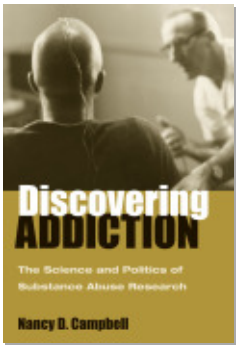
Discovering Addiction

Campbell, Nancy D.



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CHAPTER 5

"The Tightrope between Coercion and Seduction": Characterizing the Ethos of Addiction Research at Lexington

In "The Lesson of the Hospitals," Michel Foucault revealed an implicit contract between rich and poor governing the organization of clinical experience.

> But to look in order to know, to show in order to teach, is not this a tacit form of violence, all the more abusive for its silence, upon a sick body that demands to be comforted, not displayed. Can pain be a spectacle? Not only can it be, but it must be, by virtue of a subtle right that resides in the fact that no one is alone, the poor man less than others, since he can obtain assistance only through the mediation of the rich. . . . [I]t is just that the experiences of some should be transformed into the experiences of others. (1975, 84)

What was the utility of the rich offering help to the hospitalized poor? Foucault answered that the diseases of the poor were transformed into the knowledge of the rich: "[The clinic] is the *interest* paid by the poor on the capital that the rich have consented to invest in the hospital; an interest that must be understood in its heavy surcharge, since it is compensation that is of the order of *objective interest* for science and of *vital interest* for the rich" (1975, 85). The present chapter weighs how heavy that surcharge was for those who participated in the experiments of the ARC—from the perspectives of those who structured the experimental situation and those who paid the price as subjects. What were the lessons of the laboratory housed at the Lexington Hospital? What were the situated ethics, indigenous moralities, and laboratory logics at work there? What lessons does Lexington hold now that research is no longer conducted in fed-

eral prisons but is rampant under the far less controlled conditions of large-scale clinical trials?

This chapter takes an in-depth look at the social organization of knowledge production during the heyday of the world's premier addiction research unit. By distinguishing between the ethos of the laboratory logics—the actual practices, protocols, and spirit with which researchers approached human subjects—and the abstract, codified ethics supposed to govern laboratory life, it is possible to characterize the modes of perception organized at the ARC in relation to the prevailing indigenous moralities of the addiction research problem group.[1] Research networks are key sites for exerting formal and informal social controls over research. Members must adjudicate among "moral traditions for handling investigatory risk" (Halpern 2004, 41, 124). The ARC conducted research on human beings from 1935 until 1974, decades when there was both change and continuity in the conduct of human experimentation. How did laboratory logics, knowledge production practices, and ethical stances evolve at the ARC? What was the relationship of indigenous moralities to the practical logics through which scientific work of the kind the ARC conducted was made possible?

TOWARD A SITUATED ETHICS: SPECIFYING THE INDIGENOUS MORALITIES OF THE ADDICTION RESEARCH NETWORK

Some of the technologies pioneered at the ARC found their way into continuing clinical use; others were consigned to the dustbin of history.[2] The laboratory logics of Lexington were based on systematic attempts to mimic the "natural" course of events in which people encounter, use, build up tolerance to or dependence on, and withdraw from mind-altering drugs, so as to expose the underlying basis of the process. These "natural" conditions were, of course, social conditions that exceeded capture in the laboratory setting. The logic of unmasking the underlying conditions was akin to the clinical logic of nineteenth-century heroic medicine, with its high dosages and purgatives. As shown in the preceding chapter, high dosages and intravenous administration were used to mimic the natural course of addiction. Although the physiological process through which subjects progressed blurred the line between mimicry and actuality, "experimental readdiction" reenacted initiation, tolerance/dependence, and withdrawal. Because many addicts avoided the aversive effects of withdrawal, only those willing to endure it "volunteered" for studies. If researchers failed to document highs or lows, they believed their data would

be useless for revealing the basic mechanisms of addiction and for applied studies on the abuse liability of new compounds. Answering basic research questions required human subjects to reenact the very physiological and psychological conditions that landed them at Lexington. Experimental readdiction was addiction itself, not mimicry.

Taking seriously the sociological position that drug use varies according to social setting and cultural context—and is modulated by social norms, beliefs, rituals, and meanings—requires not conflating what was happening with experimental readdiction in the laboratory with what was happening on the street. The setting of the research ward, the expectations of researchers and subjects who framed their activities as the investigation of a scientific problem, and the metrics and technologies developed at the ARC worked against any simplistic reenactment of addiction's "natural course." In the research ward, skilled medical personnel who were extraordinarily familiar with opiates were on hand if anything went wrong with dosage or route of administration, and subjects who were ill were treated for their illnesses and never experimented on. Dosage and purity were carefully calibrated at the ARC, things that could not be achieved on the illegal market due to criminalization, prosecution, and the sanctioned ignorance of most physicians in relation to nonmedical use of drugs. No matter how the ARC tried to replicate "natural conditions," its social location mitigated against it doing so.

Unlike the conditions of the street, laboratory conditions required some version of informed consent. Most studies recounted in this chapter were produced during the long prehistory of informed consent, which was a historical product of the late 1960s and early 1970s (Goodman, McElligott, and Marks 2003, 4). Focusing solely on current concepts of informed consent rides roughshod over the many other social controls over human experimentation that evolved in the early to mid-twentieth century from a logic of "lesser harms" (Halpern 2004). During the Progressive period, courts and legislatures considered the issue of informed consent but did not specify its form (Halpern 2004, 97, 102, 117). Prisoners who participated in research were regularly asked to give consent as early as 1915 (Harkness 1996, 3), and parents were asked to sign consent forms when children participated in vaccine trials as early as the 1940s and 1950s (Halpern 2004, 113). However, until research sponsors began to require written consent statements in the 1950s, consent was obtained nonsystematically. Even after sponsors began consulting lawyers on language and sharing this information with each other (Halpern 2004, 100–101, 117), a "disorganized situation" permeated all scientific research on human subjects and

lasted well into the 1960s (Moreno 2001, 200). Because there was evidently wide variation between sites, the indigenous morality of the ARC research group can best be glimpsed by looking at actual practices accepted there.

Research at Leavenworth and Lexington involved a high degree of participation by knowledgeable subjects, prisoner patients who appear to have been quite aware of what was happening to them. As interviews with Himmelsbach indicated (1972, 1994), they were not "unwitting" subjects. The indigenous morality of the ARC placed such importance on consent that a brief note on voluntary participation appeared in many of its scientific publications from its inception. Participants volunteered for studies through the same routes they volunteered for work assignments, vocational education, or recreational activities. "There was no dearth of people who wanted to be subjects," Conan Kornetsky recalled of the late 1940s and early 1950s (personal communication with the author, July 28, 2006). However, questions of what motivated people to volunteer and whether true voluntarism is possible in coercive prison or military contexts contribute to the murkiness of the ethos at Lexington.[3]

One factor relevant to addressing questions of voluntary participation at the ARC was that much of the public understood addicts to have already willfully risked drug exposure outside the laboratory. Among American publics, acceptance of voluntary risk is higher than acceptance of involuntary risk (Halpern 2004, 97). The prisoner patients at Lexington could easily avoid taking on experimental risk—most neither participated in experiments nor were recruited to do so. The only known research participant still living, Eddie Flowers, estimated that less than half of residents even knew of the research program (2004). Far fewer participated in it, and those who did volunteer generally appear to have sought out the opportunity. Flowers's six months of research participation came about through word of mouth in the mid-1950s.

