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## Discovering Addiction

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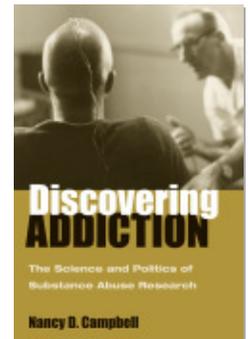
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## Notes

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### INTRODUCTION

1. Called by names ranging from “addiction” to “drug dependence” to “substance abuse,” the concept has generated a rich critical literature: see, for example, Davies 1992; DeGrandpre 2006; Forbes 1994; Fraser and Gordon 1994; Keane 2002; Lenson 1999. No synonym has gained as much traction as “addiction,” a term returning to today’s scientific parlance (O’Brien, Volkow, and Li 2006).

2. There is a flourishing literature on drug policy history: see Courtwright 1982/2001; Musto 1973/1999; Tracy and Acker 2004. Howard I. Kushner (2006) argues for looking more closely at the history of science.

3. Walsh also published an article (1973a) specifically on the institution that housed the ARC and on its transfer from the National Institute of Mental Health to the Bureau of Prisons.

4. Some social worlds cohere more than others. Behavioral pharmacologists comprise a well-defined group, but neuroimagers are relative newcomers whose networks are dispersed across social worlds. Social worlds are “universes of mutual discourse” (Mead 1938/1972, 518 [quoted in Clarke 1998, 289 n. 21]) that form the basic building blocks of the social organization of knowledge. Social worlds or arenas form coherent units of analysis despite contentious politics and heterogeneities within them (Clarke 2005, 48).

5. The field of science and technology studies has analyzed sciences as varied as biochemistry, crystallography, genetics, geology, physics, primatology, and the reproductive sciences (Clarke 1998; Frickel 2004; Fujimura 1996; Gilbert and Mulkay 1984/2003; Haraway 1976, 1989, 1997; Kohler 1991 and 2002; Latour and Woolgar 1986; Traweek 1988). Yet only Acker (1995, 1997, 2002) has written about the search for a nonaddicting analgesic. I am indebted to her landmark chronicle of the formative generation of addiction researchers up through World War II.

6. Having previously written a book on gender and drug policy, I am acutely aware of the appalling lack of women and persons of color who study addiction. Thomas Babor notes, “One thing that is clear from the selection process that brought these people into the field is that it favoured the recruitment of men rather than women,” raising the question of the “possible influence of gender bias in the disciplines from which addiction has built its workforce” (quoted in Edwards 2002, 384).

7. These conflicts often involve accusations of secrecy, betrayal, and ethical lapse (Crease 2003; Hayden 2003). The Tuskegee Study of Untreated Syphilis in the Negro Male still stands as an exemplar of the moral failing of science the world over (Jones 1981/1992; Reverby 2000). Failure to disclose covert military and intelligence drug exper-

iments has heightened distrust of government, particularly in communities of color (U.S. Congress 1973, 1975, 1977a, 1977b). A countervailing example is provided by the U.S. Department of Energy Advisory Committee on Human Radiation Experiments.

## CHAPTER 1

1. Michel Foucault's insight into the place of the "dangerous" delinquent in the "carceral continuum" rightly noted continuities between prison and "work begun elsewhere, which the whole of society pursues on each individual through innumerable mechanisms of discipline." Nothing, he wrote, distinguished the forms of authority directed toward sentencing, supervising, transforming, correcting, and improving individuals other than the "singularly 'dangerous' character of the delinquents, the gravity of their departures from normal behavior" (1979, 303). For Foucault, the power to punish was indistinguishable from the power to educate or even to "cure."

2. Harry M. Marks describes "medical individualism" as a cultural barrier to clinical research in the United States (1997, 51). Several factors exacerbated conflict between the clinic and laboratory in ways that made clinicians relatively uninterested in treating drug addiction. Once blamed for addicting patients, physicians began to avoid or underprescribe opiates even to patients in need of them (Jaffe 1985). So-called self-administration studies are discussed further in chapter 4 in the present book.

3. Himmelsbach 1972, 3. Many thanks to Jon M. Harkness for sharing copies of Himmelsbach 1972 and of his own interview with Himmelsbach, completed with Gail Javitt on November 2, 1994, under the auspices of the U.S. Department of Energy Advisory Committee on Human Radiation Experiments.

4. See Terry 1999 on a similar undertaking to define markers of lesbianism in the 1920s.

5. The federal Hygienic Laboratory was established in 1887 to pursue broader research programs than could municipal, state, or private laboratories (H. Marks 1997, 48). Although it had regulatory responsibility to test serums, vaccines, and industrial compounds, it lacked access to patients and did not have the capacity to do clinical research.

6. Although Robert Koch visualized the tubercle bacillus in 1882, U.S. clinicians integrated germ theory into practice slowly, because they still subscribed to miasmatic thought or feared that the new bacteriology would displace the medical arts. However, germ theory was popularly and commercially embraced. The personification of germs was central to the developing public health bureaucracy of the Progressive Era (Brandt 1985; Kraut 1994).

7. Carter et al. quoted in Terry and Pellens 1928/1970, 613. On hormone research in this period, see Oudshoorn 1994, 2003.

8. Terry and Pellens 1928/1970, 542. Paul Sollier's assistants at the Sanatorium of Boulogne conducted hematological studies to trace leukocyte reactions, which they interpreted as signaling a "true crisis" of the body along the lines of an infectious disease, rather than a mere "psychical breaking up of a habit." (Terry and Pellens 1928/1970, 541).

9. Some of the early psychoanalytic material is reprinted in Yalisove 1997.

10. The pharmacotoxic orgasm offered an objectless "executive process by which the

discharge of the entire psycho-sexual excitation is accomplished, like the function of onanism in children” (Rado 1926, 403).

11. Rado’s Columbia lectures, delivered from 1945 to 1955, were published in 1969.

12. Addiction researchers trace their lineage not to psychoanalysis but to behaviorism, crediting Olds with discovering the “brain reward system,” the neurological substrate for motivation and learning foundational to “brain mapping” and neurobehavioral drug-screening approaches. See Olds and Milner 1954; Olds 1955, 1956, 1958. Yet Olds was rarely cited in addiction studies until after a late 1960s review of work on drug effects on “brain-stimulation reward” (Kornetsky 2003a, 2003b).

13. On how such gendered conceptions play out in institutional settings, see Lunbeck 1994. On the longer history of such attribution patterns, see Tuana 1989, 1993.

## CHAPTER 2

1. For Becker, the term *drug* is not a pharmacological category but a reflection of “how a society has decided to treat a substance” (2001).

2. At the May 28–29, 1970, symposium that marked Seevers’s retirement, Nathan B. Eddy credited Seevers with realizing that monkeys could be used as a primary research tool, an idea that became the basis of a “world-recognized regimen for screening agents for morphine-like physical dependence capacity” (quoted in Domino 2004, 5). Eddy’s own foundational work at Michigan and, later, at the National Institutes of Health was the main basis for Seevers’s realization that monkeys could be used in this way.

3. By then, the bench program had synthesized 125 morphine derivatives and 350 other compounds and tested many in animals, according to Swain (1991, 18).

4. Reid Hunt to Charles W. Edmunds, April 30, 1936, Charles W. Edmunds Papers, box 1, Bentley Historical Library, Ann Arbor, Michigan. Edmunds lectured against patent medicine advertising and served as an expert witness in a case brought by the forerunner of the Food and Drug Administration, the U.S. Department of Agriculture’s Bureau of Chemistry, against manufacturers of “Buffalo lythia waters,” who claimed they relieved arthritis and rheumatism. He chaired the national standards committee on the desirability of biological assays for the United States Pharmacopeia and was active with the NRC committee until his sudden death in 1941.

5. Seevers later had Gerry A. Deneau translate Claude Bernard’s article “Experimental Studies on Opium and Its Alkaloids” from the French. Bernard subcutaneously injected the active principles of opium into dogs, cats, rabbits, guinea pigs, rats, pigeons, sparrows, and frogs. He mentioned that two Paris physicians had conducted similar trials in man. After explaining the difficulty of comparison due to species differences, Bernard wrote: “The animal experiments facilitate the physiological analysis which will clarify and explain the pharmacological effects in man. We will see, in effect, that everything which we establish in man will be confirmed in animals, and vice versa, except for the particulars which differences in species explain; but basically the nature of the physiological actions is the same. It should not be otherwise, because without that there would never be physiological science nor medical science” (1864, 406). The unpublished translation was in Seevers’s files at the University of Michigan. For access to this and otherwise unavailable material pertaining to Seevers, I thank James Woods.

