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From Skid Row to Main Street:  
The Bowery Series and the Transformation of Prostate Cancer,  
1951–1966

ROBERT ARONOWITZ

**SUMMARY:** Between 1951 and 1966, more than 1,200 homeless, alcoholic men from New York's skid row were subjected to invasive medical procedures, including open perineal biopsy of the prostate gland. If positive for cancer, men underwent prostatectomy, surgical castration, and estrogen treatments. The Bowery series was meant to answer important questions about prostate cancer's diagnosis, natural history, prevention, and treatment. While the Bowery series had little ultimate impact on practice, in part due to ethical problems, its means and goals were prescient. In the ensuing decades, technological tinkering catalyzed the transformation of prostate cancer attitudes and interventions in directions that the Bowery series' promoters had anticipated. These largely forgotten set of practices are a window into how we have come to believe that the screen and radical treatment paradigm in prostate cancer is efficacious and the underlying logic of the twentieth century American quest to control cancer and our fears of cancer.

**KEYWORDS:** cancer, prostate cancer, history of medicine, efficacy, risk, screening, bioethics

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Starting in 1951 and continuing for over a decade, Columbia University investigators recruited more than 1,200 homeless, alcoholic men from New York City's skid row, the Bowery, brought them to a recently opened public cancer hospital, and subjected them to many invasive tests and procedures, including open perineal biopsy of the prostate gland.<sup>1</sup> If positive for cancer, men underwent radical prostatectomy and surgical castration followed by a course of hormonal treatment. Although some kind of consent may have been obtained, these studies were conducted on poor, helpless men because investigators would and could not do these experiments on people with more autonomy, power, and dignity, such as the paying private patients at nearby Columbia Presbyterian Hospital.<sup>2</sup>

Like other cases of unethical research practices such as those exposed by Henry Beecher in 1966, the *Bowery series* was published in leading medical journals, cited frequently in the medical literature, and was the subject of popular news coverage.<sup>3</sup> These practices were ultimately forgotten and had minimal direct impact on subsequent clinical developments. Yet their history is significant because they provide a provocative and illuminating perspective with which to view subsequent events. The Bowery series was a prescient attempt to combine a set of existing practices for diagnosing and treating prostate cancer into a new early detection and radical treatment paradigm. Very similar practices would gain acceptance decades later. The difficulties of retrospective ethical judgments notwithstanding, we today respond to the invasive procedures done to ill-informed men for uncertain benefit with some disgust and disbelief. Yet very similar practices in the ensuing decades generally have not elicited similar reactions. Why? Comparing and contrasting the *Bowery series'* assumptions, goals, and limited impact to subsequent developments provides some answers. This historical juxtaposition also makes

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visible some underappreciated ethical challenges posed by the ways that mass risk-reducing interventions have gained acceptance within modern medicine and society.

It is unsatisfying to simply observe that medical technologies and practices are accepted because they are effective at saving lives and reducing morbidity. Not only is evidence of scientific efficacy only one reason why medical and lay people accept new technologies and practices, but scientific evidence is often absent or contested. Historians, especially since Rosenberg's influential 1977 essay, have researched the social and historical context within which actors determine whether medical treatments work or not.<sup>4</sup> This "social efficacy" approach, which is also central to contemporary anthropological studies of medical practice, focuses on the work done besides the direct impact on objective states of health.

Pressman noted that "a therapy's usefulness is contingent upon a particular historical era."<sup>5</sup> The corollary is also true. There may be a good deal of historical contingency to a therapy's lack of utility, i.e. its limited social efficacy. Looked at this way, the *Bowery series* is a crucial side story illuminating what needed to happen in order for cancer risk to later get into men's bodies on a mass scale.<sup>6</sup> Contrasting the failure of the Bowery practices to gain much traction with similar practices deemed efficacious in later periods allows us to identify developments—besides evidence of scientific efficacy—that changed the way these similar interventions were later understood, legitimated, and diffused throughout American medicine and society.

## **State of Prostate Cancer Prevention and Treatment Prior to 1950s**

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Efforts to prevent and treat prostate cancer until recently were suffused with pessimism. At the turn of the twentieth century, Johns Hopkins' urologist Hugh Young developed the radical prostatectomy.<sup>7</sup> Although Young claimed cures, the extensive operation was rarely taught or performed for more than a half a century except among Young's disciples at Hopkins and the few Urology programs they populated.<sup>8</sup>

The failure of the radical prostatectomy to take hold was in stark contrast to the successful diffusion of Young's Hopkins colleague William Halsted's radical mastectomy, which was also an extensive, mutilating operation. Contemporary observers cited different reasons for the limited diffusion of the radical prostatectomy.<sup>9</sup> Some observed that 95% of patients were diagnosed when the disease was already metastatic and so incurable by radical surgery. Even those few patients without obvious metastases did poorly after surgery, surviving only a few months more than men with evident metastases.<sup>10</sup> Radical surgery was also mutilating. Over 80% of men became impotent while many others suffered incontinence and other complications. A urology text from 1917 reviewed Young's operation and the occasionally used radium therapy and characteristically concluded that "a small experience with both procedures leaves us in doubt."<sup>11</sup> The disease evoked fear and hopelessness, similar to what a diagnosis of lung or pancreatic cancer evokes today.

The one positive therapeutic development was Charles Huggins' demonstration in the early 1940s that different forms of androgen (male hormone) depletion had salutary effects on patients with prostate cancer. However, it soon became clear that treatments such as surgical castration or estrogens were palliative rather than curative. But these effects were dramatic, readily witnessed, and led to some hopefulness that further progress was attainable.

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At the same time, surgery to relieve prostatic obstruction—the overwhelming reason for prostatic surgery—had revealed many “incidental” cancers in prostate glands removed for apparently benign conditions.<sup>12</sup> Along with autopsy results that found prostate cancer in an increasing number of men as they age,<sup>13</sup> a consensus developed among American urologists that there might be little reason to detect prostate cancer early, as was the push with breast and other cancers. The prognosis of these “latent” cancers was presumably very good because they appeared in living men in otherwise good health and among men who died of other causes. There appeared to be a much higher prevalence of latent cancer than clinically apparent prostate cancer. The implication was that finding such cancers and subjecting patients to surgery would produce more harm than good.

Pessimism about early detection and radical interventions existed at mid-century for reasons besides accumulated clinical knowledge. The prominent campaigns to detect breast and cervical cancer were highly gendered, linking compliance with messages about maternal and marital responsibilities.<sup>14</sup> The bodily mutilation of radical mastectomies and hysterectomies were invisible in these appeals. Perhaps the active promotion of similar prostate cancer messages to men was less appealing to male surgeons, who may have more easily empathized with the operation’s harms—impotence and incontinence—while aware of its limited efficacy.

At mid-century, the one prostate cancer detection practice that was promoted was the routine rectal examination. Besides signs and symptoms of metastatic disease (especially bone pain), an abnormal rectal examination was the most frequent presenting sign of prostate cancer. So there was reason to hope that more subtle findings on rectal examinations might lead to detecting cancer early enough for treatment to be effective. But the promotion of these exams

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was tepid and never become a major cause for urologists, who had long witnessed that rectal findings came too late to make a difference. There may have also been a gendered reluctance to promote rectal examinations on a mass scale.

Perhaps the very first blood tumor marker was one associated with prostate cancer: prostatic acid phosphatase (PAP). First described in the 1930s by Columbia University researchers, PAP's potential to aid in diagnosis and monitoring the clinical course was clear.<sup>15</sup> But, like the findings on rectal examinations, elevated serum levels were almost always associated with incurable, metastatic disease.

At mid-century, there was also a backlash to the optimism-promoting “do not delay” public health campaign in other cancers. Physicians were increasingly skeptical that existing means of prevention and treatment were effective because the mortality from most site-specific cancers had not improved. This skepticism led to a group of self-identified physician “pre-determinists” who argued that at the time of a cancer diagnosis the fate of most individuals was outside of medical care, determined by the poorly understood tumor characteristics and the individual patient's immune status.<sup>16</sup> There was also some pushback on the fear-inspiring public health messages and the bodily mutilation that resulted from radical surgery.<sup>17</sup> Predeterminism and other forms of medical skepticism were minority counter-currents at mid-century but may have nevertheless reinforced the already-dominant pessimistic attitudes held by urologists about the prevention and treatment of prostate cancer.

## **The Bowery Series**

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In the early 1950s, Dr. Perry Hudson was determined to change this prevailing medical pessimism by demonstrating a very different clinical approach. Hudson traced his interest in prostate cancer to a job he held in an interlude in his medical training, where he watched people die miserable deaths at a tuberculosis hospital, some of whom he surmised died of prostate cancer. He learned that “virtually nothing” was done for prostate cancer and “there was no way . . . to make an early diagnosis” except for rectal exam and “people weren’t very good about it. They argued about it. And it was assumed that maybe Dr. Young had a better finger than anyone else.”<sup>18</sup>

After some surgical training and a stint in the Navy, Hudson, with “all these things stuffed in the back of my head,” secured a research position in the urology department at Hopkins under Young’s successor, William Scott. Hudson was soon offered a residency position and successfully made it through their pyramidal system in which fewer spots were available in each succeeding year.

Hudson learned Young’s perineal operation for cancer but observed that there was little improvement of or interest in prostate cancer surgery in the intervening half century. People were still diagnosed with incurable disease and “simply died.” There also was no reliable diagnostic test and no one even imagined screening or early detection. The incidence and prevalence were unknown.

Wanting to pursue research into prostate cancer and eschewing private practice, Hudson took a position at Columbia University. A Damon Runyon Cancer Research Foundation Fellowship supported his prostate cancer research and allowed him to go up “the academic ladder.”

