

# All in the Details

Neeraja Sankaran

In response to "[Parallel Histories](#)" (Vol. 7, No. 1).

To the editors:

It goes without saying that a positive review of a book into which one put so much heart and time for the better part of a decade—indeed, closer to two decades if counting the pre-history of the book's inception—is extremely flattering and gratifying for an author. But an invitation to respond publicly to such a review is something of a double-edged sword. For whereas it is easy enough to write a few or even several hundred words addressing criticisms—valid or otherwise—it is more difficult to respond to praise. Expressions of gratitude and agreement, heartfelt though they might be, do not make for interesting reading.

That said, what made Raghavendra Gadagkar's sympathetic review of *A Tale of Two Viruses* such a pleasure for me were the little things he noticed and remarked upon in different chapters. Reading them roused a thrill of recognition, a kind that is hard to define more precisely, except to call it an instant mental kinship that made me glad that this person read my book.

The first episode that Gadagkar noted as standing out was how Peyton Rous—a key player in my *Tale*—was warned by his medical-school professor, the pathologist Dr. William Welch, to avoid the cancer problem at all costs. Ignoring this advice led Rous to discover the tumor-causing virus that would, 55 years later, garner him the Nobel Prize. What makes this episode even more striking is the fact that cancer research was explicitly *not* a primary goal of the Rockefeller Institute for Medical Research where Rous worked at the time. The institute's founders, holding with the view that the problems posed by cancer were "too complete a mystery for profitable attack," had decided to focus on such fundamental disciplines as cytology, chemistry, and genetics.<sup>1</sup> The general attitude there, reflected in the words of Leonor Michaelis, the head of one of its laboratories, was that "the problem of cancer will not be solved in a cancer institute."<sup>2</sup> Perhaps Michaelis had the right idea. For, while no institute can claim to have solved cancer even to this day, Rockefeller University indeed boasts of being home to the discovery of the "canonical

cancer virus" thanks to the fact that the institute's founding director Simon Flexner hired Rous in 1910.<sup>3</sup>

The second anecdote that caught Gadagkar's attention also concerns Rous and describes his reaction after Flexner erroneously attributed discovery of the tumor virus to both Rous and his colleague James B. Murphy. In writing to Flexner, Rous was anxious to set the record straight, not only to reclaim priority in the matter of discovery, but more importantly, as he would emphasize to the biochemist Joseph Fruton in response to a similar misattribution some decades later, because "This [attribution] carries by implication an indictment, namely that all along through the years I have ignored the rights of a fellow discoverer; never mentioning him."<sup>4</sup>

Rous's reactions to the remarks of both Flexner and Fruton not only offer a glimpse of the personality behind the scientist, they also serve as the perfect example of the vital role archives play in the craft of history. I'm sure that the history of science is rife with many such episodes, but this particular one would have never come to light had it not been for the infectious enthusiasm of Charles Greifstein, who had a hand in curating the Rous collection at the American Philosophical Society. Charles made a special point of sharing Rous's letter to Flexner with me during my research trip to the American Philosophical Society Library.<sup>5</sup> His sharing of this episode primed me for recognizing its reenactment in Rous's exchange with Fruton, which I found a few weeks later. To include the story in my account of Rous's discovery was a natural progression, and I'm delighted that it struck a chord with Gadagkar.

Last, but by no means least, in the list of things that I'm pleased were appreciated is my labeling of lysogeny as a lynchpin. Unlike the Rous stories, this characterization is no little anecdote, but the argumentative basis for my concluding chapter, indeed the entire book. I remember an altogether different reaction during one of my early archival research trips. An animal virologist whom I met in passing seemed surprised and somewhat unconvinced that I was considering drawing parallels between bacteriophage lysogeny and the action of tumor-inducing retroviruses. That Gadagkar—who honed his biological skills working with bacteriophages—not only seems to have grasped my point but also deemed that last chap-

ter the “most interesting of all,” means that the earlier encounter played its role in sharpening the denouement in my *Tale*.

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1. George Washington Corner, *A History of the Rockefeller Institute, 1901–1953: Origins and Growth* (New York: Rockefeller University Press, 1965), 216.
2. Corner, *A History of the Rockefeller Institute*, 217.
3. John Drake and James Crow, “Recollections of Howard Temin (1934–1994),” *Genetics* 144, no. 1 (1996): 2, doi:10.1093/genetics/144.1.1; and “Our History,” *rockefeller.edu*.
4. Peyton Rous to Joseph Fruton, May 4, 1966, as quoted in Neeraja Sankaran, *A Tale of Two Viruses: Parallels in the Research Trajectories of Tumor and Bacterial Viruses* (Pittsburgh: University of Pittsburgh Press, 2021), 19. The letter was in response to Fruton’s review of Corner’s *A History of the Rockefeller Institute*: “The Rockefeller Institute for Medical Research, an Essay Review,” *Journal of the History of Medicine and Allied Sciences* 21, no. 1 (1966): 71–77.
5. For Greifenstein’s own perspective on this episode, see also his blogpost: “Rous and Viruses,” *Blog APS Library*, February 6, 2022.

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