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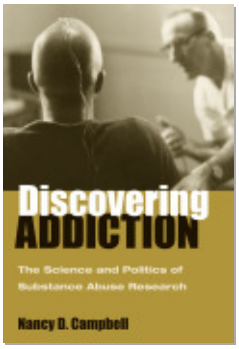
Discovering Addiction

Campbell, Nancy D.



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CHAPTER 6

"The Great Hue and Cry": Prison Reform and the Ethics of Human Subjects Research

"The Panopticon is a privileged space for experiments on men, and for analysing with complete certainty the transformations that may be obtained from them" (Foucault 1979, 204). The Panopticon was not only a surveillance mechanism but a "laboratory of power," which "could be used as a machine to carry out experiments, to alter behavior, to train and correct individuals." It could be used "[t]o experiment with medicines and monitor their effects" and "[t]o try out different punishments on prisoners, according to their crimes and character, and to seek the most effective ones" (Foucault 1979, 203). Foucault drew the dilemma starkly: there was room for neither individual nor collective motive within the Panopticon. The callous could do nothing different from the compassionate. Measured according to this nihilistic yardstick, clinical research amounts to nothing more than an inquisitorial procedure. The metaphor situates the Panopticon as an engine for disindividualizing power, which rendered inconsequential the motives of any individual involved in operating the machine: "It does not matter what motive animates him: the curiosity of the indiscreet, the malice of a child, the thirst for knowledge of a philosopher who wishes to visit this museum of human nature, or the perversity of those who take pleasure in spying and punishing" (Foucault 1979, 204).

Do the motives that animate research matter? I have contended that the motives of the researchers at the ARC shaped the research process and its outcomes in mostly laudable directions. Many people have asked me whether the ARC should be characterized along the same lines of the Public Health Service

study of the natural course of untreated syphilis in African American men (Jones 1981/1992; Reverby 2000). The Tuskegee Study of Untreated Syphilis in the Negro Male continued even after effective treatment became available, and the more I learned about Lexington, the less appropriate such comparisons seemed.

Research ethics must be situated within the social conditions, material constraints, and commitments that prevail in specific institutional contexts. Research at the ARC took place within a federal narcotics hospital set up to deliver drug treatment on a massive scale. Treatment at Lexington was not "cure" but gradual withdrawal, detoxification, and abstinence, mixed with psychotherapy and vocational rehabilitation.[1] The Lexington and Fort Worth hospitals set a humane standard of care in contrast to the abrupt withdrawal cold turkey practiced in jails without medical supervision. Unlike the Tuskegee study, treatment was not withheld at Lexington, where the primary goal was achieving a sustained period of abstinence before release. One of the troubling features of the ARC was the administration of drugs that undermined abstinence; research participants were, at least in the early days, rewarded in their drug of choice; some, no doubt, were enticed by the prisoner grapevine's promise of drugs in a "drug-free" setting. A second problematic feature was the racial profile of Lexington admissions in the post–World War II era, when the general population was increasingly composed of poor, young, racial-ethnic minorities. However, it turned out not to be the case that they ever became the majority of research participants. Research subjects tended to be older, more affluent, and white. Written informed consent was obtained; after the summer of 1949, formal written consent forms were read aloud to participants who could not read.[2] Himmelsbach also indicated that consent was obtained from the program's earliest days (1972, 1994).

Despite the fact that African American inmates were not disproportionately experimented on, there remains something unsettling to present sensibilities about systematic research programs housed within structurally coercive institutions. Should research oriented toward understanding addiction and relapse not have been undertaken? Could any research be conducted ethically in any prison? Such questions came to a head in the struggles related in this chapter. The laboratory logics, experimental practices, and ethical norms at Lexington varied over time, which should make us reluctant to issue a blanket condemnation or to measure the ARC by today's standards. We should also be cautious about uncritically adopting the frames that the 1970s prison rights

movement used to cast aspersion on all prison research in order to better advance the case for reforms. Such frameworks were not designed to differentiate between greater goods, lesser harms, and necessary evils. Ending prison research became a symbolic terrain on which reformers portrayed themselves as upholding the rights of the disenfranchised. Key Democratic congressional leaders sponsored the hearings that were the mechanism by which the scientific work of the ARC was discredited and its conduit to research subjects cut off. Negative publicity was one means by which the reformers secured their own good name as guardians or protectors of the rights of prisoners. This was, however, a contest over whose constructions of "rights" and "protections" would win out.

HOW LEXINGTON BECAME AN OLD-FASHIONED PRISON

A banner year in the outcry over prison reform and human experimentation, 1973 caught the reluctant jailers at the ARC by surprise. By then, they held the keys to the only federal facility where prisoners still served as subjects. The research staff was responsible for order and discipline among the between forty and sixty federal prisoners who elected to transfer to the ARC. The laboratory became a "miniprison" just when prison research was brought to crisis. Never nuanced, the politics of crisis created the impression that unconstrained biomedical researchers badly needed external oversight and that legislators needed to provide it.[3] The American Correctional Association (ACA), the national accreditation body for U.S. prisons, created its first informed consent protocol for correctional institutions in 1972. The ACA was inspired by a scandal involving Southern Food and Drug Research Incorporated, a "Phase I drug-testing empire" headed by Austin R. Stough, MD, a physician untrained in pharmacology (Harkness 2003, 218–30). Jessica Mitford exposed Stough in a highly publicized *Atlantic Monthly* article in January 1973.[4] She drew favorable attention to the American Civil Liberties Union's National Prison Project, which had begun litigating prison abuse cases six months before, when Alvin Bronstein became the executive director.[5] The ACA then reversed course and disallowed prison research entirely by withholding accreditation from any facility where it was conducted. That move reconfigured the terrain on which prison research took place, just before several controversies came to public notice.

News of the Tuskegee scandal broke in the summer of 1972. Senator Edward Kennedy's Committee on Labor and Public Welfare moved quickly

into investigative hearings, to which were invited former ARC director Harris Isbell and ex-Lexington inmates Eddie Flowers and James Henderson Childs.[6] The hearings led to formation in 1974 of the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research.[7] This commission provided a conduit for new forms of bioethical expertise. Over the next several years, the commission documented the scope of prison research through site visits to the ARC and other drug-testing operations in state prisons. Anticipating an election year, Senator Robert Kastenmeier held hearings in the fall of 1975 on a bill to end prisoner experimentation. Witnesses included three ex-research participants and the current ARC research director, William R. Martin, who had assumed the reins at the ARC after Isbell's departure.[8] Finally, to investigate a project dubbed MKULTRA, conducted under the umbrella of the Central Intelligence Agency (CIA), Senator Frank Church held hearings in 1975, with a follow-on hearing in 1977.[9] Church sought the Democratic presidential nomination in 1976, eventually conceding it to Ford. All of the hearings previously mentioned were convened by Democrats seeking reelection.

The National Commission for the Protection of Human Subjects never banned federal prisoner research outright. However, a new political consensus placed prisoners in the category of subjects most vulnerable to disrespect, lack of benefit, and unfairness in the conduct of research. Given their circumstances, prisoners might be selected for "administrative convenience" or because they were "cheaper than chimpanzees," as Mitford put it (1973b, 138). Three principles—respect for persons, beneficence, and distributive justice—appeared in the commission's culminating document, *The Belmont Report* (National Commission 1978).[10] Each principle was to be applied to informed consent, determination of risk and benefit, and subject selection. Fearing that "social undesirables" might be subjected to exceptional risk, the commission stated a preference that "dependent" or "vulnerable" subjects not participate in research at all.

> One special instance of injustice results from the involvement of vulnerable subjects. Certain groups, such as racial minorities, the economically disadvantaged, the very sick, and the institutionalized may continually be sought as research subjects, owing to their ready availability in settings where research is conducted. Given their dependent status and their frequently compromised capacity for free consent, they should be protected against the danger of being involved in research solely for administrative convenience, or because they are easy to manipulate as a result of their illness or socioeconomic condition. (National Commission 1978)

Drug users have historically been constructed as unreliable subjects because they are easily manipulated and are seen as themselves manipulative (Campbell 2000). Aspects of long-term, chronic drug use and the criminalization of drug use has compounded this public perception. Although addiction went unmentioned in *The Belmont Report,* both illness and imprisonment were found to undermine the capacity for free consent. When the commission required experimenters to guarantee that consent was informed, selection unbiased, and benefit direct, those conducting research with convicted drug-using felons faced insurmountable challenges in continuing their work.

When ARC researchers first told me that they were surprised at the official decision to fold the human research program at Lexington, I was skeptical, until I uncovered considerable evidence that the ARC had been exempt from the generalized political critique of prison research by everyone other than congressional staff. For instance, staff of the National Commission for the Protection of Human Subjects were dispatched to Lexington on May 3, 1976, for a site visit. According to Jasinski, who became director of the ARC after Martin retired in 1977, the commission "found that we had already been doing informed consent [with] all the safeguards." Jasinski continued:

> They didn't say you couldn't do prisoner research, but that you needed some extra safeguards. [W]e had a system which had multiple safeguards. . . . [T]here hadn't been an injury or death in forty years doing this type of research. The Presidential Commission was going to say that, to find that what we were doing was okay. (2003)

In contrast to most commercial Phase 1 studies in state prisons, many of the oversight bodies recognized that the ARC research was essential for providing data for domestic and international drug policy and considered it to be ethically aboveboard.