> There was a guy there by the name of Red [Rodney] . . . [who] shared with me, 'cause he didn't share that with a lot of other people, about the fact that he was in this drug program in Lexington, Kentucky. He kind of like laid it out to me, that they'd take him out of the main population for two or three weeks, and they'd try different drugs on him, and then they'd pay him off in heroin, 'cause that was his drug of choice. . . . [T]hrough his finagling, I was able to get in. . . . [T]hat's when I began to be a part of that whole experimentation thing. (2004)[4]

Whether or not one should refer to them as volunteers—and I think there is good evidence that they were—subjects became part of the program largely because they sought access to drugs to break their everyday routine. They were

self-identified "dope fiends" (so Flowers referred to his younger self), whose short-term goal was to get high regardless of the long-term consequences. Their prime objective dovetailed with the ARC's scientific goals, which could only be met by studying serious, seasoned, long-term opiate addicts with a tendency to relapse. Most of the ARC's subjects had been admitted to the institution multiple times before participating in experiments. Subjects possessed a range of extra-institutional drug experiences, and a range of treatment "failures." The ARC capitalized on subjects' familiarity with drugs from the category under study, and to my knowledge, never subjected drug-naive subjects to the administration of experimental drugs.

Within the indigenous morality of the ARC, knowledgeable former drug users were considered not only the best source of comparative data but the only ethical subjects. I do not mean to suggest that the ARC's laboratory logic of mimicry would be considered ethical today, since elements of it undoubtedly would not pass our era's scrutiny. For example, in a classic, 1948 ARC study that no institutional review board would now approve, Abraham Wikler set up a single-subject study of "self-regulated experimental re-addiction to morphine." The subject could ask for and receive "by any route (administered by an aide or by himself) any drug in any amount (up to a 'ceiling' judged safe by the experimenter) at any time of day or night for an unspecified period of time which, however, would not be less than one month" (Wikler 1972, 9–10). Wikler retrospectively reported on the study, which was published in 1952 (1952d):

> [T]he subject would be informed one month in advance of the termination date of this agreement. It was stressed that the experimenter had no interest in the subject's getting himself "hooked," but if he should, the experimenter would advise on how the subject might withdraw himself from whatever drug he was taking. The subject assured the experimenter that he would not get "hooked," and elected to take 30 mg of morphine i.v. as his first dose. (1972, 11)

Following the natural course of readdiction with this subject, Wikler elicited free associations and recorded the subject's manifest dream content. Morphine's euphoric effects were displaced by its dysphoric effects within a few days. The subject rapidly ascended to extraordinarily high doses of morphine, denied that he feared withdrawal and did not seek to avoid it, and elected to withdraw cold turkey in the end, apparently having done it before.

This study was extremely significant for the formation of Wikler's conditioning theory, which remains a touchstone in neurobiological investigations, which now define addiction as a chronic relapsing brain disorder (see chap. 8

in the present book). Wikler suggested that the social practice of "hustling," defined as "operant behavior directed towards obtaining opioids," was reinforcing in its own right. This conclusion "furnished a basis for construction and testing of a 'conditioning theory of drug dependence and relapse' in animals" (Wikler 1972, 11). Although supplemented by animal studies, Wikler avers that he worked out his conditioning model by observing and interacting with just one subject. From listening closely to his subject's perceived needs and cravings, dreams and desires over an extended period, Wikler gained what he considered a deeply grounded sense of the gratifications and necessities that motivated his subject to "self-regulate." This experimental design was unusual even in the context of Lexington, inasmuch as the subject himself helped conceptualize the study, was extremely articulate about his dreams and experiences, and was able to convey a great deal about the social worlds of "hustling" in a way that Wikler could translate into the language of operant conditioning. Wikler's well-recognized immersion in his work and his ongoing proximity to the subject were among the conditions of possibility that led to the emergence of conditioning theory at Lexington.

Researchers base moral judgments as well as scientific interpretations on local knowledge they derive from their familiarity with the materials and technologies used in their scientific and therapeutic practices. Thus scientific debate tends to take place through disputes over tools, techniques, and research design just as much as disputes over definitions, theories, or ideas. The "technical character of disputes over local knowledge" tends to mask not just the disputes' moral content (Halpern 2004, 124) but the way in which the requirements of political ideology converge with the requirements of medical technology (Foucault 1979, 38). Certainly, the ARC researchers were intimately familiar with the actions and effects of the potent compounds supplied them. The technical character of their published research rarely allowed them to air their moral or political views and obscured any record of scientists' attitudes toward their subjects. Evidently, they viewed their subjects as a means to advancing the understanding of how addiction worked, but they seem also to have appreciated their subjects as individual human beings (something a large fraction of the American public remains unable to do when it comes to drug addicts). Researchers' perspectives and modes of perception diverged from those of their subjects. After all, these researchers literally held the keys to unlock the secrets of their subjects' lives. Invariably, researchers I interviewed insisted that subjects volunteered in order to make their lives meaningful or "give something back" to society. Many researchers recognized that even altru-

ism may be construed as moral coercion, acknowledging that they "walked the tightrope between coercion and seduction" at the ARC. They were, after all, administering drugs to people who liked them and who would do just about anything to get them. Through their performance of ethicality, addiction researchers attempted to distance themselves from accusations that they were enticing, seducing, coercing, or coddling addicts.

Boundaries of social class in particular separated researchers from their subjects; subjects were court-mandated to Lexington and were literally captive there. The researchers were almost entirely white, upper- and middle-class professional men who experimented on poor, lower- and working-class, ethnically and racially diverse addicts. Although most research participants were white, there is no doubt that the poor were exploited for the scientific purposes of the dominant social classes who were identified with the U.S. government. The production of scientific knowledge was an exercise of social power and privilege—it was extractive, however well-intentioned or scientifically "enlightening." Flowers later said: "I began to come to grips with the fact that I was used. Let me put it that particular way. . . . I kinda like got in touch with being taken advantage of . . . because I was a dope fiend. And being a dope fiend, I used dope! . . . They used my ass and took advantage of me. . . . Back then at that time for a while there I was angry, bitter and so forth. A little further down the line, I kind of chalked it up as a bad experience." Uninterested in the consent process, the forms he signed, or the information given, Flowers was focused on the "payoff," the drug rewards that were given to participants as in-kind payment up to 1955. He characterized researchers as exploiting his vulnerability to drugs: "I was very vulnerable, . . . in the sense that if it's about drugs, I wanted drugs, okay? I recognize that not only just myself but some other people were thrown into a situation, was used, was paid off with what we as drug addicts craved—drugs. I see it from the perspective that it was wrong. It should not have happened" (2004).

Flowers's retrospective account was mediated through the lens of his later participation in drug treatment and adoption of recovery as a way of life. A pivotal moment came during his testimony before the congressional investigation that followed on the Tuskegee study, when he first heard allegations that the ARC's research was part of a project of the Central Intelligence Agency and military intelligence. He sustained a lifelong eye condition that he subsequently attributed to a hallucinogen administered at the ARC. He stands as a rare—and highly credible—witness to the perspective of those whose bodies were used, quite literally, in the name of science. Flowers embodies the "fundamental and

appalling structural reality of Lexington": that, as one of the anonymous reviewers of this book in manuscript put it, "addicts who were sent to an alleged rehabilitation center for treatment were recruited for experiments that, instead of trying to wean them from their addictions, subjected them to new drug experiences and then rewarded their voluntarism by giving them free samples of the very drugs they were supposed to be giving up." This characterization rests on the assumption that rehabilitation was taking place at the Lexington Hospital, which actually offered little or nothing in the way of what we would call treatment today. Rather than condemn the experimenters, I present readers with the very ethical conundrums uncovered in bringing this laboratory to life, in order to advance historical knowledge about how research on human subjects was actually conducted prior to the emergence of the human subjects regime now in place (see chap. 6).