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Hudson was soon appointed head of urology at the newly opened municipal hospital for cancer patients, the Francis Delafield Hospital. New York City paid more than \$7 million to build the hospital and also bore all the costs for care.<sup>19</sup> Many Columbia physicians who had clinical and research interests in cancer had appointments at Delafield and the affiliated Institute of Cancer Research, where Hudson also had an appointment. At age thirty-three, Hudson was put in charge of forty-five beds and a full floor of laboratories at Delafield plus another five urological laboratories at Columbia-Presbyterian. Hudson believed he was given these resources because no one but him knew anything about urological laboratory work. Hudson eventually ended up with seven PhD researchers and twenty-five high-level technicians “in that empire I put together.” He was the only salaried urologist at Columbia, financed by outside grants and contracts. “I was a very peculiar guy to the other doctors.” Hudson recalled that they were uninterested in his academic pursuits. Hudson was also the only person capable of and actually doing radical prostatectomies for cure: “I really was a misplaced priest in a whorehouse.”

With all these resources at his disposal, Hudson decided to launch a major clinical research project that would dispel the pessimism surrounding prostate cancer. Hudson believed that this pessimism was bolstered by mistaken beliefs about the prevalence and natural history of the disease. He noted the received wisdom that most or all men over fifty had cancer in them and that the cancer detected at autopsy or conservative prostatectomy “is of no clinical significance and rarely causes death.”<sup>20</sup> Both beliefs made any early detection program a fool’s errand. Hudson surmised that these beliefs could be undermined by demonstrating that a treatable, “early” form of prostate cancer could be diagnosed in living men.

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The early 1950s was a period of rapid change in cancer research and clinical care, resulting in many ethical and logistical challenges. The creation of a large, modern municipal hospital devoted to cancer clinical care and research was one response. The laboratory space and administrative support that Hudson received as well as the many other clinical research programs started at Delafield reflected the underlying logic of a municipal cancer hospital affiliated with and staffed by academic cancer researchers: in return for expensive and difficult-to-access cancer care, poor patients would be subjects in clinical experiments and clinical material for training.

There was also a new urgency about cancer research that led to combined research and clinical initiatives. In the 1950s, when virtually no chemotherapeutic or radiation practices had been subject to robust clinical trials, it is difficult not to see *all* cancer interventions as experimental. In the wake of wartime demonstrations of the efficacy (however short-lived) of nitrogen mustard against lymphoma and the early post-war efforts of Sidney Farber and others to treat childhood leukemia with folate antagonists,<sup>21</sup> there was tremendous medical excitement, Federal and private funding, and patient demand for quick diffusion from laboratory to patients with cancer, inevitably blurring the already fuzzy practice/experiment distinction.

The inpatient wards of NIH hospitals in Bethesda and Memorial Sloan-Kettering in Manhattan attracted referrals of largely middle class cancer patients. The moral economy of experimenting on these patients with new and potentially devastating chemotherapeutic agents was and is often built on the desperate hope for cure or extended survival among people facing near certain death. Using less sick people, which was necessary for studies of new diagnostic or preventive practices, required a different calculus. This was especially true of Hudson's plans to test highly invasive diagnostic and preventive practices in large numbers. Homeless alcoholics

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offered up their bodies for clinical experimentation in return for desperately needed and difficult-to-access medical care, shelter, and safety. The powerlessness of these welfare clients in municipal shelters and hospitals was a precondition for this exchange.<sup>22</sup>

Hudson was looking to expand the clinical approach to prostate cancer he had learned at Hopkins and was putting into practice at Delafield. Hudson routinely offered a pre-operative perineal biopsy to Delafield patients who sought medical attention for symptoms caused by an enlarged prostate. If the biopsy was positive, Hudson would do a radical operation for cure rather than one of the more limited operations done then and now for obstruction. “A lot of the patients agreed to it. Some didn’t.” Hudson eventually learned to be persuasive, “so for years, everybody who had a prostatic operation had a biopsy first . . . an open biopsy. I knew how to do that without damaging the patient.” Having established for himself the utility of pre-operative biopsy for men with obstructive symptoms, Hudson then wondered about “those people wandering about the street who don’t have a benign enlargement. What about them? . . . How do you screen for it? How do you justify doing anything to these people if you had nothing to go on?”

Hudson recalled that the idea of carrying out a study of open perineal biopsies on Bowery men occurred while taking care of a homeless patient who had previously been a Princeton history professor. Hudson then approached the owner of the Bowery flop house where his patient stayed who agreed to allow Hudson to approach men for the study “as long as they don’t riot against whatever you’re doing.”

Recruiting subjects from the Bowery was not easy at first. “I started to make a speech,” Hudson recalled, “and they throw some overripe vegetables at me.” Hudson’s luck turned when

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he befriended two Welfare department workers at the city run Municipal Lodging House, or the Muni as it was called.<sup>23</sup> They were able to convince the residents “so that just about everybody volunteered.”<sup>24</sup> Recruitment began in March 1951.

Although the Bowery men were subjects in a clinical trial, they were frequently referred to as patients and treated as such once at Delafield. “No patient sought medical attention spontaneously,” was a typical way Bowery publications blurred this boundary.<sup>25</sup> The men also provided residents and surgeons opportunities to practice their craft.

These recruiting, clinical, and teaching practices were part of a larger moral, political, and monetary economy of Bowery life. Men (and a few women) provided souls and remained temporarily sober for church missions that in turn provided meals, lodging, and a respite from street dangers; men hustled each other and outsiders for cash that they exchanged for food and cheap drink; low rent flop houses and cheap bars turned profits from the large flow of down and out men; men provided cheap and expendable day labor for small amounts of cash. The Bowery concentrated homeless, alcoholic, and physically and mentally distressed people who were often the waste material of the work economy and made them invisible and scarce elsewhere, in a part of the city long-impooverished and violent, literally under shadows from a soon-to-be removed elevated train.<sup>26</sup> The exchange of bodies for research and practice and training was only a small part of this larger, historically and geographically specific system of exploitation.<sup>27</sup>

While there is no historical record of the Bowery men’s experiences in their own voices, a partial picture emerges from interviews with Hudson and co-workers, journalist accounts, archival material, and scientific papers. Not surprisingly for a controversial study from a long

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time ago and which lasted over a decade, these sources offer fragmentary and often incommensurate accounts of the same events.<sup>28</sup>

Hudson recalled that recruitment was easy. Bowery men knew they would receive “total care” at Delafield. If someone had a hernia, it was repaired, and for free. “They were my friends,” Hudson recalled.<sup>29</sup>

A 1961 popular history, *The Bowery Man* by Elmer Bendiner, gave some detail and texture to the recruitment and experience of the Bowery men who participated in Hudson’s studies. While the account conformed to the book’s frame of the men’s tragedy and exoticism, some important details can be gleaned.<sup>30</sup>

Bendiner wrote that Hudson’s initial recruiting difficulties were overcome when “recruiting speeches” were “supplemented by the tactics of the Muni. There, once a man chosen for the study was persuaded to volunteer, the authorities lifted his meal ticket and returned it only when he had kept his appointment. That made it difficult to backslide.” The cleanliness, beds, and food at Delafield were attractive as well as the fact that “nobody bothers you and everybody calls you Mister.”<sup>31</sup>

Bendiner followed one man he called Mr. Finn from the Muni to Delafield. At the Muni, Finn and others were recruited after they showered. “The men cannot wait to wrap themselves in rags and so preserve themselves from the assaults of other men. They have traveled far to escape the impact of people, and they cannot lightly bear the harsh and alien looks of others on their stripped, bare bodies.”<sup>32</sup> After a physical exam by the recruiting doctors, there was a rudimentary consent process:

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“‘Mister Finn,’ the Muni recruiters say. His face takes on an apoplectic blush. ‘We think you ought to come to the hospital and let us give you a check-up. Will you do it, Mister Finn?’ Mister Finn smooths his limp hairs flat on his head, shifts his weight, clutches his nightgown about his paunch, and murmurs, ‘Sure.’” At the desk somebody writes his name on a pad, then hands him a card that entitles him to breakfast only—no dinner tomorrow. His regular meal ticket will be picked up. But the precaution is unnecessary. Mister Finn is delighted at being accepted into the club of distinguished prostates.”<sup>33</sup>

According to published studies, the first 141 Bowery recruits had some symptoms suggestive of urinary obstruction—difficulty urinating, frequency of urination, or painful urination—or physical signs. But the subsequent recruits were unselected, consecutive patients made part of the study irrespective of symptoms or signs. The rationale for this change was not explicitly discussed, although some mention was made of the (high) “frequency of cancer found.”<sup>34</sup> But the change allowed the Bowery series to approximate a study of cancer screening and to make inferences about the natural history of prostate cancer and its prevalence in the general population. Of course, these inferences depended on just how representative the Bowery men were of the larger population and on how Hudson’s residents selected men to go to Delafield.<sup>35</sup>

One physician who participated in the Bowery work in the early 1960s recalled that “we would meet these men as they came out of the shower . . . and say we’re doing examinations on your prostate.” He would ask the men “would you mind if you bend over? We’re going to check your prostate.” This participant recalled that he tried to recruit patients who had some prostatic abnormality (contradicting the published protocol) but did not think the other recruiters

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did.<sup>36</sup> He would typically tell the men that “there is a little irregularity of your prostate. We should test it for cancer.” The other resident physician recruiters would usually say something like “oops you have a prostate, let’s go up to the hospital.” This participant believed recruiting men with a possible prostatic abnormality was more ethical than his fellow residents’ practices.