Nevertheless, the Bureau of Prisons (BOP) cut the ARC adrift from the literally captive population of postaddicts on whom it had relied for almost forty years.

> This became a decision by the administration. We had been receiving cooperation from the Bureau of Prisons. One day Norm Carlson, who had been the director of the Bureau of Prisons, called and said that he was going to end the prisoner program. (Jasinski 2003)

Carlson's reversal changed the definition of what counted as addiction research, who conducted it, how and where they went about it, and how they

felt about it. Personally uncomfortable with prisoner research,[11] Carlson had been pressured to broker a phaseout deal with the National Institute on Drug Abuse (NIDA) in the wake of the Kastenmeier hearings, during which Lexington ex-inmates had given damaging and dramatic testimony about their participation in ARC studies.[12] Meanwhile, the broader institution of Lexington shifted away from being a destination solely for drug addicts. Under Nixonian drug policy, responsibility for drug treatment continued to devolve (today, it often resides at the county level). Although access issues remain, treatment is far more available than it was when Lexington and Fort Worth were the only providers. By 1973, Lexington had become an obsolete and "rather expensive anachronism," in the words of Jerome H. Jaffe, director of Nixon's White House Special Action Office for Drug Abuse Prevention (Walsh 1973a, 1004). The institution was transferred completely to the BOP in 1974, and the ARC was then absorbed into NIDA.[13] After 1974, the broader institution at Lexington became what its founders insisted it should never be—an old-fashioned prison that had nothing to do with treatment, rehabilitation, or research.[14] Although the ARC program continued until the end of 1976, it had become clear that change was in the offing.

RACE AND THE CONSTRUCTION OF VULNERABILITY: THE POLITICS OF REFORM AND RESEARCH

Since the first mandatory minimum sentences (instituted by the 1951 Boggs Act), incarceration has been the main plank of U.S. drug policy. Criminalization can be pegged to changing patterns in the racialization of drug-using populations and to drug law enforcement in urban settings. Enormous numbers of African Americans have been imprisoned on drug charges since the mid-1950s. By 1955, two-thirds of the eleven hundred drug offenders housed at Lexington were African Americans addicted to heroin.[15] Only seven years prior, two-thirds had been white. Although the heroin-addicted population grew disproportionately poor, urban, and black, racial conflict rarely rose to administrative attention at Lexington until the late 1960s, when the discourse of civil rights was mobilized within the institution. Racial difference was certainly a practical matter of lived experience at Lexington, where there was a flourishing African American jazz culture. Ex-inmate Eddie Flowers differentiated the racial dynamics of Lexington in the 1950s from those of northern penitentiaries, noting that membership in drug subcultures superseded racial conflict between inmates at Lexington. Racial disparities in drug conviction rates have widened

since Lexington closed its doors; they are best attributed not to consumption patterns but to law enforcement patterns, the political economy of global drug trafficking networks, and the cultural geography of drug markets. The racial politics of the U.S. wars on drugs have rendered drug laws "the new Jim Crow" (Boyd 2001).

Prison research became a civil rights target in the 1970s, when some movement leaders charged that research subjugated black bodies. At the 1976 National Minority Conference on Human Experimentation, held by the National Commission for the Protection of Human Subjects, concerns that African Americans bore a disproportionate share of the risks of prison research arose. Such allegations had previously cropped up in a highly publicized case involving a University of Maryland research program (studying infectious disease) conducted at the Maryland House of Corrections in Jessup, Maryland—despite the fact that civil rights leaders became aware that most research subjects were white early in the conflict (Gilchrist 1974). The commission's national fact-finding mission also revealed that research subjects were mainly white, better educated, and employed at the "better" jobs even in predominantly white institutions (National Commission 1976b, 9). Research participants at the State Prison of Southern Michigan in Jackson, Michigan, were also disproportionately white, older, and more experienced with prison life (National Commission 1976b, 36). At Jackson, they were overwhelmingly from the "honor block." The commission determined that African Americans comprised less than one-third of research subjects nationally. Black prisoners actually complained to the commission that there was a selection bias against their participation.

Lexington reflected this national trend. Never did blacks outnumber whites in the research ward. Entirely white study populations were not uncommon prior to when the Narcotic Addict Rehabilitation Act (NARA) went into effect in 1968. After that, the BOP supplied between forty and sixty subjects to the ARC at any given time, dividing them so that the research population would be approximately one-third white, one-third black, and one-third Hispanic. Always in search of seasoned subjects, the ARC upped its age eligibility from twenty-one to twenty-five years old in 1968, to ensure that it did not exploit adolescents. Although it failed to find that the risks of research tilted unevenly toward blacks, the National Commission for the Protection of Human Subjects certainly recognized that the benefits of research tilted toward the affluent. Adopting a language of redistribution, the commission wrote:

> Some populations, especially institutionalized ones, are already burdened in many ways by their infirmities and environments. When research is proposed that involves risks and does not include a therapeutic component, other less burdened classes of persons should be called upon first to accept these risks of research, except where the research is directly related to the specific conditions of the class involved. Also, even though public funds for research may often flow in the same directions as public funds for health care, it seems unfair that populations dependent on public health care constitute a pool of preferred research subjects if more advantaged populations are likely to be the recipients of the benefits. (National Commission 1978)

The commission allowed research to continue if it was "directly related to the specific conditions of the class involved." The category "addicts" was not a named class and in fact had multiple strikes against counting as a "class" in the calculus of vulnerability. Most drug-addicted prisoners were black, ill, or indigent, and all were institutionalized. Under the terms set by the commission, they were multiply vulnerable subjects. The fact that most participants in prison drug research had historically been white did not assure that the vulnerable would be screened out in the future.

The charge that prison researchers exploited subjects was made forcefully by the American Bar Association, which formed the Correctional Economics Center (CEC) in early 1973 to apply economics—the "science of choice" (Meyer 1975, 7)—to prisoner experimentation.[16] In early 1975, the CEC issued a report titled *Medical Experimentation on Prisoners: Some Economic Considerations.* Authored by Peter B. Meyer, the report rendered a highly politicized and emotional question as a logical and analytical matter of the fair distribution of benefits and burdens. According to Meyer, pharmaceutical manufacturers won an "implicit subsidy" when prisoners agreed to bear risks that their "outside counterparts" refused (1975, 7).[17] Pharmaceutical companies were deeply dependent on prisoners for Phase 1 testing.[18] However, the ARC was uninterested in organizing risky Phase 1 clinical trials, which often involved screening drugs that would never make it to market because of toxicity or low tolerability (Meyer 1975, 9). The unit was uninterested in testing drugs that no one was likely to take, focusing primarily on drugs that not only made it to market but remain widely used today and on illicit drugs. Moreover, the ARC was not set up to do the kind of pharmacokinetic studies of safety required in Phase 1 testing and thus never performed "first man" studies (first trials in human beings). Finally, the ARC did not profit from direct relationships with pharmaceutical companies, although it worked closely with Merck and others. Its work cannot

be said to represent an "implicit subsidy," because it was basically a form of specialized government oversight.

Although elucidating the neurophysiology of addiction was the ARC's raison d'être, experimentees often misremember the role they played in studies. Sociological studies show that participants attach significance to their role by attributing more risk to it. When social scientists sought to determine the impact of pharmacological testing programs on prisoners, they found inmates "readily report[ed] their consent to be informed" even when it was not. Like their unincarcerated counterparts, prisoners "affirmed informed consent without really possessing it," for reasons including low literacy levels, poor memory, or cognitive issues attributed to power differentials between researchers and themselves (Wells et al. 1975, 49). Two-thirds of those who signed forms listing side effects that clearly stated, "This drug has been previously given to man by mouth in doses up to 2000 mg/day," could not recall reading that statement. After two weeks, participants "claimed . . . the drug had never previously been tested on human subjects." Wells et al. took this to mean that the subjects "wanted to believe, took pride in believing, that they were the initial volunteers, the pioneers performing an act requiring courage and one which marked them for some, if not great, distinction" (1975, 52). Recommending that prison officials not only tolerate researchers inside prison walls but welcome them, Wells et al. viewed pharmacological research as a highly positive and beneficial activity that enhanced prisoners' self-esteem and productivity and reduced their "aggression": "Opportunities for the inmate to interact favorably with well-disposed individuals from the society outside the prison walls, to experience the meaningful satisfaction of having been of service, to feel an often lacking sense of importance and to supplement his often intensely frustrating lack of financial resources may be absent elsewhere in the prison environment" (Wells et al. 1975, 53).