What seems valid to me in the preceding critique is that researchers at the ARC were insensitive to how unacceptable their work might be perceived to be beyond institutional walls. This can be illustrated by an example drawn from the animal models pioneered at the ARC and the human analogues researchers sought in order to validate their work in animals. As the previous chapter showed, researchers believed that human response to drugs varied according to the social setting, cultural context, or experimental situation.[5] Among the animal models they considered valid were spinal dogs and decorticated cats. In their quest to draw parallels between animal models and human addiction, researchers did not consider the extent to which outsiders would find animal models cruel or revolting. Their goal was to find a human analogue among Lexington residents, and they identified a so-called spinal man who had been rendered paraplegic by syphilis of the spinal cord prior to admission to Lexington. For the professional network of addiction researchers, such a cross-species analogue was an opportunity for focusing on the laboratory logics of readdiction, substitution, and unmasking. Outside the research community, the moral implications of opportunistically using such subjects as decorticated cats, spinal dogs, or the "spinal man" border on horrific. Although the goal of these studies was unmasking the basic mechanisms of addiction so as to develop more generally acceptable and effective therapeutic responses and testing the potential public health threat of new compounds, the question of just how much "interest" individual subjects paid has to be raised if we are to consider the political and moral stakes at the heart of substance abuse research. The next section considers three addiction therapies for which the ARC followed up on clinical reports: methadone, today used in medical maintenance; nalorphine

(n-allylnormorphine, also known by its trade name, Nalline), the narcotic antagonist that was a first-line response to opiate overdose prior to the synthesis of naloxone in 1960; and frontal lobotomy, no longer used to treat drug addiction thanks to studies conducted by the ARC.

CALCULATING THE COSTS OF SCIENTIFIC OPPORTUNISM: THE ETHOS OF THE LEXINGTON HOSPITAL

Originally, the laboratory at Lexington was mandated to study how the U.S. government should best deal with drug addicts, a goal apparent in Himmelsbach's initial studies refuting claims of therapeutic efficacy made by nostrum makers. By the early 1950s, the ARC was struggling to preserve its basic research program in the face of industry pressure to become a drug-testing operation.[6] By dividing the workload between Michigan and Lexington, CDAN buffered the ARC, a research site that offered something no one else could—access to otherwise healthy morphine-dependent human subjects. Such clinicians and pharmacologists as, respectively, Beecher and Seevers could not replicate the conditions of everyday life in Lexington. Alone of all research facilities in the country, the ARC had access to drug-experienced subjects and a constant stream of compounds in quantities great enough to test. These were the material conditions necessary for it to mark the scientific milestones it had by the mid-twentieth century.

The ARC's first signal achievement was the initial human testing of methadone in the late 1940s. Not until the ARC established methadone's efficacy in 1947 had an effective pharmacological agent for relieving the abstinence syndrome been identified. The names *methadon* or *amidone* were assigned to a synthetic analgesic compound developed in Germany at I. G. Farben and rediscovered in a Department of Commerce investigation of German wartime industries (Isbell, Wikler, et al. 1948). Because the ARC found that methadone produced a prolonged but mild abstinence syndrome, it was put into clinical use for managing withdrawal at Lexington in the late 1940s.[7] Subjects likened methadone to heroin, displayed euphoria when they were on it, became talkative and boastful, and attempted to get more of it. Former morphine addicts expressed satisfaction with methadone even at low doses, and their satisfaction increased with dosage increases. Judging from typical responses to the injection of methadone, the ARC concluded that "narcotic drug addicts would abuse methadone and would become habituated to it if it were freely available and not controlled" (Isbell et al. 1947, 892). They con-

cluded that methadone was a dangerously addictive drug that would become a potentially serious public health problem if not controlled.

When researchers delved into subjects' responses to single doses of methadone, they found that their respondents could differentiate what they were told was a "new synthetic drug" from other opiate drugs along an axis they called "drive," defined as the "ability of an opiate drug to produce ambition to work, to engage in games, listen to music, etc." (Isbell, Eisenman, et al. 1948, 86). When researchers pointed out that subjects actually exhibited decreased activity when on methadone, the "puzzled" subjects stated that they felt ambitious after morphine but "knew they were not" after administration of methadone (Isbell, Eisenman, et al. 1948, 86). Obviously experienced and well informed, the subjects agreed that if opiates were unavailable, they would prefer the new synthetic drug to alcohol, barbiturates, marijuana, or Demerol (Isbell, Eisenman, et al. 1948, 86). An "uninformed" control group was then formed out of a group of subjects who had participated in a study on pain thresholds. The controls could not differentiate between the effects of methadone, morphine, or other synthetic opiates, such as Dilaudid. One subject said: "That was great stuff. I wouldn't have believed it was possible for a synthetic drug to be so like morphine. Can you get it outside? Will it be put under the narcotic law? I wish I could get some to kick my next habit" (Isbell, Eisenman, et al. 1948, 88; Isbell et al. 1947, 892). This statement convinced the researchers they had an abusable substance on their hands. They sounded the alarm in the publications that introduced medical professionals to methadone.

Methadone is a long-acting opiate that can be dangerous in cumulative doses. This danger was unknown until the ARC responded to accidental "methadone poisoning" (overdose) in two subjects of a large methadone study consisting of 110 white men and 15 African American men. Two African American men went into comas after being administered cumulative doses of twenty milligrams of intravenous methadone. Both subjects were particularly susceptible to methadone, since others had received similar doses without getting into trouble. Having become cyanotic, they were on the brink of death after failure of the standard responses, artificial resuscitation and Nikethemide. Realizing opiate overdose might be reversed by a narcotic antagonist, researchers reached for a bottle of Nalline (nalorphine or n-allylnormorphine) supplied to them by Merck. University of Illinois pharmacologist Klaus Unna had discovered in 1943 that nalorphine antagonized most of morphine's actions in experimental animals. Two previous attempts to use nalorphine as an antidote had been reported in the clinical literature with equivocal results: one case reported

death due to shock; the other patient revived. Thus the question of whether or not clinicians should employ the drug in cases of overdose was still open (Addiction Research Center 1978, 42). In the ARC cases, the researchers reported that the administration of nalorphine "apparently induced spectacular and, possibly, life-saving effects." They explained: "Unless N-allyl-normorphine had been given, one would have expected that both patients would have remained in coma, with depressed respiration, for at least several hours. In fact, if N-allyl-normorphine had not been available, both patients might have died" (Fraser et al. 1952, 1206). Once safe and effective dosages were worked out, methadone was put into clinical use at the Lexington Hospital to ease withdrawal, and there was never, to my knowledge, another overdose incident involving it.

The streak of opportunism that characterizes the will to knowledge was in healthy evidence at the ARC. After the overdose incident, the researchers followed up by studying nalorphine in spinal dogs (Wikler and Carter 1953). At a CDAN meeting on January 22–23, 1954, Isbell stated his intent to "get some patients pretty depressed with morphine and then come in with the Nalline" (Committee on Drug Addiction and Narcotics 1954a, 852).[8] Once it was found useful to combat opiate-induced respiratory depression in newborns and diagnose active addiction, Nalline was used as a rapid diagnostic tool for determining if a person was in fact addicted to opiates (Isbell 1953, 1954). The compound antagonized narcotic effects and unmasked the underlying physical dependence that Wikler, Fraser, and Isbell (1953) believed appeared early in the process of addiction. The nalorphine story illustrates the ARC's resourceful use of whatever substances, situations, and subjects were ready to hand. However, the press of the time represented such resourcefulness not as heroism but as barbarism.

Journalistic accounts of the ARC portrayed "guinea pig volunteers" rewarded in drugs. For example, a 1951 account by reporter Edward Mowery that appeared in a *New York World-Telegram and Sun* series on heroin in Harlem showcased Lexington: "We headed for the research unit of Narco, where gruesome experiments on voluntary guinea pig patients are conducted around the clock by scientists charged with establishing the addiction propensities of new drugs. In this 12-bed laboratory was discovered the potency of Demerol and methadone and the established fact that large doses of barbiturates cause withdrawal convulsions and hallucinations."[9] Mowery described the readdiction of "confirmed addicts beyond rational help" in an experiment with n-allylnormorphine, which he identified as "the best antidote yet devel-

oped in treating poisoning by morphine and other opiates." Although addicts "get no bang from it," a doctor explained to Mowery, they begged for more even after "doses which pharmacologists regard as astronomical." The doctor continued: "[T]heir reward for undergoing this unspeakable agony and possible death, is a grain of morphine for each month of the test or days off their sentence. These souls never waver in their choice. It's morphine." Such portrayals sensationalized the science and dealt with human subjects in a cavalier manner.