To get three recruits, the resident recruiters typically needed to examine a dozen men. They explained to the Bowery men that at the hospital they would “get an open biopsy to test your prostate.” The men who went along were attracted to staying “4 or 5 days in a hospital, clean sheets, and three meals a day.” It was easy to recruit because doctors in white coats still had authority.<sup>37</sup>

In Bendiner’s account, Finn arrives at Delafield, where he is told that “it is entirely up to you. It will require some surgery just to find out if there is any cancer, you understand. That is called a biopsy.” The dialogue continues:

“Whatever you think is right, Ma’am.”

“It isn’t for me to say, Mister Finn. It’s up to you.

Little boys frequently are allowed to decide of their own free, God-given will to do what is expected of them.”

“Sure, Ma’am.”<sup>38</sup>

Bendiner did not detail Finn’s actual medical experiences, but Finn was biopsied, found to have cancer, and treated.

With an infusion of NIH funds in 1955,<sup>39</sup> Hudson was able to employ many clerks to keep meticulous records and find Bowery men and bring them back to Delafield for study.<sup>40</sup>

Hudson recalled that it was easy to follow homeless men over ten years or more.<sup>41</sup> “They don’t

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get out of there. People don't leave that kind of life. Almost never . . . We knew them very well." As for the logistics, "the cancer society gave us a small bus that took him back and forth. They knew all about the study."

At Delafield, the Bowery men underwent X-rays of the abdomen, urine and blood analysis, intravenous pyelograms (sequential X-rays of dye collecting in the kidneys, ureters, and bladder), cystoscopy (passing a scope through the penis into the bladder to visualize abnormalities) often accompanied by trans-bladder biopsy of the prostate, prostatic massage with attempt at collecting fluid for cytological examination, and complete physical examination with special regard to rectal and prostate examination.<sup>42</sup> All these tests were preliminary to the open perineal biopsy. Under general or local anesthesia, Hudson and/or his residents cut out a one to two centimeter square piece of prostatic tissue.<sup>43</sup> Perineal biopsies involved dissecting away tissue between the rectum and scrotum to reveal the back of the prostate gland and surgically removing a core of tissue.<sup>44</sup> One half of this core was sent to the pathology laboratory for frozen section while the patient and the operating team waited (the other half was retained for permanent preparation). If positive for cancer, the patient underwent a complete perineal prostatectomy. If the frozen section did not have cancer, the patient's perineum was stitched back together.

Patients with a positive biopsy were also castrated surgically and given estrogen treatments. These hormonal interventions were commonly used to relieve the pain and dysfunction of metastatic disease, but it was not routine to combine these punishing interventions with radical surgery aimed at curing localized disease. One of Hudson's articles is a defense of this unusual and aggressive practice.<sup>45</sup> Hudson noted that there was controversy between

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advocates of surgery and those of hormonal treatment. Given that not all curable patients would be cured by surgery alone, why not use both?<sup>46</sup> Hudson and collaborators consistently favored the most aggressive clinical practices—a situation made possible by the compliant clinical material.

Hudson and colleagues reported very little clinical detail about the consequences of the many invasive tests or interventions.<sup>47</sup> Hudson claimed that impotence did not occur as a result of perineal biopsy, which inspired Carl Dahlen and Willard Goodwin to publish a case series of twenty-four patients who had open perineal biopsy for suspicion of cancer. These patients had negative biopsies and thus had no further surgery. The results were strikingly different from Hudson's claim. There were many complications. Most patients were hospitalized for a week. ten patients had serious complications during or after biopsy, including cardiovascular events, a blood clot (leading to a fifty-day hospitalization), stich abscess, rectal lacerations (three were identified and repaired during surgery; one patient took thirty-nine days to heal), urinary stress incontinence, long term drainage problems, and perineal bleeding (two patients). As for sexual function, only a third (eight patients) had no change in sexual potency after biopsy (one of these eight patients actually reported improvement); nine claimed diminution in potency; and another seven reported being completely impotent. The authors concluded that patients considering perineal biopsy should know about these serious complications and the procedure should only be done when there was a "reasonable likelihood" of cancer. The authors noted that it was "difficult to explain" why their results differed so dramatically from the Bowery series. They speculated that there might have been differences in surgical technique or, more likely, differences in the way sexual potency were determined.<sup>48</sup>

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When queried about whether the Bowery men might have been harmed in the ways suggested by Dahlen and Goodwin's study, Hudson said many years later "that's a lot of baloney."<sup>49</sup> A participant in the Bowery work in the 1960s, however, recalled that impotence was common and the most feared danger of perineal biopsy (and the perineal approach to prostatic surgery in general) was rectal perforation. Also, the surgery that followed a positive biopsy was dangerous. At least one Bowery man died after prostatectomy.<sup>50</sup>

Hudson's studies focused on many aspects of prostate cancer incidence and natural history, but the main contribution was to demonstrate the feasibility and efficacy of open biopsy for diagnosing prostate cancer at a treatable stage. The Bowery studies provided good evidence for the diagnostic accuracy of open perineal biopsy and were frequently cited as such. Hudson and colleagues reported on the much greater accuracy of the open perineal biopsy over any other means of diagnosis: rectal exams, cystoscopy, biopsies thru the rectum and bladder, blood tests such as prostatic acid phosphatase and LDH, and cytology of prostatic secretions. Most of the cancers detected by open perineal biopsy occurred in men with normal results on these tests. In other words, these other tests all lacked even the minimal sensitivity to detect cancer at the stage in which it might be curable.

Bowery publications from 1953–1966 reported on different numbers of men and subpopulations and often focused on narrow rather than big picture questions. In some studies, Bowery men were not clearly distinguished from other Delafield patients and Bowery men with symptoms were not distinguished from unselected men. In the last published report from 1966, which might have been expected to have the most complete follow-up, Hudson and Stout reported that there were 98 cancers among the 892 patients identified as survey patients,

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presumably the Bowery men without signs and symptoms.<sup>51</sup> This 11 percent detection rate was lower than some of their earlier reports. Because other long term follow-up data were never reported, it is impossible to draw even minimal inferences about the effectiveness or dangers of either screening or aggressive treatment.<sup>52</sup>

The only Bowery series publication that reported mortality rates among men diagnosed with cancer and treated was a 1957 report. After a little more than four years of surveillance, there was a high mortality rate among Bowery men with negative biopsies (20 percent) and among those diagnosed and treated for cancer (30 percent). Hudson noted that only one patient diagnosed with prostate cancer by perineal biopsy had died of it and this man had refused treatment (dying, in Hudson's view, of untreated "latent" cancer).<sup>53</sup> Much more follow-up data was apparently collected but never published because some data were lost when Hudson left Delafield in 1960 and because Hudson had been persuaded to stop the Bowery studies and publications.

But we get an incomplete idea of the work done by the Bowery studies if we evaluate them only by early twenty-first century standards of evidence-based medicine. Persuasion about the efficacy of clinical practices can also be based on the mere demonstration that something previously unimaginable (for scientific, technical, pragmatic, and/or ethical reasons) could be done. This is especially true of new and invasive surgical practices.

In the Bowery series, Hudson demonstrated that one could do open perineal biopsies on older men who had no suspicion of cancer, diagnose cancer at prevalence rates consistent with what was known from autopsies and other studies, and then do radical interventions. However convincing a demonstration that "screen and radical treatment" could be done, the fact remained

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that what was practical and workable for Bowery men was not so for other men. Hudson tacitly acknowledged this problem. He never called for a screening program. Instead, he argued that urologists carry out open perineal biopsies for men prior to obstruction relieving surgery and to do various practices that would bring more men to biopsy.<sup>54</sup> He urged doctors to perform routine biannual rectal examinations. In his own practice, Hudson claimed more rectal exams and other surveillance followed by open biopsy resulted in “the highest known rate of curable prostate cancer. This is true despite the fact that the hospital connections and personal interest of the authors are known to be such that a large number of frankly metastatic prostatic cancer patients have been sent for treatment.”<sup>55</sup>

Since the Bowery studies did not use controls, their results could not be readily used to dispute the commonly held view that detected cancers were better left untreated.<sup>56</sup> Hudson argued that because of longer lifespans, men were no longer outliving these latent cancers. Rising mortality rates were testimony to this fact. He was emphatic about the dangers of cancers others understood as latent or innocent, but his arguments were from clinical observations and common sense, not the Bowery data. Hudson and colleagues argued from first principles that metastatic prostate cancer had to have a localized beginning: “such lesions doubtless persisted locally prior to progressing to inoperability.”<sup>57</sup>

The blurred and permeable boundaries of this clinical/research project helps explain the incomplete data reported in publications. One physician participant recalled that he was only dimly aware they were participating in research. “Research was a very nebulous term.”<sup>58</sup> The study “worked” to bring patients to Delafield and then to Montefiore Hospital (after Hudson left Delafield) for residents to learn their craft.