6. Seevers later refuted his dual-action hypothesis (Domino 2004, 24–27).

7. The lecture was published in 1939 in the *Sigma Xi Quarterly*. Elsewhere, I explore antidrug reformers' use of "orientalizing" and "primitivizing" registers (Campbell 2000, 60–71). Seevers's use of the scientific register was designed to counter the caricatures of popular imagery for the purpose of grounding what he called a sane approach to drugs and drug policy.

8. While at Wisconsin, Seevers became an authority on monkey handling, as indicated by a letter of November 28, 1934, to him from K. K. Chen of the Lilly Research Laboratories in Indianapolis, Indiana. "Contemplating doing some work with monkeys," Chen sought advice on where to get them and how to feed them. Prompt and extensive, Seevers's reply included diagrams; detailed advice on housing, feeding, and handling; and a warning: "Utmost vigilance must be maintained at all times since they are always alert to escape or bite with any relaxation of the captor or at the unexpected time" (Maurice Seevers to K. K. Chen, December 3, 1934).

9. Many thanks to postdoctoral fellow Graham Florry for rescuing the monkey movies and screening them during my visit on March 19, 2005. Dated 1936, the film I saw and hereafter quote from was no. 5818.2, titled *Opiate Addiction in the Monkey*. It resembled *A Cinematic Study of Macaca mulatta*, a Seevers June 1936 publication listed in the *Journal of Pharmacology and Experimental Therapeutics*. The film depicted group-housed monkeys outside, quarreling and resisting capture, with a caption speculating about what the monkey wants—"only dilaudid, desire handling or injection?"

10. On Harlow, see Haraway 1989, 231–43; Blum 2002.

11. This undated film script from the Department of Pharmacology at the University of Wisconsin listed numbered scene shots organized by drug, time, and animal subjects—named "Cody," "Meyer," "Dillinger," "Morphy," "Jocko," and "Boss."

12. A positive stimulus had to take place within two or three minutes, the length of the monkey's "mental set" (a term used instead of the anthropomorphic term *memory*).

13. Victor Laties has posted an invaluable set of materials on Spragg at <http://www.apa.org/divisions/div28/archive/History/pan/briefspragg.html>.

14. Maurice Seevers to William Charles White, chair of CDAN, March 21, 1940.

15. This was a novel request from a researcher outside Lexington. On October 8, 1940, Seevers acknowledged receipt of five hundred grams of morphine sulfate to Lyndon Small at NIH. Small supplied the laboratories at Lexington and Michigan with morphine and other compounds, sometimes purifying as many as ten pounds at a time for research purposes (Lyndon Small to Maurice Seevers, October 31, 1941).

16. William Charles White to Maurice Seevers, April 9, 1940.

17. Clifton Himmelsbach to Maurice Seevers, January 16, 1941. The subject of the correspondence was Seevers's attempt to convene a subgroup of "morphinists" (who studied morphine) at an annual meeting of the Federation of American Societies for Experimental Biology.

18. Wailoo (1997) argues that social contexts make disease identity coherent by assigning meaning to physiological symptoms. Because the anemias have shed their moral character, his case offers a contrast to the persistence of the moral discourse of addiction.

19. Minutes from the first postwar CDAN meeting on October 2, 1947, National Research Council, Washington, DC, appendix and p. 9. Proceedings and minutes can be

consulted at the National Academy of Sciences Archives in Washington, DC, or at the National Library of Medicine in Bethesda, MD. I am grateful to Jim Woods and Louis Harris for allowing me access to their personal collections.

20. Kelsey trained under E. M. K. Geiling, who showed that a drug released by Masengill under the name Elixir Sulfanilamide caused over one hundred deaths in 1937. The event led to the 1938 Food, Drug, and Cosmetic Act that established the FDA (Stephens and Brynner 2001, 44–45).

21. Supply problems necessitate the reuse of animals. Later animal rights and antivivisectionist movements also troubled the supply of research primates (Blum 1994). The original problems Seevers faced in establishing the colony were related to the large numbers of animals used to produce and test polio vaccine and antimalarial drugs (Deneau 1970, 213). Popular protests eventually led India to cut off the traffic in monkeys in 1955, and it resumed only after the United States promised to use Indian monkeys solely for biomedical research that benefited “all humanity,” rather than in military or space research (Haraway 1989, 121). The embargo prompted the NRC to start a secure domestic breeding colony on the island of Cayo Santiago near Puerto Rico. In 1978, India again banned export of rhesus monkeys, of which it was the only supplier. On animal models, see Rader 2004; Rowan 1984.

22. Seevers spoke to the Proprietary Association in the proceedings of the Annual Research and Scientific Development Conference held at the Biltmore Hotel in New York City on December 8, 1960 (Seevers 1960, 4–12). Dedicated to steady growth and dissemination of scientific knowledge among pharmaceutical manufacturers, the society was reactivating its Therapeutic Research Foundation in response to the Kefauver hearings that led to the 1962 amendments to the Food, Drug, and Cosmetic Act of 1938. Seevers’s paper on test planning utilized Henry K. Beecher’s CDAN-sponsored studies (see chap. 4 of the present book). Seevers joked that aspirin would not have survived the animal and human screens typically used to determine safety and efficacy (1960, 10).

23. Thanks are due Jeremy Nordmoe, archivist at the Eskin Biomedical Library at Vanderbilt University in Nashville, Tennessee, which holds the archives of the American College of Neuropsychopharmacology. He remembered that this film predating 1965 was among the collected papers of Keith and Eva Killam. CDAN was listed as the film’s sponsor, and it was in 1965 that CDAN adopted the name change that made it the Committee on Problems of Drug Dependence.

24. Eddy related the following anecdote at Seevers’s retirement: “The only time Dr. Seevers and I ever exchanged sharp words had to do with a deadline. We were members of a World Health Organization study group on drug dependence. I happened to be chairman and Dr. Seevers was asked to head a subcommittee to write a description of the concept of dependence. We gave them a free day and I waited a little longer for their statement. Then I chided Dr. Seevers for the delay; we had only a week and we had to approve the statement for our report. In his usual forceful language, Dr. Seevers said that maybe we would get the statement and maybe we wouldn’t; it had to be right” (quoted in Domino 2004, 5).

25. The negative connotations of the term *dependency* in Anglophone cultures were not fully recognized by this coalition. See Fraser and Gordon 1994; Schram 2006, 136–52. As these sources make clear, the term was advanced to destigmatize

pauperism but was recycled as a “postindustrial pathology” associated with bad habits and addiction.

### CHAPTER 3

1. *Porter Act*, Public Law 203, 71st Cong., 1st sess. (May 13, 1930).
2. Research did not occur at Forth Worth until 1965, when the small Social Research Unit opened in collaboration with the Institute for Behavioral Research headed by Saul B. Sells of Texas Christian University. See U.S. Department of Health, Education, and Welfare, 1967.
3. For a sense of how this multidisciplinary research team functioned until World War II, see Himmelsbach 1972, 1994; Martin and Isbell 1978, 13–24.
4. Clinical observation led William R. Martin, ARC director from 1963 to 1977, to classify multiple opiate receptors (kappa, sigma, and mu) and correctly predict their location in the brain before they were visualized (Acker 1997).
5. The U.S. District Court ruled in 1936 that Lexington could not hold voluntary patients against their will (*Ex parte Lloyd*, 13 F. Supp. 1005 [D.C. Ky. 1936]). At first, only male opiate addicts were admitted, but President Roosevelt opened Lexington to “neuropsychiatric” patients in 1942, and many came from veterans hospitals. See *Operations Manual*, part A, chapter 1: History, NIMH Clinical Research Center, Lexington, Kentucky, 1971, found in Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, Lexington General Operations, box 8, folder “Lexington History and Operations,” NARA Southeast, Morrow, GA.
6. Soon after the women were moved into the main building, patience wore thin due to the “living and working of both men and women in the same building” (Annual Report of the General Services Section 1957, 1). Social control at the Lexington Hospital was eroding by the late 1950s, when there began to be more “adverse behavior reports,” bribery, and attempts to introduce contraband, all of which administrators viewed as disruptive to withdrawal, treatment, and research. Half the voluntary patients stayed for less than thirty days, which was considered “destructive to efficient use of staff and morale since very little treatment is accomplished” (Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, Administrative Records 1957–1974, Statistical Data Annual Statistical Summary 1957–1964, box 1, NARA Southeast, Morrow, GA).
7. The bureaucratic history is confusing due to Lexington’s hybrid status and the separation of the research unit from the larger institution. The PHS was transferred out of the Treasury Department on July 1, 1938, to the Federal Security Agency, which became the Department of Health, Education, and Welfare in 1953. The Division of Mental Hygiene was abolished when NIMH was established in 1948. After that, Lexington and Fort Worth fell under the jurisdiction of the Division of Hospitals, and the ARC became an NIMH research unit in 1949. Not until 1967 was the hospital operation transferred to NIMH and renamed the Clinical Research Center. In 1968, the Health Services and Mental Health Administration was established, along with the Division of Narcotic Addiction and Drug Abuse (DNADA), under the auspices of which fell both the ARC and the NIMH Clinical Research Center.