Even if such stories badly distorted the scientific work of the ARC, selected instances of scientific opportunism verged on preying on the vulnerable. It is important to distinguish between such instances rather than issuing a blanket condemnation from a presentist point of view. There is debate among historians about how to characterize such treatments as frontal lobotomy, which was considered therapeutic for schizophrenia and intractable pain in the 1950s (Dynes and Poppen 1959; Hamilton and Haynes 1949; Mason and Hamby 1947). Lobotomy was adopted partly because it solved certain problems of social control faced by asylum superintendents (Pressman 1998). A handful of clinical observers maintained that lobotomized addicts no longer suffered the pain of narcotic withdrawal but were in no position to measure the abstinence syndrome or to establish controls (Mason and Hamby 1948, 1039). The ARC researchers feared clinicians would come to invalid conclusions and start lobotomizing addicts out of ignorance.

Skeptical that lobotomy was therapeutic, the ARC conducted a study on whether or not it prevented the pain of withdrawal, using the tried-and-true methods through which they had studied the typical progress of the abstinence syndrome (Andrews and Himmelsbach 1944). The ARC researchers knew that predictable signs of the abstinence syndrome were "fairly reproducible in any given person," although their intensity might vary (Wikler, Pescor, et al. 1952, 515). When they learned that four subjects from Kolb Hall, the neuropsychiatric facility for nonaddicts that was also located on the grounds at Lexington, had been recommended for therapeutic frontal lobotomy, they decided to undertake an experiment.[10] Three schizophrenics and one sufferer of phantom limb pain whose arm had been amputated in a childhood accident underwent the procedure (Wikler et al. 1952). Injured in a railroad accident as a child, the latter was a forty-eight-year-old white man who had used morphine, heroin, and Dilaudid for decades. He also had undergone electroshock treatments and methadone substitution therapy in vain attempts to relieve phantom limb pain. Some of these worked for short periods, during which he was aware of the

missing limb but not of the pain; but he always returned to Lexington readdicted. The three schizophrenic subjects had no previous history of drug addiction but had been unresponsive to any previous treatment.

Before the surgery, the three schizophrenic subjects were stabilized on morphine and put through a "test withdrawal," to establish a baseline against which the same procedure, repeated after the lobotomy, could be compared. After the lobotomies were performed, the same withdrawal procedure was performed on each subject. The subject suffering phantom limb pain was treated differently: he failed to show any effects from his first lobotomy, so a second was performed. He then resumed work as a railroad payroll clerk, and nine months later, he was reportedly no longer asking for narcotics or exhibiting "concern over his condition" (Wikler et al. 1952, 3). Still, Wikler wrote, "[F]rontal lobotomy should not be considered as a generally desirable treatment for drug addiction per se, since it is not yet clear that the deficits consequent to frontal lobotomy are to be preferred to the problems associated with narcotic addiction" (1951, 163). The ARC researchers thus attempted to hold clinicians back from adopting frontal lobotomy as a treatment for drug addiction. Although they came to what we would now think of as an enlightened position through the lobotomy study, their use of human subjects in the manner described clearly raises ethical questions: Did they go too far, or were they playing a corrective role in helping base clinical practice on evidence rather than on a speculative surgery that resulted in lifelong low affect for its subjects? Did preventing wider adoption of frontal lobotomy save large numbers of narcotic addicts from the knife? Does that warrant the sacrifice these four subjects ended up making?

Many readers will be appalled on discovering that lobotomies were not only performed at Lexington but systematically and intentionally studied there in the manner described. Ethical lines were blurry in the lobotomy study: the schizophrenics had never been addicted to opiates before they were experimentally addicted (not once, but twice) and forced to undergo withdrawal (not once, but twice). Second, who can say whether or not the requirements of mental competence we recognize as so essential for informed consent today were met? Turning to the sufferer of phantom limb pain, there is the matter of offering more than palliative care to someone who had lived most of his life in intractable pain—perhaps even holding out the hope of "cure," and the possibility of a life free from pain. Finally, there is the question of whether or not the study directly benefited or enhanced the health and well-being of anyone involved. My purpose here is not to pronounce judgment retrospectively but to

clarify what the lobotomy study meant in the context of the laboratory logics and indigenous moralities of the ARC.

The lobotomy study enabled investigators to elaborate further on basic mechanisms that otherwise could not be seen. They believed that the morphine abstinence syndrome worked to unmask homeostatic mechanisms developed by the nervous system and the pituitary-adrenal system to adapt to repeated administration of opiates. They observed that former addicts quickly built up tolerance to extremely high doses of morphine-like drugs. By putting these experimental subjects into abrupt abstinence, the researchers attempted to unmask the underlying mechanisms they sought to elucidate. Studying the contrast between the schizophrenics, who had never been "naturally" addicted, and the intractable pain sufferer, who had been a regular user of opiates for decades, was a route to show that physical dependence was not "synonymous with 'addiction,' since none of the schizophrenic patients exhibited interest in, or craving for, morphine at any time during that study" (Addiction Research Center 1978, 50). By differentiating between the "purposive," or symbolic, aspects of craving and abstinence and the "nonpurposive," or nonsymbolic, aspects of it, the investigators established that changes during abstinence were "independent of symbolic significance" (Addiction Research Center 1978, 51). The study showed that although users might be "'conditioned' to meaningful stimuli," drugs were devoid of symbolic value to the lobotomized schizophrenic subjects. The experimental situation was set up to unmask conditioning by stripping away desire and symbolism, leaving only "objective" signs of abstinence. The subjects showed the lack of reactivity, or low affect, that typically followed lobotomy. Subsequently, the ARC did not recommend lobotomy or do further work involving it.

As a thought collective, the ARC played a corrective role relative to clinicians, whose ideas about what might be therapeutic were indicated by individual case reports. A compelling example of how haphazard clinical practice could be was provided by University of Michigan pharmacologist Edward J. Domino, who dramatically described nalorphine as the drug that drew him into neuropsychopharmacology.[11] During Domino's internship, he was on a cancer service where an experimental opiate, Dromoran (levorphenal), was being tested on the terminally ill.[12] When a breast cancer patient went into serious respiratory depression and became comatose after small, therapeutic doses, Domino speculated that he could revive her with nalorphine, which he had used to revive overdosed dogs during demonstrations in the medical school at the University of Illinois, where Klaus Unna had first studied the

pharmacology of the drug. Domino recounts: "While [I] ventilated the patient, a nurse called [across town] for the nalorphine to be brought. When it arrived, [I] broke the vial and injected it. I'll never forget it. . . . [I]t was remarkable. She was totally comatose and, then all of a sudden, I gave her the nalorphine and she started to breathe." Learning that the patient's cancer had interfered with her liver processing, Domino realized that cumulative doses of Dromoran had poisoned her. "[B]ut, in addition," he observed, "I saved her life" (1995, 5). This defining moment attests to the casual nature of the social organization of clinical research in the days prior to clinical trials, databases, and registries for adverse drug reactions.