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Hudson recalled that he decided to stop publishing results from the Bowery series when an editor of the journal *Cancer* wrote him “a very careful letter and asked me what protection I had from the Universities’ legal department. And what measures I had taken to guard against lawsuits. And I had to tell him “nothing, I had done nothing . . . that sort of scared me. So when I did the last of the studies I never published them.” The editor who warned him was “a friend of mine. He didn’t send that out as anything but a friendly warning. That meant that he had heard things. Other people did too. I was aware of that.”<sup>59</sup> In any event, the study was under pressure to close because of Hudson’s move out of Columbia and Delafield (according to Hudson, records from Delafield were destroyed by city administrators).<sup>60</sup>

Hudson sought NIH funding, which had previously supported his work, to continue different aspects of the Bowery series throughout the 1960s. The sponsoring institution was the High Tor Foundation, a free-standing private research facility located at Hudson’s home, whose president was Hudson and secretary-treasurer was his wife. Reviewers were generally critical of these proposals and did not recommend funding except for one 1966 study to assist Hudson in a statistical analysis of the Bowery series. Reviewers noted that Hudson “assumes a computer of itself can analyze data and divulge relationships which he himself cannot see” and generally was “naïve about epidemiological studies and has an unsophisticated view of what might be done with his data once the follow-up is completed.”<sup>61</sup>

When asked fifty years later if he thought that the Bowery series was ethically sound, Hudson replied “Oh yeah. You know these people, the ones who are persistent street people in New York, are not simple minded. They are alcoholics. They have mental disease very often. But (it) doesn’t appear when they are listening to a conversation or answering a question

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straightforwardly and honestly. No. They understood everything that was going on.” In addition to believing that the Bowery men understood the risks and benefits, Hudson staunchly defended the ethics of his study by noting he never gave money to subjects to participate. In Hudson’s view, this practice constituted coercion. He was critical of these financial incentives in the prostate cancer prevention trials of Finasteride, conducted decades later and orchestrated by Merck, in which he was a coinvestigator.<sup>62</sup>

A physician participant in the Bowery studies in the 1960s regretted the failure to provide adequate written informed consent. “I doubt very much today that interview (the discussions after the showers in the men shelters) would hold up,” he explained. He particularly regretted the failure to adequately inform men in writing of the dangers of open perineal biopsy, especially rectal perforation. “Today we wouldn’t do that in a million years but we did that.” But he added that “the goal was a noble one, to find cancer at an early stage.” He also explained that the procedure was generally safe and that he and other residents were well trained. As for the ethics of using Bowery men as subjects, this participant explained that Hudson’s private patients also routinely underwent open perineal biopsies.<sup>63</sup>

What was the reception of these studies? They were widely cited in urological journals and textbooks in the 1950s and 1960s, usually as evidence of the superior sensitivity and specificity of open perineal biopsy in the diagnosis of prostate cancer.<sup>64</sup> There were no ethical objections raised about undue risk from either the biopsies or the interventions that followed a positive biopsy or the use of an ill-informed and easily coerced population. The many reviews of Hudson’s NIH proposals were silent on the ethical dimensions of the Bowery series except for a marginal comment attached to a 1966 approval that the NIH had not yet received a response to

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their request for “documentation of the institution’s compliance with the appropriate Public Health Service policy.”<sup>65</sup>

Urologist William Parry recalled that the demonstration that a lot of unsuspected cancer could be diagnosed by biopsy made the study influential but there were no ethical condemnations.<sup>66</sup> The only hint of an ethical judgment was a 1965 article that cited the studies and made a passing remark that “irrespective of ethical problems posed by such a series, certain facts emerge that are undoubtedly germane to the present inquiry.”<sup>67</sup>

The Bowery series did not lead to using open perineal biopsy as a *screening* method. An editorial accompanying one of the Bowery series papers in 1955 objected to the idea of this use, observing that “the authors cannot propose doing routine open perineal biopsies on all old men.” Not only was generalizing the Bowery practice to free-living men unimaginable, but one would still miss half the men with prostate cancer (since some autopsy series showed approximately twice as much cancer as Hudson found). “In order to detect all the instances of cancer of the prostate in men over 55, routine radical prostatectomy would be necessary.”<sup>68</sup> There were no attempts to replicate a screening trial of open perineal biopsy or put it into general practice.<sup>69</sup> “You can’t,” Hudson explained. “You can’t talk people into something like that.” If one could, “the whole profession of medicine would come down on your head.”<sup>70</sup>

Urologists also did not generally change their practices in the more limited directions Hudson and colleagues pointed to in their publications. Open perineal biopsy did not become a routine procedure prior to surgery to relieve prostatic obstruction. Presumably, urologists believed that the open perineal biopsy was a much more dangerous procedure than Hudson had reported. A physician participant in the Bowery work in the 1960s believed that both the

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perineal biopsy and operation for cancer were difficult to perform and most urologists had few opportunities to learn perineal approaches. This was unfortunate, he recalled, because perineal biopsies were far superior to needle biopsies (which he called “horrendous” because of the risk of infection), the common method used by urologists at the time. The perineal approach allowed a much better visualization of the prostate and was less morbid, but “getting there was very tricky.” It was a difficult operation through a small opening, “between the wind and the water.” Unless you are well trained the chance of rectal perforation was high.<sup>71</sup>

Hudson recalled that some urologists objected to the studies. “Among urologists, I got called a collection of really fancy names. Not by the heads of departments so much but a few of them. . . . Reactions were all over the lot. I didn’t pay any attention to them.” The people Hudson admired “understood what I was doing. So I paid no attention to it.”<sup>72</sup>

The Bowery studies were not only regularly reported in the medical literature without ethical comment, but they were the subject of positive journalistic coverage in a 1957 photo spread in *Life Magazine* (Figure 1). The studies were also highlighted in positive terms in the annual reports of the sponsoring institutions. “During the past 24 months,” the author of the 1952–1953 Institute of Cancer Research annual report bragged, “more early cancers have been subjected to radical prostatectomy at Delafield than at any other hospital in the world, regardless of size. This surgery has been performed, mainly by the resident urologists, with a mortality rate of slightly over one per cent- the lowest on record.”<sup>73</sup>

<< FIGURE 1 HERE >>

After leaving Columbia, Hudson pursued a career that led him to tobacco research, laboratory science, continued urological practice (doing radical perineal prostatectomies and

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open perineal biopsies well into his 90s), other major preventative research, and most recently a deanship of an offshore medical school.<sup>74</sup>

## **The Past in the Present**

In hindsight, Hudson had correctly diagnosed many of the obstacles to making prostate cancer the target of an early detection and radical treatment paradigm. Pessimism, fatalism, and inaction had to be banished. Prostate cancer needed to be transformed from a uniformly deadly disease to one that could be detected early and cured. There had to be some means of diagnosing prostate cancer at this curable stage. Such means would have to be acceptable to healthy men. And they and their doctors needed to believe such practices were effective.

Hudson intended to jump start these changes by demonstrating that curable prostate cancer could be easily diagnosed by open perineal biopsy and was as prevalent as cancers found in autopsy studies and among men undergoing obstruction-relieving operations. This demonstration became even more unique and potentially persuasive when Hudson extended the study to *asymptomatic* Bowery recruits when the “clinical material” proved extremely compliant.

This demonstration nevertheless fell short in many significant ways. The desperate life situation that made the Bowery men compliant also limited any generalizing. No one could imagine screening a free-living population of American men with open perineal biopsy. As there was no comparable control group, there could be no convincing evidence whether finding and treating aggressively these cancers helped or hurt these men. Any putative prolonged survival

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was also consistent with the received wisdom that the detected cancers were “innocent” or “latent” and not destined to develop into metastatic disease.<sup>75</sup>

From a historical perspective, the practices and goals that constituted the Bowery series were prescient as much as they were unethical. In the more than half-century since these studies were started, tinkering and clinical innovation has transformed prostate cancer attitudes and practices in the directions that Hudson anticipated. Many elements of the Bowery series—screening asymptomatic people, mass biopsies, and resulting transformation of prostate cancer into a curable disease—are now in place. And this transformation has occurred without any profound new etiological understandings or dramatically new therapeutic principles and modalities.

Annually, more than 25 million American men are currently screened for prostate cancer by serum Prostatic Specific Antigen (PSA) determinations.<sup>76</sup> As a result, most American men over 50 have been screened. Well over a million American men get prostate biopsies every year. At least 220,000 of them get new diagnoses of prostate cancer via ultrasound guided core biopsies.<sup>77</sup> Most of these men will ultimately get active treatment. Approximately 88,000 men with good prognosis cancer annually undergo radical prostatectomy.<sup>78</sup>

Hudson and colleagues had demonstrated a screening plus radical intervention paradigm for which fellow urologists and ordinary men were not yet ready. It is no longer unimaginable that men without symptoms will readily consent to having bits of their prostate gland taken out and examined for cancer. In the decades since the Bowery series, there has been a great deal of tinkering with different elements of that program that have made similar practices more palatable to doctors and patients. This tinkering has catalyzed changes in medical routines and created the

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conditions—especially a large cohort of men at high risk for prostate cancer—for rapid, self-sustaining attitudinal and behavioral change.

Techno-scientific change along multiple fault lines has followed a similar strategy to Hudson's overall, if failed, program: first transform the disease by creating and diffusing diagnostic and treatment practices, and then use this demonstration as evidence of the efficacy of a new early detection and radical intervention paradigm. The perception of efficacy might then jumpstart more screening, more diagnoses and people at risk, and more apparent cures attributed to interventions (if only because there were more diagnoses relative to an unchanged mortality rate). In sum, a self-reinforcing cycle of practice and attitudinal change takes root. This cycle stabilized assumptions about prostate cancer's treatability, natural history, and orderly classification, which, while widely accepted in other cancers, were not received urological wisdom earlier in the century.

The resulting radical transformation of prostate cancer and a new confidence in the efficacy of our interventions have not followed either of the two ideal-type means of persuasion in modern medicine: (1) the demonstration of efficacy by experimental evidence of the superiority of one intervention over another, or (2) the identification of the cause(s) or mechanism(s) of disease and the use of technology or practices that lead to their removal or evasion. Instead, following the aspirations of the Bowery demonstration, there has been a gradual shift in the global picture of the disease, from hopeless to hopeful, incurable to curable, invisible to visible. A disease has changed along with the prevailing mood about our ability to intervene against it. Innovations in techno-practices have played catalytic roles rather than determinants of change. These innovations have contributed to overcoming the obstacles

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Hudson faced to the smooth working of a self-reinforcing cycle of beliefs and practices. They helped urologists and radiation therapists, among others, catalyze a dramatic change in the social efficacy of existing practices, without necessarily being efficacious in terms of their overall health impact.

