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Discovering Addiction

Campbell, Nancy D.

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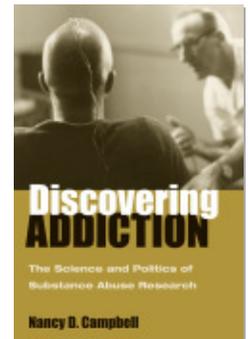
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CHAPTER 3

“A New Deal for the Drug Addict”: Addiction Research Moves to Lexington, Kentucky

Systematic research on the effects of narcotic addiction on human beings began at a federal penitentiary annex at Fort Leavenworth, Kansas, where several thousand drug addicts were housed in the U.S. Army Disciplinary Barracks. In 1930, the U.S. Public Health Service (PHS) was granted statutory authority to run medical services in U.S. prisons.¹ A medical detail was dispatched to Leavenworth the next year. There, a small biochemical laboratory was established to “determine the more exact nature of the chemico-physiological changes occurring in connection with drug tolerance and addiction” (NIMH 1971, 9). The ethical ambivalence lodged at the heart of this origin story was expressed in a caution by Assistant Surgeon General Walter L. Treadway.

It is not assumed that Federal prisoners should be used as experimental animals for the furtherance of medical knowledge. However, a large prison may be regarded as analogous to a laboratory, subject to control, where observations and scientific studies should be made possible. (1930, 8)

The injunction against using federal prisoners as “experimental animals” simultaneously gestured toward a level of social control over research subjects that was possible only in structurally coercive settings, and the urge to put captive populations to use in the production of knowledge.

The laboratory at Leavenworth came into being under the direction of a young PHS clinical investigator, Clifton K. Himmelsbach, whose work was coordinated by the NRC Committee on Drug Addiction (CDA), described in chapter 2.

[I]t immediately struck me that this was a very, very dependable kind of illness, that things happen almost by the clock. You could predict, you could just almost tell what was going to happen, when it was going to happen, and when it was going to fade. So I got the idea of reducing this to numbers of some sort and getting a picture of this illness. And I started with a 1, 2, 3, 4, plus, based on the appearance of signs that occurred, and this was the first feeble attempt to quantify the morphine abstinence syndrome. (Himmelsbach 1972, 8)

Himmelsbach developed a method to track withdrawal, which he called the “morphine abstinence syndrome” (1941). He found that all patent medicines marketed to “cure” addiction or relieve withdrawal symptoms were ineffective. His first scientific paper (1933) concerned high rates of malaria among addicts due to shared syringes. After Leavenworth, Himmelsbach conducted clinical trials of analgesics on cancer patients at Pondville Hospital in Massachusetts before moving to Lexington, Kentucky (Acker 2002, 86–89).

Laying the methodological foundation for the laboratory logic of substitution described in the previous chapter, Himmelsbach pioneered the prediction of a drug’s “addictive potential” by systematizing the progress of abstinence symptoms. If a “less addictive” candidate drug was administered to a chronic morphine user, the experimental subject could be observed for whether that drug alleviated or promoted withdrawal symptoms. This logic of substitution was applied in the unique circumstances of a laboratory that opened in 1935 under Himmelsbach’s direction in Lexington, Kentucky, on the rural, thousand-acre campus of a federal prison-hospital that served narcotic addicts who resided east of the Mississippi River.

“Narco” was one of two U.S. narcotics farms operated jointly by the PHS and the federal Bureau of Prisons (BOP), in Lexington, Kentucky, and Fort Worth, Texas. Congress changed the name from “U.S. Narcotic Farm” to “U.S. Public Health Service Hospital” soon after Lexington’s opening on May 29, 1935. The new name became effective on July 1, 1936, but the nickname “Narco” stuck. Designed as treatment hospitals to quarantine addicts far from urban temptations (Musto 1973/1999, 204–6), these hybrid prison-hospitals were presented to the public as a “New Deal for the drug addict” (Conhurst 1935, 1). The Porter Bill (1929), their enabling legislation, contained a research mandate pursued at Lexington from its opening in 1935 until 1979.² Construction of the Lexington facility cost \$3.6 million and was portrayed as an institutional solution to a social problem of national scope that crosscut racial, ethnic, and class divisions: “Dope plays no favorites and has no pet hunting grounds in this country, Government men say. Over 100,000 addicts are scattered up and down the

whole scale of life in fairly regular ratios regardless of color or race or economic position. They constitute a very real and solution-demanding problem” (Conhurst 1935, 1). This chapter brings back to life the larger institution of Lexington and the laboratory that brought into being the drug addict as a scientific subject and an object of knowledge.

Once settled in Lexington, Himmelsbach hired what became the core group of scientists in the addiction research enterprise, although World War II would split the first generation into two groups. The initial group consisted of ranking PHS officers, most of whom had worked in penal institutions where there was a high incidence of narcotics addiction. The first collaborative research team consisted of biophysicist Howard L. Andrews, who made one of the first electroencephalographs and was invited to Lexington to “find out what’s going in the brains of these addicts”; Ralph Brown; Robert H. Felix, who later became director of NIMH; Justin K. Fuller, previously chief medical officer at Leavenworth; Michael Pescor; biochemist Fred W. Oberst; surgeon William F. Ossenfort, who later became chief medical officer at the Atlanta Penitentiary; Victor H. Vogel, a clinical psychiatrist; and physiologist Edwin G. Williams.³ World War II took so many of the first group to Washington that when the second group began to arrive in the early 1940s, only fifteen staff remained in the research unit (Martin and Isbell 1978, 27). The second group included Harris Isbell, who did an internship at Lexington in the early 1940s and succeeded Himmelsbach as director in 1945; Abraham Wikler, who came as a psychiatry resident in 1940; Anna J. Eisenman, a chemist hired during the war and one of few women researchers there; biophysicist Karl Frank; H. Franklin Fraser, a clinical researcher; Harris Hill, Conan Kornetsky, and Richard Belleville in psychology and psychometrics; and William R. Martin, who became director of research in 1963, when Isbell, Fraser, and Wikler retired. In 1948, the research unit became the first basic research laboratory of the newly formed National Institute of Mental Health (NIMH) and was named the Addiction Research Center (ARC).

The history of substance abuse research is inextricable from the story of Lexington. Despite their geographic isolation—or perhaps because of it—researchers at Narco had access to a large pool of drug-experienced subjects from which they could select subjects who fit eligibility criteria. Constituted as an elite corps, they made formative conceptual contributions. Even their technical and custodial staff was handpicked, because Himmelsbach believed they had to be “thoroughly sold” on the idea that their work contributed to scientific progress if he was to achieve the levels of control he believed necessary

to the research process. Himmelsbach wanted “no one [to] ever come into the unit except [his] people”: “[They were] my eyes and ears twenty-four hours a day, seven days a week, 365 days a year, as long as we stayed open, and I was in constant touch with them, and they with me, by telephone, so that this was a continuous kind of controlled situation in which they felt perfectly free and comfortable with me and I with them” (1972, 15–17). This dream of perfect control and a total situation of round-the-clock observation were integral to the research ward at Lexington, which was the only laboratory in the world entirely devoted to the study of drug addiction. Bringing back to life the material, discursive, and organizational arrangements of this institution demonstrates how the social organization of knowledge production enabled rapid moves from clinical observation to testable hypotheses, making basic and even molecular mechanisms of addiction discernible.⁴

The central question driving addiction research was why individuals varied in the propensity to addiction and relapse. Haunted by variation in the “subjective” effects of drugs, researchers turned to animal models to produce “objective” accounts of the physiological mechanisms involved, including tolerance, addiction or dependence, withdrawal, and relapse. Yet human questions remained the driving force as the field came into being: Why were some individuals more or less susceptible to addiction than others? What accounted for high rates of relapse, which have been documented since the earliest days of institutional response? Did some people experience the pleasures of drugs or the pains of withdrawal differently from others? Did some experience the anticipation or effects of pain differently than others? Did drugs work differently in some people? To answer such questions in what they considered “objective” terms, the ARC relied on a steady stream of “research material”—including human subjects and nonhuman entities, such as animals and chemical compounds—around which institutional routines, metrics, and protocols were developed. The human subjects were supplied by the surrounding institution, the compounds by pharmaceutical companies via Nathan B. Eddy, the biological coordinator of the NRC/NAS Committee on Drug Addiction and Narcotics (CDAN).