Only the ARC was in a position to do the systematic, controlled studies that built up decades of baseline data by the early 1950s. Methadone's profile of action was established, and it remains integral to treatment today; nalorphine was discovered to work as a lifesaving therapeutic intervention; and frontal lobotomy never came into vogue as a treatment for addiction (when it might easily have). In each case, ARC researchers opportunistically availed themselves of particular subjects whose conditions shed light not only on the particular problem at hand but on the underlying dynamics of drug dependence—tolerance, abstinence, and presence or absence of desire for the drug. They wanted to see what was left once desire was stripped away, and they saw the drugs they studied as tools for doing so. How shall we retrospectively calculate the price that human subjects paid in the methadone, nalorphine, or lobotomy studies? On balance, were the studies beneficial to those very individuals or only to those who have benefited since? As the evidence on which the ARC cautioned clinicians, these studies diverged from the ARC's regulatory role.[13] The studies led to clinicians ending the practice of abrupt withdrawal, for the ARC urged methadone substitution and gradual tapering off across a ten-day period (Fraser and Grinder 1953). Clinicians now had an effective response to overdose, nalorphine and, later, naloxone, each of which were later evaluated as possible therapies for addiction. The calculus of suffering was distinctly weighted toward the greater good for the greatest number. Individuals who suffered lobotomy or overdose endured unspeakable trauma; others benefited from the knowledge thus obtained. Unresolved questions remain: Did the twice-lobotomized amputee live out his life pain-free or "drug-free"? Did the two subjects revived by nalorphine know what their near-death experiences meant? Where should U.S. government responsibility for aftercare in the case of long-term effects from research of this kind begin and end? How shall we calculate the moral and social costs of scientific opportunism?

THE DEMISE OF PATIENT-ORIENTED RESEARCH AND THE RISE OF CLINICAL TRIALS

Most of those who worked at the ARC saw themselves as basic scientists who happened to work in a clinical setting. Aware that drug responses varied in terms of individual susceptibility and psychological effects, they documented the range of individual variation but aimed to specify the common neurophysiological pathways that lay along the road to addiction, withdrawal, and relapse. They did this by working closely with subjects and by designing their studies to take advantage of their relatively unfettered access to subjects. In her study of research on a hospital ward in the 1950s, *Experiment Perilous: Physicians and Patients Facing the Unknown,* medical anthropologist Renee C. Fox tells of the "sort of investigation that entails moving back and forth in both directions, between the clinical bedside and the laboratory bench; that involves patients as subjects; and that is directed toward finding more effective modes of diagnosing, treating, and preventing the diseases and disorders from which its patient-subjects suffer" (1959/1998, 259). Although the Metabolic Group, which Fox studied, operated in a very different experimental setting from the prison-hospital at Lexington, the kind of patient-oriented research Fox described was akin to that of the ARC. Patient-oriented research differed from the pedestrian drug trials organized elsewhere, which really did employ human subjects as little more than guinea pigs.

The clearly demarcated division between the clinical and research units at Lexington was reinforced by institutional routines and practices. Researchers were not responsible for treating patients or delivering medical care, although they did monitor subjects living on the research ward. By contrast, Fox's metabolic researchers separated "laboratory life" from the practice of "real medicine." One of her subjects stated, "If you listen for it, you'll hear one or another of us saying, 'How long can I live this laboratory life anyway? I've just got to get back to *real* medicine'" (1959/1998, 27). At the ARC, the value was reversed; the real action was in the lab, which was buffered from the frustrating realities of the rest of the institution. The futility of standard treatment methods was evident to everyone associated with the place. An oral history given in 1970 by Earl Chestang, a thirty-one-year-old trainee of a Detroit methadone clinic, recounted taking three trips to Lexington, beginning in 1959.

> Most addicts knew Lexington wouldn't work the way it was set up at the time, because that place was exactly what Walter Winchell said it was in the '50s. He had only one thing to say about it, it was a multi-million dollar flophouse for

> junkies. That's what he called it, and that's what it really was, and all the addicts knew it, and it seems like the professional staff must have known it. It really was of no help to a guy unless he was right there in the institution. (1970, 10)

Chestang described encountering among his fellow patients an addicted physician who was "one of the worst addicts I ever saw in my life." The physician patient was at Lexington for the second time, in an attempt to reclaim his medical license. He claimed to have become addicted as part of a self-designed experiment to prove to his patients, who kept begging for narcotics, that they were "just weak, immature individuals" (1970, 45). Physician addicts were a regular feature on the wards of Lexington. They often volunteered for studies, sometimes staying on the research ward to record data or do other low-level tasks related to the studies. A physician patient befriended by Eddie Flowers committed suicide by throwing himself down the spiral staircase at Lexington (2004). These examples point to the overall lack of individually tailored or even appropriately specific treatment at the "multi-million dollar flophouse for junkies."

Many patient-inmates experienced the clinical staff at Lexington as uncaring and nontherapeutic, due to lack of direct contact between staff and patient-inmates. A thirty-eight-year-old African American male from Detroit who went to Lexington voluntarily in 1967 criticized the clinical program: "It lacks the type of atmosphere that would motivate, I think, anybody, any addict." He described staff as "people doing their nine to fives and their eight-to-four-thirties, going about their business" (Hall 1970, 5, 18). He explained: "Periodically, you would go before a doctor, and he would do a sort of in-depth interview with you, find out as much as he could about your background, psychiatric interview or something, but there was no closeness. Everything was done on a sort of vast scale. You never really got the feeling that you were part of a drug program and you were going to be helped with your drug problem" (Hall 1970, 18). Decades later, Flowers affirmed that a similar situation prevailed when he was there in the 1950s: "Nobody got no treatment. We didn't go to no group therapy. We didn't go to no individual therapy. We didn't do nothin'. Worked on the job down there, but [there was] nothing in the way of dealing with the individual and addiction. There was no program" (2004). Living in the general population contrasted to participating in the close-knit research ward, where subjects were paid a good deal more individual attention than clinical staff were able to pay to other patients. The benefits of such consideration must have been considerable for some research subjects, even though they knew that the research unit was not trying to "treat" them.

Basic science—or "nontherapeutic" research—was elevated over treatment at the ARC. That distinction proved the ARC's undoing when it became politically necessary to show how research directly benefited individual subjects. Having staked its claim in the making of science, the ARC research program was defined as "nontherapeutic." Unlike many clinical trials today, there was no pretense that individual subjects were being offered therapy—much less a "cure"—for what ailed them. The sociological effect of this situation was that researchers gained social status while clinicians occupied a lower position within the institutional hierarchy. Addiction was experienced as an "intractable" illness—when hopes that research would find a "cure" were alive, addiction was constructed as an acute condition, rather than a chronic, relapsing one. Administrators and clinicians were to explain relapse rates, while scientists were to study them, in hopes that relapse would reveal what caused addiction in some individuals and not others.

Because the treatment of the time was largely ineffective, relapse rates fell periodically under review. Eighty percent or more of Lexington patients relapsed after release. Thus there was a divorce between "successful" researchers and the "custodial" clinicians. This distinction became more pronounced even as higher-caliber clinicians arrived with hopes of studying treatment efficacy, for they voiced frustrations with lower social status and complained to external reviewers (see chap. 6 in the present book). As with the clinical researchers about whom Fox wrote, distinctions between research and treatment solidified at Lexington for four reasons. First, there was a congeries of uncertainties concerning the underlying biochemical and physiological mechanisms of the disease process, chemical compounds, administrative procedures, methodological techniques, clinical or "nonexperimental" aspects of diagnosis, treatment, and the course of disease. Second, everyone involved in research recognized the limitations of therapy. Third, there was the sheer difficulty of locating, recruiting, and maintaining subjects in the study (by contrast to clinicians being overwhelmed with patients). Fourth, there was social conflict between research and therapy. The social organization of Lexington produced two cultures, and over time those who worked in research and treatment began to see themselves at odds with one another.

