them differently:

On the evening of Thursday, the 7th of September, the vestrymen of St. James’s were sitting in solemn consultation on the causes of the visitation. . . . He [Snow] had fixed his attention on the Broad-street pump as the source and centre of the calamity. He advised the removal of the pump-handle as the grand prescription. The vestry was incredulous, but had the good sense to carry out the advice. The pump-handle was removed, and the plague was stayed (xx-xxi).

Bradford Hill’s view (1955) of Richardson’s penchant for the dramatic touch is evident in this passage. Snow’s description of his meeting with local authorities contains nothing about their state of mind or the impact on the progress of the epidemic that resulted when they ordered the pump-handle removed.

Another difference between the two accounts is noteworthy. Richardson knew enough about local affairs not to repeat Snow’s error in saying he met with the Board of Guardians. In 1854 the Parish of St. James, Westminster still enjoyed an exemption from implementing the New Poor Law, which had replaced parish authorities with Unions administered by Boards of Guardians. Richardson is correct when he stated that Snow met with vestrymen; more precisely, the vestrymen with whom Snow spoke would have been the Governors and Directors of the Poor, sitting as a special Sanitary Committee whilst cholera visited the metropolis.

Richardson could be a careful reader, so it’s difficult for me to believe that he completely missed the fact that the worst of the outbreak had passed when the authorities intervened. Granted, in MCC2 this information appears eleven pages after the paragraph about Snow’s appearance at their meeting on the 7th of September and is buried in an analysis of a table depicting fatal attacks that Snow amassed during the fall, long after the Broad Street outbreak had ended. Snow (1855) eventually determined that there had been:

- 56 attacks on 31 August
- 143 on 1 September
- 116 on 2 September
- 54 on 3 September (when he collected his first water sample in the evening)
- 46 on 4 September
- 36 on 5 September
- 20 on 6 September
- 28 on 7 September

“On September the 8th—the day when the handle of the pump was removed—there were twelve attacks; on the 9th, eleven; on the 10th, five; on
the 11th, five; on the 12th, only one; and after this time, there were never more than four attacks on one day” (49, 51).

Even if Richardson missed this point in MCC2, indirect evidence suggests that he would have known Snow’s mind on this subject from two publications that chronologically bookend this monograph. On 23 September 1854, the Medical Times and Gazette published a letter from Snow containing almost identical language (about his meeting with parish officials and the pump-handle removal) to that in MCC2; but this paragraph contains the following clause: “The number of attacks of cholera had been diminished before this measure was adopted, . . .” (322; transcribed in Appendix B).

In the biographical memoir, Richardson (1858) cited the report produced by the St James, Westminster parish Cholera Inquiry Committee (1855), particularly “the demonstrative evidence of the Rev. Mr. Whitehead,” as evidence that Snow’s perception of the cause of the Broad Street outbreak “become each day better and better appreciated” (xxi; Appendix F). Richardson was likely alluding to Whitehead’s exhaustive research into the drinking habits of Broad Street residents, his identification of the probable index case at 40 Broad Street, and the subsequent dis-interment of a leaking drain between the cesspool at that address and the Broad Street pump-well. Given this endorsement, Richardson studious avoidance of the following remarks is damning to his own account in the memoir: Whitehead (1855) stated that “The pump handle was taken off on Friday, September 8th. But by that time the epidemic had evidently subsided” (153). Moreover, Richardson chose to ignore that portion of the parish report in which Snow (July 1855) commented on this matter for the third time: “It will be observed that the daily number of fatal attacks was already much diminished by September the 8th, the day when the handle of the pump in Broad Street was removed . . .” (118).

Once I had established that Richardson had falsified Snow’s accounts of the 1854 London cholera epidemic when summarizing them in the biographical memoir, my next task was to sort out why he chose to do it. The hurrah-moment came after many readings of the memoir, when it occurred to me that the tone in the three and a half pages about Snow’s cholera researches seemed to differ from the rest of the memoir.

In these pages Richardson (1858) is downright hostile to anyone he deemed unappreciative of “the labours of our friend” (xxi). He expressed dismay that most people seemed unaware of the dangers Snow had confronted when conducting house-to-house investigations, as well as the toll they had on his physical health and personal finances (xx). Richardson is especially disgusted with those who failed to acknowledge Snow’s priority and achievements in the South London study (xxi-xxii). He does not mention
anyone by name, but the bill of particulars fits the five members of the Committee for Scientific Purposes within the Medical Council to the General Board of Health. This committee had published a Report on the Last Two Cholera-Epidemics of London, as Affected by the Consumption of Impure Water (Simon 1856). The Report included a retrospective analysis of cholera mortality in South London sub-districts served by two competing water companies, one providing water tainted by sewage from the tidal zone of the Thames, the other with a supply source situated beyond the impact of tidal flow. Sound familiar? Yet, the Simon Report (named after John Simon, the lead author and medical officer to the Board of Health) never mentioned Snow, MCC2, or any of his other publications about the study he had initiated in South London while the 1854 epidemic was actually underway.