Widening inmates' research participation would not have been the advice of three former ARC subjects—Kenneth Matthews, Richard Alexander, and Otis Clay—who testified on September 29, 1975, the first day of the Kastenmeier hearings. These witnesses presented themselves as vulnerable and exploited, recounting having been lured to Lexington by the promise of drugs, better food, and easier living. Jon Harkness reports: "They also conveyed to the House Subcommittee a perception that, once in the research facility, they had not received adequate explanations of proposed research projects from ARC scientists. And they alleged that ARC researchers had not treated them with kindness and respect" (2003, 278). Two days later, William R. Martin refuted

these allegations in a formal rebuttal to Kastenmeier that relied on experimental data and remained almost entirely within the discursive restraints of scientific discourse.

Under court order, Martin refrained from mentioning that Otis Clay was personally motivated to portray the ARC in a negative light because Clay had filed an "inartistically drafted" pro se lawsuit in 1971 against Martin, Carlson, Jasinski, and other high U.S. government officials.[19] The lawsuit was ultimately dismissed, but at the time of the Kastenmeier hearings, an appellate court had just reinstated Clay's complaint and reversed a lower court's dismissal.[20] Clay's case was reopened when the U.S. Court of Appeals for the Second Circuit permitted him to amend his complaint in April 1975. Martin's response to Clay's congressional testimony revealed that Clay's answers in the drug study were at odds with his testimony. While Clay told the congressional committee that he left an experimental pain study because he could not stand it, Martin contended that Clay had characterized the electroshocks he received as not painful, weak, slightly painful, or average. Whereas Clay had testified to having been physically restrained, Martin insisted that subjects were not restrained. Martin stuck close to the facts as he saw them and studiously avoided the far more important question—restrained or not, why were human beings being subjected to electroshock while on drugs?

Failing to communicate the scientific rationale for the studies, much less deliver a defense of them, Martin was unable to counter the ex-inmates' testimony. Nor did he marshal the relevant scientific communities that later rallied around the ARC when it was, to all appearances, too late. The ex-inmates won a discursive and political victory that provided the nails in the coffin of the ARC at Lexington. Although the information in Clay's court documents is limited, it is also instructive in terms of the construction of vulnerability then under way. Claiming he had been treated inhumanely and had suffered a heart attack a week after participating in a two-dose naltrexone study, Clay sought two million dollars in damages. Fifty-one years old when, in 1968, he requested a transfer to the ARC from the Atlanta Penitentiary, where he was serving a ten-year sentence on federal narcotics charges, Clay stated that he was "attracted by the better living conditions at the ARC and the possibility of receiving narcotics under the experimentation program" (*Otis Clay, Plaintiff-Appellant v. Dr. William R. Martin et al. and The United States Surgeon General et al. The United States Defendants-Appellees* 509 F.2d 109–14 (2d Cir. 1975)). During his two-year stay, Clay consented to studies involving morphine, pentazocine, naltrexone, and chlorpromazine. His lawsuit claimed that he had par-

ticipated involuntarily, and he charged the researchers with having a conflict of interest between their research, administrative, and custodial roles. His testimony played up his personal vulnerability and the structural conditions within which the ARC experiments were conducted.

Following the hearing, in a letter written on November 11, 1975, Kastenmeier urged Carlson to end his "continuing commitment" to supplying research subjects for research conducted at Lexington for the Department of Health, Education, and Welfare (DHEW).[21] Carlson then appointed a task force to determine what the conditions of true voluntarism for biomedical research should be. That body came up with five conditions.[22] First, rewards could not include "meritorious good time" off a sentence. Second, volunteers had to come from "less restrictive circumstances" at the time the choice to participate in research was made. These two conditions alone would have created an entirely new set of administrative circumstances and recruitment logistics for the ARC, by giving prisoners little incentive to transfer. Third, a general fund into which research sponsors were to place supplementary monies for the well-being of prisoners was required, so that prisoners would cost researchers the same amount as free-living volunteers. Unlike the terms on which commercially sponsored prison research was conducted, the ARC had always "borrowed" subjects, paying them nothing beyond the wages they would have received for sewing uniforms, woodworking, or doing agricultural, custodial, or food service work elsewhere in the institution. Given that pharmaceutical companies did not pay the ARC but instead funneled paltry, voluntary donations to the NRC committee that coordinated communication between industry and the ARC, the proposed fee structure would have prohibited the ARC from relying on prisoners even if the BOP continued supplying them. Fourth, the task force required establishment of a "subject advisory group" consisting of prisoners themselves. Lastly, subjects were to be compensated for "all lasting injury or loss of earnings suffered as a result of participation in a research project," a stipulation that would open the ARC to litigation.

Although it set the bar high, the BOP task force unanimously agreed that "the Bureau [should] continue to participate in the valuable research being done at the Addiction Research Center" (Harkness 2003, 286). But Carlson discontinued sending federal prisoners to the ARC and quietly worked out a deal with Robert DuPont, the director of the new NIDA, to end research by the end of 1976.[23] On March 1, 1976, Carlson justified this incremental approach to Kastenmeier as showing respect for the "significant research that has resulted in the past from this program" and permitting the researchers "to continue with

the programs they have already initiated at that facility." Attempting to gain some political capital by hurrying things along, Kastenmeier leaked Carlson's letter to the *Washington Post* before dropping the bill. DuPont placed an "irate" call to Carlson to complain that the BOP was "reneging" on its commitment to a gradual phaseout.[24] In the end, political expediency, rather than "abstract arguments concerning the ethical validity of experimentation with prisoners," ended biomedical research in federal prisons (Harkness 2003, 304). The political spectacle displayed in the congressional hearings also played a part in ending the practice, by embarrassing the scientific community and overshadowing its reasoned responses.

Clearly, Carlson believed that continued medical experimentation on federal prisoners was unwise. In a letter of October 2, 1975, to Harold R. Tyler, Deputy Attorney General Carlson conveyed the gist of his testimony before the Kastenmeier committee. Stating that he had "serious doubts about the ability of prisoners to volunteer," Carlson noted that he supported the bill's "general thrust" to ban medical research on prisoners altogether. Dutifully, he explained that the ARC used federal prisoners "to test abuse potentialities of opiate-like drugs" and search for "antagonistic drugs to counteract the effects of addictive drugs," while he distanced the BOP from the project, evidently considering it NIDA's problem. Like Kastenmeier, Carlson had become convinced that most biomedical research then being conducted with prisoners could be conducted more ethically on nonprisoner populations. Diametrically opposed, the scientific community considered the turn to other populations neither feasible nor ethical.

SCIENCE STEPS IN: RESPONSES FROM RELEVANT SCIENTIFIC COMMUNITIES

A deluge of letters praising the ARC research program as essential to the global drug policy regime and to the growth of scientific knowledge on the effects of drugs on brain and body came from the scientific community. An NIMH research task force gave this evaluation in its report "Program of the Addiction Research Center, 1935–Present":

> By any measure, the program of the ARC has been an outstanding success for 38 years and is currently as vigorous as any time in its illustrious history. There have over the years been natural changes in programs and goals of the ARC and changes in emphasis, which were due to changes in either the staff or interests of the principle [*sic*] investigators. Without any reservation, I believe that the

> drug assessment program of the ARC has been, from every point of view, one of the most outstanding and effective public health endeavors in preventing drug abuse. No drug that has been evaluated at the ARC and has been judged to have a low abuse potentiality has given rise to any significant abuse problem. There have been a very large number of drugs, however, where pharmacologic properties are closely similar to those of heroin and morphine and dilaudid, which if they had been uncontrolled most certainly had the potential for creating major problems of abuse. (29–30)[25]

This report is typical: it lauded the ARC for playing a positive role in public health. By contrast the Clinical Research Center (CRC)—as the therapeutic and custodial side of Lexington was called after 1968—did not enjoy such esteem. Its reputation was low within the addiction research enterprise. On January 30, 1973, director Harold Conrad's memo to all ARC and CRC employees assured them that there were no plans to close the ARC but indicated that the CRC might close down. Why, then, was the internationally known, venerable human research program of the ARC shut down in 1976?

Scientists expressed considerable resistance to ending research not only at Lexington but in prisons more generally. For them, a major factor was the regulatory climate set up by the 1962 amendments to the Food, Drug, and Cosmetics Act (1938), which made large-scale clinical trials mandatory. Not only did the amendments require proof of a drug's safety and efficacy, but the particular disease or condition for which it was effective had to be named. This regulatory requirement presented a dilemma to which moral repugnance and prisoner rights could only partly reply: large-scale drug testing of the type that was then largely carried out in state prisons was required for the regulatory process. What would happen if prison research was banned? Some members of the National Commission for the Protection of Human Subjects believed that prisoners should be allowed to participate if trials were conducted off prison grounds—as long as nonprisoners also joined the study for the same compensation. Others felt prisoners would then participate just to get off-site. The commission encouraged researchers to develop "alternative populations" for Phase 1 trials (National Commission 1976b, 11). Although the commission recognized that FDA regulatory requirements triggered pharmacologists' involvement in prisoner research, they provided no guidance as to how the mandates would be met once prisoners were no longer eligible.