8. Sanford Bates to Walter Treadway, May 28, 1935, Record Group 129, Bureau of Prisons, National Archives II, box 746 NC-43, correspondence file 4-13-0, 1930–1937, College Park, MD.

9. Bates to Treadway, May 28, 1935. This prophecy held only until the mid-1970s, when the facility reverted to a federal prison.

10. Sanford Bates, director of the BOP, to Lawrence Kolb, May 28, 1935, NARA Record Group 129, file 4-13-0, box 746 NC-43.

11. James V. Bennett, commissioner of prison industries, to Lawrence Kolb, November 4, 1936, NARA 4-13-3-29, box 47.

12. Lawrence Kolb, “Drug Addiction among Women,” n.d. Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, Public Relations, box 1, NARA Southeast, Morrow, GA.

13. Lexington staff were reluctant to engage policy debates without definitive data, although they testified on behalf of more humane treatment on June 2, 3, and 8, 1955 (U.S. Congress 1955).

14. Sanford Bates to Eleanor T. Glueck, Institute of Criminal Law, Kendall House, Cambridge, MA, December 17, 1930, NARA 4-13-0.

15. WHO 1950 was submitted by Nathan B. Eddy, National Institutes of Health, and was based on Isbell’s data produced at the ARC, illustrating how tightly interwoven CDAN was with the WHO expert committee.

16. *Annotated Bibliography of Papers from the Addiction Research Center, 1935–1975* (DHEW no. [ADM] 77-435, 1978) includes addenda covering the years 1976 and 1977.

17. Bates to Treadway, May 28, 1935.

18. Bates to Treadway, May 28, 1935.

19. Chestang 1970. Many thanks to Professor Gwendolyn Hall for allowing me to use the materials she gathered in the spring of 1970.

20. The research staff held different political beliefs than clinical and support staff. These were often implicit, because they marked researchers as “outsiders.” For instance, Wikler’s eldest daughter Marjorie Senechal, who lived on the Lexington grounds from the age of one to the age of fourteen, has noted that though her parents, Eastern European Jews from New York’s Lower East Side, forbade their children to voice the family’s left-liberal political views, “the glaring absence of Monopoly among the board games on our porch would have tipped off the politically aware” (Senechal 2003, 189).

21. Sociologist Howard S. Becker taped the book-length oral history of Marilyn Bishop, which was edited by Helen McGill Hughes, wife of Everett C. Hughes, and published as *The Fantastic Lodge: The Autobiography of a Girl Drug Addict* (1961). Bishop’s pseudonym was “Janet Clark.”

22. Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, box 6, folder “Forms,” NARA Southeast, Morrow, GA.

23. Correspondence addressed to attorney general, Washington, DC, 18 October 1942, NA Record Group 129, box 25, by Perry and Gladys Youts.

24. Availability of drugs other than those used in ARC studies depended on fluctuating levels of security. Senechal recalls finding drugs and paraphernalia on the grounds as a child (2003, 192). The pharmacist who mixed preparations for the ARC in the 1970s noted changes over time (Johnson 2005). An inmate, Eddie Flowers, inter-

viewed in 2004 by J. P. Olsen and Luke Walden in the course of making their film *The Narcotics Farm*, recounted entering the research program specifically to get drugs.

25. Dropout rates as high as one-third were recorded in studies of narcotic antagonists, which most people do not experience as pleasant. Participants could and did opt out, according to National Commission 1976a.

26. The earliest instruments, the Morphine Abstinence Syndrome Intensity (MASI) scale developed by Himmelsbach and Kolb's K-classification system, categorized addicts in terms of psychological profile, behavior, and physiological state. In the mid-1950s, Harris Hill, Charles Haertzen, and Richard Belleville developed the Addiction Research Center Inventory (ARCI), which is still in use (Martin and Isbell 1978, 161).

27. The second interview was conducted as part of the Oral History Project of the U.S. Department of Energy's Advisory Committee on Human Radiation Experiments (Himmelsbach 1994). Himmelsbach did not return to addiction research after he left Lexington in 1944.

28. The concepts "normal" and "abnormal" shift in relation to time and place: "It must be admitted that the normal man knows that he is so only in a world where every man is not normal. . . . The normal man is he who lives with the assurance of being able to arrest within himself what in another man would run its course. In order for the normal man to believe himself so, and call himself so, he needs not the foretaste of disease but its projected shadow" (Canguilhem 1991, 286).

29. The meeting was held on April 29, 1961, as part of a three-year PHS project that culminated in a report issued by the Law-Medicine Research Institute titled "A Study of the Legal, Ethical, and Administrative Aspects of Clinical Research Involving Human Subjects," issued on March 31, 1963. Thanks to Jon M. Harkness for sharing the meeting transcripts, which identify speakers by initials. The transcripts are housed at the Mugar Memorial Library at Boston University.

30. Wikler wrote about the neuropathology of Horner's syndrome, cases of which he encountered between 1938 and 1940 at the PHS Marine Hospital in St. Louis, Missouri.

31. Soon after Lexington opened in 1935, Robert H. Felix became persuaded that the electroencephalograph would be useful for studying the effects of addiction on the brain. He went to Providence, Rhode Island, for training, where he met biophysicist Howard L. Andrews and convinced him to move to Lexington. There, Andrews installed the first electroencephalograph west of the Alleghenies (Kay and Andrews in Martin and Isbell 1978, 140–54). Andrews did not leave Lexington until 1942, when he departed "with a distinct sense of disappointment and personal failure" because the technology had not enabled him to draw conclusions of basic significance (Martin and Isbell 1978, 153). Wikler took over Andrews's lab in 1943 and expanded the facility with a government surplus ink-writing EEG machine that was used to study sleep patterns, metabolic and tissue tolerance to alcohol and barbiturates, and the effects of mescaline and psilocybin.

32. From 1942 to 1943, Wikler went to the University of Chicago, the Illinois Neuropsychiatric Institute, the Northwestern University Institute of Neurology, the Yale University Laboratory of Physiology, and the Rockefeller Institute for Medical Research at the New York State Psychiatric Institute and New York Hospital.

33. Wikler's first publication of national scope was coauthored with Jules H. Masserman, with whom Wikler studied at the University of Chicago (1943). They conditioned responses using conflict in an operant conditioning situation to produce "experimental neuroses" and recorded the experiments on film. Masserman had a psychoanalytic orientation and an "avowed anthropomorphism" (Iversen and Iversen 1981, 35).

34. He cited Lawrence Kubie and Roy R. Grinker, Sr., both experimental psychiatrists, although Grinker had been psychoanalyzed by Freud himself. Grinker studied anxiety as an objective, observable behavior (1979, 45, 50).

35. The idea resembles Rado's "pharmacothymic orgasm," which he claimed was equivalent to the pregenital alimentary orgasm of a baby at the breast. Robert D. Chesick (1960) later attempted to confirm the existence of this phenomenon.

36. The Social Science Section found high relapse rates in a "fairly low percentage" and argued, "[G]enerally discouraging conclusions which have been drawn from other studies may, therefore, be based largely on the fact that these studies have chosen to use the most negative of the possible measurements" (O'Donnell 1964).

