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“Break-Bone” Fever in Philadelphia, 1780: Reflections on the History of Disease

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SUMMARY: In the Autumn of 1780 an epidemic hit the city of Philadelphia. The symptoms of the disease resembled those of present day dengue fever, and subsequent observers argued that the disease was in fact dengue. But was it? The question forces us to confront the challenges of retrospective epidemiology and how we examine the history of a disease. This paper examines the 1780 epidemic from two perspectives. First, it looks at evidence that the disease was dengue and examines what this tells us about the epidemic and the conditions that caused it. Second, it looks at the disease from the perspective of Dr. Benjamin Rush, who treated hundreds of patients during the epidemic. In other words, it examines the disease through the lens of eighteenth century medical ideas. The paper concludes that each approach is valuable and reveals different aspects of the relationship between society and disease.

KEYWORDS: breakbone fever, dengue, Benjamin Rush, Philadelphia, epidemics

The summer of 1780 was filled with promise for the residents of Philadelphia. While July and August were unusually hot and punctuated by...
the usual outbreaks of fever and dysentery, along with disturbing reports from the war, the British occupation of the city had ended and the naval blockade of the Delaware River and Chesapeake Bay had been broken. Merchant ships were slowly returning to the port of Philadelphia. While the war continued, it had moved south. These events, combined with the expectation of an exceptionally good wheat harvest in the Delaware Valley, signaled the return of better times.¹

On the 19th of August, however, the air turned suddenly cool. The next day an epidemic broke out among the city’s residents. Dr. Benjamin Rush, who provided a detailed account of the outbreak, claimed that scarcely a family, and in many families, scarcely a member of them, escaped it.² The diaries and letters of other city residents confirm Rush’s observation. Rush identified the disease as a true “bilious remitting fever.”³

He observed that the fever remitted every morning and sometimes in the evening and that the exacerbations of the disease were worse every other day, with two exacerbations observed in one day. Many patients also developed a rash in the third or fourth day. The rash was accompanied by a burning in the palms of the hands and soles of the feet. Some patients reported nausea and vomiting. Yet the most dramatic symptom of the disease was pain.

The pains that accompanied this fever were exquisitely severe in the head, back, and limbs. From these circumstances, the disease was sometimes believed to be a rheumatism; but its more general name among all classes of people was the break-bone fever.⁴ Rush noted that in some cases the disease produced hemorrhagic symptoms, leading occasionally to the death of the patient. Many of the patients who recovered from the disease suffered from weakness and “a dejection of the spirits” for an extended period of time. Because of this, one of his female patients suggested that the disease should be called “break-heart” instead of “break-bone” fever.

². In his published accounts it is unclear whether his comment on the extent of the epidemic applied to the city as a whole or to Southwark where the epidemic began. However, his journal notes indicate that the reference was to the whole city. Benjamin Rush, “An Account of the Bilious Remitting Fever as It Appeared in Philadelphia in the Summer and Autumn of the Year 1780,” in Medical Inquiries and Observations, vol. 2, 2nd ed. (Philadelphia: J. Conrad and Company, 1805), 119–20. Benjamin Rush, Accounts of Epidemic Diseases and Chronic Diseases, vol. 1, 1779–1789 (Yi2/7263 #1/Historical Society of Pennsylvania).
⁴. Ibid., 122.
The disease was widespread, affecting young and old, men and women, blacks and whites. Rush reported having treated upward to a thousand patients suffering from the disease between mid-August and early October. In a letter to John Dickinson he noted, “I have not known much more than half the number of people sick at one time in this city during the 20 years that I have known it in the line of my profession.” Rush and several other local physicians contracted the disease themselves.5

Rush attributed the outbreak to excessive buildup of bile, influenced by climatic factors, in those who came down with the disease. In particular he noted the importance of the southwest winds passing over the marshes to the south and west of the city. He also observed that “moschtesoes were uncommonly numerous during the autumn. A certain sign of an unwholesome atmosphere.”6

The disease persisted until early October, when the weather turned cold, accompanied by rain and an easterly wind. With the change in weather the epidemic subsided.

What Was the Disease?

Since the end of the nineteenth century medical authorities have asserted that the cases of “bilious remitting fever” that Rush treated in Philadelphia in 1780 were in fact cases of dengue fever, the symptoms of which appear to be similar to those described by Rush in his account. Rush is often credited with having produced one of the first detailed clinical descriptions of dengue fever.

Scott Halstead, perhaps the leading authority on dengue fever today, commenting on Rush’s account, concluded, “There is only one etiology known to produce this constellation of acute phase and convalescent symptoms—the dengue virus.”7

5. Rush, *Accounts of Epidemic and Chronic Diseases* (n. 2). Rush noted in his journal that he saw 255 patients in September and on a single day in that month “visited upward of sixty patients, all in different—and some in very remote—parts of the town and most of them had one disorder, viz. the bilious remitting fever. . . . [Never before had I seen] more than half the number of people sick at one time in this city during the twenty years that I have known in the line of my profession.” BR to John Dickinson, September 3, 1780, Dickinson Papers, Historical Society of Pennsylvania, cited in David Hawke, *Benjamin Rush: Revolutionary Gadfly* (Indianapolis: Bobbs-Merrill, 1971), 241–42.

6. Rush, “Account of the Bilious Remitting Fever” (n. 2), 119. Dr. Lind refers to the Scottish physician James Lind (1716–94), who developed a cure for scurvy and wrote extensively about tropical diseases in the Caribbean.

According to the CDC,

Dengue fever is characterized by sudden onset after an incubation period of 3–14 days (most commonly 4–7 days) of high fevers, severe frontal headache, and joint and muscle pain. Many patients have nausea, vomiting, and a maculopapular rash, which appears 3–5 days after onset of fever and can spread from the torso to the arms, legs, and face. The disease is usually self-limited, although convalescence can be prolonged.8

Like the victims of Rush’s bilious remitting fever, dengue patients frequently experience some form of bleeding, which in extreme cases can give way to dengue hemorrhagic fever (DHF), or dengue shock syndrome (DDS), both of which can be fatal.

Today we believe that dengue fever is caused by a flavivirus closely related to the virus that causes yellow fever. It is most commonly transmitted among human hosts by the *Aedes aegypti* mosquito, which is also responsible for the transmission of yellow fever. According to the WHO, Dengue is the world’s fastest-spreading tropical virus and represents a “pandemic threat” across large areas of the globe, infecting perhaps as many as four hundred million people a year.9

I stumbled upon Rush’s description of bilious remitting fever as part of a larger project on the history of dengue. I was excited to find a description of a dengue outbreak from the eighteenth century and decided to combine it with other references to the outbreak contained in letters and diaries of people living in Philadelphia at the time to construct a detailed account of this early dengue epidemic for the first chapter of my book.

With this goal in mind, I boldly stepped back into the world of colonial Philadelphia and early modern medicine, a world with which I had little prior experience or knowledge. Most of my research has focused on the late nineteenth and twentieth centuries, a world in which diseases are defined by microscopes, blood slides, x-rays, and sputum cultures, and more recently ELISA and PCR tests.