Regulatory requirements were a side issue for the ARC, which had far more at stake. Scientific associations flooded the commission with letters of support. Rather than write to the commission chair, Martin appealed directly to fellow

pharmacologist and commissioner Joseph V. Brady several times in late 1975. In a letter of September 3, 1975, Martin tried to reframe the ACLU's interpretation of individual civil liberties, warning, "Those who wish to preserve liberty and freedom of the individual by curtailing therapeutic research on psychopaths may find that their efforts will have quite the opposite long-term impact." Pointing out how the "disease process" of "prisoner psychopaths" overtaxed national resources by increasing mortality and morbidity from alcoholism and addiction, Martin warned that repressive laws were likely to be put into place if further research on the disease of addiction and its treatment was hindered. Emphasizing the therapeutic nature of research, Martin urged "sensible reform," rather than outright abolition.[26]

Martin wrote another letter to Brady on December 5, 1975, listing specific ways that skilled clinical investigators could "minimize harm" to participants: "Our society should recognize that participation in human experimentation is an altruistic act for the good of society and should be rewarded as other socially constructive acts." Reframing the question of whether prisoners were capable of informed consent, Martin replayed the ongoing discussion as to whether naive or drug-wise subjects were best for research. Not surprisingly, he favored knowledgeable subjects: "It is my opinion that narcotic addicts who have been the major participants in drug studies of the Addiction Research Center probably give a more knowledgeable consent than do most other patients. From their practical experience they have much more knowledge about what the drugs will do than most other subjects and they understand much of the pharmacologic jargon." No longer referring to subjects as "postaddicts," Martin played on public fears of "addict recidivists," calling them the best research tools because they were uninterested in rehabilitation or perhaps even beyond its reach. Commenting on the idea of finding alternative populations, he wrote, "I cannot think of another population of participants in which the potentiality of inducing an increase in or worsening drug-using behavior would be less."[27] Martin predicted that without studies on addicts, pharmaceutical companies would be freer to place new, uncontrolled drugs on the market, thereby increasing the magnitude of the drug abuse problem.

Dense and closely argued, Martin's letters conveyed a tone of desperation. On January 9, 1976, he traveled to a public hearing held by the National Commission for the Protection of Human Subjects, where he warned that ending prisoner research would "retard development of therapy for addicts and . . . prohibit the evaluation of the addictive properties of analgesics." Martin portrayed participation in the ARC studies as individually beneficial, arguing it

was a "safe and constructive experience" that "often improves health" and serves as a "source of pride." Although he agreed that practical measures could reduce the "seductiveness" of the research environment and so reduce subtle coercion, he felt that prisoners and nonprisoners were "equally knowledgeable" about the conduct of research. Citing evidence that prisoners made "informed judgments," Martin stated that prisoner participants should be compensated for their altruism as well as in cases of harm (National Commission 1976b, 45). His position was not paternalistic, and he did not portray prisoners as vulnerable or exploited. Instead, he represented them as adults who could make their own decisions, despite the prison setting.

The spring of 1976 brought a chorus of similar letters from pharmacologists' professional associations, including the Committee on Problems of Drug Dependence (CPDD), the American College of Neuropsychopharmacology, the American Society for Clinical Pharmacology and Therapeutics, and the American Society for Pharmacology and Experimental Therapeutics. Modeled on each other, these associations targeted the chair of the National Commission for the Protection of Human Subjects, Kenneth John Ryan, MD, chair of obstetrics and gynecology at Harvard University. For example, Eddie Leong Way, chair of the Department of Pharmacology of the University of California at San Francisco, stated in a letter dated March 24, 1976, that the phaseout of the ARC was a "devastating blow to progress." Praising the ARC as the "best facility in existence in the world for conducting drug abuse research," he claimed that "studies there have abided by every ethical principle established with respect to human subjects."[28] Given the high public visibility of drug abuse, pharmacologists cast themselves as apprehensive that the bulwark or backbone of drug abuse research was to be eliminated.

The National Academy of Sciences relayed a letter from the chairman of the CPDD, Leo Hollister, who wrote on April 9, 1976, that closing the ARC would present a "major handicap" to the fight against drug abuse.[29] Hollister defended the scientific value and reputation of the ARC, placing the very honor of pharmacology at stake in this struggle. In a letter dated March 26, 1976, Keith F. Killam, Jr., president of the honorary society the American College of Neuropsychopharmacology, depicted the ARC as a "model coupling excellence of research with impeccable regard for the welfare of the subjects."[30] His letter detailed the workings of the CPDD, pointing out that "in practice, the CPDD has acted as a buffer between pharmaceutical companies proposing new narcotic analgesics and those who evaluate them for dependence liability." He argued, "Without the facility at Lexington, this valuable program, which pro-

tects the public against the commercial introduction of new drugs with high abuse potential, would be completely devastated." Yet this important argument for protection of the public good was buried in a paragraph on his letter's second page, where it garnered little attention.

These letters were responsible for the decision of the National Commission for the Protection of Human Subjects not to ban biomedical and behavioral research in U.S. prisons. Some members still favored a ban but agreed to allow prisoner research if strict guidelines were met: adequate living conditions, separation of research participation from parole considerations, effective grievance procedures, public scrutiny, the significance or importance of the research, compelling reasons to involve prisoners, and overall fairness (National Commission 1976b, 13). These restrictions would not necessarily have tolled the death knell for prisoner research, for the commission recognized that "in some cases research in prisons is going to be necessary" (Cohn 1976; this article by *Washington Post* reporter Victor Cohn showcased the commission's dismay at the BOP undercutting its authority).

Not wanting to hamper the production of data useful to its reform agenda, the commission encouraged sociological and psychological research into the effects of incarceration or prison conditions if it posed minimal risks. Careful to guard against discrimination resulting from withholding treatment that would directly benefit individual prisoners, the commission distinguished between biomedical research that was related to individual health and well-being and research that was "unrelated to the health or well-being of prisoner-participants" (National Commission 1976b, 15). Interestingly, the ARC served as the commission's prime example of research that was considered "unrelated" to prisoner health and well-being (National Commission 1976b, 23). The commission was equivocal about whether developing new addiction treatments or investigating the nature and causes of addiction to narcotics or alcohol abuse was individually beneficial or not (National Commission 1976b, 26). The lack of clear benefit to the individual, rather than to a class of people, was viewed as problematic given the calculus of risk and benefit put into play at that time.

To document the scope of prisoner research, the commission asked prison administrators and pharmaceutical companies how much drug testing they did on prisoner volunteers. Only sixteen out of fifty-one companies admitted relying on prisoners, yet prisoners still comprised between 85 and 90 percent of subjects in Phase 1 trials overall (Adams and Cowan 1971; National Commission 1976b, 47). Thirty-six hundred prisoners were then participating in one

hundred protocols studying seventy-one substances (National Commission 1976b, 31). When the commission made four site visits to prisons where testing occurred, Lexington was not among them. They visited what was then the largest penitentiary in the country, the State Prison of Southern Michigan in Jackson, Michigan, where Upjohn and Parke-Davis, two Michigan-based pharmaceutical companies, had built a research facility on the prison grounds. Prior to the FDA's promulgation of the 1962 amendments, Upjohn had proposed a dedicated facility to the Michigan Corrections Commission; Parke-Davis had gotten involved once the FDA regulations went into effect.[31] For meeting the new FDA requirements, these companies had access to a research pool of about eight hundred subjects who met eligibility criteria (including having an IQ over seventy). The National Commission for the Protection of Human Subjects interviewed eighty Jackson inmates, both participants and nonparticipants, and found that participants "valued the research opportunity" and that nonparticipants did not mind others taking up the opportunity but preferred not to do so themselves (National Commission 1976b, 35).

Prisoners at Jackson apparently valued participating in pharmaceutical drug research. One prisoner participant told Robert J. Levine, a consultant on ethics:

> You tell us you've come here to protect us from the risks of research. But one thing we've noticed is that when you are in those research units, you don't die. . . . But at any moment in the prison yard you could be killed by a fellow inmate for no reason at all that you can identify. You die out here in the yard. If you want to keep us out here in the yard, you are not protecting us. You don't seem to understand that we are living in a place where random death is a way of life. We have noticed that the only place where people don't die here is the research unit. Just what is it you think you're protecting us from? (Levine 1981, 73)

When FDA regulations barring prisoner research were issued in 1980, a group of Jackson prisoners brought suit, and Upjohn soon joined them.[32] Although settled out of court when the FDA issued the indefinite stay of its guidelines that remains in effect, the lawsuit marked some resistance to the de facto moratorium brought about by the correctional community. Even this kind of support from unlikely allies—subjects themselves—could not revive the flagging credibility of the addiction research community. The moral authority of science had been deeply challenged; scientists had been unable to make a politically compelling public case against the charge that they were exploiting prisoners. Disserved by their very modesty, the scientific experts were met by a

politicized counterexpertise that contested their motivations, interests, ethics, and findings.