#### CHAPTER 4

1. Sigmund Freud quoted in Greenacre 1953.

2. Psychopharmacology's usefulness was contested; for instance, Felix was reluctant to establish the NIMH Psychopharmacology Service Center. Yet historians claim, "[I]ntroduction of chlorpromazine and reserpine in the mid-1950s held out the promise of healing the long-standing division between biological and psychodynamic psychiatrists and promoting the reintegration of the specialty with medicine generally" (Grob 1991, 154). The Psychopharmacology Service Center's Early Clinical Drug Evaluation Unit conducted clinical trials until the 1970s, standardizing data collection, protocols, and rating scales. David Healy claimed, "This was almost a new form of science, one that acknowledged that techniques drive progress as much as, if not more than, anything else—a form of science that was looked down upon by university-based scientists, for whom experiments were conducted to test already existing theories" (2002, 282). See Healy 1996, 239–63; Cole 1970.

3. We have been "becoming neurochemical selves" for a long time (Rose 2003).

4. Benedict (1960) quoted heavily from Huxley's "Drugs That Shape Men's Minds" (1958), while stressing the "deadly amorality" of government mind control research, which he correctly insisted was neither science fiction nor prophecy.

5. The first edition of Nelson Algren's *The Man with the Golden Arm* (1951) won the National Book Award in 1949. It became a major motion picture—directed by Otto Preminger and starring Frank Sinatra—that was notorious for getting around the Hollywood censorship codes. Addiction researcher Conan Kornetsky (2003b) remarked on its accurate portrayal of heroin.

6. The figure of the monkey encodes a stunning array of social phobias, polarities, and political agendas about the "enemy within," controlled by external forces as is *The Manchurian Candidate*'s Raymond Shaw, who embodies qualities opposite to those of the democratic citizens of the free world. Drug issues provide "chemical curtains" for racist sentiments.

7. Internalist accounts refute serendipity. See Dews 1985, 3.
8. Donald Klein, interview by Jackie Orr, March 29, 1996, quoted in Orr 2006, 171. Thanks to Jackie Orr for sharing her interview with Klein, who worked at Lexington as a psychiatrist and assistant surgeon from 1954 to 1956.
9. Evelyn Fox Keller reminds us that claims to universality are political, not scientific (1992, 180–81). The putative universality of ethnopharmacology still surfaces in debates over how to regulate psychoactive substances in the face of global inequality.
10. The movie *The Snakepit* was made at the Rockland State Hospital prior to Kline's arrival (Healy 2002, 104).
11. This passage is from a guidebook to the "new territory" (Barchas et al. 1977, 528).
12. In *Useful Bodies: Humans in the Service of Medical Science in the Twentieth Century* (2003), Jordan Goodman, Anthony McElligott, and Lara Marks argue that focusing on informed consent "skews the study of human experimentation toward an ethical analysis rather than a practice" (4). More significant, they say, is how the "modern state increasingly used its prerogative to lay claim to the individual body for its own needs, whether social, economic, or military" (2).
13. Pharmacologist Harry Gold of Cornell is credited with developing the double-blind test "virtually alone" (Shapiro and Shapiro 1997; cf. Kaptchuck 1998, which argues for a much longer history).
14. Beecher's Anesthesiology Laboratory at Massachusetts General Hospital was the world's first facility for the clinical study of anesthetic agents (Harkness 1999).
15. See Ellison C. Peirce, Jr., "Anesthesia Safety and Mortality Studies in the 1950's through 1970's," at <http://www.apsf.org/about/rovenstine/part3.msp>.
16. Beecher studied drug effects on performance of physical and mental tasks. According to Tousignant 2006, the U.S. Army Medical Research and Development Board funded Beecher's first large-scale clinical trial of methadone, and he conducted military-supported field trials of methadone in Korea.
17. The expansion of experimentation by clinicians was not legally recognized as a legitimate part of the physician's activities in the late 1950s. Beecher regarded it as essential and believed its necessity should be legally recognized.
18. Information on Louis Lasagna (deceased) is taken from Lasagna 1994 and Healy 2002. There is little information on Jane Denton. See Anthony Petrosino's entry on Charles Frederick (Fred) Mosteller in the James Lind Library (<http://www.jameslindlibrary.org>). See also Beecher et al. 1953; Lasagna et al. 1954.
19. The Cornell group published dozens of papers from 1940 to the mid-1950s, when Hardy turned to other interests (see Hardy, Wolff, and Goodell 1940, 1952; Schumaker et al. 1940). The dolorimeter enjoyed a brief heyday in the early days of Lexington, falling out of favor by the early 1950s (Tousignant 2006).
20. Rothman argues that due to the construction of the Nazis as fundamentally different, U.S. clinical researchers did not perceive implications for their own work (1994, 62–63).
21. Bridgman's concept of "operationalism" was famously attacked by Herbert Marcuse in *One-Dimensional Man* (1964).
22. Wikler participated in a phenomenology reading group convened by Erwin Strauss, who edited *Phenomenology: Pure and Applied* (1964), to which Wikler contributed.

23. Wikler wrote: “In achieving an impressive degree of mastery over the world about us, the growth of the natural sciences has been characterized by an ever-increasing supplementation of ‘private operations’ (sensing, feeling, inducing, deducing) with ‘public’ ones (control and manipulation over measurable variables). As one result, even our ‘private’ ways of perceiving the world have changed from those of our prescientific ancestors, so that at sunset, we no longer ‘see’ the sun sinking into the sea, but ‘see’ it disappearing beneath the horizon” (Wikler 1965, 85).

24. Beecher here cites two letters received from Wikler, penned August 7 and September 6, 1956.

25. The thalidomide controversy broke during Senator Estes Kefauver’s hearings on price-fixing and profit margins in the pharmaceutical industry (Stephens and Brynner 2001).

26. The committee explored Puerto Rico, a common pharmaceutical testing ground, but never followed up (Committee on Drug Addiction and Narcotics 1954b). On the use of Puerto Rico as a “laboratory,” see Briggs 2002).

27. Under the auspices of the National Institute on Drug Abuse, Charles Gorodetzky added a voice-over in the 1970s.

## CHAPTER 5

1. Sydney A. Halpern has defined an “indigenous morality” as a set of statements and practices that pervades the scientific problem groups and social networks that comprise a clinical research. She has defined “scientific problem groups” as networks of “researchers who address common questions, share materials and techniques, review one another’s scientific papers, and debate the meaning of empirical findings” (2004, 9–10). The concept is useful for studying the social organization of science (Mulkey, Gilbert, and Woolgar 1975).

2. Harry M. Marks criticized sociologists of science for focusing on the laboratory and ignoring the clinic (1997, 8). Movement was bidirectional at the ARC, where researchers took cues from clinicians and corrected the evidence base on which they acted. Although oriented toward basic research, ARC findings were useful to clinicians.

3. Frank Jewett, president of the National Academy of Sciences, responded thusly to a 1943 proposal to introduce venereal disease into a prison population in hopes of finding an effective chemoprophylaxis: “[P]rison populations are not free populations and . . . so-called volunteers are not true volunteers in the ordinary sense. Their volunteering is or can be alleged to have been brought about by reasons which are entirely absent in a free population” (quoted in H. Marks 1997, 104).

4. Flowers testified in the Kennedy hearings (U.S. Congress 1973) beside Harris Isbell and Lexington inmate John Henderson Childs, who worked for Isbell. Flowers has been interviewed several times by filmmakers J. P. Olsen and Luke Walden, and I remain indebted to their willingness to share the fruits of their labor. John Marks also interviewed Flowers for his book *The Search for the Manchurian Candidate* (1979), which I discuss in chapter 6 of the present book.

5. For instance, Wikler wrote: “[C]linical and electroencephalographic effects of these drugs are determined not only by the chemical properties of these agents but also by other factors which are not clearly defined. However, the ‘personality’ of the individ-

ual, his past experiences with drugs and the meaning to him of the experimental situation appear to modify drug effects” (1954, 174).

6. Industry had to look elsewhere to build its clinical research infrastructure. It often looked to poor populations within the United States and Puerto Rico (Briggs 2002; Fisher 2005; Petryna 2006; Shah 2006).