In the second postlude, I depict Richardson as very exercised when he recalls the appearance of the Simon Report. It not had only stolen Snow’s thunder, it had diminished the alleged influence of impure water on the communication of cholera by asserting that the mortality disparity between the two water companies was only 3.5 x 1. That figure was low enough to satisfy sanitarians that any impurity, not just inadvertent ingestion of cholera discharges, could have caused the disparity by weakening the constitutions of susceptible victims. Snow had sent rejoinders (1856; 1856b; 1857) eviscerating the Simon report: In

Snow’s view, the committee had analyzed addresses in which cholera deaths, not fatal attacks, had actually occurred (many victims were moved to hospitals and workhouses before they died); some house numbers and addresses were ascribed to the wrong sub-districts; the analysis did not break the 1854 epidemic into periods of varying mortality, resulting in misleading averages; and so forth.

Richardson’s hostility (1858) also extended to critics of the decision to remove the pump handle on the popular Broad Street pump. The parish’s action, based on Snow’s recommendation, ostensibly elicited “much discussion amongst the learned, much sneering and jeering even; for the pump-handle removal was a fact too great for the abstruse science men” (xxi). Instead of providing evidence to support this claim, Richardson offers a refrain with a vengeance: “But it matters little . . . [what] little men [think] . . . It matters little, for the plague was stayed” (xxi).

On the other hand, anyone who agreed with or listened to Snow about removing the handle of the Broad Street pump fared much better in Richardson’s memoir (1858). The vestry, for example, are depicted as “incredulous, but had the good sense to carry out the advice” (xxi). Richardson concluded his summation of Snow’s epidemiological contributions by tooting his own horn: “It was my privilege, during the life of Dr. Snow, to stand on his side,” he wrote. “It is now my duty, in his death, as a biographer who feels
that his work will not be lost, to claim for him” the “entire originality” of the oral-fecal theory of cholera transmission, as well as “the discovery of a connection between impure water supply and choleraic disease” (xxii).

Me thinks, Dr. Richardson, that thou protest a tad too much. When I rewind the tape of your professional life to 1855, I find a distinctly tepid endorsement of Snow’s theory:

Dr. Snow and his objectors are both right in the main; and while the specific poison of cholera . . . may, by accident, be carried into the intestinal canal by the medium of water, it may also be wafted into the lungs by the medium of the air.

Although . . . we are prepared to go great lengths with Dr. Snow in support of his peculiar views, we are obliged to stop whenever we meet this absolute conclusion [“that the morbid material producing cholera must be introduced into the alimentary canal”] (Richardson 1855, 134).

I can only imagine how disappointed Snow must have felt when he read this review, for he was adamant that the physiology of cholera communication did not permit both mechanisms. Richardson also poured salt on this wound by allying himself with the man who was the co-discoverer of the oral-fecal theory (but who always insisted that Snow deserved priority in publishing the idea): “The view that the specific cause of cholera may be carried by the air into the lungs, we are glad to see supported by Dr. Wm. Budd . . .” (135).

Richardson (1855) also had reservations about Snow’s South London study, an “experiment based on a grand scale . . . [in which] three hundred thousand persons were as regards water supply divided into two groups: one group drinking water containing the sewage of London, the other having water quite free of such impurity” (140). Then came two qualifiers: “If the facts collected with great labour by Dr. Snow are to be trusted as we now have them before us [in MCC2] . . .”; and “should time prove the statement [the 14:1 mortality ratio between the two supplies during the first four weeks of the 1854 epidemic] to be mainly correct, it must be set down as one of the most important demonstrations ever brought forward of the effects of an impure water on a vast population” (140). Perhaps Richardson felt that he, too, had wronged Snow when he was alive. To disagree about theory was part of the game, but to question a man’s investigative integrity and defer acceptance of his results until some unspecified time in the future is an odd way “to stand on his side.”