"HUMAN GUINEA PIGS": LIABILITY OF A DIFFERENT KIND

There have been surprisingly few public representations of the ARC's scientific work or the ethical dilemmas crystallized by its laboratory logics. Journalists often devote an obligatory paragraph to condemning research at Lexington, detailing the exploitation of vulnerable subjects at the hands of caricatured "mad scientists." Tarring the ARC with the brush of guilt by association, these accounts link the ARC to the far less systematic drug research conducted by military, law enforcement, and intelligence organizations, notably the CIA, the Army Chemical Corps, and the forerunner of the Drug Enforcement Administration, the Federal Bureau of Narcotics (FBN). Consider this sarcasm:

> One of the first MKULTRA studies conducted was at the National Institute of Mental Health Addiction Research Center in Lexington, Kentucky. At the time it was working hand in hand with the CIA to test and develop new, mind-altering drugs. Young patients, usually drug addicts serving various sentences for drug violations, were offered a chance to volunteer as guinea pigs in exchange for the drug of their addiction. Naturally, the CIA got inundated with eager volunteers jumping at this wonderful opportunity to get free drugs while they were in prison. Each was given a physical examination, administered one of eight hundred or so hallucinogenic drugs, and observed for a few days. They were then given heroin, morphine, or anything else they wanted as payment for their participation. (Goliszek 2003, 158)

Not only does the paragraph just quoted suggest that the CIA ran the ARC, that both organizations developed drugs, and that participants got free rein to decide their rewards, but it fails to acknowledge the ARC's practice of limiting subjects to drug categories with which they had prior experience. It implies that rather than using informed, seasoned felons as participants, the ARC exploited vulnerable youth who would do anything to get drugs.

The Church committee hearings were the source of some of the sensationalistic claims in the paragraph just quoted. Such unsubstantiated and undocumented claims often reappear almost verbatim when the ARC comes up in the hearing documents. Witnesses in the Church hearings described test subjects as "volunteer prisoners who, after taking a brief physical examination and signing a general consent form, were administered hallucinogenic drugs." They explained, "As a reward for participation in the program, the addicts were pro-

vided with the drug of their addiction." Yet the next sentence in the hearing documents concerned FBN agents surreptitiously administering LSD to "unwitting nonvolunteer subjects in normal life settings by undercover officials," without concern for dosage or controls (U.S. Congress 1975, 391). Readers of the Church hearing documents could easily mistake all drug research as unethical, rather than sorting out the implications of the very different enterprises in which the FBN and the ARC were engaged.

During the period when the MKULTRA studies were conducted throughout the country, the ARC studied LSD-25 to determine its usefulness as a temporary or "model" psychosis. Well into the 1970s,[33] the ARC studied LSD to discover whether "bad trips" could be cushioned or curtailed by tranquilizers, reserpine, or chlorpromazine and to discuss how its effects compared to psilocybin or mescaline. The laboratory logics of the ARC stood in marked contrast to those of the military and intelligence community, which contracted or conducted LSD research in at least eighteen other sites. However one might define "science," what the CIA and the FBN were doing with hallucinogens was not it. The FBN's drug-testing activities raised "serious questions of command and control within the Bureau," according to the Church committee (U.S. Congress 1975, 421–22). One hesitates to apply the term *studies* to such informal, nonsystematic, and unethical activities.

My initial exposure to the testing program at Lexington came in the form of investigations laced with conspiracy theories, which elided distinctions, erased nuance, and characterized the work in ways that led readers to misinterpret the work of the ARC as science run amok. *The Search for the Manchurian Candidate: The CIA and Mind Control* (1979), by investigative journalist John Marks, delivers a damning judgment for Harris Isbell.

> As Director of the Addiction Research Center at the huge Federal drug hospital in Lexington, Kentucky, he had access to a literally captive population. Inmates heard on the grapevine that if they volunteered for Isbell's program, they would be rewarded either in the drug of their choice or in time off from their sentences. Most of the addicts chose drugs—usually heroin or morphine of a purity seldom seen on the street. The subjects signed an approval form, but they were not told the names of the experimental drugs or the probable effects. This mattered little, since the "volunteers" probably would have granted their informed consent to virtually anything to get hard drugs. (66–67)

Upon first reading this description from Marks, I failed to notice the slippage between witting and unwitting subjects, ethical and unethical practices, and scientific laboratory logics and the amateurish enthusiasms of law enforce-

ment. Representing the ARC studies as "quick and dirty," Marks made it seem as if subjects were in unlimited supply and thus disposable, when few were in fact eligible. Highlighting one "chilling" and "astonishing" study, Marks wrote, "To Dr. Isbell, it was just another experiment, for his 'intense curiosity' and 'relish for the task' shone through his 'dull scientific reports.'" Marks continued: "No corresponding feeling shone through for the inmates, however. In [Isbell's] few recorded personal comments, he complained that his subjects tended to be afraid of the doctors and were not as open in describing their experiences as the experimenters would have wished" (68–69). This description stands in stark contrast to how my interviewees characterized Isbell, who was frequently called on to handle complex cases and had a reputation for being particularly compassionate toward addicted persons (Kornetsky 2003a, 2003b; Mansky 2006).

Testifying before the Kennedy subcommittee in 1975, Isbell described a drug payoff system that had been in place from the institution's founding days (he had first come to Lexington as an intern in 1934–35). "The ethical codes were not so highly developed," he said. Marks made much of this when he located Flowers, one of the ex-inmate research subjects who had testified in the Kennedy hearings. Marks outlined a point system that Flowers claimed had been used to determine drug payoffs: "All he had to do was knock on a little window down the hall. This was the drug bank. The man in charge kept a list of the amount of the hard drug each inmate had in his account" (Marks 1979, 68–69). There is little archival evidence to substantiate the workings of the "drug bank," and my interviewees flatly deny the existence of any payoff scheme during their tenure at Lexington. The drug bank was discontinued in 1955 when the government authorized the ARC to make cash payments as long as they did not exceed what inmates could make when working elsewhere in the institution. Such practices certainly did not exist by 1963, the earliest year for which I can triangulate using multiple interviews of proven veracity.

Investigatory accounts leave open important unanswered questions about the long-term effects of drug exposure, liability, and the need for lifelong aftercare in cases of government experimentation. Unfortunately, such questions were not opened by the social network of prison reform advocates, who were new to the problems of abuse liability testing and pharmaceutical trials. Meanwhile, the social network of addiction researchers who had long labored to find a nonaddictive painkiller became acquainted with questions of legal liability and ethical responsibility. In the clash between politics and scientific expertise

that was the prisoner research debate, scientists, lawyers, and prison advocates evidenced little resonance with one another's animating motivations, commitments, goals, or tactics. The National Commission for the Protection of Human Subjects was supposed to resolve this clash but instead left it in limbo.

UNSETTLED ACCOUNTS: THE NATIONAL COMMISSION FOR THE PROTECTION OF HUMAN SUBJECTS OF BIOMEDICAL AND BEHAVIORAL RESEARCH

As a federal intramural research program, the ARC was considered a liability not only because of its geographic isolation in contrast to the other intramural programs but because of the nature of its facility and its subjects. In a report dated April 30, 1976, on NIH intramural research programs, the President's Biomedical Research Panel (under DHEW) offered proposals to overcome both problems.[34] The panel mentioned Lexington only to advise strengthening its clinical and laboratory facilities.

> Location of the intramural program of the NIMH amidst the intramural programs of the NIH appears to have been significant for mutual enrichment. The intramural programs of the other Institutes of the ADAMHA [Alcohol, Drug Abuse, and Mental Health Administration] have not faired [*sic*] as well. The intramural program of the NIDA, located at the Addiction Research Center in Lexington, Kentucky, and the intramural program of the NIAAA [National Institute on Alcohol Abuse and Alcoholism], located at St. Elizabeth's Hospital in Washington, DC, have been relatively isolated and have. . . suffered from inadequate facilities.

The panel advised improving and enlarging NIDA's intramural research program and physically relocating it to the NIH campus (U.S. Department of Health, Education, and Welfare 1976, 31).

In the spring of 1976, William Pollin, director of NIDA's Division of Research, appointed a blue-ribbon subcommittee of the National Advisory Council on Drug Abuse. Headed by eminent Harvard behavioral pharmacologist Peter B. Dews (whose work is discussed in the next chapter of the present book), the subcommittee's main task was reviewing the ARC program and considering a proposal by the Alcohol, Drug Abuse, and Mental Health Administration to combine separate research facilities at one, unnamed location. The NIDA Advisory Council Task Force on Intramural Research met on June 11, 1976.[35] In its hands was a new report from the National Commission for the Protection of Human Subjects on its May 3, 1976, site visit to Lexington.

The report included the operations manual of the Organizational Review Committee at Lexington and illustrated divergent perspectives between researchers and their subjects.

According to the commission, the ARC did not deserve the charge that "administrative convenience" led to the choice of prisoners as research subjects.

> The decision to use prisoners who are ex-addicts for these was not a matter of chance or convenience; rather they were selected because experienced addicts were considered to be the best reporters of the subjective effects of new drugs in comparison with narcotics, and best able to understand what administration of these drugs meant in order to give informed consent. Non-addicts were considered unacceptable for tests involving administration of narcotics. (National Commission 1976a, 4)

The commission explained that ARC researchers considered it ethically acceptable to use "hard-core addicts" with a documented relapse history so that "they are not doing anything to the subjects that the prisoners wouldn't do to themselves if they had a chance" (National Commission 1976a, 5). Although conducted after the BOP stopped transferring prisoners to the ARC, the site visit included interviews with each of the remaining sixteen prisoner volunteers, and the report characterized prevailing perceptions and beliefs of participants. Most of these prisoners had transferred to the ARC to be closer to family members or out of the mistaken belief that participating would positively affect parole eligibility. The commission reported that this belief, "[p]assed by word of mouth in the prisons of origin," was supported not by statistical evidence but by "knowledge of particular men who made parole after returning from the ARC" and was "fed by the men's conviction that participation in research is considered to be a socially beneficial thing to do" (National Commission 1976a, 10). The commission staff attributed the circulation of this mistaken belief not to the ARC but to the prisoner culture.