It did not take me very long to realize that like Alice I had fallen down a rabbit hole, or as Dorothy notes in *The Wizard of Oz*, “Toto, I don’t think we are in Kansas any more.”

The more I tried to understand what Rush and his contemporaries were experiencing, how they understood “bilious remitting fever,” the more I began to question the translation of this concept into dengue fever. Was Rush’s bilious remitting fever dengue fever? On what basis could I draw this conclusion?

To answer, or even ask these questions, forced me to come face to face with the challenges of historical or retrospective epidemiology—something I had carefully avoided in my previous work. Having been trained as an African historian and formally entered into medical history late in my career, I had also avoided engagement with the cultural wars that have surrounded retrospective epidemiology and diagnosis within the history of medicine.

I want to use the case of break-bone fever to explore some of the issues raised by retrospective epidemiology and to reflect more generally on the practice of writing the history of disease.

**Retrospective Epidemiology and Its Critics**

In 2000 Adrian Wilson published a paper titled “On the History of Disease-Concepts: The Case of Pleurisy.” Wilson identified two approaches to the history of disease. One he labeled the *historicalist-conceptualist* approach, which he argued “takes concepts of disease as objects of historical study.” The other he called the *naturalist-realist* approach, which he claimed “excludes disease concepts from historical investigation, because it takes modern disease concepts as the mirror of natural reality.” For the naturalist-realist, historical disease concepts are merely cultural constructions of present-day diseases, in Charles Rosenberg’s words, ways of framing disease.

Perhaps no clearer example of the contrast between these two approaches can be found than by comparing the first two volumes of the Johns Hopkins Biographies of Disease series: Steve Peitzman’s *Dropsy, Dialysis, Transplant: A Short History of Failing Kidneys* and my own book, *The Making of a Tropical Disease: A Short History of Malaria*. Peitzman’s book is very much in the vein of Wilson’s historicalist-conceptualist approach, carefully tracing the history of concepts of disease of the kidneys. By

11. Ibid., 276.
14. It is not clear to what degree Peitzman views the various ailments he describes through the course of his book as historically situated understandings of a single disease entity, which we now know as nephritis. Is he engaging in an exercise in framing disease to use the series editor’s much-written-about construct? In other words, does he believe that Bright was studying and treating nephritis? I wish to thank Lisa Boult for pointing out the limitations of Peitzman’s conceptualist approach in this book.
contrast, my study is grounded in the assumption that a disease, which we now call malaria, has existed as a distinct biological entity for centuries. The book examines the conditions that have shaped its distribution over time and space as well as efforts to control it.

All my work on the history of disease has in fact taken a naturalist-realist approach. While I have described how understandings of disease and responses to them have changed over time, I have always assumed that the diseases I have studied have had a long history and that the etiologies and pathologies of malaria or tuberculosis have changed little over centuries.¹⁵

I am of course not alone in this approach. A number of historians and historical demographers have assumed that it is possible to trace diseases and their effects on human populations over time using historical sources. Most recently, John McNeill’s prize-winning book on ecology, disease, and war in the greater Caribbean, *Mosquito Empires*, uses historical sources to reconstruct the histories of malaria and yellow fever from the seventeenth century to the early twentieth. He bases his history on current understandings of the epidemiology of these two diseases, assuming not only the historical existence of both diseases, but that they interacted with human populations in much the same way that they do today.¹⁶ McNeill’s argument resembles those of his father, William McNeill, whose *Plagues and Peoples* became a model for naturalist-realist approaches to the history of disease.¹⁷

McNeill acknowledges that this approach is not without problems, stating in a footnote, “Retrospective diagnosis is always uncertain to some degree. . . . Even with yellow fever, there is a real possibility of confusion and mistaken historical diagnosis.”¹⁸ And yet his reservations are not mentioned again in the rest of his study.

¹⁸. “Retrospective diagnosis is always uncertain to some degree. Dengue is also a hemorrhagic fever, with all the same symptoms except the black vomit. Louse-borne relapsing fever also mimics yellow fever symptoms fairly closely, and even malaria and typhoid can be mistaken for yellow fever. Infections manifest themselves differently in different victims; pathogens evolve; seventeenth- and eighteenth-century observers leaped to conclusions. Even with yellow fever, there is a real possibility of confusion and mistaken historical diagnosis.” McNeill, *Mosquito Empires* (n. 16), 34n54.
Closer to home, Philadelphia has been the focus of a number of studies by historians and demographers who have traced changing patterns of disease, mortality, and population growth. These studies have employed local bills of mortality and other historical sources to reconstruct the causes of mortality in Philadelphia from the seventeenth century through the early nineteenth. All of the studies acknowledge the difficulty of using historical sources and “the ambiguous nature” of eighteenth-century disease categories. Yet they put these reservations aside when making arguments about the changing demography and disease ecology of the city.

Retrospective epidemiology has of course also been at the center of debates over the identity of the black death. One of the most vocal critics of retrospective epidemiology has been Andrew Cunningham, who argued that comparisons of current and historical descriptions of plague make no sense. For Cunningham the laboratory revolution radically transformed how we understand and define disease, creating disease entities for which there are no equivalent earlier concepts or categories. What counted as a disease and precisely how a disease was diagnosed changed beyond recognition. He concluded that retrospective epidemiology tells us very little about the past. To the contrary, it distorts the past by projecting backward present-day issues and concerns. Cunningham in effect staked out what one may label a fundamentalist historicalist-conceptualist position regarding historical epidemiology. In essence he tells us, don’t do it. David Harley and others have made similar arguments.


22. Ibid., 242

23. David Harley, “Rhetoric and the Social Construction of Sickness and Healing,” Soc. Hist. Med. 12 (1999): 407–35: “The accounts of internal ailments in the past are incommensurable with modern accounts because they were framed by a very different understanding. … Even if greensickness, chlorosis and anorexia nervosa can be regarded as physiologically identical, the ascribed aetiology, cultural meaning and therapeutics are so different that they are not the same disease” (417–18).
Historicist arguments pervaded the field of medical history in the 1970s, 1980s, and 1990s. They were a kind of litmus test that distinguished “serious” historians of medicine (read: those with Ph.D.s) from physicians who wrote history and often engaged in retrospective diagnosis. They were part of the culture wars that fractured the field and led to the unfortunate departure of many MD historians from the AAHM.

Yet, historicist criticisms of retrospective epidemiology have not gone unchallenged. For example, Cambridge-based paleopathologist and physical anthropologist Piers D. Mitchell acknowledges that there are serious methodological limitations to retrospective epidemiology when all one has to go on are historical descriptions of disease. Yet he insists that with care one can navigate these obstacles. More importantly, he argues that whether or not retrospective epidemiology can tell us anything about the past is dependent on what you want to know about the past.