Although the ARC was the only place in the United States where drugs were tested in human subjects, its paramount scientific goal was to understand the underlying neurophysiology of drug addiction. However, in the course of pursuing basic questions, the ARC provided a much-needed drug-testing service to the U.S. government, the pharmaceutical industry, the World Health Organization, and the United Nations. Small-sample clinical drug trials used exper-

rienced drug addicts to compare the abuse potential of new compounds. The roster of companies whose drugs were tested on human and animal subjects at Lexington by the mid-1950s includes Abbott Labs; Burroughs-Wellcome; Ciba; Endo; Hoffman-LaRoche; Lederle; Eli Lilly; Merck; Parke-Davis; Schering; Smith, Kline and French; Squibb; Upjohn; Winthrop-Sterling; Wyeth; and others. The ARC's evaluative capacity was crucial to the pharmaceutical industry, which lacked the infrastructure, research capacity, and techniques to determine whether its products were addictive or not. Industry submitted compounds to the NRC coordinating committee for administration at Lexington—there was only indirect contact through CDAN between researchers and industry representatives. Compounds tested at Lexington included many in regular use today: alcohol; barbiturates; buprenorphine; clonidine; codeine; cyclazocine; Demerol; Dilaudid; heroin; LSD; mescaline; methadone; nalorphine, naloxone, naltrexone, and other narcotic antagonists used to reverse opiate overdose; major and minor tranquilizers, such as Miltown and Equanil; sedative-hypnotics, such as Seconal; marijuana and delta-9-THC; and cough syrups. Buprenorphine, a pharmacotherapy for opiate addiction not FDA-approved until 2003, was pioneered at the ARC by Donald Jasinski, who first glimpsed its potential in the 1970s.

Postaddicts were the ARC's primary "research material," a role not unusual for U.S. prisoners even after the 1949 adoption of the Nuremberg Code (Rothman 1994, 62–63). Postaddicts were experienced drug users who detoxed on entry to the institution and whose sentences exceeded six months (no subject could be administered a drug within six months of release). Lexington housed several kinds of patients, among them a couple hundred neuropsychiatric patients who were not drug addicts, several hundred convicted felons with a history of drug taking, and "volunteers," addicts whose peers or relatives urged them into treatment but who were not serving sentences.⁵ People who voluntarily sought treatment at the Lexington Hospital never participated in research. They could not be held against their will, and nearly 70 percent signed out against medical advice (Rasor and Maddux 1966). By the late 1950s, administrators perceived voluntary patients as thorns in their sides, regarding women housed in the "Jenny Barn" as especially troublesome (Campbell 2000, 120–25).⁶ Women were not used in research, for they were considered "unreliable" subjects or worse, as the term *jenny* is a pejorative signifying a female donkey. Nor were young people, the mildly addicted, or the mentally ill considered to provide valid testimony necessary for research. Neither their words

nor, apparently, their bodies could be trusted, and thus they were saved from the exploitation to which seasoned male narcotic addicts were invited.

Implementation of a federal civil commitment program for narcotic addicts in 1967 relaxed security at Lexington, a topic explored further in chapters 5 and 6 of this book. After 1968, convicted felons who volunteered to participate in research were transferred to the ARC from federal penitentiaries elsewhere, and the research ward expanded its operation and heightened its security. Reluctant jailers, the ARC researchers were far more interested in science than security, yet they were caught holding the keys to the miniprison that was their laboratory in the scandal-saturated atmosphere of the 1970s. Born in the crucible of New York State’s murderous assault on Attica prisoners in 1971, the Stanford Prison Experiment of 1971, the coverage of the Tuskegee Study of Untreated Syphilis in the Negro Male in the summer of 1972, and Jessica Mitford’s 1973 book *Kind and Usual Punishment: The Prison Business*, the debate over prison research undid the ARC. Federal prison research ended in 1976, after the PHS had turned Lexington over entirely to the BOP, which cut off access to human subjects.⁷ The social context did not lend itself to careful understanding of what was actually happening in the laboratory life of Lexington, nor did it address who should organize, monitor, or pay for clinical trials of abuse liability once they became impossible to conduct at Lexington. Forced out of bluegrass country, the ARC was absorbed into the National Institute on Drug Abuse in 1974 and was renamed the Intramural Research Program in the 1990s.

INSTITUTING ADDICTION RESEARCH AT THE NARCOTICS FARM

The Lexington narcotics farm came out of the modern project to infuse penalty with a moral or rehabilitative mission. This new form of restraint—or “discipline,” as Foucault famously dubbed it—brought in a “whole army of technicians”: “warders, doctors, chaplains, psychiatrists, psychologists, educationalists.” Foucault wrote, “[B]y their very presence near the prisoner, they sing the praises that the law needs: they reassure it that the body and pain are not the ultimate objects of its punitive action” (1979, 11). Psychiatry occupied a central position within the proliferation of this swarm of subsidiary authorities through which the state extended its power. As the quote from Walter L. Treadway at the beginning of this chapter indicates, the institutionalizing urge

out of which Lexington came was propelled as much by the will to knowledge as by the attempt to reform prisons as sites for rehabilitation and vocational training.

The narcotics farms were the brainchild of two reformists—Treadway and James V. Bennett, an up-and-coming assistant director of the BOP who later directed the agency for a quarter century (J. Roberts 1996). Appointed to the Committee on Drug Addiction in 1929, Treadway shortly became head of the newly created Division of Mental Hygiene. From this position, he oversaw site selection, construction, and the opening of the narcotics farms. The sites were selected to support agricultural activities with “some degree of satisfaction or profit” (NIMH 1971, 3). The first director of the BOP, Sanford Bates, who held that position from 1930 to 1937, credited Treadway with educating the government about the “wisdom and importance of professionalizing the type of public service” involved in treatment of the “dependent and delinquent classes.”⁸ Bates emphasized that Lexington was established and run as a hospital, institutionalizing a new therapeutic approach to the management of drug addiction: “No matter who succeeds to its administration, it cannot ever become an old-time prison.”⁹

The administrative staff at Lexington referred to “patients,” rather than “inmates” or “prisoners.” They carried out rehabilitation and vocational therapy through such agricultural industries as farming and dairying and such prison industries as the “needle works” (for sewing prison uniforms and “going-home clothes”), a woodworking shop that manufactured chairs and office furniture for federal institutions, a laundry, a “microphotography” unit, and a print shop and book bindery. This hybrid prison-hospital delivered congregate care at an immense scale through the routines of a hospital with tight security. Although its rehabilitative mission was at odds with the broader criminalizing trajectory of U.S. drug policy, part of the project was to position the “dangerous classes” to receive “moral therapy” (Tomes 1994). Not everyone greeted the institutionalizing urge with enthusiasm, despite the political consensus out of which the narcotics farms were built. Until shortly before he assumed the helm at Lexington, Kolb had not supported specialized facilities for addicts (Acker 2002, 155, 163), despite advocating a strong federal role in mental health and hygiene. Lexington was the bricks-and-mortar incarnation of the idea that treatment, research, and rehabilitation should be linked.

By all accounts, Kolb assembled an enviable staff to run the institution, “raiding” both of the bureaucracies that oversaw Lexington’s construction. In congratulating Kolb on the “fine character of [his] staff,” Bates noted he was

"somewhat aghast at the number of fine subordinates that you have selected from our institution."¹⁰ The laboratory occupied a unique niche in the institutional ecology of the Lexington Hospital, which provided it with space and with laundry and dining services and "allowed us to borrow some of their patients" (Martin and Isbell 1978, 29). After attending a CDA symposium at Lexington on October 14, 1936, Bennett, by then commissioner of prison industries, complimented Kolb: "It seemed to me that you have a well-rounded, feet-on-the-ground research program which cannot but add much to our knowledge of drug addiction. For the first time in the history of the problem we are in a fair way to finding out at least what not to do and what remedies to abandon."¹¹

Although prison officials apparently believed that the ARC was engaged in treatment research, it was not a clinical unit, nor did it conduct clinical research. It was a small, semiautonomous research unit that was unique in conducting basic and behavioral research on humans and animals in the midst of a large clinical and custodial facility. The unit also assessed the abuse potential of new drugs from the pharmaceutical industry, supplied by CDAN. The ARC never conducted research directly for the pharmaceutical industry, nor were there any contracts or financial arrangements with industry. The ARC had very different goals from the Lexington Hospital, which sought addicts who were good candidates for rehabilitation.