Hudson never argued that screening biopsies be done on all elderly men. He understood that what was needed was a “simple, economical and reliable test for early prostatic cancer by means other than surgical biopsy. . . . Such tests have not yet been developed.”<sup>79</sup> It would take decades to create and diffuse such a test. By the mid-1980s PSA screening would be used to identify a subpopulation of men at high risk who might undergo less mutilating biopsies in order to find cancers whose potential for harm is about as unknown today as in the 1950s.

Before PSA, however, there were decades of tinkering with the so-called male PAP test, first developed by some of Hudson’s collaborators and one of the many diagnostic tools studied but found wanting in the Bowery series, to create a tool that was up to the job of identifying men whose prostate cancer risk was high enough to justify biopsy or other interventions. In the 1970s and 1980s, just prior to the development and introduction of the more sensitive PSA test, there was a good deal of expectation that the new PAP blood tests would do this work.<sup>80</sup> But more PAP testing in itself did not stabilize into a self-perpetuating system of attitudinal and behavioral change. A more acceptable nexus of practices, along with witnessed evidence of their efficacy, would be necessary to get men to consent to getting biopsied for cancer on a mass scale.

Over the 1970s and 1980s, older debates over the value of rectal exams for screening, rectal versus perineal biopsy approaches, and core versus needle biopsy techniques, were eclipsed by the development of relatively safe spring-loaded biopsy guns and transrectal

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ultrasounds, which allowed easier sampling and suspicion-driven biopsies. Surgery and radiation were made more palatable and safe, but perhaps no more effective, by the discovery and promotion of new techniques such as nerve sparing surgery, combination external and internal radiation methods, and robotic assisted surgery.

These technological changes were important improvements in safety and palatability over the open perineal biopsy and radical perineal prostatectomy used on asymptomatic men in the Bowery series. They led to much more testing, more prostate cancer diagnoses and men at risk, and greater numbers of men who survived for long periods of time with a prostate cancer diagnosis. The rise in apparent incidence and greatly improved survival led to a perception that the different elements of the screen and treat program “worked,” leading to more compliance, and more uptake of these practices.<sup>81</sup>

But we remain ignorant about the overall scientific efficacy and safety of the resulting mass experiment that impacts millions of American men each year.<sup>82</sup> These practices developed in ways which are typical of late twentieth and early twenty-first century styles of medical knowledge production and the behavioral change/consumerist logic of our present health care system, especially as it regards risk and cancer. The constitutive parts of this style and logic are an exaggerated confidence that “a cancer is a cancer” and faith that earlier means of detection represent progress. Also influencing these changes have been the economic and other benefits for urologists, device manufacturers, test makers, and others.

Since the mid-1980s, increasing numbers of men received a prostate cancer diagnosis via PSA screening and biopsy, submitted to active intervention, and lived long cancer-free lives. Many such men as well as outside observers interpreted this sequence as diagnosis, treatment,

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and cure, and touted it as such, leading to more faith in the paradigm, more screening, more decisions for active treatment, and more recruits into the mass of cured cancer patients. This logic has often resisted skeptical challenge. Not only is progress undeniable, but the selling of fear and uncertainty makes the technologies which promise to banish them irresistible.

Without much agency by any one group of individuals, the very conditions for a more “evidence-based” approach to the effectiveness of different prostate cancer interventions have been undermined by the wide diffusion of practices and ideas prior to any definitive experiment. There is now almost no chance that a purposeful clinical trial, however robust, can offer a definitive answer. The history of prostate cancer interventions has not followed the dictum “randomize the first patient.” The widespread diffusion of mass screening followed by aggressive intervention, catalyzed by decades of incremental tinkering prior to rigorous clinical experiments, has undermined the interpretation of randomized controlled trials of screening. Too many people are being screened and treated for there to be an uncontaminated enough control group with which to interpret experimental evidence of no benefit. We are thus likely to remain ignorant of the net health benefit of screening and radical treatment. It is hard to say that men considering screening (and many men first learn they have been screened when they are told they have a high PSA) can really make an informed decision, yet another troubling parallel to the Bowery series.

There has been a disturbing continuity between the Bowery series and our current paradigm. The Bowery studies were prematurely deployed, crude technopractices that anticipated and followed a similar logic undergirding many subsequent incremental developments in prostate cancer. These studies, forgotten perhaps because they reminded later

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practitioners of the violence and dubious ethics of subjecting men to invasive procedures without good evidence of benefit, are a window into the long halting history of how we have come to accept as efficacious a set of very invasive prostate cancer practices, developments that are themselves part of an ever larger quest to control cancer and our fears of cancer and that have brought millions of American men into state of prostate cancer risk.

The Bowery series practices were unethical judged by present standards and raised questions for some of Hudson's contemporaries. Hudson recruited men from Bowery shelters because they were either not competent to fully understand the risk/benefit calculation and/or they were desperate enough to make a calculation that someone less vulnerable was unlikely to make. The former reason ran against the ethical principle of informed consent that had been explicitly codified as a result of the Nuremberg trials in 1947 while exposing vulnerable populations to undue risk might arguably be understood as a less established norm prior to Beecher's paper and the multitude of regulations and codes that followed upon the public airing of the Tuskegee experiments and other scandals starting in the 1970s.<sup>83, 84</sup>

At the same time, the history of the Bowery series does not neatly fit within existing historiography of human experimentation or medical therapeutics. It occupies the space between them. While historians have explored different aspects of the research/practice boundary<sup>85</sup>, the Bowery series represents an underappreciated nexus of different practices. Aggressive proto-screening transformed marginal men into patients who then became more legitimate objects of clinical tinkering at the border of therapeutics and experimentation within a public hospital whose very existence was built on the exchange of access to care for the socio-economically disadvantaged in return for teaching and research opportunities.<sup>86</sup> Furthermore, the ethical

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challenges posed by transforming people into patients via well-intentioned screening and clinical practices, are compounded when individual practices together create a system whose overall efficacy is never established, visible, or even the responsibility of any one person or group. This complex and historically new reality may require a wider scope of collective ethics and responsibility rather than a strict focus on the individual doctor–patient and researcher–subject.

In other words, we obscure the significance of these practices when we view them solely through bioethical lenses, however well contextualized. A facile moral superiority toward past actors and events also robs history of its potential to provide insight into current ethical problems, which are unlikely to appear in the same guise as past ones. The Bowery practices occurred within a system that provided care to people with little or no access in exchange for using their bodies for practice, learning, and experiment. Not recognizing these continuities in the “fertile soil” within which unethical practices develop can lead to a false sense of security about the protections we enjoy from codified ethical standards, informed written consent, and institutional review boards (IRB), especially when some protections, like the IRB, are focused on explicit clinical experiments rather than the wider and more obscure boundary zone present in the Bowery and at Delafield, and within which a lot of medical activity still occurs.

As or more disturbing than imposing significant dangers on an uninformed, vulnerable population in the not so distant past, the Bowery practices also anticipated our present experience with similar technologies and ideas and can help make visible their significant ethical challenges. That past is in this present. What is troubling about that past cannot be neatly tallied in separate ethical and scientific accounts or between research and practice. To do so would limit the kind of criticism and self-awareness we need to make sense of and respond thoughtfully to present

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challenges. It is not just a scientific matter that we deploy practices and technologies that have not been rigorously studied beforehand. It is also an ethical problem, as this deployment can constitute a lien on the adequacy of future informed decision-making and consent. The complex historical processes by which we come to accept that a set of interventions *work* deserve moral scrutiny even if there is no singular bad actor or actors. Neither is promoting or imposing dangerous practices on an ill-informed population solely an ethical problem. Such practices can and have transformed a disease and what we believe about it.

Robert Aronowitz is professor and chair, history and sociology of science department, and professor of family practice and community medicine at the University of Pennsylvania. He also directs the Robert Wood Johnson Foundation Health and Society Scholars Program at Penn. He is an internist and historian of medicine who works mainly on the history of disease in the United States. Aronowitz is the author of *Making Sense of Illness: Science, Society and Disease* (Cambridge University Press, 2008) and *Unnatural History: Breast Cancer and American Society* (Cambridge University Press, 2007).

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Sara Aronowitz, David Asch, David Barnes, Alan Blinder, Allan Brandt, Dan Clawson, Steve Feierman, Susan Lindee, Jane Mathisen, Jonathan Moreno, Mark Neuman, Charles Rosenberg, Jason Schnittker, Jason Schwartz, and the RWJF Health & Society Scholars and faculty at the University of Pennsylvania read and commented on earlier versions of this manuscript. This research was done when I was a visiting scholar at the Russell Sage Foundation; thanks to the scholars and staff there for comments at a workshop based on this research. Deanna Day, Claire Gabriel, and Katy Winograd provided crucial research assistance. The National Archives (College Park, MD) and Archives & Special Collections, A.C. Long Health Sciences, Columbia University Medical Center granted me access to their collections. Special thanks to the historical informants cited in the text.