7. At Lexington, methadone was used to ease withdrawal, not as a form of maintenance. Not until the 1960s did Vincent Dole and Marie Nyswander, architects of methadone maintenance, develop the clinical logic of the “methadone blockade” at Rockefeller University. Dole was a highly respected scientist trying to solve the “opium problem” evident on the streets of New York City, but Nyswander’s credibility in the scientific community was low. During her 1945 residency at Lexington, she was perceived as “threatening” or “naive” in the tiny professional enclave at the ARC (Senechal 2004). One especially telling anecdote presaging his views on the “methadone mess” was recounted late in life by Harris Isbell in an interview with Marjorie Senechal, Abraham and Ada Wikler’s daughter, who lived on the Narco campus until age twelve. Isbell recalled preventing Nyswander from distributing morphine shots as Christmas presents to Lexington inmates (Senechal 2003, 193). Outsiders to the “research establishment,” Dole and Nyswander were criticized for prematurely announcing methadone’s efficacy as a maintenance agent and for being “not pharmacological” enough (Courtwright, Joseph, and Des Jarlais 1989, 337). They in turn disparaged prior addiction research, as evidenced by Dole’s remark that there “was no research talent in the field, just some pharmacologists working with animals who didn’t have a concept of human epidemiology” (quoted in Courtwright, Joseph, and Des Jarlais 1989, 332).

8. At the January 1953 CDAN meeting, Isbell urged Beecher to include nalorphine as a control and to run clinical trials of a nalorphine-morphine combination to establish nalorphine’s analgesic efficacy in cases of postoperative pain. According to May and Jacobson, this suggestion had come up in the late 1940s (1989, 190).

9. The series ran from November 26 to December 1 and catalyzed conferences sponsored by the New York City Office of the Mayor, the New York City Welfare Council, the New York Academy of Medicine, and the Josiah Macy, Jr., Foundation (Campbell 2000, 98–102).

10. Lobotomy was atypical at Lexington because it did not have discernible therapeutic effects. Kolb Hall housed a couple hundred veterans or retired members of the Coast Guard who were neuropsychiatric patients (the population for whom lobotomies were commonly recommended at the time).

11. My account is drawn from an informal interview with Edward F. Domino in March 2006, as well as from Domino 1995 and my formal interview with him in 2006.

12. The drug was also tested at Lexington (Isbell and Fraser 1953).

13. As mentioned earlier, the ARC evaluated the abuse potential of many branded compounds central to the medical market of the 1950s, such as Seconal and other barbiturates, Dromoran, or Miltown (meprobamate, the first popular minor tranquilizer). See Isbell 1951a, 1951b; Hill and Belleville 1953; Isbell and Fraser 1953. Isbell 1951a reported on a study in which ten barbiturate-addicted patients were maintained on large doses of secobarbital (Seconal) for lengthy periods (Addiction Research Center 1978, 53). Severe impairment was found, leading to warnings that consumers could not anticipate emergencies and should not operate machinery on the drug.

14. Cancer research at the Jewish Chronic Disease Hospital in Brooklyn, hepatitis research at Willowbrook School on Staten Island, and the Tuskegee Study of Untreated Syphilis in the Negro Male stand as examples of unethical human subjects research. The Tuskegee scandal catalyzed the National Commission for the Protection of Human Subjects of Biomedical Research (Reverby 2000). Soon after the scandal broke in the summer of 1972, another human subjects scandal regarding military and Central Intelligence Agency (CIA) testing of psychoactive drugs as “incapacitating agents” implicated the ARC, which studied LSD-25 and other hallucinogens. Most research on LSD occurred at the Edgewood Arsenal; Fort Detrick, Maryland; Fort Bragg, North Carolina; Fort McClellan, Alabama; Fort Benning, Georgia; and Dugway Proving Ground, Utah (Moreno 2001, 256). By contrast, the ARC studied these drugs through their usual protocols and techniques and in comparison to the opiates with which they usually worked (Wikler 1954).

15. Unlike opiate research, LSD research was not centrally coordinated. Instead, the army and the CIA competitively funneled money through private foundations for clandestine LSD studies, in which prisoners served as unwitting subjects of convenience. See Lee and Schlain 1985; Campbell 1995; Goliszek 2003; Hewitt 2002; J. Marks 1979; Moreno 2001.

16. Kennedy’s Interdepartmental Committee on Narcotics recommended that the federal narcotics hospitals shift “from their present emphasis on treatment of Federal narcotic prisoners and probationers as well as volunteers, to full-time research-oriented programs, examining all aspects of narcotic and drug abuse” (memorandum from the secretary of the Department of Health, Education, and Welfare to the surgeon general, n.d., Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, Study of Narcotic Problems—Forms—Civil Commitment Reports, box 6, folder “Policy 1963–1966,” NARA Southeast, Morrow, GA). Although President Johnson reportedly viewed the committee’s report skeptically, on July 15, 1964, he directed all units into maximum activity (Office of the White House Press Secretary, Statement by the President, July 15, 1964 Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, Study of Narcotic Problems—Forms—Civil Commitment Reports, box 6, folder “Policy 1963–1966,” NARA Southeast, Morrow, GA). His directive spurred a study of the PHS neuropsychiatric and narcotic hospitals as part of the attempt to devolve responsibility for treatment to states and municipalities, while continuing the federal research mandate. Ironically, since voluntary patients had never participated in ARC studies, the president’s commission recommended that they be accepted “only to advance research aims” (briefing memorandum to the secretary of the Department of Health, Education, and Welfare from Rufus E. Miles, Jr., assistant secretary for administration, “Design for a Study on the Future of the Neuropsychiatric and Narcotic Hospitals of the Public Health Service,” 1964, Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, Study of Narcotic Problems—Forms—Civil Commitment Reports, box 6, folder “Policy 1963–1966,” NARA Southeast, Morrow, GA, 3).

17. Likening responses to drug addiction to past responses to insanity or witchcraft, the *Robinson* opinion advocated modern medical treatment. Addicts, Justice Douglas wrote for the majority, were under the sway of compulsions they could not manage

without professional help. An addict was defined as “a person who habitually takes or otherwise uses to the extent of having lost the power of self-control any opium, morphine, cocaine, or other narcotic drug” (*Robinson v. California* 370 U.S. 660 [1962]).

18. Most NARA patients were considered “too antagonistic or disruptive to participate in the institution treatment program” (Maddux in Martin and Isbell 1978, 239).

19. Sidney Cohen, acting director of DNADA, formed a panel to transition Lexington to a “model treatment facility” and named Harold Conrad chief of the CRC.

20. Bertram S. Brown to the Secretary of the Department of Health, Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, box 5, NARA Southeast, Morrow, GA.

21. Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administration Office, Clinical Research Center, Lexington, KY, box 4, NARA Southeast, Morrow, GA.

22. Paul Q. Peterson, acting deputy surgeon general, “Program Review of the Clinical Research Center, Lexington, Kentucky,” August 7, 1970, Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, Administrative Records 1957–74, box 4, NARA Southeast, Morrow, GA.

23. At the request of Jerome H. Jaffe, director of the White House Special Action Office for Drug Abuse Prevention and an alumnus of Lexington, William Bunney, director of DNADA, dispatched William Pollin and Richard Belleville (once at the ARC) to Lexington the week of October 16, 1972. Earlier that year, the CRC shifted priorities from treatment to research at DNADA’s behest. Their report reflected the dissatisfaction of the CRC researchers with their lower prestige relative to the ARC. On November 30, 1972, Bunney responded to Jaffe’s request for recommendations. He noted that the ARC was necessary to carry out the Department of Health, Education, and Welfare’s responsibility for assessing the abuse potential of new compounds, warning “there is no other federal laboratory which can carry out the present or expanded responsibilities” (Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, Lexington General Operations, box 8, NARA Southeast, Morrow, GA). Believing that the ARC might have to take on tasks carried out in Ann Arbor, Bunney advocated merging the ARC and the CRC. Nowhere did he mention the possibility of discontinuing the ARC’s human research program.

## CHAPTER 6

1. See Joseph Sturgell, “Description of Hospital Treatment Program,” appendix B, in Committee on Drug Addiction and Narcotics 1955, 1033–36.

2. Atlanta Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY Study of Narcotic Problems—Forms, box 5, NARA Southeast, Morrow, GA.

3. Early sociology of bioethics documented social norms among biomedical researchers. Barber et al. (1979) analyzed the first national survey of the institutional review boards mandated in 1966 at every institution that conducted research on human

subjects with PHS funding. Originally published in 1973, the book revealed a “permissive” minority of biomedical researchers that held low-status positions in the social hierarchy of science and who would approve studies that most considered ethically questionable. Barber et al. wrote: “The research community is itself pathogenic, at least to a degree, and perhaps we will never adequately regulate the use of humans in research until we better understand the pathology” (1979, x). Emphasizing the pattern through which “‘good guys’ [were turned] into ‘bad guys,’” the sociologists contrasted their approach to the individualistic terms of Beecher and others who did not situate ethical actions within social context (Barber et al. 1979, xiii).