If you want to use diagnosis to understand the people and social context of that society, then a modern diagnosis may convey a modern social perception of that disease if people are insufficiently critical. However, if we want to use that diagnosis to understand the micro-organism causing that disease, how it was spread around the world, and whom it chose to infect, then making a modern biological diagnosis is not only reasonable, but is a key part of the research process.\(^\text{24}\)

This argument sidesteps Cunningham’s objections by making a kind of ends-justifies-the-means argument. But it tells us a great deal about what is at stake in these debates over the utility of retrospective epidemiology and why they persist. It is the work that retrospective epidemiology does for a diverse group of scholars interested in tracing disease, or population shifts across time, or in using disease to better illuminate the changing social and economic contours of societies over time, that causes them to at once acknowledge the limitations and pitfalls of this practice, and yet insist that it can be done. This is also why I have followed this path in my own work.

I came to the history of disease as a way of interrogating the impacts of colonial and later postcolonial development on the lives of people living in Africa by tracing the changing contours of sickness and health. More generally, I have employed the history of disease as a way of illuminating the complex ecological relationships that link social, economic, and biological processes together to produce disease states. It is obviously more difficult to trace these relationships over time if one has to continually

question the historical existence of the disease one is studying. So like others I have acknowledged the methodological problems inherent in this approach, but pushed ahead as if they did not exist.

In this essay I want to suggest that we need to take seriously the historicalist-conceptualist critique of retrospective epidemiology and do more than simply acknowledge that a methodological problem may exist. I am not suggesting that we should stop engaging in retrospective epidemiology. There are clearly historical insights to be gained from this approach. Rather I argue that we need to be more cautious in our use of historical sources and more aware of how simple translations of historical descriptions of disease into current biomedical categories can distort these sources as well as prevent us from fully appreciating the world from which they came. Retrospective epidemiology is possible and useful when done carefully. At the same time, I want to suggest that those of us practicing historical epidemiology have much to learn from historicalist-conceptualist approaches to the history of disease.

To do this I propose to revisit Rush’s account of the 1780 outbreak in Philadelphia from two perspectives. I will begin with my realist hat set firmly on my head and make the case that Rush was describing an epidemic of dengue fever. I will also highlight how a realist approach can illuminate various aspects of life in Philadelphia at the end of the eighteenth century.

I will then change hats and try to read Rush’s account through the lens of eighteenth-century understandings of disease and medicine. What was it that Rush saw? I want to suggest that what Rush saw only roughly corresponds to what we now call dengue fever. Rush was describing a disease that existed in a different conceptual universe from that in which dengue exists today. This is not to say that dengue did not occur in Philadelphia in 1780, merely that it is difficult to draw a straight line between Rush’s bilious remitting fever and present-day dengue fever. Moreover, to read bilious remitting fever through the lens of dengue fever prevents us from appreciating the complexity of Rush’s understanding of disease and the richness of the world of medicine and disease in which Rush and his contemporaries lived.

Finally, I will ask how Rush’s account of bilious remitting fever became an iconic description of dengue fever, given the difficulty of translating eighteenth-century disease concepts into twentieth-century disease categories.

I will end by reflecting briefly on what this case tells us about the writing of the history of disease. I want to suggest that we need to move beyond the culture wars between realists and historicists and recognize that each approach, applied critically, can make important contributions to our understanding of historical disease experiences.
The Realist View

Is Rush’s account of bilious remitting fever in Philadelphia consistent with what we now know about more recent dengue fever outbreaks? I have tried to answer this question by going beyond the similarities in symptoms, which most physicians have relied on to make their claims that Rush was describing an epidemic of dengue fever. I will employ evidence provided in Rush’s published and unpublished accounts and other writings and journals, along with the writings of others in the city, and local newspaper reports, to reconstruct how the epidemic spread and the conditions that may have given rise to it. In other words I will try to reconstruct the epidemiology of the outbreak.

The first thing to note is that the sudden onset of the epidemic and its very rapid spread through the city were characteristic of dengue outbreaks, which are often described as explosive. Table 1 charts Rush’s visits during the period of the epidemic.

The fact that mosquitoes were unusually abundant in the city at this time raises the possibility that the people of Philadelphia were suffering from one or more vector-borne diseases. While there is no way for us to know for certain what species of mosquitoes were present in Philadelphia in the summer and fall of 1780, there is reason to suspect that the \textit{Aedes aegypti} mosquito was a frequent visitor to the city. The mosquito is generally assumed to have been introduced into the Americas from Africa through the slave trade and to have produced outbreaks of yellow fever in Philadelphia earlier in eighteenth century. Today the \textit{Aedes aegypti} mosquito occurs widely in the eastern United States. Its normal range is limited to the southeastern United States, but it has been known to extend its range as far north as New York in particularly warm years.

The pattern by which bilious remitting fever spread in 1780 is also consistent with a vector-borne disease. According to Rush, the disease began in Southwark, a port area located on the south side of the city along the Delaware River. Diseases introduced by ship frequently found a fertile breeding ground in Southwark, and the neighborhood became a launching pad for a number of epidemics in Philadelphia.

The conditions that existed in Southwark may well have encouraged the breeding of \textit{Aedes} mosquitoes. \textit{Aedes} mosquitoes are notorious urban

25. \textit{Aedes} mosquitoes are skittish and frequently bite several people in the course of a feeding. This accounts in part for the explosive nature of dengue epidemics.

breeders, and they prefer to breed in collections of clean water associated with human habitation. Southwark was a poor working-class neighborhood, occupied by dockworkers, sailors, and other members of Philadelphia’s laboring poor, who could not afford the higher rents of central Philadelphia neighborhoods. Incomes were low, housing poor, sanitation and diets inadequate. There was no piped water, so water was collected in open barrels and cisterns, which could provide ample breeding grounds for *Aedes* mosquitoes.\(^{27}\)

Another source of the mosquitoes that were so present in Philadelphia in the summer and autumn of 1780 may have been the area to the south of the city where the Delaware and Schuylkill Rivers joined. In his “Account of the Climate of Pennsylvania and Its Influence upon the Human Body,”

\(^{27}\) Benjamin Rush, “Account of the Climate of Pennsylvania and Its Influence upon the Human Body,” in *Medical Inquiries and Observations*, vol. 1 (Philadelphia: J. Conrad and Company, 1805), 74–75. It is unlikely that these marshes would have provided breeding places for *Aedes aegypti* mosquitoes, which prefer clear water sources. However, the swamps may well have provided breeding grounds for *Anopheles* mosquitoes and thus contributed to the annual autumnal epidemics of malaria.
Rush observed that these lands had been covered with woods, which the British had cut down during the winter of their occupation. The denuded countryside had turned into marshland. In a classic example of Hippocratic environmentalist thinking, Rush noted, “The influence of winds upon health, depends very much upon the nature of the ground upon which they pass. Winds which pass over mill-dams and marshes in August and September generally carry with them the seeds of fevers.” He further concluded, “It is a well known fact, that intermitting and bilious fevers have increased in Pennsylvania in proportion as the country has been cleared of its woods, in many parts of the state.”