Participants indicated that they selectively avoided agreeing to studies of drugs or routes of administration they did not like (National Commission 1976a, 11). Many reported having withdrawn from studies without interference from researchers, although they believed that if they withdrew too often, they would be removed from the ARC. Despite being unable to identify any actual cases where refusal to participate had resulted in removal, they argued, "If we all refused, you know they would not keep us here" (National Commission 1976a, 12). This belief led the commission to conclude there was a "significant

coercive element in the unit" since "the same people who are responsible for the research are also responsible for decisions regarding the circumstances of the men's incarceration at the ARC, and that decisions to remove a prisoner from the ARC are viewed by prisoners as having a negative impact on chances for parole." Because researchers—not "distant prison officials"—were responsible for discipline, prisoners were "unable to ignore completely the fact of this power when they are asked to participate in a particular study" (National Commission 1976a, 13).

Although commissioners were unable to identify any complaints about particular studies, they turned up four general complaints. The first of these negatively compared the amount of money participants could make at the ARC to what prisoners thought they could have made at their "prison of origin" (National Commission 1976a, 14). The second and third were closely related concerns about the lack of education, training, entertainment, recreation, and therapeutic programs at the ARC as compared to the "model prison," or Federal Correctional Institution (FCI), next door.[36] Implementing NARA had effectively "unlocked" the main entrance to Lexington and brought in "800 male and female young offenders, primarily serving short terms for relatively minor offenses," setting up a situation in which the ARC was perceived as unfairly restrictive relative to the rest of the institution. The commission report pointed out: "The FCI next to the ARC is a facility run for a very different population, so much so that during daylight hours, there is no locked door between many inmates there and the outside world. To allow the ARC greater access to the FCI would require special security for which funds are not available, and which would be counter to the concept of the FCI" (National Commission 1976a, 15). ARC researchers were concerned that their subjects might gain access to contraband drugs through contact with individuals at the FCI. This would not only compromise the scientific value of their studies but pose to participants a risk that the researchers could not control.

A fourth general complaint was the failure to provide aftercare or follow-up to former participants. Couched within a legal climate of increased liability and litigiousness, this concern took on new weight in relation to studies on hallucinogens that might cause "flashbacks" or have unknown effects (National Commission 1976a, 15). The commission staff conveyed the fears underlying participants' arguments for continuing aftercare, which researchers cynically attributed to inmates' desires to transfer to hospital facilities viewed as more desirable than prisons (National Commission 1976a, 16). Health concerns of subjects were sometimes viewed by researchers as faked leverage to gain privi-

leges. Although conducted in the program's waning days, the site visit turned up differing perspectives that suggest research participants did not uncritically absorb but instead actively resisted or negotiated what they were told by those who studied them. This is significant given their construction as vulnerable and exploited, for it indicates that they retained at least some of the wherewithal to make informed decisions.

Although the National Commission for the Protection of Human Subjects had unusual powers due to its birth in the crucible of controversy, it did not use them to ban prisoner research. Something more confusing happened. Ordinarily, the recommendations of national commissions are not binding, but President Jimmy Carter's secretary of DHEW, Joseph Califano, was legally compelled to respond to this commission's recommendations, which set the bar high but left the door open to continued research in federal prisons. Califano tried to arrange an accreditation program through the American Correctional Association.[37] Recall that the ACA's Ad Hoc Committee on Medical Experimentation and Pharmaceutical Testing had not produced a protocol until 1972 and had reversed its stance in February 1976, when its newly approved "Position Statement on the Use of Prisoners and Detainees as Subjects of Human Experimentation" stated that a prisoner was "incapable of volunteering as a human subject without hope of reward" (Harkness 1999, 289). After that, the ACA denied accreditation altogether for facilities that conducted research. Blocked by this move, Califano tried to broker a deal with the FDA to issue uniform rules in 1980. These were stayed as part of the Jackson prisoners' suit.

Still unsettled, the decision lay neither in the hands of the commission nor in the hands of scientists. There were powerful research proponents at NIH, where director Robert Q. Marsden had established the Study Group for Review of Policies on Protection of Human Subjects in Biomedical Research in 1973, when the issue heated up. The three-member Subcommittee on Prison Volunteers in Biomedical Research included one of the most famous public figures in pharmacology, Frances O. Kelsey (Stephens and Brynner 2001). After considering an outright ban, the subcommittee instead opposed one in March 1973, and the full study group accepted the subcommittee's conclusion (Harkness 1996, 267). But none of the arguments advanced by the expert community achieved the public visibility necessary to reassure those worried about whether addiction researchers should be allowed to experiment as they saw fit on vulnerable subjects.

Despite many compelling examples of their regard for the humanity of

their prisoner patients, the ARC researchers simply could not see them as vulnerable subjects. The researchers' indigenous morality predisposed them to see prisoners as capable of making reasonable decisions about participation. They saw subjects as seasoned, drug-wise, and possessing knowledge about drug experiences outside the laboratory that were far riskier than those that were ever conducted inside it. The researchers' perspective meshed poorly with the emerging national consensus. There was simply no public discourse available to make sense of the science and politics of the ARC in the face of a critical mass mobilized against prison research on human subjects. The reform discourse positioned prisoners as the unlikely heroes and surprising beneficiaries of a new degree of public compassion. Researchers I interviewed depicted scientists as the victims of this formative moment. Divergent perspectives are to be expected as long as experts see themselves as speaking for others, rather than collaboratively producing expertise better tailored to the social contexts in which it is generated and used. Social distance between researchers and their subjects has only widened since the closure of the ARC at Lexington.

THE DEMISE OF THE "GOLDEN YEARS": LAMENTING THE LOSS OF LEXINGTON

Arriving fresh from Avram Goldstein's high-status neuropharmacology laboratory at Stanford University,[38] Tsung-Ping Su was called into Bill Martin's office in 1976. The ARC was about to shut down due to the ACLU's attempts to end prisoner research, Martin explained. Su laments:

> The glory of Lexington was that from a description of human pathology could come a pharmacological hypothesis. Lexington was a network of researchers asking very basic questions all the time, totally protected from political factors in ways that allowed us to be free to explore anything. That period has influenced me so much that I always try to link the molecular to the global picture, the global addictive process. (2004)

Martin himself was on the cusp of retiring from the PHS, so it fell to Jasinski to cultivate alternative populations. Jasinski later reported: "There was no way we could do human research in the middle of a prison in Kentucky. Lexington was a relatively small town at that time. You didn't have a huge addict population and there would be a lot of hue and cry if we tried to import addicts into Lexington." Jasinski noted that it was due to the "great hue and cry about clinical research" that the ARC did not relocate to Washington, D.C., or Bethesda.

"The NIH campus didn't have any room, and, secondly, the NIH people didn't want to bring addicts and alcoholics into Bethesda," Jasinski recalled, continuing: "At that time, the deputy director of the division of research was a guy named Dick Belleville, who had started out at Lexington. He was a psychologist who did a lot of the original work on drug reinforcement and conditioning. Dick had gone to NASA [where] he was the project officer on the first monkey in space, . . . and [was friends with] the guy who had been responsible for that, Joe Brady, who was at Hopkins" (2003). Sitting on both the NIDA Advisory Council and the National Commission for the Protection of Human Subjects, Brady prepared the ground for the move to Hopkins.

Several false starts later, Jasinski and a handful of researchers moved to Baltimore on July 1, 1979. There was disagreement within NIDA about whether it was desirable to create and maintain intramural research. The move refocused research priorities: there emerged a new emphasis on the clinical pharmacology of nicotine, which had been underexplored when the ARC was rooted in the heart of tobacco country, where everyone received a standard-issue glass ashtray. Researchers reinvented protocols to accomplish studies with free-living volunteers. Abuse liability studies were no longer the ARC's bread and butter, due to lack of support within NIDA for spending research dollars to benefit private industry. The ARC was absorbed into a vast, decentralized, extramural addiction research enterprise.

Fresh from lobbying for drug policy to stay in the White House, Robert DuPont, first director of NIDA, had instead presided over the devolution of responsibility for the nation's drug abuse treatment and prevention capacity (Musto and Korsmeyer 2002, 153). Back in the throes of the "hue and cry," the ARC had commemorated its fortieth anniversary at Lexington on June 9–11, 1975. DuPont sent Robert C. Petersen, then assistant director of the Division of Research for NIDA, to acknowledge the unit's scientific contributions. At the time, Petersen was gearing up to run the extramural funding mechanism. Although he lauded the ARC cadre, who he designated "a brilliant handful of scientists" who had demonstrated the relevance of science to social problems, he also called for greater balance between basic and applied research than that exhibited by the ARC. He characterized the research unit's commitment to basic research as "what comes most easily or comfortably."