4. See U.S. Congress 1969, 5689; Mitford 1973a; Rugaber 1969.

5. I support the National Prison Project, which has been crucial for expanding prisoners’ rights and changing prison conditions in the United States.

6. U.S. Congress 1973.

7. Commission members were Joseph V. Brady, professor of behavioral biology, Johns Hopkins University; Robert E. Cooke, vice chancellor for health sciences, University of Wisconsin; Dorothy I. Height, president of the National Council of Negro Women; Albert R. Jonson, associate professor of bioethics, University of California at San Francisco; Patricia King, associate professor of law, Georgetown University; Karen Lebacqz, consultant in bioethics, California State Department of Health; David W. Louisell, professor of law, University of California at Berkeley; Donald W. Seldin, professor and chair of the Department of Internal Medicine, University of Texas at Dallas; Elliott Stellar, provost and professor of physiological psychology, University of Pennsylvania; and Robert H. Turtle, attorney. Only Brady was familiar with the ARC.

8. U.S. Congress 1975.

9. MKULTRA was the code name for a CIA contract research program on the controlled alteration of human behavior. It ran from 1953 to 1963 and used research materials obtained through “standing arrangements with specialists in universities, pharmaceutical houses, hospitals, state and federal institutions, and private research organizations.” Testing was carried out in many sites other than the “Lexington Rehabilitation Center,” as it was called in the Church committee hearings.

10. *The Belmont Report* built on ten reports issued by the committee between 1974 and 1978. Its principles—respect for persons, beneficence, and justice—remain in effect (Callahan 2003). See <http://www.nihtraining.com/ohsr/site/guidelines/belmont.html>.

11. Biographical information on Carlson was taken from Keve 1991.

12. Correspondence dated March 1, 1976, from Norman A. Carlson, director of the BOP, to the Honorable Robert W. Kastenmeier indicated that use of prisoners with a history of narcotic abuse in tests of the addictive properties of new drugs would be phased out at the ARC in Lexington, Kentucky. Carlson’s letter was mentioned several times in *Research Involving Prisoners: Report and Recommendations*, by the National Commission for the Protection of Human Subjects (1976b).

13. NIDA was established in September 1972 and given statutory authority in 1973. The ARC was not absorbed into it until 1974. Until 1992, when it became part of the NIH, NIDA was administered by the Alcohol, Drug Abuse, and Mental Health Administration (ADAMHA) under the auspices of the U.S. Department of Health, Education, and Welfare (DHEW).

14. Lexington went through a particularly deplorable incarnation not long after the ARC's departure. My thanks to Scott Christianson for pointing out the notorious "experimental" basement unit where women political prisoners were subjected to sensory deprivation and behavior modification designed to break their will. The "Lexington Unit" closed in 1988 in a major victory for prison activists.

15. James V. Lowry, "Opening Remarks," in Committee on Drug Addiction and Narcotics 1955, 1031–32.

16. Norm Carlson sat on the advisory board of the CEC.

17. The ARC had no direct fiscal relationship with the pharmaceutical industry. CPDD and the FDA have small testing programs that are widely regarded by the scientific community as inadequate to prevent such public health crises as those involving Oralflex (Ronald 2006) or OxyContin, both painkillers of the class that the ARC would have once investigated.

18. The industry reconsidered drug research in prisons in August 1973 at a conference held in Airlie, Virginia (Harkness 2003, 253–55). Pharmaceutical Manufacturers' Association president C. Joseph Stetler announced the conference at the Kennedy hearings (U.S. Congress 1973) as he indicated how deeply dependent the industry was on prisoners.

19. *Otis Clay, Plaintiff-Appellant v. Doctor William R. Martin et al. and The United States Surgeon General et al. and The United States Defendants-Appellees*, 509 F.2d 109–14 (2d Cir. 1975). A government motion to dismiss Clay's 1975 appeal was granted in June 1977. On September 8, 1978, the U.S. government filed a motion for summary judgment, which is considered a harsh remedy that is granted only where material issues of fact no longer remain to be tried. The Court partly granted this motion after determining that Clay's heart attack was not caused by drugs administered at the ARC. However, the Court viewed as unsettled the issue of whether Clay's consent was voluntary and informed, and thus it allowed an investigation to determine whether the naltrexone experiment was conducted in a "negligent and reckless" manner. Three days after these matters were tried on March 12, 1979, the Court dismissed Clay's complaint in its entirety with prejudice.

20. "Prisoner Claims Inhuman Treatment in Medical Experimentation," *Citation* 31.12 (October 1, 1975), 138; *Clay v. Martin*, 509 F.2d 109.

21. I am grateful to Jon M. Harkness for sharing a copy of this letter, obtained through Freedom of Information Act request no. 91-3171, with me. Unless otherwise noted, all letters quoted in this chapter came from this source.

22. The last three conditions first appeared in Morris and Mills 1974 (quoted in Harkness 2003, 319 n. 114).

23. Political and bureaucratic pressures converged on Carlson: his task force on medical research wanted to phase out ARC participation, and the ACA called for abandoning all such projects in a position statement issued by its board of directors on February 20, 1976. Thus the ACA terminated use of federal prisoners as research subjects before the National Commission for the Protection of Human Subjects decided what its approach would be.

24. In his March 2, 1976, letter to the deputy attorney general, Carlson described receiving "an irate call" from DuPont indicating "we had reneged on an earlier com-

mitment concerning phasing out of the project.” Carlson wrote, “I firmly believe, however, that we should get out of the project as soon as possible.” His March 1, 1976, letter to Kastenmeier explained that all other research on federal prisoners had already been phased out over the preceding five years. Carlson received inquiries—including one (dated January 4, 1974) from Senator Sam J. Ervin, Jr., chair of the powerful Committee on the Judiciary—about the scope of prison research and the nature of BOP policy.

25. The authorship of this report, which is dated December 19, 1972, is unclear. With the help of archivist Jeremy Nordmoe, it was located among the collected papers of Heinz Lehmann, in a folder titled “Lexington Talk,” in the archives of the American College of Neuropsychopharmacology, Eskind Biomedical Research Library, Vanderbilt University, Nashville, Tennessee.

26. William R. Martin to Joseph V. Brady, September 3, 1975, Georgetown University, Kennedy Institute of Ethics, Papers of the National Commission for the Protection of Human Subjects, meeting 11, box 3. My thanks to Jon Harkness for sharing copies of these letters with me.

27. William R. Martin to Joseph V. Brady, December 5, 1975, Georgetown University, Kennedy Institute of Ethics, Papers of the National Commission for the Protection of Human Subjects, meeting 14, box 4.

28. E. Leong Way to Kenneth John Ryan, March 24, 1976, Georgetown University, Kennedy Institute of Ethics, Papers of the National Commission for the Protection of Human Subjects, meeting 17, box 7.

29. Leo Hollister to Philip Handler, President, National Academy of Sciences, April 9, 1976, National Academy of Sciences Archives, Record Group 78-016-1. “ALS: D.Med: CPDD: Chairman’s letters (3) on Human Experimentation Transmitted by President, NAS, 1976,” box 1.

30. Keith F. Killam, President, American College of Neuropsychopharmacology, to Kenneth John Ryan, March 26, 1976, Georgetown University, Kennedy Institute of Ethics, Papers of the National Commission for the Protection of Human Subjects, meeting 17, box 7.

31. Phase 1 testing at Jackson halted after twelve years; nearly thirty thousand participants were involved, only sixty-four of whom experienced a “medically significant event,” mainly adverse drug reactions from which all recovered completely (Harkness 2003, 203–10).

32. *Henry Fante et al. v. Department of Health and Human Services et al.*, U.S. District Court, Eastern District of Michigan, Southern Division, Civil Action no. 80-72778. Records from this case, which I obtained through Jon Harkness, are housed at the Great Lakes Regional Archives in Chicago, accession no. 21-88-0016, location no. 331792-332283, box 269. FDA officials announced the indefinite stay, which remained in effect at the time this book went to press. See *Federal Register* 46 (July 7, 1981): 35085.