Rush tells us that from Southwark the disease moved north and east, along Front Street, keeping to the city districts closest to the Delaware River. For a while it was known as “Front Street Fever.” He also notes that the winds were from the southwest. These winds would have pushed infected *Aedes* mosquitoes toward the city’s central districts and may explain why the disease did not spread very far west of Fourth Street. *Aedes* mosquitoes are notoriously weak fliers. Figure 1 shows the location of the cases Rush treated during the outbreak. Interestingly, the earlier yellow fever outbreak in Philadelphia in 1762 followed this same geographical pattern. In both cases, as well, the epidemics ended with the coming of colder weather as one would suspect with a vector-borne disease.

Dengue outbreaks, of course, need more than the presence of appropriate vectors; the virus also needs to be present. The presence of dengue virus in Philadelphia in 1780 can be explained in two ways, which are consistent with Rush’s description. The first possibility is that the virus was introduced into the city from some other community. Here the most likely possibility is that the disease was introduced by the arrival of a ship from a port in the Caribbean or a southern colony where dengue was present. In more recent times dengue has spread across the globe from port to port in this manner. The initial appearance of dengue in Southwark, a port area, is consistent with this possibility. But from where and how did it arrive in Southwark?

28. Ibid., 44–45.


30. We have no descriptions from this time period of dengue-like epidemics anywhere else along the Eastern Seaboard or the West Indies, from where the overwhelming majority of the ships arriving in Philadelphia during the Revolution came. Of course that
One intriguing possibility is suggested by this passage in the *Philadelphia Gazette* on August 23, 1780: “Last Thursday the brig Bellona arrived here as a flag from Charlestown. … By the flag we learn, that the enemy as well as citizens were very sickly.”31 “Flag” was a term used to describe a neutral ship. Charlestown referred to Charleston, South Carolina. From other sources we know that the British forces that had captured Charleston after a siege in May 1780 suffered greatly from “fever and ague” during the summer months of July and August. McNeill claims that malaria was a major problem for Cornwallis’s troops, but he notes that neither yellow fever nor malaria was a serious problem in Charleston.32 So what does not mean that dengue was not present in these areas. The presence of a large number of African slaves in both the Caribbean and the southern United States makes it likely that dengue was present in these areas. The absence of epidemics may reflect low population densities. Dengue transmission cannot be sustained in populations under ten thousand people.

was causing the residents of Charleston to be “very sickly” in the summer of 1780, as reported by the crew of the Bellona. Was it dengue? If so the Bellona may have conveyed a passenger suffering from dengue, or an infected mosquito. The Aedes aegypti mosquito demonstrated a remarkable ability to travel by ship around the globe from the seventeenth century onward, by breeding in water barrels. Aboard ship A. aegypti could sustain the transmission of dengue or yellow fever among crew members for weeks. The “last Thursday” in the Philadelphia Gazette report would place the arrival of the Bellona in Philadelphia on August 17. The incubation period for dengue is four to seven days, so the timing is a bit tight. But as I will suggest shortly, Rush claimed that the epidemic began on August 20, with hundreds complaining of diarrheas and fever. Diarrhea is not a normal symptom of dengue, and it is possible that the actual cases of dengue began later in the month.

This scenario assumes that the Bellona carried infected mosquitoes, which subsequently bit residents of Southwark. If it carried infected passengers who would then have had to infect local Aedes mosquitoes, the new cases could not have occurred for a longer time.

It is also possible that the dengue virus was already endemic in Philadelphia. While we have no other detailed descriptions of dengue-like fevers prior to Rush’s 1780 description, this does not mean that cases did not occur. Rush did not claim to be treating a new disease. In fact, as we will see in a moment, he claimed elsewhere that cases of bilious remitting fever had occurred in Philadelphia in a milder form earlier in the century. His account refers to mild cases appearing in June and July 1780 as well. What was different was the intensity and severity of the epidemic. If dengue had occurred earlier, the upsurge in cases described by Rush in August and September could have resulted from a combination of the increased breeding of Aedes mosquitoes, and/or the introduction of large numbers of susceptible persons.

Rush and other residents in Philadelphia noted that Friends from the Delaware Valley, who visited Philadelphia to attend the Friends annual meeting, were particularly hard-hit by the outbreak. This would support the theory that the epidemic fed on the presence of nonimmune visitors. It should also be noted that the Friends Meeting House was located at the epicenter of the epidemic.

34. “First Day John and Rachl. Watson and Sammy Trimble left us to day the last of our lodgers—Sarah Carry John Balderston, James and Grace Steers, and B. Woodcock went yesterday, John Brinton and Wife and Jessy Richardson the day before H Bornsel—12 lodgers—and daily much Company at meals &c—tho’ not so many as last Year—many of the
The Friends were not the only newcomers to the city. Many traveling poor moved into the city following the end of the British occupation.

They would have been susceptible to dengue infection and are likely to have moved into the poor working-class districts like Southwark.

Rush’s account of bilious remitting fever is largely consistent with what one would expect, had Philadelphia been hit with an epidemic of dengue fever. There are strong similarities between both the symptoms and the epidemiology of the two diseases.

Viewing the epidemic of break-bone fever through the lens of modern dengue, putting aside for the moment the methodological problems involved in using historical sources to reconstruct earlier disease patterns, we are able to see how Philadelphia’s changing political and economic fortunes shaped its physical environment and disease ecology at the end of the eighteenth century. Specifically, it highlights the ways in which changing patterns of commerce, the movements of different groups of people in and out of the city, the removal of trees and creation of swamp lands south of the city, the unsanitary living conditions that existed among the city’s working poor, and shifting wind and temperature patterns interacted to affect the health of Philadelphia’s residents. These interconnections are made visible when one views break-bone fever through the realist lens of dengue fever. It is more difficult to see or understand these connections if one refuses to engage in retrospective epidemiology and discounts the possibility that Rush was describing an epidemic of dengue fever. A realist analysis of the 1780 outbreak can thus contribute to our understanding of the history of sickness and health in colonial Philadelphia.

The Historicalist-Conceptualist Perspective

I now want to change hats and examine this episode through the lens of a historicalist-conceptualist perspective. How did Rush and his compatriots understand bilious remitting fever? What does this perspective reveal

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Country Friends are taken down with this fever which at present is much among us—tho’ none that I have heard of dangerously ill.” Diary of Elizabeth Sandwith Drinker, October 1, 1780. Elizabeth Sandwith Drinker, *The Diary of Elizabeth Drinker: The Life Cycle of an Eighteenth-Century Woman*, ed. Elaine Forman Crane (Boston: Northeastern University Press, 1994), 375.


36. The problem with the endemic model is that dengue does not normally exist in temperate regions of the globe. Bilious remittent fever cases, moreover, appear to have been limited to late summer and fall in Philadelphia. One would have to explain how the virus survived during the rest of the year for it to have been endemic in the city.
about the history of disease and medical practice in eighteenth-century Philadelphia? What does it tell us about the link between bilious remitting fever and dengue fever?