Despite U.S. drug policy resting largely on criminalization, the Lexington Hospital was set up to detoxify on entry, treat underlying illnesses, and attend to routine medical conditions (including dental problems typically encountered by addicts, among whom the dentistry practiced at Lexington had a fine reputation). After these basic issues were addressed, patient-inmates could engage in therapeutic vocational and recreational activities, including such skilled activities as haircutting, dairying, sewing, woodworking, or photography. Such activities were not typically encountered in prisons except through prison industries, so difficult interactions with the criminal justice system plagued the hospital from the outset. Determining which convicted felons would be assigned to Lexington was contentious because the institution had a reputation as a "country club." Federal marshals and judges were confused about how to define addiction, which drugs were addictive, and what kinds of addicts made good candidates for rehabilitation. At first, Kolb sought candidates among prisoners serving lengthy sentences, but sentence length turned out to be a poor guide for deciding who was to go to Lexington. The founding medical officer in charge did not interface well with law enforcement; he

deplored criminalization as a “bad solution [to] a problem that has in a sense been created by governments.”¹²

Later, as assistant surgeon general, Kolb testified before Congress that criminalization produced addicts as criminals. Echoing him, Conan Kornetsky, who started working at Lexington as a University of Kentucky graduate student in 1948, said in a 2003 interview with the author: “The government screwed up completely. If anything these long sentences made criminals out of noncriminals. They defined them as criminals because they’re using drugs. Even in Kolb’s definition, they were defined as criminals. . . . they were defined as psychopaths. . . . If you read [the Kolb classification scheme] carefully, they really weren’t psychopaths, they were psychopathic-like. Only 5–10 percent were classic psychopath, and the others were various other types of personality disorders. Their psychopathy was that they didn’t have the same sort of ethical ideas that the rest of us had. Basically, they used drugs. It was sort of a self-fulfilling prophecy. [They were] psychopaths because they [were] in jail for using drugs.” Lexington produced a particular kind of addict identity and the behaviors, interpretations, and social definitions to go with it. The institution circulated a vernacular argot through which addicts and their parents, partners, and physicians understood addiction (Maurer and Vogel 1967).

Administrators and researchers at Lexington opposed the punitive direction of national drug policy engineered by the Federal Bureau of Narcotics (FBN) in the 1950s (McWilliams 1990). ARC personnel tried to direct federal policy toward a public health approach. In a 1959 speech before an audience of doctors and lawyers, Isbell argued, “A tug-of-war between one group advocating ‘extremely severe repressive measures’ and another group favoring liberality in dealing with addicts has been a block to progress on the problem.” From a medical and scientific perspective, he stated, penalties for drug addiction were “far too severe, far too repressive” in the United States. Even as the principle investigators at the ARC argued against criminalization, lengthy sentences and more accurate screening enlarged the pool of eligible subjects, and the unit’s scientific productivity rested on a regular supply of knowledgeable test subjects. The glory days of ARC research coincided with the mandatory minimum sentences imposed by the Boggs Act (1951) and the stiffened penalties that came out of the Daniel hearings of 1955–56.¹³ A structural contradiction derived from the conflict between Lexington’s rehabilitative mission and the ARC’s distinctly unrehabilitative practice of experimentally readdicting people known to have been recently addicted to illicit drugs.

From the outset, Lexington administrators tried to influence law enforce-

ment to populate the institution with only “suitable” convicts—those deemed capable of rehabilitation and reliable enough to serve as research subjects. World War II changed the addicted population with which Lexington dealt. Prewar morphine addicts and opium smokers differed from postwar addicts, who were mainly heroin users, younger, poorer, increasingly African American, and more commonly involved in minor, nonviolent criminal offenses. The war also took much of Lexington’s administrative and research staff to Washington, due to Kolb’s aspirations to build a federal infrastructure to conduct basic neuropsychiatric research. His efforts and those of his protégé Robert H. Felix, whose career began at Lexington, were central to the establishment of NIMH (Felix 1939, 1944; Felix 1979, 17; Grob 1991, 68; Harden 1986; Kleinman 1995). The laboratory at Lexington became the only active NIMH unit doing basic research in 1948, an affiliation that bought it more autonomy from the prison-hospital, and gained it a powerful ally that was more oriented toward basic biomedical research than toward custodial care. The research mandate was shaped by the imperatives of CDAN to find “a chemical substitute for opium which will give substantially the same amount of relief and not be habit-forming.”¹⁴ The laboratory at Lexington was the site for the coordinating committee’s human studies, gaining a unique hold on scientific credibility, material support, and a steady supply of subjects in the wake of the war.

Rapid innovation coupled with lack of industrial capacity to test pharmaceutical products in human beings “forced” the ARC into the area of abuse liability assessment (Martin and Isbell 1978, 32). Such government entities as the Office of Naval Research and the U.S. Army also contracted with the ARC to evaluate drug potency (Wikler 1960, 17). Compounds tested at Lexington were first evaluated elsewhere, then brought to Lexington by Nathan B. Eddy of the NRC committee, the main external influence on the ARC testing program (May and Jacobson 1989). Testing served the purpose of decision making for domestic and international drug control, to which the ARC played an advisory and even regulatory role. ARC data and recommendations were tightly coupled to those of the Expert Committee on Drugs Liable to Produce Addiction of the World Health Organization (WHO; see World Health Organization 1950).¹⁵ The WHO began contributing to CDAN’s annual budget in 1961, turning to CDAN for advice on psychotropic drugs from the mid-1950s through the 1960s. During this time, the ARC was the main testing body for the WHO, the FDA, the FBN, and the United Nations, a role that ended with passage of the Controlled Substances Act (1970). Prior to that, the ARC enjoyed continuous access to a wide variety of compounds until industry began to develop its own

evaluation capacity.¹⁶ One observer noted: “Nathan Eddy used to come with his bag of medicines to try on the addicts, and they used to rate them. In fact the technique of drug discrimination in animals is really a technique of what they were doing in humans. They would rate them compared to morphine” (Kornetsky 2003b).

The testing load was overwhelming and time-intensive. Martin and Isbell wrote, “Our capacity to test these drugs was strained to the limit and only the development of screening methods in monkeys at the University of Michigan prevented us from being completely overwhelmed” (1978, 32). Not until the 1960s did animal models become all that useful for drug screening. Until then, human beings were the most valuable source of information about drug effects. Despite the testing program, however, researchers were never in thrall to the pharmaceutical industry; they were buffered from fund-raising and administration and granted latitude to do basic research by virtue of their PHS positions. They defined addiction in physiological terms, as the predictable outcome of social and psychological conditioning, and sought to unmask neurophysiological effects in isolation from “psychopathological” effects. That science was not done anywhere else, so it is worth a close look at how people found themselves part of the institution that surrounded the laboratory at Lexington.