> Before we become too self-congratulatory while wearing our researchers' hats, we might also remember that we too have a tendency to do what comes most easily or comfortably. It is often easier to limit oneself to basic research or to well-controlled laboratory settings than to attempt to cope with the messy

> problems of the real world. . . . Too often the most gifted researchers have scorned practical problems as either inappropriate or poorly suited to the well controlled research conditions their career contingencies lead them to prefer. Nevertheless, it is people who abuse drugs, and not rats. And drug abuse must be prevented not in laboratory animals but in people living in a complex world. (DuPont 1978, 263–64)

DuPont's reminder that it is people, not rats, who abuse drugs, can be taken as indicative of the NIDA leadership's need to distance the new agency from a potential political liability at a time when animal and prisoner research had both come under attack.[39] Although there was clearly continued respect for the ARC among the scientific community, the science side of NIDA was at times portrayed as a mafia by those concerned with education, treatment, prevention, and evaluation research.

Despite the venerable ARC's foundational work, NIDA portrays substance abuse research as moving from infancy to adolescence in the compressed time frame of the mid-1970s. From September 1973 to this day, NIDA estimates it has coordinated 85 percent of the world's scientific research on drug abuse. During this time, degrees of respect for the intramural program have fluctuated.[40] When at Lexington, the ARC had a great deal of autonomy in its research trajectory in coordination with the CPDD. By contrast, NIDA had to be extremely responsive to national priorities, legislative mandates, and external pressures, such as the emerging parents' movement. The agency concentrated on large-scale quantitative surveys, such as the household survey; the emergency-room-based Drug Abuse Warning Network; a treatment unit survey; and, beginning in 1975, the national high school survey "Monitoring the Future," implemented by the University of Michigan. NIDA was national in scope, tightly bound to policy, and interested in epidemiological and applied research. It had less use for the hybrid kind of "basic" and clinical research undertaken by the ARC.[41] Although many within NIDA were aware of the proud tradition of the ARC and its scientific preeminence, this legacy was portrayed as both an asset and a liability after the ARC moved to the site of the old Baltimore City Hospital in the summer of 1979. Animal research continued at Lexington until the early 1980s, when the unit reconsolidated at the Baltimore campus (then called the Francis Scott Key Medical Center and now known as the Bayview Medical Center at Johns Hopkins University). The ARC morphed into NIDA's Intramural Research Program, the official name given to the program under Alan Leshner in the 1990s.

One casualty of the move to Baltimore was the intensity of focus achieved

through daily interaction about the conceptual basis of drug addiction. As ARC researcher Charles Gorodetzky describes, the ARC had the ideal "low walls" of problem-based, interdisciplinary knowledge formation.

> I always felt that the whole problem of drug abuse, the whole concept was a multidisciplinary issue. It took in everything from sociology to molecules. . . . [The directors] felt that we should span the entire gamut. My own feeling was that it was the ideal bridge between pharmacology, basic science, and medicine, which was one of the things that attracted me to the field. One of the beauties of the ARC was that interdisciplinary research was so easy to do. (2003)

Despite this, the hierarchy of credibility within the institution owed something to disciplinary formation. Tensions between social science and basic research seem to have increased over time; my interviewees attributed these to ideological differences. Some sociologists clearly felt pressured by "bench scientists," as did John Ball, who says he left Lexington because his publications were delayed or censored by "biomedical scientists believing that social science is not scientific enough" (Ball 2002, 31–32).

Contrary to the image of unproductive government scientists, those whose laboratory lives began at the ARC recalled their time at Lexington as highly productive. They were shielded from administration, fund-raising, and advocacy, and they inhabited a research culture that was completely devoted to the problem at hand. Gorodetzky recalls:

> [T]here was some sadness that what we knew had been really golden years of research. There were times when Don and I used to agitate with Bill [Martin], "Let us go up to Washington more with you, we want to mix with those folks" and he said, "Forget it, stay where you are, you don't know how good you've got it, stay a thousand miles away." He was right. We were unbothered, we were not really bogged down with all the kinds of committee work and administrative work that we would have had if we were at headquarters. . . . [and] we were a very, very productive group. (2003)

The loss of the prisoner research program meant that researchers had to adapt to outpatient populations, which changed the nature of their scientific inquiry. "[B]ecause we had prisoners who were institutionalized, and the government was already paying for their upkeep, we could do more complex and long-term studies that you couldn't do in other types of units" (Jasinski 2003).

The research suffered when researchers lost daily contact with subjects. Of his time at the Lexington ARC in its heyday, Conan Kornetsky remarked: "We

knew all the subjects by name, we interacted with them daily. We drank coffee with them. We were concerned about their pain" (personal communication with the author, August 3, 2006). With the Lexington ARC's shutdown, close observation, once central to this basic research facility, was displaced. The interactive process of interpreting clinical phenomena, inner sensations, and subjective effects was gone. With the exception of profoundly literate artists who have translated their inner sensations into communicable form, the words of drug users have rarely been listened to, recorded, or taken as seriously by anyone as they were at the ARC. This irony was apparent to Charles R. "Bob" Schuster, a NIDA director who recognized Lexington's value: "I understand [the ethical concerns that ended the Lexington research] but I really do believe that the research that came out of Lexington was some of the most important research on addiction and it could not have taken place anywhere else" (2004). The pharmacist who compounded medications at the ARC, Rolley E. "Ed" Johnson, felt, like other Lexington veterans, that the move to "protect" prisoners took something away from them.

> I think it's appropriate to make sure you have a prisoner advocate on an IRB [institutional review board], but to just say outright that no prisoner should be allowed to volunteer is taking away a right instead of protecting a right. There are different sides to the question. How free are you to volunteer? How much coercion or how much seduction can be used? You can look at those private rooms, or other things that subjects got, and say "that's seduction." Nobody put a gun to anybody's head and said do it, but if you knew you were going to get a private room, even though it may only be three feet wide and six feet long, but you're not sharing it with four people, that certainly could be looked at as seduction. If you have a private TV and can watch any station you want instead of what everybody else is watching, that certainly can be looked at as seduction. There's seduction in everything we do if you stop and think about it. In the real marketplace you're trying to seduce people into buying what you've got all the time. It's hard to reconcile that in a prison situation, so the easiest thing was to just say they can't do it. (2005)

The last drug-addicted prisoner was transferred out of the ARC on December 31, 1976. By then, the broader institution had become the ordinary prison that James Bennett, Walter Treadway, and Lawrence Kolb had insisted it never become. Portrayed as the "guys who did research on prisoners," ARC researchers faced the dispiriting loss of their laboratory at Lexington, which created a vacuum into which the new science of behavioral pharmacology stepped with a new set of laboratory logics.

MAKING SENSE OF THE DEMISE OF PHARMACOLOGICAL TESTING AND PRISONER RESEARCH

Resistance to the emerging form of the new human subjects regime issued from pharmaceutical manufacturers, scientists, clinicians, and even prisoners themselves, some of the last of whom protested the removal of their rights to participate in research. Social scientists, legal scholars, and corrections professionals debated professional practices and ethical conduct. Civil rights organizations, consumer groups, the women's health movement, and, later, the HIV/AIDS movement engaged in concrete struggles for and against access to particular drugs and therapies. Yet philosophical abstractions predominate in public deliberation of complex ethicopolitical issues in the United States. In retrospect, the prison research episode was a struggle over whose expertise would prevail in a moment of political turmoil over the proper connections between science and governance.

A report dramatizing the ethical dilemmas of prison research appeared in 1980 to restage the struggle between proponents of continued prison research: namely, William R. Martin, still in Lexington but by then at the University of Kentucky; Robert J. Levine, chair of the Yale University Human Investigation Committee and consultant to the National Commission for the Protection of Human Subjects; Henry K. Beecher's onetime coauthor Louis Lasagna of the University of Rochester; and Alvin J. Bronstein, the ACLU National Prison Project's chief litigator. Commissioned by the Center for the Study of Drug Development at the University of Rochester, the report was authored by Seymour Shubin. It traced the contours of an epic clash between a certain kind of scientific expertise and a certain kind of politics, neither of which recognized the nuances of a socially situated ethics.

Research proponents barely gained a moral toehold against those arguing against continued federal prison research. Marshaling research-promoting arguments, Shubin framed the report with a loaded rhetorical question: "Should prisoners be permitted to volunteer as subjects in scrupulously-run, carefully-reviewed research projects that require their informed consent and permit them to withdraw at any time?" Scattered throughout the report were dire discursive flags: medical research in prisons was said to be in its "death throes," "killed" by DHEW and the FDA; its quiet death "without headlines" was expected to "cripple" pharmaceutical companies (Shubin 1980, 1–4). Shubin's report was structured to showcase research programs that did not deserve to die, instead of the "few widely publicized cases of thoughtless or unscrupu-

lous prison research" (1980, 4). It offered two exemplars: the Malaria Project at the Stateville Penitentiary in Joliet, Illinois, phased out in 1975; and the ARC in Lexington, Kentucky, phased out in 1976.