So what did Rush see in 1780? This question immediately raises methodological problems. Rush did not keep casebooks per se. He wrote down observations in notebooks and then drew from these descriptions to produce journals. The journals formed the basis of his later published accounts. The notebooks for this period are unfortunately missing. However, a journal for this period, titled *Accounts of Epidemics and Chronic Diseases*, does exist. It reads like a rough draft of the published accounts, but it contains details that do not appear in the published account of the bilious remitting fever outbreak. I will return to this point later.

To better interpret both his journal and his published accounts I read them against his other writings on disease and particularly fever. Here again there was a problem. His writings on fever and climate were published in *Medical Inquiries and Observations*, which first appeared in 1789. These accounts provide a good deal of information on Rush’s views of the causes, symptoms, and treatment of fevers. Yet this source, like his account of bilious remitting fever, was published nine years after the outbreak. So we need to use these accounts carefully. Ideas about fever were in a state of flux in the late eighteenth century. Moreover, it is apparent from his autobiography that Rush’s understanding of fever was evolving at this time. He seems to have become increasingly unitarian in his view of fever, seeing all fevers as manifestations of a single disease by the 1790s. This view challenged earlier classifications of fever, such as that developed by his Edinburgh mentor William Cullen, who proposed distinct categories of fever along with appropriate treatments.

Just where Rush stood in his changing understanding of fevers in 1780 is difficult to know for certain. That said, what do his published writings tell us about how he understood bilious remitting fever?

To begin with, it is clear that you cannot read his descriptions of fever as if you were reading a set of patient records at the University of Pennsylvania or Johns Hopkins Hospitals. Not only are there no biological markers, blood work, or ELISA or PCR tests, but also eighteenth-century classifications of fever do not correspond to current disease classifications.

Bilious remitting fever was part of a symptomatic nosology. Diseases were known by the symptoms they produced and could vary from person to person depending on an individual’s constitution. They were not distinct entities with specific causes and sets of symptoms, though a number of physicians in Europe and America were moving in this direction, and certain diseases, such as gout, were more delineated than others. For
Rush, fevers were often stimulated by climatic conditions or patterns of behavior—changes in diet, drinks, and exercise. These conditions caused an overexcitation of the blood vessels. In this, Rush was influenced by Cullen as well as by his Edinburgh classmate John Brown. Rush’s belief in the stimulating effect of climate explains why he began his description of the 1780 Philadelphia outbreak with the following passage: “Before I proceed to describe this fever, it will be necessary to give a short account of the weather.”

Bilious remitting fever referred to any fever that rose and fell but did not completely disappear and that was accompanied by the copious expulsion of bile in the form of vomiting and/or diarrhea. It was applied to a range of ailments. For Rush, yellow fever was not labeled as such until the end stages, marked by true black vomit or death. Until that point is was simply another bilious remitting fever. Thus Rush diagnosed the first cases of yellow fever in Philadelphia during the infamous 1793 epidemic, as cases of bilious remitting fever. For Rush, break-bone fever and yellow fever were both manifestations of bilious remitting fever.

Fevers have assumed several new forms since the year 1766. The mild bilious fever has gradually spread over every part of the city. In the year 1780, it prevailed as an epidemic, in Southwark, and in Water and Front streets, below Market-street. In the years 1791 and 1792, it assumed an inflammatory appearance and was accompanied, in many cases, with hepatic affections ... it appeared in 1793 as an epidemic, in the form of yellow fever.

The idea that break-bone and yellow fever were different manifestations of bilious remitting fever reflected Rush’s evolving view that all fevers were but phases or states of a single disease and that fevers could morph from one state to another. Rush described his view of the unitary nature of fever in the following passage from his “Outlines of a Theory of Fever,” originally published as Outlines of the Phenomena of Fevers in 1789.

There is but one fever. However different the predisposing, remote, or exciting causes of fever may be whether debility from abstraction or action, whether heat or cold succeeding to each other, whether marsh or human miasmata, whether intemperance, a fright, or a fill, still I repeat, there can be but one

38. According to Rush’s understanding of the causes of bilious remitting fever, described above, this meant that yellow fever was not contagious. This was a point of considerable medical and political conflict at the time.
fever. I found this proposition upon all the supposed variety of fevers having but one proximate cause. Thus fire is a unit, whether it be produced by friction, percussion, electricity, fermentation, or by a piece of wood or coal in a state of inflammation.40

He went on to observe,

The pulmonary consumption is sometimes transformed into head-ache, rheumatism, diarrhoea, and mania. … The bilious fever often appears in the same person in the form of colic, dysentery, inflammation of the liver, lungs, and brain in the course of five or six days.41

Similarly he argued that typhus was a “state of fever,” not a separate disease, and could shift into other fever states. From this, he concluded,

Many observations of the same kind might be made, to show the disposition of nearly all other diseases to [merge] with each other. To describe them therefore by any fixed or specific characters is as impracticable as to measure the dimensions of a cloud on a windy day.42

Rush’s use of the image of the cloud clearly revealed his view of diseases as entities that took on different forms at different times, their nature constantly shifting, requiring the trained physician to constantly adjust his diagnosis and treatment.

Rush’s increasingly unitarian view of fever was captured by one of his students in a letter to another student in 1795:

Dr Rush who you know is so indefatigable in theoretical pursuit tells us this season that there is but one fever in the World. That Fevers may arise from direct or indirect Debility; from Marsh or human effluvia, but that the Proximate cause is the same. That the multiplicity of fevers according to the system of many Nosologists has done immense mischief &c. That fever is an Unit. That all the different names of fever in the nomenclatures of Nosologists are only different modifications of one disease in the arterial System. … That the Fevers which have been divided into continued, remittent, and intermittent are not distinct fevers, but so many different states of fever, or so many species of one genus.43

41. Ibid., 34.
42. Ibid., 34. Though here again it is important to note that this passage does not appear in his earlier journal account of the outbreak. However, the following passage from the earlier account appears to capture the same sense of the fever passing through stages:
“When the fever did not terminate on the 3rd or 4th day—it run on the 11- 14- & 20 putting on before the 22nd all the symptoms of the putrid & afterwards the usual symptoms of a nervous fever.”
If Rush viewed Cullen’s classes of fevers as but different presentations of a single condition in 1780, it would also explain why he claimed “no other febrile disease was observed during this time in the city.”

Clearly by the 1790s, Rush had come to believe that there was but one fever that could manifest itself in multiple ways, depending on a range of external stimuli, bodily conditions, behaviors, or medical treatments. In diagnostic terms, this meant that the appearance of symptoms that would have led his mentor or contemporaries to reclassify a patient’s disease was viewed by Rush as but a reflection of the evolving states of a single fever.