33. NARA was used as the vehicle for the criminalization of LSD (Hewitt 2002). See Isbell et al. 1956 for an example of what the Lexington group published about LSD in peer-reviewed scientific journals and the medical press. This research was not covert. See also the entry on Harris Isbell’s Lilly Research Prize lecture, given on March 15, 1956, in Indianapolis and titled “Studies on the Diethylamide of Lysergic Acid: Development of Tolerance and Effects of Tranquilizing Drugs on the Reaction” (Annotated Bibliog-

raphy of Papers from the Addiction Research Center, 1935–75, DHEW no. [ADM] 77-435, 1978, 70).

34. The few relevant pages of this report were found in the files of Heinz Lehmann, a member of the National Advisory Council subcommittee, among his collected papers in the archives of the American College of Neuropsychopharmacology, Eskind Biomedical Research Library, Vanderbilt University, Nashville, Tennessee.

35. Materials considered by the task force included the NIDA ARC's annual report for the period July 1, 1974, to June 30, 1975, and Wikler 1972. A similar review of the clinical, pharmacological, physiological, and biochemical investigations was submitted in the same time frame by Martin.

36. The only prison industry for which ARC participants were eligible was the printing trades, and they could not earn meritorious compensation for both participating in studies and working in the print shop. A "Schedule for Meritorious Compensation" appeared as attachment 3 in the report of the National Commission for the Protection of Human Subjects (1976a). Compensation was set at five dollars per study day and could not exceed six study days per month for single-dose studies or forty dollars per month for chronic studies. "Routine jobs" were capped at twelve dollars per month. At the end of their stay at the ARC, patients received a fifty-dollar bonus for each year of participation but could not receive more than one hundred dollars. This schedule responded to former criticisms—made in the pre-NARA days—that the ARC environment was "seductive."

37. Information in this paragraph appears on the Web site of the Advisory Committee on Human Radiation Experiments, at [http://www.hss.energy.gov/healthsafety/ohre/roadmap/achre/chap9\\_4.html](http://www.hss.energy.gov/healthsafety/ohre/roadmap/achre/chap9_4.html). In 1971, DHEW produced an institutional guide to its policy on protection of human subjects, in an attempt to regularize federal policy on the use of human subjects.

38. This laboratory was involved in the visualization of opiate receptors, which catapulted neuropharmacology into public visibility in 1973. See Goldstein 1997.

39. Despite his pioneering animal models, Wikler never believed they could "furnish a complete inventory of the variables that determine human adaptation," although they served as "limited models of 'learning' that may apply to man as well" (1957, 225–26).

40. Petersen compiled a list of all extramural projects, which was published in July 1975 as the first two volumes of the NIDA Research Monograph series. *Findings of Drug Abuse Research, 1967–1974* illustrates both rapid expansion and lack of coordination in the addiction research enterprise, summarizing over thirty-five hundred studies supported by over ten federal agencies and conducted by 650 researchers. Neither volume acknowledged the existence of an intramural program.

41. Although the intramural program became a poor second cousin, working in it had its advantages for those whose approaches were not underwritten by the more publicly visible extramural program of this highly politicized field. Two examples of such approaches suffice: Tsung Ping Su's career-long research on kappa receptors, which did not appear to play a role in opiate addiction but may hold the key to understanding the action of amphetamines (Su 2003); and genetic research undertaken in George Uhl's laboratory shortly after NIMH's retraction of claims about the genetics of schizophrenia (Uhl 2003).

42. Bernard Barber, “Prepared Statement,” in U.S. Cong. 1973.

43. Six experiments involving mixed viruses and mycoplasmas were done on humans in a Ramsey, Texas, prison unit between September 19, 1970, and June 2, 1974; two more were done in 1976; and recombinant DNA experiments took place from 1974 through 1979, when little was known about its effects (Tabenanika 2002). Similarly, infectious disease research at the Maryland House of Detention in Jessup, Maryland, indicated lack of respect for human life and dignity (Gilchrist 1974). Yet in August 1979, *Bailey et al. v. Lally*, the ACLU test case at Jessup, was concluded in favor of the University of Maryland researchers who were the defendants. The ruling held that prisoners could volunteer even in generally poor prison conditions. The scientists had already withdrawn from the prison in January 1976, before the trial got under way and long before the ruling on their behalf. Finally, the Jackson program, although ethical, involved direct contact between commercial interests, researchers, and subjects, with few layers of oversight. It shut down in 1989; the stigma of prison research rendered its continuance impractical.

## CHAPTER 7

1. Those who follow scientists at work speak of “epistemic cultures” (Knorr-Cetina 2001) or “epistemic communities,” defined by Haas as professional networks whose members share “recognized expertise and competence in a particular domain and an authoritative claim to policy-relevant knowledge within that domain or issue-area” (1992, 3). Although members of an epistemic community hail from a variety of disciplines, they share a consistent set of normative principles and beliefs that provide the rationale for their activities, a set of causal beliefs derived from how they go about analyzing problems central to their domain, and a basis for linking their findings to policy. They also share ideas about how to validate knowledge in their expert domain. Finally, they see themselves as engaged in a common research and policy enterprise to enhance human welfare. The social worlds (or arena analysis) approach points to the translation of shared beliefs and commitments into practice (Clarke 1998, 2000, 2005).

2. Behavioral pharmacologists I interviewed include Robert Balster, George Bigelow, Thomas Crowley, Roland Griffiths, Chris-Ellyn Johanson, Charles R. (Bob) Schuster, and James (Jim) Woods. The present chapter is based on their interviews and publications, visits to their laboratories, and a literature review that included all extant internal histories of behavioral pharmacology. The generosity of these scientists was tangible, and I hope that this account does justice to the complexity of their views, practices, and politics.

3. This statement, made in the course of my 2005 interview with Becker, captures the spirit of the behavioral pharmacology enterprise, despite Becker himself being a critic of behaviorism (as was his mentor, Herbert Blumer). See Plummer 2003.

4. Publications of interest include Dews 1955, 1958; Morse 1955; Ferster and Skinner 1957.

5. CPZ is a major tranquilizer marketed in the United States by Smith Kline and French Laboratories under the trade name Thorazine. See Brady’s foreword to Thompson and Schuster 1968; Dews 1985, 3–5.

6. CPZ was synthesized in the attempt to find an antihistamine to better manage stress. Invented in the 1920s by David Macht, the “rope-climbing test” was the pharmaceutical industry’s first screening technique using animals. CPZ rendered rats “indifferent” to food rewards; they refused to climb a rope even to escape aversive shock. The drug was included in medical kits to manage “battlefield stress” in Korea (Healy 2002, 82).

7. The formalization of behavioral pharmacology included the First International Conference of Neuropsychopharmacology, held in Rome in 1958, and the formation of the Behavioral Pharmacology Society in 1957.

8. Thompson and Schuster cite a decrease in LD-50 (lethal dose for half of the subjects) when animals are on amphetamines and subjected to stress or crowding (Weiss, Laties, and Blanton 1961).

9. For a similar attempt to map such a convergent interdisciplinary domain configured around mutagenesis, see Frickel 2004.

10. Actively working on a “two-factor learning theory” of relapse, Abraham Wikler cited presentations on animal self-administration by Weeks, Schuster, and Thompson at the 1963 CDAN meeting in Ann Arbor, Michigan. Wikler’s first factor was “temporal contiguity” between onset of abstinence and specific environments; his second was a version of hustling called the “reinforcement of instrumental activity” or “morphine acquisitory behavior” (Wikler 1965, 89). Wikler saw the new behaviorist vocabulary as a route to operationalize “mentalistic” concepts or such cultural activities as hustling.

11. Red Rodney was known as a “musician’s musician.” He was a long-term heroin addict who made several trips to Lexington.

12. On associations between heroin and jazz, see Davis 2003; Jonnes 1996.

13. This meeting was held in 1963, prior to when the NRC committee changed its name to the Committee on Problems of Drug Dependence in 1965.

14. A lifelong cigar smoker, Seevers knew something about the strength of desire and underwent a notable conversion in the late 1960s when he was appointed to chair the American Medical Association’s Committee on Tobacco and Health. He prohibited smoking in the department, ended sale of tobacco products in the University of Michigan hospital, and encouraged Schuster and Lucchesi to study the effects of intravenous nicotine on human subjects (healthy adult volunteers of both genders). The study attracted the interest of Jerome H. Jaffe, serving as the basis for Schuster’s subsequent move to Chicago.