1. The actual number of Bowery “subjects” who underwent perineal biopsy is unclear. Perry Hudson and Arthur Stout reported that 891 “survey” patients had prostatic biopsies; “Prostatic cancer. XVI. Comparison of Physical Examination and Biopsy for Detection of Curable Lesions,” *New York State J. Med.* 3 (1966): 351–55. These 891 men are presumably in addition to the first 141 selected subjects; Perry Hudson, “Prostatic cancer. XIV. Its Incidence, Extent and Behavior in 686 Men Studied by Prostatic Biopsy,” *J. Amer. Geriat Soc.* 5 (1957): 338. Hudson (interview by Robert Aronowitz, December 2011) reported that there were many more Bowery recruits than those cited in publications. In 1966, an NIH review of a Bowery series grant, which included a site visit, reported that “a total of 1611 men have been admitted to the study and 1229 have been biopsied.” Summary Statement, re: R01 CAD8693-01; from RG443, Records of the National Institutes of Health, National Cancer Institute Office of the Director, Correspondence, Hosier-Hooxsey #3, Box No 8, N-3-443086-1 (hereafter, “NIH file”). *Dr. Williams* (pseudonym) (interview by Robert Aronowitz, December 2011), reported

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recruiting Bowery men for admission to another hospital. These subjects, not identified in any publications, appear to represent additional men.

Williams, who was a urology resident under Hudson, requested anonymity out of concern for legal or other complications that might arise from bringing details of the Bowery series into public view. His recollections are neither offered nor cited in such a way to make an unaccountable accusation against Hudson (whom he remembered with fondness and great respect), which I believed would raise questions of fairness and responsible scholarship. Williams's recollections are each supported by other published and archival sources, and the one substantive difference with Hudson's recollections—on the dangers of perineal biopsy—were shared by some contemporaries and cited below. All interview notes with Williams and other informants are available for review upon request.

2. Men were asked to participate and could refuse. For example, Hudson ("Prostatic cancer. XIV," n. 1) reported that 138 of the 824 Bowery men admitted to Delafield, after extensive and invasive clinical testing, either did not agree to or were not offered open perineal biopsy.

3. Henry Beecher, "Special Article: Ethics and Clinical Research," *New England J. Med.* 274 (1966): 354–60.

4. Charles Rosenberg, "The Therapeutic Revolution," *Perspect. Biol. & Med.* 20 (1977): 485–506. For an extension of this approach, see Judith Levitt's historical analysis of why women entered hospitals for childbirth in *Brought to Bed: Childbearing in America 1750–1950*, (Oxford: Oxford University Press, 1986). The social efficacy idea was elaborated by Rosenberg

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in the introduction to *Our Present Complaint: American Medicine Then and Now* (Baltimore: Johns Hopkins University Press, 2007).

5. Jack D. Pressman, *Last Resort: Psychosurgery and the Limits of Medicine* (New York: Cambridge University Press, 1998), 415.

6. Cancer risk has diffused along gendered fault lines. While historians have begun to understand why American women welcomed routine pelvic examinations and screening breast x-rays. See Kirsten Gardner, *Early Detection Women, Cancer, and Awareness Campaigns in the Twentieth-Century United States* (Chapel Hill: University of North Carolina Press, 2006); Robert Aronowitz, *Unnatural History: Breast Cancer, Science, and Society* (New York: Cambridge University Press, 2007); Ilana Löwy, *Prevention Strikes: Women, Precancer, and Prophylactic Surgery* (Baltimore: Johns Hopkins University Press, 2010); Barron Lerner, *The Breast Cancer Wars: Fear, Hope, and the Pursuit of a Cure in Twentieth-Century* (New York: Oxford University Press, 2003). James T. Patterson, *The Dread Disease: Cancer and Modern American Culture* (Cambridge, Mass.: Harvard University Press, 1987). Much less attention has been paid—comparatively or historically—to the analogous transformation of male—[sic] prostate—cancer, and to why this transformation did not occur until decades after developments in the largely female cancers.

7. Hugh H. Young, “The Early Diagnosis and Radical Cure of Carcinoma of the Prostate,” *Bull. Johns Hopkins Hosp.* VXVI (1905): 315–21.

8. Hudson interview, (n. 1), and William Parry interview by Robert Aronowitz, December 2011.

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9. Hudson and colleagues emphasized the mistaken belief that the operation had a high mortality. Experience, blood banks, and antibiotics made the operation in 1955 safe as well as effective. They also lamented that in Europe the operation had been “practically abandoned.” Perry B. Hudson, Alex L. Finkle, and J. A. Hopkins, “Prostatic cancer VI. Mortality rate of Radical Surgery for Adenocarcinoma of the Prostate,” *J. Urol.* 73, no. 1 (1955): 139–41.

10. Hugh H. Young and D. M. Davis, *Young’s Practice of Urology* (Philadelphia, 1926), 613–71.

11. Edward L. Keyes. *Urology: Diseases of the Urinary Organs, Diseases of the Male Genital Organs, the Venereal Diseases* (New York, 1917), 16.

12. Keyes “found evidences of carcinoma in 14 out of 100 cases of specimens of supposedly non-malignant prostate removed at operation.” Keyes concluded that such cancers had “no bearing upon clinical carcinoma of the gland . . . do not appear clinically as carcinomatous.” Keyes, *Urology* (n. 11), 312.

13. Arnold Rich, “On the Frequency of Occurrence of Occult Carcinoma of the Prostate,” *J. Urol.* 33 (1935): 215–23.

14. Robert A. Aronowitz, “Do Not Delay: Breast Cancer and Time, 1900–1970,” *Milbank Quart.* 79 (2001): 355–86.

15. Ethel B. Gutman, Edith E. Sproul, and Alexander B. Gutman, “Significance of Increased Phosphatase Activity of Bone at the Site of Osteoplastic Metastases Secondary to Carcinoma of the Prostate,” *Amer. J. Cancer* 28 (1936): 485–95.

16. Robert A. Aronowitz, *Unnatural History: Breast Cancer and American Society* (Cambridge: Cambridge University Press, 2007). Barron H. Lerner, “Fighting the War on Breast

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Cancer: Debates over Early Detection, 1945 to the Present,” *Ann. Internal Med.* 129 (1998): 74–78.

17. George Crile, Jr., *Cancer and Common Sense* (New York: Hale, 1955).

18. Hudson worked at this hospital after dropping out of medical school when his father stopped paying tuition. He returned to medical school after finishing graduate study in physiology at the University of Chicago. These and subsequent biographical details from interview with Hudson conducted in 2011 (n. 1).

19. Delafield contained three hundred beds and cost \$7,800,709 to build. New York City also paid for the latest cancer equipment, such as a 2 million deep volt X-ray machine at a cost \$1,394,000 (Anonymous, “New Modern Units to Aid Cancer War,” *New York Times*, July 9, 1950).

20. Perry B. Hudson, Alex L. Finkle, J. A. Hopkins, Edith E. Sproul, and Arthur P. Stout, “Prostatic cancer. XI. Early Prostatic Cancer Diagnosed by Arbitrary Open Perineal Biopsy Among 300 Unselected Patients,” *Cancer* 7, no. 1 (July 1954): 690–703.

21. Siddhartha Mukherjee, *The Emperor of All Maladies: A Biography of Cancer* (New York: Scribner, 2010).

22. Homeless Bowery men constituted a devastating social problem in itself that attracted social science studies, journalistic accounts, films, and photographs. For example, Columbia University researchers interested in social welfare issues began their own “Bowery Project” in 1963 that involved the aid of Welfare Department administrator Morris Chase, who Hudson credited with gaining access to subjects at the Muni. Howard Bahr and Theodore Caplow, *Old Men Drunk and Sober* (New York: New York University Press, 1973).

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23. Men from the Muni were recruited to other dubious clinical experiments. In 1942, the Rockefeller Foundation funded a “louse lab” in the Lower East Side because of its proximity to the Muni. The first field trials of anti-louse powders were planned on “Bowery bums.” But expectations of compliant Bowery subjects proved wrong. Bowery men proved too unreliable and uncooperative to be research subjects. Alison Bateman-House, “Men of Peace and the Search for the Perfect Pesticide: Conscientious Objectors, the Rockefeller Foundation, and Typhus Control Research,” *Pub. Health Rep.* 124, no. 4 (July-August, 2009): 594–602.

24. Hudson interview, n. 1.

25. Perry B. Hudson, Alex L. Finkle, Aristides Trifilio, Harold M. Jost, and Edith E. Sproul, “Prostatic Cancer. VIII. Detection of Unsuspected Adenocarcinoma in the Aging Male Population,” *J. Amer. Med. Assoc.* 155, no. 5 (1954): 426–29. Patients/subjects were also characterized as avoiding medical care, “and received what they had with the rather detached attitude people have on the Bowery; they may be considered resistant to helpful suggestion, particularly when it involves the prostate gland.” R. Totten, “Some Experiences with Latent Carcinoma of the Prostate,” *Bull. New York Path. Soc.* (July 1953): 579–82.

26. There is a considerable historical, sociological, and economic scholarship on the Bowery and the skid rows of other American cities. See, for example, Carl I. Cohen and Jay Sokolovsky, *Old Men of the Bowery: Strategies for Survival among the Homeless* (New York: Guilford, 1989) and Bahr and Caplow, *Old Men Drunk and Sober*, (n. 22). The remarkable neo-realist film *On the Bowery* (L. Rogosian, Milestone Films, 1957) follows a down-and-out railroad worker for three days, himself and other “actors” Bowery residents.

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27. Many twentieth century bioethical tragedies have involved racial minorities, institutionalized juveniles, and prison and other captive populations. The Bowery men were almost uniformly white and their vulnerability resulted from their poverty, mental and physical illness, alcoholism, and the resulting homelessness. These demographic factors may partially explain why the *Bowery series* has not previously attracted any bioethical attention. Urologists may also have been embarrassed by the continuities highlighted in this paper's conclusion.