Because a fever could shift in character, Rush believed that physicians needed to be attentive to their patients’ changing condition and treat their patients accordingly, revising treatment as needed. He further argued that physicians, who were wedded to fixed categories of diseases and related treatments, often missed or overlooked the changing condition of the patient and the need for flexibility in treatment. This meant that Rush would often visit patients several times during their sickness, modifying his treatments in relation to their changing states.

Both his account of the epidemic and his ledger of treatments reflect this pattern of flexibility in treating patients over time. He frequently began with emetic of tartar in order to induce vomiting and reduce the buildup of bile to which he attributed the symptoms of his patients. He indicated that he administered emetic of tarter to nearly all of his patients during the epidemic. If the fever lasted more than four days he applied blisters to the neck and behind the ears. He also noted that he was not opposed to employing opium to reduce the pains associated with the fever, though he also observed that it had salutary effects in promoting sweats and a remission of the fever. Interestingly, he treated a higher percentage of women with opium (24 percent) than men (14 percent), suggesting perhaps the kind of gendered differences in the acknowledgment of pain that Olivia Weisser describes in her work on early modern medical narratives in England.

Rush noted that he did not resort to bleeding in any of the cases he treated, a curious decision given his massive use of bleeding to treat yellow fever patients in 1793. In explaining his decision not to bleed in 1780, Rush indicated that the nature of his patients’ pulses (full but never hard) counterindicated the use of bleeding.

Of course Rush’s services were normally not free, or even cheap. While he provided free care for some patients, his minimum charge for a

44. Rush, “Account of the Bilious Remitting Fever” (n. 2), 121.
46. Rush, Epidemic and Chronic Diseases (n. 2), 1:71.
household visit was 15/-, not an insignificant sum. During the summer and fall of 1780 he visited many households on multiples occasions, resulting in cumulative payments of £1 to £3. Some households, which he visited frequently during the period, had much higher bills. For example, for the month of October Henry Lalor paid £15.\textsuperscript{47} This would equal £1,245 in 2012.

While much has been made of Rush’s services to the poor, it is clear that the majority of his clientele were relatively well off. This fact raises interesting questions: Did the nature of his practice shape his thinking about fever? Were his ideas about the shifting nature of fevers a product of a medical practice that allowed him to visit patients almost daily? In other words, were they a product of “rich man’s medicine”?\textsuperscript{48} While carefully tracking changes in a patient’s condition was classic Hippocratic medicine, the ability to actually do so and earn a living was facilitated by a clientele that could afford repeated services. Rush claimed in his autobiography that he owed much of what he knew about medicine and his contributions to medical science to his experience treating the poor. Yet it may well be that his evolving ideas about fever owed more to his treatment of his well-off patients.

So what are we to make of Rush’s description of bilious remitting fever? To begin with it is rich in detail. Clearly for Rush the devil was in the details, and he carefully recorded the changing state of his patients. As a result, his descriptions of cases are much richer than any current description of dengue fever, a point I will return to below. Second, his description includes a remarkably wide range of symptoms. In fact his published account of the outbreak included only a sample of the total array of symptoms he evidently encountered. A fuller list can be found in his journal \textit{Account of Epidemics and Chronic Diseases}.

Many of these symptoms are difficult to correlate with the symptoms included in present-day descriptions of dengue. Some appear to be incompatible with dengue. Thus some of Rush’s cases experienced jaundice, some intermittent fevers, and still others hemorrhaging from the bowels. Similarly he claims the epidemic began with many hundreds of complaints of diarrheas. All of these symptoms are uncharacteristic of dengue.\textsuperscript{49}

In order to translate bilious remitting fever to dengue fever, one has to explain away these apparent anomalies, to eliminate the noise. One way

\textsuperscript{47} The purchasing power of a British pound in 1780 was equivalent to £83 in 2002 according to Walter Issacson’s biography of Benjamin Franklin. \textit{Benjamin Franklin: An American Life} (New York: Simon & Schuster, 2003), 396.

\textsuperscript{48} I want to thank my colleague Mary Fissell for having pointed this out.

\textsuperscript{49} His journal description of the outbreak in fact begins with this passage: “The air became suddenly cool on Saturday Aug’st 19. Next day 100’ds complained of dyarrheas – fevers &c/- There comp’s is increased during the remaining part of Aug’st & all September.”
to do this is to argue that Rush’s tendency to view different sets of symptoms as stages of a single disease led him to merge a number of what we would view as different diseases into a single description.

Others residents of the city, in fact, suggested that the city’s inhabitants were afflicted with several ailments at this time. For example, Elizabeth Sandwith Drinker, in her diary entry for September 11, observed, “2 or 3 different kind of Fevers are at present prevalent in this City. Numbers are taken off. There are very few Houses, but one or more of the Families are unwell—in some the fever is thought to be puterid.” Putrid fever covered a wide range of conditions, including, according to William Buchan’s *Domestic Medicine; or, A Treatise on the Prevention and Cure of Diseases by Regimen and Simple Medicines*, a number of the symptoms that Rush ascribed to the 1780 epidemic of bilious remitting fever, including severe body and head pains. This manual was a common furnishing of homes on both sides of the Atlantic. Drinker, a keen observer of medical conditions, may well have thought that Rush’s bilious remitting fever was a putrid fever if she read Buchan.

Samuel Rowland Fisher, a successful Quaker merchant who was at the time imprisoned in Philadelphia’s Walnut Street Prison on the charge of being a Tory, and for refusing to recognize the authority of the court, wrote in his prison journal in September,

> The City from the beginning to abt. the 20th of this Month was very sickly, considerable numbers died, I heard of 17. 19. & 27. persons being buried on three several days, great numbers have had a fever, which I conclude from what I hear is of a putrid kind, but most of them have had it but lightly. My Father, each of my Brothers & Sisters have had it & got well thro’ it,—It is a great favour we have our health so well in this house & tis remarkable the sickness has not got amongst any of the prisoners here, tho’ they are crowded in many of the Rooms being about 100 persons in all Men & Women, who live in a very dirty manner& some of them seem to be much abandoned to almost every vice. I heard of three persons who died of the Yellow Fever & Joseph Reed’s wife is said to have died of the flux.

There can be little doubt that the residents of Philadelphia experienced a myriad of health problems for much of the eighteenth century. As

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Curiously, this mention of diarrhea does not appear in his published account of the epidemic, nor is it characteristic of dengue. His journal also contains this passage:

> “for some the disease fell w’th whole force upon the face producing swellings which in some cases terminated in abscesses under the jaw & in the ear.” Rush, *Epidemic and Chronic Diseases* (n. 2), 1:41.

50. Drinker, *Diary* (n. 34), 375.


William Currie noted in 1792, “Some one or more contagious disease, being always more or less prevalent in the city, is one reason why a greater proportion of children die annually in the city than in the country.” Rush’s ledger for the months preceding and following the bilious fever outbreak also reveals that his patients suffered from a steady stream of sickness.