The day-to-day corridor talk and staff meetings of the ARC have vanished from the historical record. What strategies did researchers use to maintain clinical distance from their subjects? How did they deal with moral uncertainties generated by the fact that they were administering to human beings unknown drugs of unknown potency with unknown effects? How did they respond to the

certain knowledge that they lacked effective treatment or to knowing precisely how agonizing withdrawal can be? These questions must have encroached especially on those who were physicians. As Fox found with the physician researchers of the Metabolic Group, coping mechanisms were "group-patterned," involving a pattern of "ritualized optimism" about the potential that basic research might yield therapeutic innovations (1959/1998, 135, 277). Such optimism was ironically based in social distance between research staff and clinical staff, meaning that researchers did not come into contact with the vast majority of patients—only with the self-selected few who participated in studies. This is similar to what we can infer happened at the ARC. To this day, researchers recall friendships with participants or remember with sadness departures of participants to whom they had grown close. Strangely, an ethic of care seems to have pervaded the ARC researchers despite the barriers of class, creed, and sometimes color between them and their subjects. The differences between the researchers and their subjects were in many ways narrower than they are in the clinical trials of today. Clinical trials now take place at increased social distance among primary investigators, researchers, staff, and participants. The scale of the studies alone works against the formation of an ethic of care and the social bonds that go with it. The latitude for exploitation of vulnerable human subjects in clinical trials is great, a topic to which I turn in the concluding chapter of this book. The next section of this chapter lays the groundwork for understanding human subjects regulation.

REGULATING HUMAN SUBJECTS: THE EMERGENCE OF A NEW REGIME OF GOVERNANCE

Federal human subjects regulation sprang from the military public health apparatus in the early 1950s, when the Armed Forces Medical Council established a policy "for the use of human volunteers (military and civilian employees) in experimental research at Armed Forces facilities" (quoted in Moreno 2001, 172–73). Pentagon policy TS-01188, modeled on the Nuremberg Code, was signed by Secretary of Defense Charles E. Wilson on February 26, 1953, but its top secret classification limited its impact. The U.S. Army's Office of the Surgeon General also adopted the Wilson policy in 1954 (Moreno 2001, 243). The policy supposedly applied to extramural clinical research contractors, but there was actually no education, enforcement, or follow-up. When Army Regulation 70–25 restated the policy in 1962 and the U.S. Army inserted the "Principles, Policies, and Rules of the Surgeon General" into its contracts,

contractors seemed to have no knowledge of the previous version (Moreno 2001, 179, 243).

Resistance to the army's attempt to impose a set of "rigid rules" based on the Nuremberg Code was especially strong at Harvard University, where Henry K. Beecher drafted alternative rules that became known as the Beecher-Army Compromise (Moreno 2001, 243). Although it is now accepted as the ethical basis for governing human subjects research, the Nuremberg Code was contested by Beecher and others throughout the 1950s, due to perceptions that it was so rigid as to prevent human experimentation altogether and severely restrict investigators' autonomy. Beecher doubted that most subjects understood science well enough to give truly informed consent, and he did not believe that a priori rules could be laid down to govern clinical research. Drug researchers considered full disclosure of the drug under study counterproductive due to the placebo effect. An example of a study in which full disclosure would be counterproductive was suggested by Isaac Starr, who chaired the 1954 CDAN meeting: "Since many people in this country had been taking small amounts of sedatives over long periods of time, [I] would like to see studies initiated on the withdrawal of barbiturates. Would these individuals develop abnormal behaviors as soon as the barbiturates were stopped?" Starr cautioned the committee that valid results would be obtained only if the sedatives were withdrawn without patients' knowledge (Committee on Drug Addiction and Narcotics 1954a, 693). Other participants at the first 1954 CDAN meeting expressed similar concerns about the validity of studies where subjects knew what was happening to them. As shown in previous chapters, Beecher's laboratory and the ARC favored making subjects aware in most situations but cautioned that results could be affected by the experimental setting, the observer's presence, and the questions asked. Theirs was a contention over how to interpret the meaning of awareness for the experimental subject. This was an ongoing contention over whether knowledgeable or naive subjects were best suited for the types of studies undertaken in these research sites.

Better placed than those at the ARC to inscribe views in public policy, Beecher traveled to the Pentagon with a delegation from Harvard Medical School in 1962. There, he offered a compromise that avoided the strict language of the Nuremberg Code and instead established flexible guidelines that retained the cultural authority of biomedical and clinical researchers. The academicians left assured that the new regulations were simply suggested guidelines. No one from the ARC was personally involved in the tussle over the form that human subjects regulation was to take. However, the ARC was implicated

in the debate because the Army Chemical Corps was then widely involved in drug research on LSD, mescaline, and other substances that were also studied at Lexington. Army Regulation 70-25 contained exemptions that enabled military researchers and contractors to avoid full disclosure if they thought it would invalidate experiments (Moreno 2001, 244). Disclosure exemptions also applied to "ethical medical and clinical investigations" that were of potential benefit to subjects—as Moreno points out, a tautology at best (2001, 244). The exemptions allowed investigators to decide how much to tell subjects when they assumed the research directly benefited subjects, but it forced full disclosure in instances of indirect benefit. While today's process of informed consent vests the power to discern benefit in the subjects, the nascent regime offered in the Beecher-Army Compromise placed it in the hands of researchers. This compromise over how fully informed subjects had to be in order to meet standards of informed consent was reinterpreted in due course during the events discussed in chapter 6 of the present book, particularly the 1975 congressional investigation of research conducted by the Department of Defense and the Central Intelligence Agency.

Scholars who have brought the historical sociology of bioethics into being have skipped over Beecher's scientific work as a source of his preoccupation with ethics (cf. Moreno 2001, 242). As shown in chapter 4 of the present book, Beecher's clinical logics and his concern with securing high social status and continued funding played a role in his performance of ethical subjectivity. When, in 1966, Beecher published the landmark papers that brought attention to what Moreno has dubbed the "'homegrown' American ethics scandals" (2001, 247), Beecher's involvement with military and intelligence contracts went unmentioned. Deeply invested in guaranteeing that the virtues of the individual investigator would secure ethical practice, Beecher served his own interests by emphasizing the need for continued professional autonomy and the prerogative power of professionalism.

The military was the first source of human subjects regulation, but in 1966, a second stream of regulation issued from the NIH, then and now the main U.S. government sponsor of health-related research. NIH director James Shannon pushed for standards of informed consent and for review committees that consisted of not just professionals but members of the public. Although a uniform policy to protect human subjects went into effect in 1966 (Mishkin 1993), site visits revealed uneven compliance and widespread disarray about what it meant among the research community. Consent declarations were used at many sites, but they effectively allowed even hazardous research to proceed

(Halpern 2004, 119). For the entire time that the ARC operated at Lexington, the legal climate was gray, and the rules, ethics, and customs governing use of human subjects were murky.

A series of public scandals involving unethical human subjects research catapulted human subjects research policy into public view.[14] Prison research programs were implicated in exposés of military and intelligence testing, such as those of the Army Chemical Corps research contracts on the effects of hallucinogens (Moreno 2001, 195). Contractors were supposed to include training lectures so subjects knew what to expect from LSD, but concerns that the power of suggestion would influence outcomes led to noncompliance at most sites (Moreno 2001, 256). In Army Chemical Corps studies of LSD at Holmesburg Prison in Pennsylvania, inmates and scientists quickly found themselves in over their heads: "The researchers at Holmesburg didn't know what to make of LSD's effects, and the inmates were familiar with street drugs but not hallucinogens. They also couldn't be told much about the drug, including its name, because at the time the research was classified" (Moreno 2001, 228). This clandestine LSD research network was funded by the U.S. Army and the Central Intelligence Agency through the Geschickter Foundation and the Josiah Macy, Jr. Foundation.[15] The ARC had no need of foundation support and never conducted research on "unwitting" subjects. The ARC studied development of tolerance to LSD, as well as whether tranquilizers could ameliorate the effects of "bad trips."

Also spurring stronger protection of human subjects were social movements for civil rights, prisoners' rights, and patients' rights, which changed the very nature of clinical care and medical research. Indeed, Halpern attributes the emergence of research abuse as a public problem to clashing historical sensibilities. Shaped by the experience of World War II, an older sensibility justified human experimentation as a sacrifice for the common good. Shaped by the social movements of the 1960s, a newer ethos represented human experimentation as exploitation of the powerless by the powerful. Lexington was a casualty of that clash. The rest of this chapter introduces the policy context that changed Lexington's clinical side in ways that altered both its therapeutic and research missions.