15. Tomoji Yanagita, the Japanese scientist responsible for the technical innovation of the backpack apparatus necessary for behavioral study of monkeys, concurred with this statement in an interview that Schuster conducted with him.

16. Douglas Candland contrasted the physical sciences, where the goal is to eliminate variance because it confounds predictive accuracy, to the behavioral sciences, where the goal is to accept and measure variance as a descriptive technique. “Measures of variance,” he argued, “can be just as reliable as formulas that strive to eliminate or reduce variance” (1993, 357), yet popular conceptions of the physical sciences as reliable or rigorous remain deeply interred in the distinction between the “hard” and the “soft.”

17. Analysis of specific sites, or receptors, in the brain where drugs exert rewarding and reinforcing effects did not become technically feasible until after behavioral tech-

niques and procedures were developed and validated (Cozzens 1989; Pert 1997; Snyder 1989).

## CHAPTER 8

1. By joint resolution in 1989, the Decade of the Brain (1990–2000) was designated by the U.S. Senate and House of Representatives and a presidential proclamation by George Bush, Sr., in July 1990 (Jones and Mendell 1999).

2. Evelyn Fox Keller describes sociotechnical “borrowings” between physics and molecular biology that are not unlike how substance abuse research has incorporated neuroscience and genetics: “I want to argue that physics and physicists provided a resource of far greater import for the success of molecular biology than any particular skills; namely, they provided social authority. That authority was, of course, acquired in the first place through the formidable displays of technological and instrumental power issuing from physics itself, but this initially technical authority soon became available for deployment far beyond the domain of their technical triumphs; it became, in short, an authority that could be called upon for the essentially social process of reframing the character and goals of biological science. This borrowing proceeded in a variety of ways—first, through the borrowing of an agenda that was seen as looking like the agenda of physics; second, by borrowing the language and attitude of physicists; and finally, by borrowing the very names of physicists” (1992, 98).

3. I thank Kathryn Keller for bringing to my attention the special issue of *Science* with Bloom’s article.

4. The field of science and technology studies offers a vast literature on eugenics and genetic determinism (e.g., Duster 2003; Gould 1981; Kevles 1985; Rafter 1997; Roberts 1997; Stepan 1982).

5. The extent to which it is accurate or legitimate to refer to cycles of drug use as “epidemics” is debatable. The importation of epidemiological discourse into the drug field was furthered by Hughes and Jaffe 1971 and Hughes et al. 1972. Courtwright (1982/2001) is an example of a drug policy historian who makes great use of epidemiological mapping.

6. The 1990 OTA report claimed, “That drug abuse is a chronic relapsing condition and that drug abusers are a heterogeneous population with other social and behavioral problems pose obstacles to effective treatment” (1). The OTA report repeated the phrase “chronic relapsing disorder” without citing its origins or examining its meaning, while castigating thirty years of federally funded addiction research for failing to produce “studies that attempt to conform more closely to research principles” (10). Suggesting that the lack of rigor and unsophisticated, anecdotal, and uncontrolled studies were endemic to the field, the OTA advised new studies designed to “dissect” treatment programs so as to determine which components were effective for which “client groups.” The report advanced targeted study and individually tailored treatment as the solution: “Ultimately, research on drug abuse treatment should lead to what has been a common practice in medicine, namely a case management approach with an individual tailored plan to maximize the likelihood of treatment effectiveness” (10). The OTA report cited NIDA favorably for embarking on randomized, controlled trials. Although this damn-

ing assessment was hardly recognizable by those within the field, it galvanized the turn toward neuroscience.

7. Campbell 2000 provides a detailed critique of biological determinism in drug policy discourse. For instance, the CRBD could be used to cast punishment as ineffective, inhumane, or nonscientific. By contrast, it could be used to justify congregate care or orphanages for the children of addicts or to write off continued allocation of public resources toward treatment and research.

8. These claims are made within specific structural and social contexts that have professionalized the frontline treatment workforce (Payne, Schreiber, and Riley 2004). Although anecdotal accounts circulate about resistance to science and evidence-based thinking among addiction treatment professionals, my ethnographic observations have led me to believe that there has been a sea change. Many treatment providers regularly appeal to such scientific constructs as the CRBD, tout neuroscience as the path to enlightenment, and cite science as a “tool” for dealing with difficult people and complex problems.

9. This can be best glimpsed through a comparison to the form that neuroscience has taken elsewhere (Xie 1999).

10. Ehrlich’s earliest reference occurred within a 1913 speech before the general session of the Seventeenth International Congress of Medicine in London; subsequent references can be found in the first volume of his collected papers (1956).

11. On previous preoccupations with unveiling the “deep femininity” lodged in the brain, see Ludamilla Jordanova 1989, 56–58. In “Nature Unveiling Herself before Science,” Jordanova discusses the “physiognomic mentality” that encouraged the move from “visual signifiers to other, invisible, inner signifieds” (1989, 92). She writes: “The process of looking is central to the acquisition of valid knowledge of nature. From classical times, science and medicine have been explicitly concerned with the correct interpretation of visual signs, and skill in those fields was pre-eminently seen as a form of visual acuteness” (1989, 91). Carolyn Merchant refers to the figure of *Nature Revealing Herself to Science*, a statue by French sculptor Louis-Ernest Barrias that Merchant says “suggests the sexuality of nature in revealing her secrets to science” (1980, 190).

12. The quotation echoes Francis Bacon’s “Enough if, on our approaching her with due respect, she condescends to show herself” (quoted in Keller 1992, 57). Merchant (1980) and Keller (1985, 1992) have shown that courtship metaphors are gentle versions of the scientific assault on feminized nature.

13. Lecturing at Stanford University in the late 1960s, Vincent Dole motivated molecular pharmacologist Avram Goldstein to take up opiate biochemistry. Dole and Nyswander hypothesized that addiction was a metabolic disease in which genetics played some role (1967, 19–24). Goldstein (1976) postulated existence of an endogenous reward system.

14. The clinically effective dosage is believed to be the dose that occupies a certain fraction of receptors for that class of drugs.

15. Information on Charles P. O’Brien comes from O’Brien 1998 and 2005.

16. My thanks to Graham Florry for sending me this paper along with similar retrospective accounts by Harris Isbell and Maurice H. SeEVERS.

17. Information on fMRI is taken from Savoy 2001. I would like to thank Rachel Dowty, Colin Beech, and Sal Restivo for illuminating conversations on fMRI.

18. Information can be found at <http://www.uphs.upenn.edu/trc/conditioning/studies.html>. I also relied on a segment of the Bill Moyers special “The Hijacked Brain,” which involved Childress explaining her research, as well as on Childress 2006.

19. Record Group 511 ADAMHA Alcohol, Drug Abuse, and Mental Health, Administrative Office, Clinical Research Center, Lexington, KY, Public Relations, 1939–1973, folder 2, 2. Jurgensen presented this paper on August 30, 1966, in Baltimore, Maryland. He was departing Lexington to head the narcotics farm in Fort Worth, Texas.

## CONCLUSION

1. Chartered in 1970 as a component of the National Academy of Sciences, the IOM is a nonprofit science advisory board and honorific membership organization that provides science-based advice to government agencies by relying on unpaid, volunteer experts.

2. NIDA was subsumed into NIH in 1992, leaving the public service aspects of treatment and prevention in the Department of Health and Human Services division of the Substance Abuse and Mental Health Services Administration (SAMHSA). SAMHSA created the Center for Substance Abuse Treatment (CSAT) to expand access and enhance quality of treatment services and established the Center for Substance Abuse Prevention for the purpose of technology transfer of prevention materials to treatment providers.

3. The concept behind the Practice Research Network is a social innovation used in many fields, including social work, psychiatry, and medicine. I conducted an institutional ethnography within a statewide PRN initiative that was part of CSAT’s 1999–2003 Practice Research Collaborative grant program. Primary investigators were John Coppola, executive director of the Alcoholism and Substance Abuse Providers Association, and Frank McCorry of the New York State Office of Alcoholism and Substance Abuse Services. My position as a participant-observer from fall 2001 through fall 2005 offered a fascinating vantage on how the social machinery of credibility of substance abuse research operates in the field of clinical practice. I thank everyone involved for their generosity in allowing me both to observe and participate.