It is thus possible that multiple diseases were occurring in Philadelphia in the autumn of 1780. It is also possible that some of the patients Rush treated suffered from multiple afflictions. This would explain why his account of bilious remitting fever included symptoms that do not correspond to those of dengue fever. Yet to argue that Rush’s description contains symptoms from several diseases involves putting on a realist hat. It entails pouring his account through a filter of current understandings of disease.

We make bilious remitting fever look like dengue by finding a way to filter out those symptoms that do not fit our understanding of dengue, to filter it until it conforms to what we know as dengue. In other words, we must engage in a process that distorts bilious remitting fever beyond recognition. Or more precisely, to turn it into something Rush never saw. It in fact requires us to turn the category of bilious remitting fever into “break-bone” fever, a popular term that defined the disease by those symptoms that most resemble dengue. Remember Rush did not call the disease by this name. He mentions the term only once in his description, noting that this is what many people in the city called it. Yet it is the term that is often used by later observers who wished to link dengue fever with the disease Rush described.


54. A number of the residents who kept diaries reported having experienced on and off bouts of fever, or “fever and ague,” over the course of the summer and autumn of 1790. They noted similar experiences among their relatives and neighbors. Philadelphia residents also seem to have recognized the existence of different types of fever, though their main distinction appears to have been between fevers that were “dangerous” and those that were not. Dangerous fevers could kill you. Several observers noted that the outbreak that occurred in the fall of 1780 was severe but “not dangerous.”

55. It is of course impossible to know for certain how an epidemic of dengue fever would have affected a population exposed to the specific pathogens that were common in Philadelphia and whose nutritional state may have been quite different from those of contemporary populations. On the other hand, it could be argued that their overall health condition is unlikely to have been worse than that of populations living in slum communities in Delhi who suffer from dengue fever today.

56. This raises the possibility that those with the fever viewed it through the lens of the pain it created and that their view of the disease fits more closely to dengue than Rush’s bilious remittent fever. The problem with this arguments is that try as I might I have found very
Finally, to say that bilious remitting fever is dengue is doubly problematic. It requires us to distort what Rush saw by focusing on only part of his description in order to make it fit our current definitions of dengue. But in doing so, it also inhibits us from engaging with and exploring the world of medicine in which Rush worked. Taken on its own terms, viewed through Rush’s eyes, the epidemic of bilious remitting fever in 1780 allows us to enter into a very different world of medicine. There are no set disease entities with standard cures. Diseases are protean and treatments constantly changing in response to a shifting array of symptoms. Following Rush as he navigated his way through the growing number of cases provides a fascinating doorway into his medical practice, about which, surprisingly, relatively little has been written, compared to what has been said about his theories.57 Trying to turn bilious remitting fever into dengue, on the other hand, discourages us from passing through this door.

Becoming Dengue

Given the disparities between Rush’s descriptions of bilious remitting fever and current definitions of dengue, I found myself wondering how Rush’s bilious remitting fever came to be dengue. I want to end this essay by trying to answer this question.

The term “dengue” was first applied by physicians working in the West Indies and the southern United States in the 1820s to a constellation of symptoms similar to those described by Rush. The physicians describing these outbreaks questioned whether they were seeing a new disease or one that had been seen before. In this context they frequently compared what they were seeing with Rush’s earlier description. As Katherine Arner has recently shown, physicians in the Caribbean and eastern United

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57. For example, in his account book (Journal B) he records the following visits and treatment of one John Bogart: “Sep 6 (pulv:antimony), Sep 8 (pulv:nitros), Sep 9 (p.c.p.), Sep 10 (sal Glauh, pil:anod), Sep 12 (pulv:antimony), Sep 13 (pulv:antimony), Sep 14 (pil:anod, episc:carp), Sep 15 (bol:vol), Sep 16 (bol:vol), Sep 19 (pil:anod, bol:vol, episc:col), Sep 20 (pulc:met, bol:vol, c.p., ing:corrob).” While the journal does not indicate the various forms that Bogart’s illness took, the diversity of the treatments employed by Rush to treat Bogart reflects the iterative nature of Rush’s method of treatment.
States were part of an emerging community of “America” physicians who exchanged ideas regarding diseases that were common in the region.\(^{58}\) As often as not, these physicians distinguished the two diseases. In other words, they questioned whether what they were seeing was what Rush had described. Writing in 1839, Henry Dickson, M.D., compared epidemics of dengue that had occurred in the Caribbean and southern U.S. cities in the 1820s with Rush’s description of bilious remitting or break-bone fever. He concluded,

> On the whole, it seems reasonable to conclude, that the two maladies, although presenting some curious coincidences, are in nature essentially distinct and different. One being an eruptive fever, new, specific, and peculiar, while the other is nothing more than an autumnal remittent, a malaria fever, somewhat modified by an unknown agency.\(^{59}\)

Dickson pointed particularly to the differences in the nature of the rash that accompanies the two diseases.

Among the most prominent differences between nineteenth-century descriptions of dengue and Rush’s account of bilious remitting fever was the nature of the extended period of severe physical debility that often followed the termination of the acute phase of the disease. In Rush’s account, symptoms lingered and were associated with fatigue, malaise, and a dejection of the spirits. By contrast, as Donald Carey has noted, many of the early descriptions of dengue fever in India and the Americas during the nineteenth century included severe rheumatic sequelae and disability. He argues that the disease that was labeled dengue was in fact chikungunya, a viral infection closely related to dengue fever that is also transmitted by the *Aedes aegypti* mosquito and that is characterized by a long aftermath of physical disability.\(^{60}\)

Yet oddly, and despite these differences, Rush’s description is viewed as one of the first detailed accounts of a dengue fever epidemic. Why? How did break-bone fever merge with dengue, and how did Rush’s account of bilious remitting fever become one of the earliest descriptions of dengue fever?

The answers to these questions appear to lie partly in transformations in medical practice that occurred during the second half of the nineteenth

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century and the early twentieth century, and partly in the growing reputation of Benjamin Rush among American physicians researching dengue at the beginning of the twentieth century.

Rush’s account of bilious remitting fever represents a style of medical description that was common in the eighteenth and early nineteenth centuries. Physicians devoted pages to carefully detailing patients’ symptoms. This style of medical description reflected the view that a disease was known by its symptoms and allowed physicians like Dickson to distinguish what he was seeing from what Rush saw.

This view of disease contrasted sharply with the view of physicians later in the nineteenth century who increasingly saw disease symptoms as the product of bodily lesions and, following the bacteriological revolution, as the product of specific pathogens. The shift to an ontological model of disease transformed relations at the bedside. What was important was the causes of the disease, not the symptoms it produced. The scalpel and later the microscope gradually replaced the patient’s description of symptoms as the key to clinical diagnosis. The changing view of what constituted a disease led over time to increasingly simplified descriptions of patients’ symptoms. The change occurred slowly, but by the early twentieth century medical descriptions of dengue were in general much less detailed than they had been a century earlier. As descriptions became less detailed, the distinctions among a number of dengue-like diseases diminished, or were dropped from medical descriptions.