Casting Martin as an exemplary scientific proponent of prison research, Shubin depicted Bronstein as disrespecting the very prisoners whose rights he supposedly championed. Highlighting Levine's claim that most prison research was not unduly risky, Shubin cited actuarial calculations indicating that risk levels were as minimal as those of office secretaries—much lower than risks faced by window washers or coal miners (1980, 16). Minimization of harm and risk did not play well in the climate of the 1970s. However, what historical sense is to be made of the ethos at the ARC depends on some calculus of the risks to which participants were exposed. By limiting studies to small groups of subjects, the ARC reduced exposure to drugs known to be risky. They rarely exceeded one hundred subjects and did not randomize; nor did they often use placebos, which would have been recognized by the drug-wise subjects who comprised their pool of eligible subjects.

The indigenous morality by which ARC researchers worked calculated risk in the laboratory by comparing it to the much higher risks encountered by those who used illegal drugs of unknown provenance in unknown dosages on the streets. The ARC emphasized the lack of mortality and morbidity associated with its research, because dosage was precise and drug supplies unadulterated: "In over 40 years of using human subjects we had had no mortality at all and virtually no morbidity" (Shubin 1980, 5). Foregrounded was the indigenous morality against coercion or seduction at the ARC, a frame often echoed by my interviewees. Martin conceded that prisoners were more vulnerable to coercion and seduction because almost anything could be used as an incentive. He argued against using parole as the sole incentive if research were the only way a prisoner could shorten his sentence. But he contended:

> [I]f it's just one of many ways, I see nothing wrong with it. In fact, I think that by denying [prisoners] the opportunity to volunteer for research, we're denying them still another freedom. And I also think that if a prisoner wants to provide a very useful service to society, he shouldn't be denied it. (Quoted in Shubin 1980, 7)

Laying out a different ethical yardstick for prison research, Martin argued that captivity was itself an important safeguard: [I]f we were to make someone dependent on morphine or a new analgesic that turns out to be addicting, and the subject has the right to leave the unit any time he wished, he could of course

cause harm to himself and to other people. If he left the Center, say, and he was dependent on drugs and became sick and robbed a drugstore, I think the investigator would have to bear the responsibility. . . . I would never conduct an experiment in which I chronically administered a potentially addicting drug to a patient who could leave the setting at will" (quoted in Shubin 1980, 7). This statement replied to the pivotal question of whether or not clinical trials could be conducted on free-living volunteers, as maintained in the Katzenmeier hearings by John D. Arnold, once a proponent of prisoner research until he discovered he could gain participation from college students.

The existence of populations of free-living volunteers willing to participate in clinical trials was not just a matter of logistics but a scientific issue haunting the halls of Lexington. Were prisoners physiologically and psychologically similar to those living outside prison walls? Bronstein argued that differences invalidated prisoner research on scientific grounds: "There is a lot of data now which suggests that much of the testing that's been done on prisoners is not scientifically valid for free world people because of the physiological changes that take place in prisoners—blood changes, metabolism changes, as a result of the kind of life you are living in a closed institution" (quoted in Shubin 1980, 15). Portraying prisoners as unreliable witnesses, Bronstein said:

> We also know that prisoners are the greatest con artists in the world. They have to do it to survive—and they were doing it, many of them, before they got to prison. They are screwing around with placebos and things. We have documentation of some tests at Connecticut State Prison. We've got affidavits from prisoners that, with only 24 prisoners in the test, only two were taking the pills each week; the others were hiding them or getting rid of them. And the two shared their urine specimens for the week with the other 22. (Quoted in Shubin 1980, 15)

Kornetsky counters: "Bad research is done in non-prisons, as well as prisons. In fact, there has been badly designed human experimentation done and sponsored by some of our most prestigious universities. Generalizing from that example is logically absurd. It is obvious that the experiments were poorly designed and poorly supervised" (personal communication with the author, August 3, 2006). However, deciding if prisoners were physiologically or psychologically different from nonprisoners was a political question. By the early 1980s, the social stakes involved had changed due to the ACLU Prison Project, which was at heart a reform movement focused on prison research as a vehicle to change prison conditions and expand prisoners' civil liberties.

The sociology of bioethics that emerged in the 1970s lent credence to the prison reform movement by representing biomedical researchers as collectively conservative and resistant to scrutiny. In the book *Research on Human Subjects,* Bernard Barber explained:

> Just as powerful businessmen in the past resisted, and still do, the "encroachments" of governance on their autonomy and self-defined expertise, so the medical research community today feels itself beleaguered by an excessively intrusive general public and the government as well. Even small requests for change, for more effective self-regulation, are viewed by the "powerful profession" as fundamental threats. (Barber et al. 1979, xiv)

Soon after *Research on Human Subjects* initially appeared, the Tuskegee scandal broke, and Barber testified in the Kennedy hearings.[42] Because they emphasized systemwide effects, the sociologists saw themselves as separate from the bioethicists (Barber et al. 1979, xv). Still, the National Commission for the Protection of Human Subjects modeled its survey research on Barber's methodology and identified a similar overall pattern: ward and clinic patients were "differentially poor, less educated, and members of minority groups"; most likely to be involved in studies with the least favorable risk-benefit ratios; and less likely to be able to "give a free and informed consent" (Barber et al. 1979, viii). Contra Barber et al., the commission did not find that "children, women, minority, or low income persons were more likely than others to participate in projects that were above average in risk" (Cooke, Tannenbaum, and Gray 1977, ix).

Barber et al. identified a faulty assumption made by many biomedical researchers: "[I]n order for medical knowledge to grow, some people have to serve as subjects for risky but important research. These people, it is assumed, should rightly be the ward and clinic patients who receive their medical care either free or at reduced charge. In return for cheaper care they will provide the crucial ingredient for medical knowledge to grow" (1979, 57). The ethical problem of this assumption bears a precise resemblance to Foucault's insight into the implicit contract governing clinical research (quoted at the beginning of the present chapter). Overcoming this structural unfairness required the new social role of an "informed outsider" who could represent the stakes of current patient-subjects, future subjects, and researchers (Barber et al. 1979, 196). Informed outsiders would ideally know more than the man in the street about biomedical research, as well as understanding the laws, codes, and social norms relevant to effective and ethical use of human subjects and social-psychological techniques for discovering the feelings and values of representative publics.

> The uninformed man-in-the-street cannot know what "the community" thinks about catheterization of the heart, or the use of levodopa in the treatment of parkinsonianism, or the transplantation of vital organs, or the injection of live cancer cells into terminal cancer patients. The informed outsider would have access to the techniques and resources that could provide such information from the community. (Barber et al. 1979, 196)

The lack of informed intermediaries in the prison research debates was responsible for their discursive and political impoverishment. The moral compass of the time inscribed only two, starkly drawn positions: those who were for it and those who were against it.

Social location and social status played a role in determining positions within the debate. The ARC researchers held high social status within scientific communities close to them, because they were extremely competitive in terms of productivity and placement of their research. They enjoyed preeminent status in the pharmacological sciences and among psychiatrists and clinicians. Their institution was the mecca of substance abuse research. This position rendered them unused to explaining themselves, for they had received little external scrutiny or challenge from any quarter. Misunderstanding the playing field, they pitched their responses within the narrow parameters of the scientific terrain they sought to defend. If anything can be learned about the political contestation of knowledge and expertise issuing from this turbulent moment, it is that science and scientific research are inevitably political acts taking place on social terrain. They are as much about the performative enactment of ethical science as they are about the content of scientific claims or the logics of laboratories.

In conclusion, this chapter has sought not to defend the overall practice of prison research but to distinguish among widely varying research practices and laboratory logics that prevailed in different research settings. Under no circumstances would I now turn back the clock to the "bad old days" of federal prisoner research, for abundant unethical experimentation occurred both before and after the ARC was quietly phased out.[43] No one should be given free rein to conduct research in total institutions. However, the state of suspended animation that federal prison research entered in the early 1980s effectively did away with public scrutiny over private research conducted in state prisons. The issues raised in this chapter hence remain unresolved. While free-living volunteers are the human subjects of clinical trials and scientific studies today, this situation creates new risks that may someday be considered unethical.

Addiction research took new forms in the post-Lexington era, and the tra-

jectories it took remind us that a concept like "addiction" comes into being only through a confluence of several lines of collective thought (Fleck 1979, 23). Some of these will be privileged over others in ways that lead them to become the dominant frame that renders all contending systems of thought "alien."

> Whatever is known has always seemed systematic, proven, applicable, and evident to the knower. Every alien system of knowledge has likewise seemed contradictory, unproven, inapplicable, fanciful, or mystical. May not the time have come to assume a less egocentric, more general point of view and speak of comparative epistemology? (Fleck 1979, 22)

Leaving Lexington, the ARC lost some of its privilege to channel the conceptual confluence that is "addiction," and new streams of thought became dominant tributaries. The "great hue and cry" over prisoner research ultimately shut down the research program at Lexington but opened the floodgates to new laboratory logics for substance abuse research and new practices in the social organization of clinical trials.