My comparison of passages in the memoir with Richardson’s 1855 review essay was the final stage in sorting possible motivations for Richardson’s two canards. Two slight and deft alterations to what Snow had established during the 1854 London cholera epidemic would assuage a guilty conscience and slam Snow’s critics. By making the 14:1 mortality ratio that Snow (and Whiting) had found for the initial four weeks stand for the
entire epidemic, Richardson expressed belated trust in his friend’s investigation and forcefully rejected the 3.5:1 ratio in the Simon Report. And by claiming that removal of the handle on the Broad Street pump had halted a horrific local cholera outbreak, Richardson now said there was no need to wait for further evidence; the vestry’s decision to follow Snow’s recommendation proved “the effects of an impure water on a vast population.” Nothing need be said about other vehicles of transmission.

Additional documentation & unpacking
The map detail showing Richardson’s address in Hinde Street is from Godfrey, 7.51 (St. Marylebone 1870) in the London Large Scale Series; David Zuck scanned and sent it to me as an attachment.

All internal speech ascribed to Richardson (1858) is based on documentary evidence in the memoir. In particular, I emphasized the sense of loss he expressed in the preface (iii-iv), the manner in which he described Snow’s critics in the section on cholera (xix-xxii), the way in which he discussed Snow’s fondness for controversy (xxx-vii-xxxvii), and the final paragraph of the memoir where he described Snow’s burial (xliv). Richardson last saw Snow alive on Wednesday evening, 9 June 1858 (xlii).

The October date for publication of On Chloroform is taken from an advert <http://johnsnow.matrix.msu.edu/work.php?id=15-78-15A>.

Murchison’s summation of the autopsist’s findings appear in Richardson (1858, xliv). My formulation of Richardson’s critical reaction to Snow’s adoption of a vegan diet and teetotalism is based on the memoir, where Richardson described the former as “unfortunate,” “the experiment did not answer; the health of our pure vegetarian gave way under the ordeal”; “he [Snow] admitted that in his own case his health paid the forfeit of his extreme adherence to an hypothesis” by John Frank Newton. Richardson, a temperance man himself, wrote that Snow “took the extremity of view and of action” when he “joined the ranks of the total abstinence reformers” (all quotes from 1858, iii).

Snow frequently mentioned that he tested the effects of anaesthetic agents on himself before administering them to patients. He often preceded such tests with experiments on various animals (for example in 1858, 23).

I purposely made the source of the meditation on Snow’s 1857 photographic portrait vague. Richardson may have had access to it, but it would be a stretch to invoke historical possibility. I pose two questions about the appearance of Snow’s hands in the photograph: the first reflects notions of blending inheritance and the inheritance of acquired characteristics, both of which could fit comfortably within Richardson’s pre-genetic worldview. The second question could be Richardson or myself pondering the post mortem findings. Richard Ellis used this photograph as a
Historiographical Unpacking — Prologue

by AMJ.

The narrative form

Historical narration in the second postlude is similar to the first in featuring a combination of reported indirect and free indirect styles. Sometimes I report what I found in documentary evidence: “At first, Richardson dealt with the sudden loss and disappointment by tidying the project Snow was working on when the fatal stroke occurred.” Richardson wrote in the preface to On Chloroform (1858) that “the completion of the work was his last act and deed. In editing the book, therefore, all that remained to be done consisted in the construction of the index, and in this a plan suggested by the author himself has been followed” (iii). Richardson also added “a word or two” to the last sentence Snow was writing when the stroke occurred (423).

Sometimes I use a sentence or two in reported style to set up the identified person’s internal speech which follows in free indirect style (minus designations of reported indirect style like he said, he thought, etc.) For example: “Richardson had just skimmed the entire draft of the manuscript. Why was he so angry? Something’s off. What could it be?”

The second postlude is different in two respects from the first. There is only one narrative perspective, Benjamin Ward Richardson’s. Second, I did on occasions quote directly from documentary evidence in the second postlude.
For example, I use Blackadder font to convey the process of Richardson writing portions of the biographical memoir; everything in Blackadder is transcribed directly from Richardson’s printed memoir (1858) at the front of *On Chloroform*. Another example of direct speech occurs when the narrative depicts Richardson reacting to Dr. Murchison’s written summation of the autopsy of John Snow:

Richardson noted that “both the kidneys were much contracted and granular, with numerous cysts, the right organ being almost entirely converted into cysts; with the uriniferous tubes either denude, or containing granular disintegrating epithelium” (xliv).

The quotation marks indicate that I have exactly reproduced Murchison’s remarks as quoted by Richardson in a footnote to the biographical memoir.