This trend was further advanced by efforts to codify diseases. Critical to the development of a simplified clinical description of dengue was the Royal Society of Physicians’ publication of a *Nomenclature of Diseases* in 1869. The *Nomenclature* was created to perfect “the statistical registration of diseases.” In describing dengue, the *Nomenclature* attempted to describe its essential characteristics and thus mediate the diversity of medical opinion regarding the nature of the disease. The result was a much-condensed description of dengue. “An ephemeral continued fever or febricula characterized by frontal headache, and by severe pains in the limbs and trunk, and sometimes by an eruption resembling that of measles; over the body; occurring in the West Indies.”

The growing simplification of descriptions of dengue removed distinctions between similar diseases, allowing for the merging of cases of dengue

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and break-bone, along with a few other diseases, including presumably what became chikungunya fever, into a single disease entity.\textsuperscript{62}

By the early twentieth century dengue researchers regularly linked the diseases they were studying to Rush’s account of bilious remitting fever. They recognized that the fit was not perfect, but they made it work through the selective filtering of Rush’s description of bilious remitting fever.

Many of these researchers were Americans working in a time when there was a growing adulation of Benjamin Rush as the father of American medicine. Rush’s reputation had come under attack during the early nineteenth century in the wake of the advances in anatomical pathology, which challenged his theories about the unity of disease. However, he received renewed acclaim as a clinician during the latter half of the nineteenth century and the early twentieth century, and his theories were revived with particular enthusiasm within the field of psychiatry.\textsuperscript{63}

These trends combined to tie dengue firmly to break-bone fever. I should note again that physicians trying to link Rush to dengue almost always referred to Rush’s disease as break-bone fever, not bilious remitting fever. In other words, they identified the disease by the name that highlighted the symptoms that appeared to link Rush to dengue.

Dr. E. R. Stitt, writing in the \textit{Johns Hopkins Hospital Bulletin} in 1913, concluded that “there would seem to be good grounds upon which to give the credit of priority to our great American physician and statesman Benjamin Rush, who, under the designation of break-bone fever, gave us a true picture of dengue as it manifested itself in Philadelphia in 1780.” He went on to describe the elements in Rush’s description that do not fit with dengue fever, but then concluded by stating, “it is remarkable that notwithstanding this fact we should be given such a clear picture of the most striking features of the disease under consideration.”\textsuperscript{64}

Fielding H. Garrison in the first English textbook on the history of medicine, first published in 1913, simply asserted that Rush was the first after Bylon of Java, to describe dengue fever.\textsuperscript{65}

\textsuperscript{62} Chikungunya remerged as a separate disease only in the 1950s, when medical technology allowed medical researchers to identify and distinguish the chikungunya virus from the dengue virus.


\textsuperscript{65} Fielding H. Garrison, \textit{An Introduction to the History of Medicine} (Philadelphia: W.B. Saunders, 1929), 379.
Henry Siler and his colleagues who conducted research on dengue fever in the Philippines for the U.S. Army at the beginning of the twentieth century wrote one of the earliest textbooks on Dengue. Like Stitt, they noted the anomalies in Rush’s description, but concluded,

The account of Benjamin Rush, describing the 1780 epidemic in Philadelphia, is so clear there can be no doubt as to the identity of the disease. His description is lucid and corresponds closely with the symptoms of dengue as we have seen in Manila.66

They went one step further and suggested that the descriptions of dengue-like fever in Java and Egypt, which occurred in 1779, may not have been dengue at all, in effect making Rush’s description the oldest detailed account. Thus the linking of Rush’s bilious remitting fever to the modern dengue fever occurred over time as medical narratives became streamlined, blurring distinctions between similar yet different fevers, and as a result of the work of a set of American researchers who explained away the anomalies that exist between Rush’s description and their own to credit one of the founding fathers of “American medicine” with having been the first to describe dengue fever.

Conclusion

So where does this leave us? Was Rush describing an epidemic of dengue fever? I think the answer to this question is clearly no. Rush was describing an epidemic of bilious remitting fever, a disease that existed in a cultural and intellectual world that is distant from that inhabited by dengue fever.

What I am suggesting is more than a process of framing disease as described by Rosenberg. Framing implies the existence of a disease entity that is interpreted or constructed differently at different points of time. What I am suggesting is that bilious remitting fever is a concept that existed independently of dengue fever. It was composed of a constellation of changing symptoms, some of which may have been caused by the dengue virus, but its existence was not dependent on the presence of a dengue virus. It would have existed even if dengue has not been present in Philadelphia in 1780.

Did the people of Philadelphia experience an epidemic of dengue fever? Here I think the answer is less clear. In the absence of paleogenetic

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Evidence, one is reliant on historical texts. One can make bilious remitting fever look like dengue by arguing that Rush mixed diseases together. Whether we choose to do so depends very much on what uses we wish to make of this epidemic. Those of us who wish to trace the history of diseases over time, or who wish to explore the disease ecology of eighteenth-century Philadelphia, may find this selective strategy useful. Those who are interested in understanding the experience and worldview of those who lived through the epidemic and the physicians who treated them have less need to engage in such a calculus. Turning bilious remitting fever into dengue in many ways distracts us from this kind of social and cultural work.

For me both paths are attractive and important to follow. We do not need to choose. Those who prefer to focus on actors’ categories and experiences might benefit from at least considering the possibility that those suffering from bilious remitting fever did in fact have dengue fever. Doing so permits us to better understand the connections that existed between colonial Philadelphia’s changing political and economic fortunes, its evolving population, changing physical environment, and patterns of sickness and health. It also allows us to compare the experiences of Philadelphia with those of other cities where dengue appears to have occurred. Such comparisons allow us to make generalizations about the ecology of the disease. Viewing bilious remitting fever as dengue thus tells us things about colonial Philadelphia that would be less apparent or difficult to see through the lens of eighteenth-century understandings of disease.

At the same time, I would urge colleagues who share my interest in historical epidemiology to occasionally abandon their biomedical categories and stop for a moment to explore the worlds they try to connect across time. Doing so forces us to take seriously the challenges of using historical documents to reconstruct past epidemics, rather than just giving a nod to them. Retrospective epidemiology needs to be based on a more critical use of historical sources. In this case, moreover, seeing the epidemic through Rush’s eye provides a way to interpret the noise of discordant symptoms that fills his account. It provides a historical rationale for pursuing a reductionist strategy.

Engaging critically with one’s sources also opens up a world of medicine that is largely hidden if one insists on viewing the world through the lens of twenty-first-century medicine.

In the end, each approach to the history of disease contributes in its own way to our understanding of historical disease experiences. Each approach also requires a careful use of sources. In exploring Rush’s views of disease it was important to come to terms with the changing nature of his ideas and
the time-sensitive nature of the available historical sources. These sources have to be used with as much care by historicists as by realists.

For a realist like myself, viewing the epidemic that occurred in 1780 through the eyes of Benjamin Rush and his compatriots has given me a new appreciation of the world of colonial medicine and those who practiced it. It was a trip well worth the taking.

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