PATHWAYS TO LIVING AND WORKING AT THE “FANTASTIC LODGE”

Lexington was regarded as an almost mythical destination. It was well off the beaten path. Patients were mandated there by courts east of the Mississippi, bused in from Chicago and New York City. Both naive researchers and addict “volunteers” described winding up at Lexington as if magically drawn to the spot. Federal prisoners sought to transfer there due to its reputation. Different hierarchies of credibility, social status, and evolving values developed between the research side of the institution and the custodial and clinical aspects of Lexington. Early in the institution’s life, administrators sought to educate state and federal governments on the “wisdom and importance of professionalizing this type of public service” in order to keep Lexington from ever becoming an “old-time prison.”¹⁷ In this context, “professionalization” referred not only to the application of psychiatry to the “dependent and delinquent class” treated at the institution but also to the research effort.¹⁸ Initially, it was hoped that professionalization would bring about moral as well as physical rehabilitation,

through systematically implemented moral treatment regimens to "readjust" addicts (Acker 2002, 166–67). Over time, enthusiasm for modern moral therapy gave way to bureaucratized routines, more coercive procedures, and a "hardening" of the clinical staff's attitudes toward addicts and their beliefs about addiction (Acker 2002, 167). Despite Kolb's commitments to humane treatment, Lexington soon displayed a typical disjuncture that historians have identified in other large-scale institutions founded on humane visions: founding ideals give way to practices of social and behavioral control designed to manage large numbers of unruly subjects (Acker 2002, 162–63; Braslow 1997; Lunbeck 1994; Pressman 1998; Scull 1989, 22; Tomes 1994).

Ambivalent representations populate the historiography of "Lexington and its discontents" (Courtwright, Joseph, and Des Jarlais 1989, 296–318). Bipolar characterizations of the prevailing custodial and clinical environment at Lexington abound from both sides of the wall. The popular press promoted Lexington to country-club status: "Ringed by the fabulous Kentucky racing stables, the USPHS hospital (nicknamed Narco) stretches over a green hill like a country club. The inmates are called 'patients'; the guards, 'security aids'; and the disciplinary board is gently titled 'the adverse-behavior clinic.' The iron gates and window bars are painted soft colors of turquoise and rose" (Salisbury 1951, 60). Such depictions did not curry favor with the public, due to popular disregard for addicts. As radio personality Walter Winchell remarked in the 1950s, Lexington was regarded as a "multi-million-dollar flophouse for junkies."¹⁹ The cultural milieu in which opiate addiction took root in the United States made the therapeutic culture at Lexington a celebrity culture. Musical instruments were purchased by the institution, and well-known jazz musicians played in a large auditorium dedicated to talent shows and concerts (Davis 2003). There was a branch of a medical society for physicians who succumbed to their occupation's typically high rates of addiction. Residents worked at a variety of jobs: families of the scientists and clinicians who lived on the grounds had access to Chinese cooks and African American domestics drawn from the inmate population (Senechal 2003).

During most of the institution's life, there was substantial differentiation between researchers and clinicians in terms of constraints, routines, and expectations.²⁰ The clinical environment was more bureaucratized. A heroin addict from Southside Chicago, Marilyn Bishop, whose audiotaped memoirs were published under the pseudonym "Janet Clark," described her arrival at Lexington: "It was a long, tiresome procedure. . . . They make you fill out all kinds of forms, form after form after form. Have you ever been in the institution

before? Have you ever attempted cures before? What's your habit? How much were you using? That was the first time I ever had any contact with the Lexington attitude about junk [heroin]; you know, just very matter-of-fact, as though he would ask someone how many cigarettes they smoked in a day" (Hughes 1961, 210).²¹ Indeed, the folders of forms in the federal archives in Morrow, Georgia, reveal that Lexington's highly bureaucratized day-to-day operation tracked everything from milk production to the whereabouts of every syringe. Application forms summarized which drugs qualified one for admission to Lexington ("opium, morphine, heroin, Demerol, methadone, dolophine, codeine, coca leaves, cocaine, Novocain, isonipecaine, and Indian hemp") and which did not ("sodium phenobarbital, amytal, nembutal, Seconal, luminal, chloral hydrate, bromides, paraldehyde, Benzadrine, elixir terpin hydrate, any other barbiturate, and ALCOHOL"). There was a two-page application to be filled out by the applicant, a two-page Medical Certificate of Drug Addiction to be filled out by the applicant's physician, and an additional form to be filled out by women applicants. The latter indicated the paternalism and institutional sexism of Lexington: a sample version was filled out by "Mrs. Violet Rose Buttercup" and granted the surgeon general, the medical officer in charge, or their designated representatives the authority to communicate with the applicant's next of kin.²² Treatment was regimented; prisoners and volunteers were perceived as difficult if they did not comply with institutional routines (Hughes 1961, 216). The degree of social control reached coercive levels despite therapeutic intent. Clinical and support staff were hardly immune from the generalized social stigma pertaining to addicts; if anything, conflicts between staff and patients confirmed addicts as undesirable, unruly, or otherwise abnormal.

Over time, addicts who habitually made the trip to Lexington noticed both demographic shifts among the general population and attitudinal shifts among staff. "Brenda" noticed more African Americans from Washington, D.C., by the mid- to late 1950s. She recounted a drastic change in a young, white psychiatric aide who had been "very nice and sociable" during a previous hospitalization. By 1956, she was so changed that "Brenda" stated: "I couldn't believe it was the same person. She had toughened, and hardened, and wouldn't smile" (Courtwright, Joseph, and Des Jarlais 1989, 307). Despite this, Lexington occupied an almost mythic status for patients, who sometimes begged for admission. In a letter to "Dr. Cobb" (*sic*) addressed to the attorney general in Washington, D.C., an addict from Terre Haute, Indiana, wrote on behalf of himself and his wife: "This will be the last request I'll ask. The last time I was there complications arose, which forced me to leave before I was cured. . . . [This time] I

feel sure we will manage to stay until we are completely cured."²³ The view of a well-run, genuinely therapeutic Lexington contrasted to portrayals in which addicts chafed against the strictures of institutional routine or invented ingenious ways to get around the rules—such as “kiting,” the practice of sending notes between inmates (Maclin 2004).

The patient-inmate population became more diverse over the life of the institution. The diversity among residents outstripped the diversity among staff. Harris Isbell started the Social Science Section in 1962 to study demographic shifts in admissions to Lexington. That unit did a study in 1966 of all admissions to Lexington and Fort Worth from their respective openings in 1935 and 1938 through 1964, showing a marked drop in the age of male admissions (only 16 percent were under age thirty in the 1930s, compared with 50 percent in the 1960s). Southern admissions had fallen off, while those from northern cities (notably New York and Chicago) had climbed. The percentage of non-white inmates (which included Chinese) was less than 20 percent in the 1930s but more than 40 percent by the 1950s. By the 1960s, there were many more admissions among people with prior criminal records who were regularly engaged in illegal activities requiring more cash than most postwar addicts could muster. Postwar addicts were younger, less skilled, and less educated—they faced such structural constraints as the disappearance of viable employment. By 1960, the racial-ethnic transition was clear: out of roughly one thousand Lexington patients, eight hundred were male; 50 percent were white, and 48 percent were African American. Over 70 percent were addicted to heroin, less than 10 percent to morphine, and more than 13 percent to synthetic opiates (Rasor and Maddux 1966).

Negative impressions of Lexington often center on initial impressions of the institution. Heroin addict Marilyn Bishop said: “You never get over that first shock. After a while, they start looking like people to you, and everything, and you get used to it. You get used to looking at the sores, at women that are so thin that it just shouldn’t be. I mean, they look like those pictures from Dachau and the concentration camps, of people who have been starving for hundreds and hundreds of years or so, and all hunched over and huddled-up and sick-looking” (Hughes 1961, 213). Despite this description, Bishop praised the food as “above jail par”—eggs, fresh fruit, dessert, and salad (Hughes 1961, 221). She noticed what Becker came to call “labeling,” by which residents came to identify themselves as “junkies” and assume an identity they had not previously called themselves prior to Lexington: “After the first six, eight months that I was making it, I never said, ‘Well, I’m a junkie,’ as an excuse or as any-

thing. But now I say it constantly. I always refer to myself as a junkie, even when I'm not hooked on anything. And when you're introduced to somebody for the first time, the first thing you find out is whether he's a junkie or not. It's like belonging to some fantastic lodge, you know, but the initiation ceremony is a lot rougher" (Hughes 1961, 214–15). Lexington produced "junkies" who were initiated into a "fantastic lodge," sharing a common language and a set of social norms that marked them as a separate class.