DEVOLUTION AND REVOLUTION: THE ROAD TO CIVIL COMMITMENT AND THERAPEUTIC COMMUNITIES

Ethical concerns were not solely responsible for ending human subjects research at Lexington. Two broad policy shifts in the administration of crimi-

nal justice and mental health profoundly altered institutional routines at Lexington. These changed the material conditions of the ARC well before prison research became a national issue. The first policy shift was the Kennedy administration's commitment to community mental health, which encouraged federal hospitals to concentrate on research and devolve treatment to "communities"—states, counties, and municipalities.[16] Even more consequential for Lexington was federal passage of a civil commitment policy, which responded to the evolving social consensus that drug addicts be treated more humanely than they were in jails. The U.S. Supreme Court interpreted addiction as a condition akin to illness in *Robinson v. California* (1962), opining that "even one day in prison would be a cruel and unusual punishment for the 'crime' of having a common cold."[17] Deeming it "unlikely that any State at this moment in history would attempt to make it a criminal offense for a person to be mentally ill, or a leper, or to be afflicted with a venereal disease" (*Robinson v. California,* 666–67), the Court held that the state of California could not criminalize a condition, status, or "affliction." While declaring itself to be "not unmindful of the vicious evils of the narcotics traffic," the Court found that states already possessed sufficient means to attack them (*Robinson v. California,* 665). Indeed, the Court argued that "prosecution for addiction, with its resulting stigma and irreparable damage to the good name of the accused, cannot be justified as a means of protecting society, where civil commitment would do as well" (*Robinson v. California,* 677).

Despite the equivocal results of civil commitment in California and New York, the federal Narcotic Addict Rehabilitation Act (NARA) passed on November 8, 1966.[18] Faced with the daunting task of scaling up civil commitment, the U.S. surgeon general saw Lexington and its sister narcotics farm in Fort Worth, Texas, as quick and dirty solutions and renamed each a "National Institute of Mental Health Clinical Research Center" in 1967. The bars came down at Lexington, which stopped admitting convicts and voluntary patients in favor of those committed under Titles I and III of NARA.[19] Problems surfaced in immediate response to NARA's new disciplinary approach. Implementation difficulties were the strongest contributing factor to the closure of Fort Worth in October 1971 and to the demise, in February 1974, of Lexington as an institution singularly devoted to drug addicts.

NARA introduced changes that put the ARC into a double bind. To quell fears that Lexington was releasing actively addicted individuals, research protocols had long stated that subjects would not be treated with experimental drugs within six months of release. Civil commitment shortened sentences to six

months or less, so after 1968, the ARC had to recruit subjects from the smaller pool of those serving longer sentences at federal penitentiaries, such as Leavenworth or Atlanta. Faced with a sudden disappearance of research subjects, the research unit acted to secure new sources through a memorandum of understanding (dated February 15, 1968) between the BOP and NIMH, concerning eligibility criteria for the transfer of prisoners to the ARC (National Commission 1976a). When they volunteered to transfer to Lexington, prisoners had to be at least twenty-five years old and have eighteen months more to serve.

Civil commitment brought minimum security to most of Lexington, but placed the ARC in the position of having to import federal prisoners to serve as test subjects. By the summer of 1970, the only federal prisoners left at Lexington were the ARC's research subjects. Since most people housed at Lexington were civilly committed under NARA, security was relaxed, and unauthorized departures and increased trafficking in "contraband chemicals" became common. Maximum security was maintained at the ARC to prevent such problems from tainting the research. Ironically, those who had long advocated for treating drug addicts less like prisoners and more like patients were thrust into the role of running a miniprison. Meanwhile, the rest of the institution morphed into the more open environment of a model "federal correctional institute." The difference was palpable to those working at the ARC: "What I didn't like about it was that I had to carry these keys and every morning I got locked in. The guards were friendly to me, but I didn't like all those locked doors. I went down there with bell-bottomed trousers on, a beard, octagonal clear glasses, just after seeing *Easy Rider*" (Mansky 2006). This shift in organizational culture exacerbated existing tensions between the ARC and the treatment side of Lexington, called the Clinical Research Center (CRC) after 1968 despite the fact that little research was done on the clinical side. Treatment evaluation research was widely perceived as scientifically weak, a perception that led to social antagonisms and substantive conflicts between the larger institution of Lexington and the ARC.

Despite its new name, the CRC remained engaged primarily in treatment and occasional rehabilitation, whereas the ARC was solely a research operation. Although interviewees differed about the politics of the CRC and its research potential, most mentioned antagonism due to the very different missions of the two units. An ARC researcher at the time, Peter Mansky, said:

> They were separate entities, run separately. We had locked doors and prisoner patients, or prisoner subjects. It was a very, very different experience in the

> CRC. Since I was young, I wandered around both and didn't have a problem dealing with both. But the clinical people over there weren't as welcome at the research center. Bill Martin wanted people there who were very seriously interested in research. . . . The ARC under Bill Martin was more questioning and challenging every aspect of treatment in the field, whereas the CRC had to accept some treatments as effective in order to be operative. . . . The CRC's task was to treat the people that were there and to help them stay off of substances. The ARC's task was to question all the treatments to find which were effective and which weren't, and to hopefully get better treatments over time. (2006)

This tension between the thoroughgoing skepticism of the research enterprise and the pragmatic orientation on the treatment side was structural, ideological, and enduring. It came from an unresolved contradiction between the will to know the Other and the therapeutic processes that supposedly work to "normalize" so-called deviant behavior.

Lexington administrators and NIMH officials made several moves designed to strengthen or modernize both treatment and the kind of clinical research that was conducted at the CRC. Proposals were floated in the summer of 1970 to import the Overholser Division of Clinical Research from St. Elizabeth's Hospital in Washington, D.C., so as to make the CRC a true clinical research center. The ARC supported the plan to overhaul the CRC, but it was never realized. The CRC nevertheless explored new treatment approaches, most notably so-called therapeutic communities, modeled on Synanon in California and Daytop in New York City. In January 1969, Stanley Yolles, then director of NIMH, recommended implementing self-help approaches at the CRC, and transforming Lexington from a security-oriented institution to a modern therapeutic community. The resulting self-organized and self-governed therapeutic communities—with such names as Numen House, Excelsior House, or Ascension House (for women)—were regionally or racially homogenous.

The most notorious of these experiments was the short-lived Matrix House, which opened in January 1970. Later that year, when the *Lexington Herald* declared, "Narco Dead: Clinical Research Center Revamped to Replace 'Terror Prison' of Past" (October 13, 1970), it used Matrix House to exemplify the change. Reporter Bill Powell favorably noted a "cheerful" visage and a staff ratio of almost one-to-one. By July 1971, administrators had phased out Matrix House because it appealed "only [to] a limited and atypical segment of the addict population."[20] Subsequent accusations of misconduct by the ex-addicts who ran Matrix House led to a civil suit in which a federal grand jury criticized NIMH management of the CRC and, on November 10, 1972,

indicted two ex-addict former staff members and two patients involved in Matrix House.

Social change came late to Lexington, arriving in forms that many local staff members found threatening or unsettling. Given the racialization of narcotics problems and drug law enforcement in the United States, a substantial cultural divide between clinical staff and the patients they treated had been growing. In the fall of 1970, a committee for equal employment opportunity investigated allegations made by one of the only African American staff at the CRC of "an ideology that keeps the Negro at the bottom rung of the authorative [*sic*] and economic ladder."[21] Although an internal investigation determined this allegation was unfounded, the incident indicates rising institutional awareness of the changing political climate regarding civil rights.