28. Hudson's contemporaries, for example, offered inconsistent explanations of recruitment practices. "The word we had," recalled William Parry, urologist contemporary of Hudson's (interview, n. 8), was that subjects were paid \$25 or \$50 "just to have a little incision." Another urologist, acknowledging this recollection of a colleague as hearsay, noted that "street people were brought in for exams and if they were found to have prostate cancer they were told they could stay in the hospital through the winter months if they would agree to a radical perineal prostatectomy. I was told that there were times when the final pathology showed no evidence of prostate cancer." Email from David Zornow to Tupper Stevens (forwarded by Stevens to Robert Aronowitz) December 13, 2011). The differing accounts suggest some ambivalence about the study's ethics.

29. Blending or confusing clinical research and patient care, the costs of medical care could be borne by the City. According to Hudson (interview, n. 1), "no patient ever paid for hospitalization. No patient ever paid a doctor there." Some of the costs for follow-up examinations (\$35 per exam) were covered by Hudson's initial NIH grant (William H. Haenssell to N.B. Hen, December 2, 1954; RG 443/National Institutes of Health, Principal Researcher

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Investigator File, 1938–1990 File: Perry B. Hudson—CS 9378; 130, 69:34:1/FRC Box 106, National Archives (hereafter NIH file 2).

30. The book’s genesis was a conversation between the author and Hudson. Elmer Bendiner, *The Bowery Man* (New York: Nelson, 1961).

31. *Ibid.*, 169.

32. *Ibid.*, 171.

33. *Ibid.*, 172.

34. Hudson et al., “Prostatic cancer. XI” (n. 20).

35. Hudson’s aims were diverse and in flux. In a progress report (February 1960, Field Investigation Grant CS 9378:C4, NIH file2), Hudson recalled that his original purpose was to determine “the frequency of the disease in asymptomatic men over age fifty,” but then new aims were added such as the “evaluation of various surgical techniques for diagnostic and therapeutic purposes. Endocrine management and the combination of surgical and hormonal treatment for the disease was appraised. It was seen that this series presented an ideal medium, original in both its concepts and methods, for a long-term study of the biological behavior of prostatic adenocarcinoma.” Referring to homeless men as an “ideal medium” reflected the “series” ethical problems.

36. In one progress report (February 1960, Field Investigation Grant CS 9378 (C4), NIH file #2), Hudson contradicted his repeated published assertion that after the first 141 subjects men were recruited irrespective of any signs or symptoms. Perhaps more aware by 1960 of others’ perceptions of ethical problems, Hudson wrote that “screening begins with a brief physical appraisal and rectal palpation of the older clients at the Men’s Shelter as they come

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from their showers in the evening. When the urologist's findings are suspicious for cancer, recommendation is made for an interview at the hospital." Once at Delafield, "the prospective patient is told the possible significance of the cursory examination he received at the Shelter and is encouraged to enter the hospital to evaluate his prostatic condition." While selection compromised generalizability, it potentially mitigated some of the concern about the harm of biopsy (i.e. there was potentially more to gain for the individual with a higher chance of having prostate cancer).

37. Williams interview (n. 1).

38. Bendiner, *The Bowery Man* (n. 30), 174.

39. Hudson's initial NIH funding, which was mostly for follow-up work, was approved with two dissenting votes. The dissenters were concerned that the study population was so mobile and had such high mortality that few subjects would remain for the 10 years it would take to get meaningful results (Summary Sheet, CS-9378, 1/7/1955; NIH file 2)).

40. According to Hudson ("Prostatic cancer. XIV" [n. 1], 342), follow-up was done by a special unit that coordinated its activities with various government and medical institutions and used "marginally punched tabulation cards."

41. Bendiner (*The Bowery Man* [n. 30], 169) emphasized the difficulty of follow-up, comparing the effort to that of bird watchers banding wild geese.

42. Perry Hudson et al., "Prostatic cancer. VIII" (n. 25), 1954.

43. Hudson and colleagues experimented with anesthetic techniques. One report discussed their experience using pudendal block (local) anesthesia for open perineal biopsies. Perry Hudson, Alex Finkle, and Jean Henley, "Prostatic cancer. V. Improvements in Pudendal

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Nerve Block Anesthesia for Surgical Perineal Biopsy of the Prostate,” *Amer. J. Surg.* 87, no. 4 (1954): 604–7. Williams (interview, n. 1) reported that the residents were well trained by Hudson, who sometimes allowed them to operate while he smoked a cigar or had his hair cut in a nearby lounge.

44. The size of the open perineal biopsy is reported differently in published reports and in interviews with participants. An early study (Totten. “Some experiences with latent carcinoma,” [n. 25], 1953), gives the size as 2.5 X 1 x .5 cm.

45. Perry Hudson, “Prostatic Cancer IV. Combined Surgical and Endocrine Management of Curable Lesions,” *Surg. Gyn. & Obstet.* 96, no. 2 (February 1953): 233–34. Later clinical trials would show the dangers without additional efficacy of adding DES treatment to surgical castration.

46. Hudson recalled (interview, n. 1) that only patients with suspected metastatic disease were so treated. The 1966 NIH review (summary statement, re:R01CAD8693-01, NIH files), however clearly stated that all of the Bowery men who were found to have “latent lesions,” approximately 10% of 1229 men biopsied, “received prostatectomy, orchiectomy, and subsequent estrogen therapy.”

47. Hudson published a short report on the first fifty-two radical perineal prostatectomies at Delafield, noting only one death (Hudson, Finkle, and Hopkins, “Prostatic cancer VI.” [n. 9]). In one NIH progress report covering the September 1, 1955–August 31, 1957 period (NIH file 2), Hudson referred to a paper in preparation entitled “Prostatic Cancer XVII. Complications from Perineal Prostatic Surgery in 1000 Consecutive Cases.” This paper does not seem to have ever been published.

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48. Carl P. Dahlen and Willard E. Goodwin, "Sexual Potency after Perineal Biopsy," *J. Urol.* 77, no. 4 (1957): 660–69.

49. Hudson interview (n. 1). Such differences could always be attributed to differences in skill, experience, and technique and the historian—and often the contemporary observer—is at a loss to accurately evaluate these claims.

50. Williams interview (n. 1).

51. Hudson and Stout, "Prostatic cancer. XVI" (n. 1).

52. In the absence of a control group, even if such follow-up data were complete, inferences about efficacy would necessarily be very limited. In various NIH progress reports, but not in publications, Hudson presented survival estimates for treated Bowery men, sometimes stratified by his idiosyncratic staging system and age. Hudson surmised that these survival rates were better than what would have occurred if cancer had not been detected and treated. He also claimed without supporting data that "the perineal anatomical approach has been proven to be the simplest surgically with regard to urinary incontinence and sexual potency (progress report, February 1960, Field Investigation Grant CS 9378 (C4); NIH File 2)."

53. Hudson, "Prostatic Cancer. XIV" (n. 1).

54. Obstructed men diagnosed with cancer by pre-operative biopsy could then have a radical perineal operation rather than more limited surgery. As a result, some men would be spared cancer later. More generally, Hudson urged urologists to establish the diagnosis of prostate cancer in any number of clinical situations. He pointedly noted that prostate cancer "is perhaps the sole remaining area in which malignant tumors are particularly important and for which surgeons do not willingly embrace the philosophy of determining, by tissue diagnosis,

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what disease is present before surgical therapy is indicated.” Hudson and Stout, “Prostatic cancer. XVI” (n. 1).

55. Ibid.

56. In 1950s progress reports (e.g., one dated 2/11/1958, NIH file 2), Hudson said he was following a control group composed on men who had the full work-up short of biopsy. But comparisons with these controls were not reported in publications.

57. Hudson et al., “Prostatic cancer. XI” (n. 20).

58. Williams interview, (n. 1).

59. Hudson interview, (n. 1).

60. Hudson filed a wrongful dismissal suit against Urology chair John Lattimer and Institute of Cancer Research head Alfred Gellhorn in 1960. Hudson also alleged that Lattimer and Gellhorn sabotaged his appointment at Massachusetts General Hospital by writing defaming letters to his potential employers. Lattimer had asked that Hudson not be reappointed in the late 1950s. The main charge was insubordination. Hudson had published research without prior approval. Lattimer also argued that not reappointing Hudson was important “for the reputation of his own Department as well as of the School and Hospital (800 file Perry Hudson, memo, May 25, 1959, “Committee on Administration Medical Faculty,” Archives & Special Collections, A.C. Long Health Sciences Library, Columbia University Medical Center, hereafter “Hudson file.”) The main instantiation of insubordination and the reputational issue was Hudson’s study of the efficacy of a proprietary product for hospital use to reduce staphylococcal infections, which involved claims and counter-claims of poor science, financial profit, and conflict of interest. Nowhere is the Bowery series mentioned as the cause of dismissal.

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Hudson's suit was settled without trial. He retained a faculty appointment in the department of Zoology, an unusual place for a urologist, and laboratory space to finish the Bowery series (Stanley Salmon to Dr. Charles Drake June 22, 1960, memo titled "Space requirements for Dr. Perry Hudson," Hudson file). Other letters attest to Lattimer's concerns about the time and space Hudson needed to complete the Bowery series (Lattimer to Dr. H. Houston Merritt, March 9, 1960, Hudson file). Columbia pathologist Edith Sproul defended Hudson's need for continuing space and resources for the Bowery series in an undated letter to Merritt from the same period (Hudson file): "Dr. Hudson has developed a unique study of prostate cancer in this city which must inevitably require years of follow-up. For this to be dropped at the time it is becoming statistically valuable would be a great medical loss."

61. From summary statement, re: R01 CAD8693-01, "NIH files."

62. Hudson interview (n. 1).

63. Williams interview (n. 1). The comparison is problematic because the Bowery men were poorly informed asymptomatic recruits, not men seeking medical care for prostatic obstruction or anything else. This participant also recalled that another clinical experiment was embedded in the Bowery practices. An anesthesiologist tested a new epidural anesthetic that led to many men having seizures and was eventually stopped.