Classification by drug of choice and diagnosis was a major part of the Lexington routine, not only among medical personnel but also among residents. Old-style "medical junkies," whom Bishop described as Southern hypochondriacs, were distinguished from the new class of "illicit junkies" who considered themselves "members of the underworld" (Hughes 1961, 219). The two intermingled at Lexington, although they shared neither social experiences nor language to interpret them. Gradually, the number of "accidental" or "medical addicts" declined. To make the boredom of the institutional routines bearable, illicit junkies shared information about policing, drug markets, and technique; smoked cigarettes (which were ubiquitous among residents, staff, and researchers, all of whom received standard-issue heavy glass ashtrays on their desks on arrival); talked about shared interests in dope and jazz; did work assignments; or sought dental or medical care. "It's not exactly what you'd call an exciting routine," admitted Bishop, "but it's pure luxury, compared to most prisons" (Hughes 1961, 225). The construction of Lexington as "luxurious" was common among locals, U.S. marshals, convicted criminals, and potential patient-inmates (Senechal 2003, 184).

Routine was socially supportive for those who lived at Lexington. Postaddicts recounted feeling at sea upon leaving the institution, returning to the familiar life after taking "the cure." As noted earlier, Lexington was to junkies an initiation rite through which they became members of a "fantastic lodge." Lexington provided a sense of belonging that ironically transformed people into "incurable junkies" for the first time in their lives. Addicts were produced according to persistent and widely held beliefs in the underlying psychological—and psychopathological—basis of addiction. The "psychogenic" basis of drug addiction, established by Kolb, was in the process of being investigated and undermined by the basic scientists in the neurophysiological laboratory next door. Relationships replayed the typically hierarchical division between educated, largely white, middle-class male scientists in white coats and subjects from the ranks of the poor and working classes. With the exception of physician addicts, well represented at Lexington and somewhat favored at the ARC

because they could assist in data processing in the age before computation, subjects came from very different social circumstances than did those who studied them. However, subjects possessed a broad range of experiences with the social circumstances surrounding drugs and drug markets, on which researchers depended for the production of valid results.

Becoming a subject in a research study marked a Lexington resident with distinction as a “real” addict whose condition was important enough to merit scientific inquiry. Subjects were housed on a separate ward when part of a study and not released into the general population. Separate housing was one of the chief incentives for participation in studies—even a small private room varied the tempo of institutional life. Financial incentives were minimal—the ARC never paid any more than other prison industries. Access to drugs clearly attracted some subjects, although they could not know if they would receive an active compound or a placebo.²⁴ Nor could they predict what drug would be administered.²⁵ Researchers reiterated beliefs that participants volunteered out of an altruistic desire to give something worthwhile back to society. A researcher who began in 1963 put it: “Obviously, they were gaining benefits from us. It wasn’t treatment benefits. We never stated this in any way as being a therapeutic benefit. But these people were serving long sentences, they liked to have the variety, and some of them were altruistic. I believe that some of them really felt for the first time in their lives, where they had never done much good, that they could actually do something that was a benefit” (Gorodetzky 2003). The belief in the altruistic motivations of prisoner patients was central to researchers’ construction and maintenance of their self-identity as ethical subjects.

Such safeguards as informed consent, eligibility criteria, protocols, and the Organizational Review Board were central to researchers’ performance of ethical science. To participate in ARC studies, prisoner patients had to meet eligibility criteria: they had to be healthy; they could not be “naive” to the drug being tested (which meant they had to have formerly consumed drugs of its class); they could not be administered experimental drugs within six months of release; and they had to “volunteer” for studies about which they could know little beyond the fact that drugs would be administered, that they would be monitored physiologically, and that they would be asked to answer extensive sets of questions or to engage in various exercises. Undergoing pencil-and-paper tests was called “being on the sawmill,” and these tests included a variety of psychometrics (Johnson 2005). Such instruments and protocols enabled the ARC to amass an unrivaled data set on the effects of drugs on humans. Thus Lexington has taken on an almost mythical status among researchers.

AN INTELLECTUAL MONASTERY: THE MEANING OF LEXINGTON TO ADDICTION RESEARCHERS

Members of the founding generation of addiction researchers were naive to the social contexts in which drug use took place. While later generations of addiction researchers entered the field knowing a bit about the social context of drug use or even being acquainted with peers who used narcotics or marijuana, none of the founding generation had social or familial connections to the “drug scene.” They were completely reliant on their informants’ veracity for narrative accounts of subjective effects and life histories, having no choice but to observe closely and listen attentively if they wanted to learn anything about addiction. They soon became involved in the process of building objective scales to measure the intensity and specificity of addiction, scales that became the ARC’s hallmark.²⁶ The research ward was the researchers’ primary conduit to their experimental subjects, on whom they relied to an unusual degree (Himmelsbach 1972, 1994). Those whose scientific careers began at Lexington recount flashes of insight garnered from casual conversation with participants in the dayroom of the research ward.

The topic of ethical limitations on work with human beings arose immediately due to the nature of clinical research on opiate drugs, such as morphine, Dilaudid (dihydromorphinone), codeine, hypnotics, and barbiturates, all of which Himmelsbach studied in the formative years of his career. His precocious awareness concerning informed consent was revealed in interviews separated by two decades, in which Himmelsbach maintained that gaining informed consent was a normalized practice in the research programs he built at Leavenworth and Lexington. The first interview was conducted in the late spring of 1972 (before the sensational story of the PHS role in sustaining the Tuskegee syphilis experiment broke).

Early in research at Leavenworth, it became clear to me that the individuals participating in the research as subjects deserve some credit and deserve some consideration as well. I think this had been unheard of by any of my predecessors, but it seemed to me that they ought to do this willingly, not because they had to, not because they had lost their citizenship and were prisoners. So I got informed consent. I would tell them what we had in mind, the good, the bad, and the indifferent of it to the extent that I was able, and get their informed signatory consent before I would accept them as steady subjects. I don’t know that that was the first time people got informed consent from study subjects, but it was right early in the course of research on man. I kept that up there and at Lexington as long as I had anything to do with clinical investigation, and I still do. (Himmelsbach 1972, 17)

Questioned shortly before his death (which occurred on March 20, 1995) about why he was thinking about informed consent in the mid-1930s,²⁷ Himmelsbach revealed that a lawyer with whom he had been friendly, James Kelly, had suggested that informed consent procedures could be easily built into the research process at Leavenworth. His 1972 interview detailed the paternalistic nature of what informed consent meant to Himmelsbach: subjects knew they could withdraw from studies if they so chose; subjects knew that he, the investigator, would not let anything "adverse" happen to them; and subjects trusted that he "would not let them suffer unnecessarily or to suffer any permanent damage" (1972, 18). Remorsefully, Himmelsbach recounted an "individual who died in my arms at Lexington from causes that I could never understand," eight to ten hours after withdrawal, despite administration of morphine. "Other than that instance," he claimed, "I don't know of a single individual that ever was harmed or was permitted to harm himself" (1972, 18).

Echoed through the years by researchers, such statements about the lack of mortality have been part of the ongoing construction of the research at Lexington as an ethical enterprise. Although there were occasional suicides at Lexington, and there was a morgue there, no deaths were directly linked to the administration of a drug under study. As recounted in chapter 5 of the present book, there were some close calls, but the ARC researchers pioneered the use of nalorphine and other narcotic antagonists to counter opiate overdose, the most common source of danger. The occasional suicides occurred among the general population, not among the small cadre of research subjects. The construction of ethical identity derived from status hierarchies in the PHS, of which most researchers were commissioned officers, and also from the researchers' position as physicians who espoused the injunction to "first do no harm." The informed consent process was applied not only to postaddicts but to the so-called normal individuals who served as controls to establish baselines. According to Himmelsbach, controls received no more than a single, ten-milligram dose of morphine, yet they, too, were asked for consent (1972, 18).