Researchers at the ARC constantly negotiated boundaries between acceptable and unacceptable risk, justifiable and unjustifiable research. On balance, they did so in ways that were ethical within the parameters of their time. Researchers who began careers there recall a reverence for human life, an appreciation for addicts as human beings, and a research culture based on relationships of mutual respect and social intermingling. Most had come to Lexington knowing little about addiction or research. The social meaning of an "addict" was not the same at Lexington as elsewhere, due to the pervasive familiarity between "addicts" and "nonaddicts" and the lack of any sense of threat from addicts or judgments toward them. Researchers also remember discussions about how to ensure that consent was truly informed, how to design studies so as to safeguard subjects from harm, and how to guarantee the integrity of results. Charles Gorodetzky, a twenty-year Lexington veteran put it:

> I can never think of any time at ARC when the ethics of informed consent research were not impressed on me. I think there was every bit as much concern for the rights of people, the rights of privacy, the dictums of do-no-harm, the dictums of doing beneficial research that was well-constructed that did not harm, the issues of risks versus benefits. (2003)

While at Lexington, Gorodetzky learned to walk what he called "the tightrope between coercion and seduction," without tipping toward either extreme.

> You can't coerce people into research—it has to be free informed consent. I think we went out of our way to get free informed consent. Of course, we were getting informed consent from prisoners, and that's where the ethical climate was different. Because after it developed in 1977, by definition it was agreed that a prisoner could not give free informed consent, because they were in prison. I

> thought that was a very narrow point of view. I thought they did give free informed consent. I think we went out of our way to make sure that they could give their consent freely.

Coercion was not, however, the main situation to which the indigenous ethics of the ARC was structured to reply. The main concern, as Gorodetzky explained, was "seduction," the principled avoidance of enticements or compensation that would be considered disproportionate in the institutional context.

> The other thing we could not do is try to seduce them into research. Seduction had the very practical operational definition of not being able to offer them money or extra time off their sentences. What we were able to offer was exactly the same as they could earn by working in prison industry. We could not offer them anything more. Now they did get more personal attention. Obviously, there were people paying attention to them all the time. They probably got somewhat better medical care when they were with us, because we were concerned with doing frequent physicals and keeping track of vital signs and all the things you would do especially in a chronic study. [But] those were never offered to them in that way, it was never presented to them in that way. (2003)

A shared ethical discourse concerning the need to avoid unethical coercion or seduction was indigenous to the ARC. This ethical discourse was a narrowly technical discourse that evolved far from the political currents of civil rights, prisoners' rights, and patients' rights that subsequently came to pose a serious challenge to the ARC (see chap. 6 of the present book). The indigenous morality of the ARC never extended to the broader question that would be posed in the mid-1970s: Are prisoners free to give uncoerced consent from a position of structural coercion? This question did not arise within the laboratory logics of the ARC but instead arose forcefully from a political space beyond them.

By documenting what scientists who worked at the ARC said and did to contribute to public science and public health, this chapter has shown how they enacted what they strongly believed to be ethical research. The next chapter contrasts the ARC's standards for informed consent with those of the military and intelligence "drug research programs" to which the research program at Lexington was publicly compared during the prison research debates. A new governing bioethics regime emerged from the political struggle between the performative politics of congressional hearings and the "modest witnessing" of scientific expertise (Haraway 1997). This clash changed the meaning of "expertise" and "ethical science" by painting scientists, particularly those who worked

for the Public Health Service, as needing congressional oversight and regulatory discipline. Those responsible for the ARC—namely, Isbell and Martin—became subjects of ad hominem attacks, and their careers were essentially sacrificed to the political process. Martin retired from the PHS in 1977 but, like his predecessors Isbell and Wikler, stayed on in Lexington, where he worked at the University of Kentucky until his premature death in 1986. The tragedy of this moment was that there was so little room for careful deliberation about how research on drug addiction could further involve multiple publics.

Much of the ARC research took place beyond the public gaze, and administrators there were proud of their ability to buffer researchers from the political currents that flowed from Washington, D.C. Thus the ARC's relationship with its congressional sponsors was tenuous. Few elected representatives understood the nature of the research well enough to have a sense of the stakes involved in ending it. The data-driven nature of the ARC made the pharmaceutical industry an undependable ally, because new products often turned out to be addictive according to the ARC. Such findings did not earn the laboratory friends among Big Pharma. Nor did ARC scientists have academic counterparts or peers among state-run prison research programs, including industry testing programs, which engaged in neither the kind of basic research nor the kind of public health research and regulatory science conducted in Lexington. For decades, social isolation had protected the research programs; reviews of both the clinical and the research programs cast them as essential to the public health.[22] However, the frequency of program reviews increased in the early 1970s. Growing tensions between the ARC and the CRC became evident as Lexington began to come apart at the seams.[23]

The very isolation of the ARC became a liability in the 1970s, when activist groups fueled several rounds of congressional scrutiny relevant to the laboratory. Congressional hearings are performative arenas that display conflict for political purposes; they are not structured to provide a forum for negotiating in a deliberative or judicious fashion. Various task forces, advisory committees, and congressional investigations were set up to evaluate the research program. According to Gorodetzky, each of them concluded "what we were doing at the ARC was really state-of-the-art in terms of ethical considerations," but "they still threw out federal prisoner research in '76, and they still gave Harris Isbell a hard time . . . for the things that occurred in the '50s" (2003). Although I am persuaded that there was an indigenous ethics at the ARC, it is clear that ethics were conceptualized as of an individualistic, rather than a systemic, character.

Researchers there had a relatively narrow repertoire of collective ethical positions. Gorodetzky put it:

> I think all of us who were involved with the ARC always felt that we were doing things really with the highest ethical and moral standards. And I don't think any of us ever really felt personally that we were stretching the lines, doing things that were dangerous, doing things that were not scientifically justified. Because everything was reviewed and rereviewed. We were under very stringent regulations on what we could reward and not reward. . . . I never remember any treatment of a prisoner that was less than humane. (2003)

One need not question the sincerity of researchers' ethical commitments to recognize that they did not focus on the kinds of questions that came to the fore when prisoner research was politicized: whether it is possible to volunteer in structurally coercive contexts, how to safeguard privacy and confidentiality in a situation devoted to observation and surveillance, or whether access to health care should be conditioned upon participation in research. Such concerns evidently did not arise at the ARC, despite its precocious attention to informed consent and experimental design with knowing subjects.

Why should one expect such questions to have been raised in the terms that came to prevail in the 1970s? To impose such expectations retrospectively is to commit the error historians refer to as "presentism"—viewing the past through the lens of the present. The scientific ethical imaginary that prevailed at Lexington prohibited coercion or seduction of subjects. Lack of seduction—under the rule that subjects could be compensated no differently from work in the laundry, kitchen, grounds, morgue, or "needle works" (the sewing room)—was taken to signal that coercion was not taking place. Coercion was defined not as structural coercion (the kind that is inevitable in the prison environment or in any highly unequal situation) but as individual coercion. Negotiating "the tightrope between coercion and seduction" was an everyday matter at the ARC. Readers may think that there was less reflection than there should have been on power relations between researcher and researched, white and black, rich and poor. But that does not mean that observers in our era can reasonably expect people in the historical situation to have behaved according to standards that evolved in the political crucible of the mid- to late 1970s.

This chapter has broadened the range of questions concerning the calculus of suffering with which it began. Did lack of ethical reflection place subjects in situations of harm? Were the risks that accrued to the laboratory logics of sub-

stitution and mimicry too high? Or were the risks so great that no one could knowingly consent to them? Conversely, might there not have been unseen or incalculable benefits for participants? How should we weigh the fact that narcotic addicts regularly subjected themselves to high risk outside the laboratory? Ultimately, should the scientific experiments described in this chapter not have been done? The outcomes of these studies—modulating the pain of withdrawal with methadone, responding effectively to opiate overdose, or saving narcotics addicts from frontal lobotomy—did not simply benefit the rich. We must then ask whether participation in this kind of research can be reduced to a form of "interest" paid by the poor. My analysis has shown how essential these studies were to establishing a knowledge base. Far from being unethical, the research program yielded broadly distributed benefits to persons from the addicted classes.