64. One sympathetic citation was co-authored by William W. Scott, Hudson's mentor at Johns Hopkins. "As far as the authors know, there is no biopsy method at the present time that can afford a more accurate diagnosis of early prostatic carcinoma (1186)." But the authors made no recommendation to actually use open perineal biopsies and concluded that "there is still a great need for a "good test" for early prostatic cancer (1194)." William W. Scott and William N.

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Toole. "Carcinoma of the Prostate," *Campbell's Urology* 2nd ed. (Philadelphia, 1963), 1173–226.

65. From summary statement, re: R01 CAD8693-01, NIH files. A 1963 NIH review of another Hudson proposal (rejected; 2R01 CA09803-04 summary statement, NIH files) to continue follow-up made a passing comment that "the investigators have demonstrated the value of *the rather radical measure* of perineal biopsy when prostatic enlargement is found (italics mine)."

66. Parry interview (n. 8).

67. J. D. Fergusson, "The doubtfully malignant prostate," *Brit. J. Surg.* 52 (1965): 746–50.

68. Frank Hinman, "Open vs. Needle Biopsy in the Early Diagnosis of Prostatic Cancer," *Amer. Med. Assoc. Arch. Surg.* 70, no. 4 (1955): 475.

69. One of Hudson's co-authors did publish a paper in which open perineal biopsies were studied as a routine procedure on men contemplating prostatic surgery for obstruction. See Otto M. Lilien, J. A. Schaefer, V. Kilejian, and V. Andaloro, "The Case for Perineal Prostatectomy," *J. Urol.* 99 (1968): 79–86. These authors, in contrast to Bowery series reports, noted that open perineal biopsy can cause rectal perforation and impotence and discussed the merits and downsides of biopsies for men undergoing these procedures.

70. Hudson interview (n. 1).

71. Williams interview (n. 1).

72. Hudson interview (n. 1).

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73. From *Scientific Report of the Institute of Cancer Research, 1952–1953*, College of Physicians and Surgeons, Columbia University, N.Y., 50.

74. One focus of Hudson’s career remained innovating and promoting perineal approaches to biopsy and cancer surgery, including developing perineal needle techniques and creating atlases of prostatic surgery and videos of radical perineal surgery.

75. NIH reviewers noted that Hudson “tends to place little emphasis on the control subjects, i.e. those who were never biopsied. If worthwhile results are to be obtained from this study, information only on survival rates and major cause of death would be insufficient, especially in this kind of population with an atypical life situation and related atypical mortality (Summary statement, re:R01 CAD8693-01, NIH files).”

76. Jason Constantinou and Mark R. Feneley, “PSA Testing: An Evolving Relationship with Prostate Cancer Screening,” *Prostate Cancer & Prostatic Dis.* 9 (2006): 6–13.

77. M. S. Steiner and C.R. Pound, “Phase IIA Clinical Trial to Test the Efficacy and Safety of Toremifene in Men with High-Grade Prostatic Intraepithelial Neoplasia,” *Clin. Prostate Cancer* 2, no. 1 (2003): 24–31.

78. See <http://www.cancer.gov/ncicancerbulletin/080911/page4>, accessed 2/2012. Each of these estimates is based on extrapolations and is imprecise. One reason this National Cancer Institute report was issued was to emphasize the increase in prostatectomies following the introduction of robotic surgery.

79. Perry B. Hudson, Manuel T. Ty, and Otto M. Lilien. “Prostatic Cancer XV: Incurable Cancer Following Conservative Prostatic Surgery for Clinically Benign Obstruction,” *Ann. Surg.* 152, no. 2 (August 1960): 308–12.

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80. R. Gittes, "Acid phosphatase reappraised," *New England J. Med.* 297 (1977): 1398–399.

81. Other factors were also increasing the apparent incidence, e.g., more prostate surgery was being done to relieve obstruction and was accompanied by pathological review, leading to a rapid increase in the apparent disease incidence. See Ray M. Merrill, Eric J. Feuer, Joan L. Warren, Nicki Schussler, and Robert A. Stephenson, "Role of Transurethral Resection of the Prostate in Population-based Prostate Cancer Incidence Rates," *Amer. J. Epidemiol.* 150, no. 8 (1999): 848–60.

82. See Gerald L. Andriole, Robert L. Grubb, Robert L. Saundra S. Buys, David Chia, Timothy R. Church, Mona M. Fouad, et al. "Mortality Results from a Randomized Prostate-Cancer Screening Trial," *New England J. Med.* 360, no. 13 (2009): 1351–354; Fritz H. Schröder, Jonas Hugosson, Monique J. Roobol, et al., "Screening and Prostate-Cancer Mortality in a Randomized European Study," *New England J. Med.* 360, no. 13 (2009): 1320–328. These are two of the best clinical trials of PSA screening followed by standard interventions, the former finding no statistically significant effect and the latter showing a marginal impact on prostate cancer mortality but at great financial and health costs. The meaning of these trials is of course open to interpretation.

83. Finely calibrated, retrospective ethical judgments are difficult to make and for the historian fraught with problems. For an interesting discussion of this issue in light of recent discovery of U.S.-sponsored Guatemalan syphilis experiments, see <http://bioethics.gov/cms/sites/default/files/Ethically%20Impossible%20%28with%20linked%20historical%20documents%29%202.7.13.pdf>, especially p. 91. See also Susan Lederer's survey of

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research controversies from the nineteenth to the mid-twentieth century that emphasized that contemporary skeptics couched their objections to human experimentation in an anti-(human) vivisection framework. This emphasis on the low benefit and high risk to human subjects weakened with the inter-war rise of the prestige of clinical medicine and greater faith in medical research's ability to deliver therapeutic benefits (Susan Lederer, *Subjected to Science: Human Experimentation in America before the Second World War* [Baltimore: Johns Hopkins University Press, 1995]). Susan Reverby (*Examining Tuskegee: The Infamous Syphilis Study and its Legacy* [Chapel Hill: University of North Carolina Press, 2009]), James Jones, (*Bad Blood: The Tuskegee Syphilis Experiment* [New York: The Free Press, 1981]), and Allan Brandt ("Racism and Research: The Case of the Tuskegee Syphilis Study," *Hastings Center Rep.* 8, no. 6 [1978]: 21–29), and others have described the social and clinical context and mindset of participants in the now infamous Tuskegee syphilis study. These scholars have emphasized prevailing social and political realities—racism, Jim Crow laws, wartime and cold war politics—as strong permissive conditions.

84. There is an extensive historical and bioethical literature on the problematic use of captive and other vulnerable populations in biomedical research. Especially relevant are A. Hornblum, *Acres of Skin: Human Experiments at Holmesburg Prison* (New York: Routledge, 1998) and Joel D. Howell and Rodney A. Hayward, "Writing Willowbrook, Reading Willowbrook: The Recounting of a Medical Experiment," in *Using Bodies: Humans in the Service of Medical Science in the Twentieth Century*, Jordan Goodman, Anthony McElligott, and Lara Marks, eds. (Baltimore: Johns Hopkins University Press, 2003), 190–213.

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85. In the wake of President Clinton's sponsored review of cold war radiation experiments, a great deal of ethical attention and policy response has been directed at the conflicts which arise when clinicians who treat very sick and often desperate patients are at the same time experimenting with therapies or otherwise using patients as subjects. See, for example. Gerald Kutcher, *Contested Medicine: Cancer Research and the Military* (Chicago: Chicago University Press, 2009). But the Bowery men's situation was different and more problematic than desperate cancer patients because their medical illness and desperation, such as it existed, were created by Hudson's proto-screening practices. Once patients, these men were given standard (for Hudson at least), if aggressive and protocol-driven, care.

Closer to the *Bowery series* in the way that experiment and treatment were blurred but also to my analytic perspective is Ilana Löwy's illuminating comparison of early twentieth century yellow fever experiments in Brazil and mid-twentieth-century sexually transmitted disease experiments in Guatemala ("The Best Possible Intentions: Testing Prophylactic Approaches on Humans in Developing Countries," *Amer. J. Pub. Health*, 103, no. 2 [February 2013]: 226-37). Löwy emphasized the underlying structural and local factors that gave rise to experiments that were later understood by nonparticipants as unethical. These two bioethical tragedies were public health experiments with noble goals, made possible by poverty, sickness, and inadequate medical care, and built on exchanges (cigarettes, food, medicines, medical care, medical training, and career opportunities for local investigators) not strictly part of clinical treatment or experiment. This "fertile soil" for troubling practices, which Löwy emphasized are present in the Global South today, have their parallels in the Bowery studies, and similarly point to the limits of

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a bioethical gaze narrowly focused on the plans and goals of investigators conducting an explicit experiment and the protections afforded by contemporary bioethical principles and practices.

86. While historians have provided much needed social context for how now infamous unethical medical experiments were carried out on vulnerable populations, the social and structural realities in which routine medical innovation, training, and practice have occurred have received much less attention. A public hospital, teaching wards with charity cases, or even the occasional nonpaying patient within a private hospital, have provided teaching and research possibilities, especially after the expansion of medical care and research post-World War II. Doctors and trainees have practiced on poor people in return for their gaining access to medical care. And while desperately sick cancer patients of any class were often willing to become research subjects to gain access to new cancer treatments, poor patients with or without cancer could be induced to take part in practices unlikely to directly benefit them in return for access to basic medical care. So a public cancer hospital was ideally set up for research/practice on patients/subjects who did not—yet—have cancer.

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Figure 1. Examination of group of “Bowery bums” for prostate cancer at Francis Delafield Hospital, 1957. (Photo by Walter Sanders//Time Life Pictures/Getty Images). Used with permission.