Postaddicts were not considered "normal" individuals within the ethical economy of Lexington or Leavenworth, because they had once been addicted to narcotics.²⁸ Instead, they were considered always already ill. Himmelsbach reported: "[T]hey all came in heavily addicted, and they were sick when they came in or about to get sick. . . . They were not normal, certainly, they were volunteers, they participated in what we wanted to do, willingly. As a matter of fact, they knew more about it than I did, much more. I learned from them. They gladly told me what they do. They gladly participated in these studies. As a matter of fact, they were enthusiastic about it" (1994, 11). Learning from those

who knew the most about narcotics addiction—addicts themselves—was a basic tenet held by researchers who spent their early years at Lexington. Himmelsbach referred to this as “dealing the patient in,” a locution clearly based on the card games that were a ubiquitous activity at all levels of the institution.

Years after departing Lexington, Himmelsbach participated in an elite gathering of clinical researchers convened in Atlantic City by the Law-Medicine Research Institute (LMRI) of Boston University to discuss the “concept of consent in clinical research.”²⁹ He there argued: “[W]e must deal the patient in . . . so that he can participate in the judgment. There are some derivative values to him as a human being, and to the extent he can understand these, I think he should understand them. His consent should be in this frame of reference” (LMRI 1963, 36). Contrasting the broad responsibility of informing patients to the narrow act of gaining informed consent, Himmelsbach attested to “values that derive for the benefit of the individual that participates in research, the satisfaction that he gets from it when he has some comprehension of what he’s done.” Himmelsbach claimed: “I’ve seen this thousands of times. I’ve seen it in prisoners, and I’ve seen it in other people.” A forceful proponent of this view, Himmelsbach stood in marked contrast to his fellows, who framed the benefits that accrued to research participants solely as “plain ordinary money” (LMRI 1963, 17). Insisting that the value of research to the participant-subject transcended money was prevalent at the ARC. It was one of the chief ways in which the white, male, middle-class physicians and scientists who worked there safeguarded their reputations and secured their social relations as ethical subjects.

The ARC attracted a succession of researchers of considerable scientific acumen. Jasinski remembered: “You’re talking about an era when the Public Health Service could be extremely selective. The people who got into the Public Health Service in the 1930s and the 1940s—before the Second World War and through the Depression—were the best of the best. The smartest group of people I ever met in my life was at Lexington. Wikler was a genius, and Martin was probably among the most creative scientists I ever met. Probably the best of them all intellectually was Isbell. Abe used to describe [the ARC] as like an intellectual monastery because it was a wonderful place to do science. We were isolated from everything. You had a conglomeration of very bright, creative people, and you had a coalescence of forces happening at the same time, which led to a golden era” (2003). Those who contributed to the team enjoyed a sense of prestige. Himmelsbach recounted: “I think we learned some things together that we probably wouldn’t have learned individually. Certainly the sum was greater than the total of the parts. This was one of the early multidisciplinary approaches in human research” (1972, 18).

Well into the 1960s, almost all significant drug addiction researchers spent time at the ARC at the inception of their careers. Budding researchers enjoyed an atmosphere of intellectual curiosity about how addiction worked, unremitting attention to research design, and the "low walls" touted for collaborative, interdisciplinary research environments today. Many recall Lexington as formative to their subsequent intellectual and professional development (Gorodetzky 2003; Jasinski 2003; Kleber 2004; Kornetsky 2003a, 2003b; Jaffe 2002, 2007). At the ARC's fortieth anniversary, Felix said: "As one stands here in 1975 and looks back at the beginning of this great research program as described in the words of the investigators, one can appreciate how frontiersmen in any field of endeavor must feel. Leaving familiar paths of endeavor which are accepted and 'respectable' the adventurers launched forth into an uncharted wilderness, hardly knowing which way was north and sure only that they were alone and they were expected to think better of their rashness after a while and return to 'civilization.' Certainly many of us had moments when we felt somewhat that way at Lexington" (Martin and Isbell 1978, 6). The symbolic status of Lexington as an origin story for the field should not be underestimated, despite its status as a total institution (Goffman 1981).

Lexington placed novice researchers in close contact with subjects and senior scientists. The latter were a close-knit group, whose familial bonds continue to this day and who remember the research culture in highly favorable terms. Their working environment was physically and conceptually separate from the rest of the institution; the ARC was described as a completely different universe. Although most researchers were PHS officers, social hierarchies at the ARC were flatter than those in the rest of the institution. Inexperienced researchers might suddenly find themselves in relationships of apprenticeship and mentorship to more experienced scientists. While they sometimes experienced their superiors as authoritarian or paternalistic, they retained reverence toward them.

Hired in 1948 as a psychology graduate student to administer clinical tests at the Lexington Hospital, Conan Kornetsky soon became involved in doing similar tests for the ARC and was able to wander freely about the research ward (unlike his imprisoned counterparts).

I spent the evenings hanging around with them on the wards, just chatting with them. And they got to sort of accept me, I became sort of one of them. . . . I was called the "young doc" even though I was not a doctor. They would chat and tell me their experience. At first I thought I'd figure out how I was going to cure them, and really quickly decided I wasn't going to cure them. They'd tell me

about their life's experience and where they grew up. At first, most of them were white, but then there was a big influx of black urban youth. A lot of the white patients were not from urban centers. Their life experience was they were drifters. . . . I got to be friendly with a lot of them. After a while they accepted me. First they would always try to tell me stories and exaggerate like mad, and after a couple of months the stories got less wild and more reality-focused. (Kornetsky 2003b)

Although such casual interchanges were permitted, formal research design was tightly controlled by senior scientists. Young researchers were granted latitude to toss ideas around informally during morning coffee sessions in the lab or the legendary Saturday seminars.

Researchers found their way to Lexington through either an informal social network or the accident of PHS assignment. It offered one of the first psychiatric residencies in the country. A rotating position as medical officer attracted “two-year wonders” just out of medical school who rarely knew what they were getting into. One of them, Charles Gorodetzky, reports:

I got a phone call from Harris Isbell, must've been around October, November of 1962. I was an intern at Boston City Hospital, having graduated from Boston University medical school. He told me, “We've got a two-year position down here for medical officer, would you like to come down to Lexington?” Because I had asked for a research position, I was a candidate. . . . I said, “Where is Lexington, Kentucky? What is the Addiction Research Center?” (2003)

Gorodetzky's anecdote captures the happenstance with which many found their way to Lexington. First-contact stories are common in the interviews. Gorodetzky recalled arriving during a periodic renovation: “Everybody was moved out of their offices. They were all put up on the third floor where we had the volunteer research ward. All the desks were crowded side to side in the day room and they gave me a desk next to Abe Wikler, which is one of my dominant memories—to put me next to this giant in the field, me, this kid out of nowhere” (2003). The arrangement was temporary, as the retirement of Wikler, Isbell, and Fraser loomed. However, Gorodetzky's acquaintance with Wikler grew into a close personal and professional relationship, as they belonged to the same synagogue and spent two overlapping decades living in Lexington. When Isbell retired in 1963, neuropharmacologist William R. Martin took the reins, which he held until the “great hue and cry” of the 1970s (see chap. 6).

A DISEASE SUI GENERIS: THE CONCEPTUAL CONTRIBUTIONS OF ABRAHAM WIKLER

The founding generation at the ARC made addiction more tractable to the biologically oriented, experimental methods embraced by the postwar group that became the core of the addiction research enterprise. The career of Abraham Wikler, associate director of the ARC and chief of the section on experimental neuropsychiatry, exemplified the “basic” orientation of the postwar core. Raised in a close-knit, working-class Jewish family in New York City, Wikler was an intellectual whose writings reflect an awareness of his position between different generations. He credited his forebears with establishing addiction as a real physical and psychiatric disorder while deflating myths about “sex-crazed dope fiends” (Wikler 1944, 4). He valued the animal studies that Kolb had done at the Hygienic Laboratory in collaboration with A. G. DuMez to refute the theory that autoimmune disorder resulted in addiction (DuMez 1919; DuMez and Kolb 1925, 1931). This work had laid the “ground work for Himmelsbach’s investigations at the Leavenworth Penitentiary and subsequently those of the Research Division at the Lexington hospital” (Wikler 1960, 2).

Knowing neither addiction nor research prior to doing his psychiatry residency at Lexington, Wikler listened closely to addicts’ stories about relapse when they returned to old neighborhood haunts after leaving Lexington “cured.” Based on cues and conditioning, Wikler’s model of addiction remains an important touchstone. Although his ideas often emerged in conversations with prisoner patients, his passion for Pavlovian conditioning theory structured his experimental design.

Abe, in talking to a number of patients, recognized the phenomenon for the first time that people could be detoxified for a long period of time, then in certain circumstances could experience what appeared to be withdrawal, triggered by a number of external stimuli. . . . Wikler viewed drug seeking and withdrawal [as something that] could be learned. He first saw this in 1948 and did both animal experiments and some human stuff on the idea that craving and withdrawal could be conditioned, à la Pavlov. . . . Abe would arrive at the same conclusion you would, but by a different logic, a circuitous logic that was always amazing. (Jasinski 2003)

Integrating insights drawn from conversation and observation was typical of the Lexington group. This capacity was central to Wikler’s integrated model of

the interplay between physiological and “psychogenic” factors, external “cues” and internal sensations.

Laboratory logics of substitution and mimicry were based on access to seasoned drug users, who were used as bioassays to gather data on drug effects. Rating scales and experimental techniques were designed at the ARC to translate “subjective” effects into quantitative, “objective” scales. These ultimately became the Addiction Research Center Inventory (ARCI), a scale still used in modified form today for assessing drug abuse liability (see chap. 7). Describing the ARC program to Congress, Wikler wrote that the research relied on prisoner patients “with histories of repeated relapses to narcotic drug use and very poor prognoses for cure who volunteer for such research” (1960, 10). Compounds were administered so that if tolerance were going to develop, it would do so within a month, a tedious and time-consuming method that required “a supply of eligible patients that is not always readily available” (Wikler 1960, 11). The ARC developed an efficient “substitution technique,” also called the “Lexington test,” but “direct addiction” was also used (Wikler 1960, 9–10). Experimental readdiction worked according to a laboratory logic that mimicked the process leading up to addiction, instead of the process of withdrawal that was central to the laboratory logic of substitution.

That Lexington researchers were in the business of readdicting prisoner patients for the sake of science was as clear to Congress as to the researchers and their subjects. Experimental readdiction was openly used to assess how “addictive” a given compound might be. This determination provided information used by pharmaceutical companies seeking to bring drugs to market. However, as indicated earlier, that was not the main reason for experimental readdiction. Answering the basic questions to which Wikler devoted his scientific career—defining the neurophysiological mechanisms of drug addiction based on a model of classical conditioning underpinned by “cues” central to social learning—required a laboratory logic in which the physiological process of experimental readdiction mimicked the process of addiction within its social and cultural context. He saw physiology and psychology as inextricably linked in the process of addiction, and he sought a method to disentangle their separate contributions.

Early in his career, Wikler became skeptical of purely psychogenic approaches to mental disorders. During the first year of his psychiatry residency at Lexington, Wikler diagnosed a basal ganglion disorder in a professional billiard player by using an unconventional diagnostic technology—a movie camera. The patient, a fifty-four-year-old white male, had repeatedly

relapsed and been admitted several times in 1940 and 1941. Doctor and patient attributed each return to morphine's calming effect on a tremor that affected the patient's exercise of his profession. He first noticed the tremor after the death of his wife, to whom he was "greatly attached" despite her disdain for the "unfortunate associations" necessitated by his chosen profession (Wikler 1942, 399). Preoccupied with resolving whether the tremors and tics were of an organic or psychogenic nature, Wikler administered standard bioassays, such as the Wasserman test for syphilis, as well as various drug preparations, including morphine itself. He took moving pictures of the patient, which were shot at regular speeds and in slow motion, before, during, and after the administration of morphine. "The slow motion pictures revealed fine coordination and rhythmicity of the tremor characteristic of an organic disorder. After injection of morphine the patient was able to write, bring a glass of water to his lips without spilling and to perform test acts fairly well, but the tremor remained unchanged objectively" (Wikler 1942, 400). Wikler concluded that the patient's apparent grief masked the basic organic picture.

Undiagnosed brain lesions, Wikler became convinced, were often responsible for "mental disorders" but were masked by "psychogenically determined emotional factors" (1942, 400).³⁰ Accurate diagnosis depended on eliminating confounding emotional tensions that complicated patients' lives and finding the true disease, whether it be malaria, a brain tumor, or the surprising instances of cerebral *Candida* infection found among drug addicts (Wikler, Williams, and Weisel 1943). Thus fortified, Wikler embraced experimental approaches and set out to revise the basic concepts and terminology of Freudian psychoanalysis, to "bring closer together the now widely separated so-called 'organic' and 'psychogenic' schools of psychiatry" (1942, 402). His goal was conceptual integration—not elevation of one school of thought over another. He was steering clear of errors of diagnostic classification. For instance, he noted a characteristic loss of emotional inhibition among the addicts with whom he spent his working life. Sounding strangely prescient in his very first published talk, Wikler listed conditions that could account for lessened inhibition, many of which looked "psychogenic" but were not: trauma, epilepsy, disturbances of brain metabolism, hormonal changes, toxic psychoses from other drugs (e.g., bromides or barbiturates), multiple sclerosis, and neurological disorders. He argued, "[W]e still do not know how many 'constitutional psychopaths' or 'feeble-minded' cases may be attributed to birth injury, unrecognized intracerebral hemorrhages at birth or cerebral complications of childhood virus diseases." Citing promising results from elec-

troencephalography, the imaging technology of the day,³¹ he noted that such factors might account for a large proportion of “problem children” (Wikler 1942, 404). Rather than turn to an elaborate analysis of psychogenic motivation, Wikler urged more thorough neurophysiological assessment as a way to account for patients’ turn to narcotics.

Himmelsbach sent Wikler on a yearlong training sabbatical before putting him in charge of the neuropsychiatric laboratory at Lexington.³² During this time, Wikler gravitated toward experimental attempts to produce states resembling human neuroses through autonomic, somatic, and behavioral disturbances in animals (1942, 400).³³ Despite believing animal models to be limited in explanatory utility, Wikler developed practical techniques to get them to work (1948b). He focused on designing experimental situations to test his hypothesis that emotional disturbances could change “body chemistry” (1942, 401). Drawing on behavioral work, including conditioning theory, scheduling, and experimental extinction of conditioned responses (Anderson and Parmenter 1941; Pavlov 1927, 1941), he also read and cited the psychiatric literature on “war neuroses” and experimental production of anxiety.³⁴ He came to divide the world of psychological research into work based on “unassailable” psychoanalytic theories and work based on conditioned reflexes (1957). Both explanatory frameworks—psychoanalysis and conditioned reflex theory—relied on social learning and environmental adaptation but adopted distinct narrative practices, laboratory logics, and techniques. Wikler maintained that both the neural theories of Pavlov and the mental theories of Freud had led to misunderstandings and “dissipated the energies of investigators in endless polemics about the ‘mind-body’ pseudoproblem” (1957, 209). He came to see physiological mechanisms as embedded in complex patterns of change that depended on the meanings attributed to them, as well as individual biography, social environment, and observational goals.

Something about addiction evoked such hybrid approaches, which also appealed to Wikler’s synthetic mind. The multidisciplinary thought collective that formed at the ARC perceived it to be impossible to get anywhere on the “opium problem” by taking any one route. There was not a deep conceptual split between concepts of physiological and psychological dependence: the goal was to integrate physiology and the “psyche,” a division Wikler questioned so thoroughly that skeptical quotation marks littered his writings. For him, “psyche,” or “personality organization,” shaped addiction and the abstinence syndrome, the intensity and duration of which varied in relation to the personality of the addict (Wikler 1948a). Indeed, he argued that addiction only became

a public health problem for persons whose "emotional need for morphine, or drugs like it, is so strong that it overbalances the personality defenses (i.e., 'super-ego' structure) against addiction" (Wikler 1950, 506). Describing most postaddicts as "extremely infantile, narcissistic individuals," he conceded that results from "psychiatrically inferior" individuals were invalid for "normal" persons. Retrospectively, he described his research trajectory as interrelating "'psychic' and 'organic' factors in the genesis of drug dependence," titling his 1974 Nathan B. Eddy Award lecture (presented to the Committee on Problems of Drug Dependence and published in 1977) "The Search for the Psyche in Drug Dependence." The problem was that the psychic and physical aspects of addiction could not be disentangled in intact animals or humans, and psychic effects could not be verified through observation. Only through Pavlovian methods did Wikler believe the "psyche" could be supplied an "operational definition" (1974, 2).

Awareness of the limitations of animal experimentation stemmed from Wikler's conclusion that personality organization corresponded to individual variations in "feeling tone." He inferred that "in man, the effects of morphine on overt behavior, affect, and phantasies depend to a large extent on the personality organization of the individual" (Wikler 1948b, 330). To explain this, he turned to émigré psychoanalyst Sandor Rado, who believed that individuals used opiates to self-medicate for "tense depression" (see chap. 1 in the present book). Wikler had more than a passing encounter with psychoanalysis: he himself underwent analysis in Cincinnati in preparation for briefly opening a private practice as a psychiatrist in the town of Lexington (Senechal 2004). Seeking to reconcile psychopathy and physiology, he became committed to the conditioning hypothesis and "learned adaptive response," because these explanations were founded on an operational definition of the psyche. For Wikler, personality factors played predisposing roles: psychopaths used drugs to gain a "positive pleasure," and neurotics used them only to reduce anxiety, which he dubbed "negative pleasure." Although he saw the ARC's efforts as confirming—not disconfirming—the validity of Kolb's classification system, he regarded personality studies as necessary but insufficient explanations of addiction.

Wikler argued that personality factors warranted continued human study: "Clinical experience indicates that other dynamic mechanisms, such as the use of a forbidden drug to express hostility and by the same means to acquire an infant dependent relationship, are operant in some individuals. Such specific psychodynamic factors cannot be investigated by animal experimentation" (Wikler 1948b, 331). He turned to Rado's theory of psychodynamics, a science

of individual variation explicated in articles and lectures (the latter were collected and published by Rado's students in 1969). On first reading, Wikler's article on this theory (1948b) appears to shift abruptly between experiments on chronic decorticated spinal dogs and speculations on the social milieu and psychic organization of human beings. Wikler argued that the meaning of drug effects differed from individual to individual—that whereas morphine released fantasies of omnipotence and grandiosity in “highly narcissistic, egocentric individuals,” who then experienced a “feeling tone” of unusual well-being and overt behaviors (e.g., garrulity, boastfulness, and psychomotor activity), its sedative effects were more attractive than its euphoric effects for “dependent” persons who were depressed or anxious, who resembled “satiated infants” when on the drug (Wikler 1948b, 330).³⁵ He concluded that the motivation to use morphine differed according to whether it satisfied deep emotional needs or not, how strongly defended an individual was against gratification, and prevailing attitudes in their social milieu. Such heterogeneity preceded the conditioning process in ways that precluded conditioning to all drug effects in all individuals. Conditioning, in other words, worked as an explanation despite individual variation. Conditioning meant that individual variation need not be explained.

Explaining the long-term persistence of physiological changes and subjective experiences associated with conditioning remains a significant scientific problem. Individuals varied in how they responded when threatened with the disappearance of a drug; anxiety about withdrawal did not appear to motivate all relapse. Most postaddicts relapsed soon after leaving Lexington, a phenomenon that was a persistent public relations headache for the institution.³⁶ Relapse became Wikler's terrain when he found that unconditioned responses might become “conditioned to various situations or memories associated with taking the drug, thereby evoking subjective experiences similar to those associated with morphine withdrawal, namely anxiety with craving for the drug” (1948b, 337). Conditioning—or learned adaptation to drug effects—accounted for relapse long after withdrawal. Conditioning and learning performed integrative work for Wikler, whose abiding desire was to “describe the indivisible organism in terms of many frames of reference.” Psychodynamics and neurophysiology were the most developed frames of reference through which to describe drug effects. Although Wikler hoped that biochemical or anatomical frames of reference would be further developed, he believed that “no individual frame of reference is any more ‘fundamental’ than any other” (1952a, 11). The problem was that psychoanalytic concepts—such as id, ego, superego, or

Oedipus conflict—could not be confirmed or disconfirmed (Wikler 1952a, 11). Thus he preferred the conditioning hypothesis, in which relapse was a response to environmental stimuli or “cues,” over explanations based on “pre-addiction impulse[s]” (Wikler in Martin and Isbell 1978). Although he suspected there was a psychogenic basis to addiction, he dismissed psychoanalysis in favor of testable hypotheses modulated through the concept of “conditioning” (1957, 87). To Wikler, conditioning emerged as the most promising integrative concept because of what subjects said and did.

The ARC played a formative role in constituting addiction research as a specialized enterprise because it was the only place where experienced drug users regularly came into contact with clinicians and researchers. Elsewhere, addicts were treated in ways that foreclosed their becoming patients or human subjects. At Lexington, researchers enjoyed close encounters with postaddicts who, they saw, were not unlike themselves in terms of intelligence, resourcefulness, and creativity. The social structure of Lexington drew attention to the range of individual responses to drugs and the diversity of those who had chronic struggles with addiction. Subjects varied in the meanings they attributed to drug use, social and professional backgrounds, and psychological configuration. What scientific sense was to be made of these variations? The ARC was an engine for individualizing and then aggregating drug effects. The exercise of disciplinary power took the institutional form of “objectification” within this vast prison-hospital, the study of which provokes an echo of Foucault’s sardonic question, “Is it surprising that prisons resemble factories, schools, barracks, hospitals, which all resemble prisons?” (1979, 226–28). The slippage between the prison, the hospital, and the laboratory at Lexington results from recognizing the impossible necessity of differentiating between them.

Drug addicts, who occupy the social category of unproductive or even antiproduktive, were rendered “useful” through the exercise of scientific discipline at the ARC. Perhaps the most succinct and accurate definition of what Foucault meant by discipline was the “unitary technique by which the body is reduced as a ‘political’ force at the least cost and maximized as a useful force” (1979, 221). As Foucault reminds us, however, “any mechanism of objectification could be used in [the hospital, the school, and later the workshop] as an instrument of subjection” in the course of the “formation and accumulation of new forms of knowledge” (1979, 224). When the disciplines “crossed the technological threshold,” Foucault maintained, they converged to augment the “effects of power through the formation and accumulation of new forms of

knowledge” (1979, 224). During the decades when the Lexington and Fort Worth hospitals existed, these institutions were the only sites for treatment, rehabilitation, and research on drug addiction in the United States. At the ARC, researchers sought to account for the mystery of individual variation, especially susceptibility to relapse, turning directly to their subjects for insight.

Loss is a recurring theme among those who did research time at Lexington. As a model of federally funded scientific collaboration, the ARC was a specialized enclave cut off from other centers of knowledge production. Social proximity to research subjects made a difference but also imparted a sense of illegitimacy when the ethical questions taken up in later chapters in this book arose. This chapter sought not to avoid such dilemmas and contradictions but to build toward the full-scale inquiry that commences in the next chapter. When the ARC’s work is placed alongside that of a peer institution, the Harvard Anesthesiology Laboratory run by Henry K. Beecher at Massachusetts General Hospital, interesting parallels and refractions appear. According to CDAN’s design, the laboratories conducted similar work. The committee expected Beecher’s lab to mirror the experimental situation set up at the ARC. Yet one laboratory was ultimately disgraced, and the other was upheld as the birthplace of the randomized, controlled clinical trial methodology used to evaluate new drugs today. Given their similar practices, laboratory logics, and attitudes toward human experimentation, what factors made the difference? Was it the “stigma” that accrues to illegitimate research enterprises? Was it presumptions about exploited research populations? Was it the different levels of prestige enjoyed by the larger institutions of which each thought collective was a part? Finally, how different was the performance of ethical subjectivity by scientists who worked in these very different social locations? Science is more of a social privilege for some. As the next chapter shows, scientific discipline is differentially enacted in different social spaces.