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Discovering Addiction

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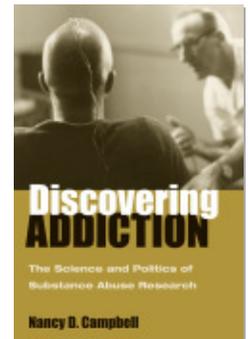
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CHAPTER 1

Framing the “Opium Problem”: Protoscientific Concepts of Addiction

What American publics and institutions define as worthy cures for drug addiction depends on who is perceived to be addicted, on what drugs addicts depend, on the meanings attributed to addiction, and on patterns of social status. The modal late nineteenth-century American addict was an upper- or middle-class white woman maintained on morphine by her physician. Respectable “medical addicts” gave way to an urban underclass that used narcotics for “nonmedical” purposes or “recreation.” These new addicts were culturally distinct from their precursors: these poor, working-class, increasingly African American and white ethnic males were viewed as part of the “dangerous classes.”¹ How addicts are treated very much depends on their membership in specific social groups; they cannot be lumped together as raceless, classless, or genderless. Historian David Courtwright has written, “What we think about addiction very much depends on who is addicted” (1982/2001, 4). The associations between illicit narcotics and “delinquent” subcultures worked against recognition that addiction might be a matter for scientific research or clinical investigation and that even “nonmedical” addicts might use narcotics to medicate themselves in absence of legitimate alternatives. Today, the distinction between “medical” and “nonmedical” has become blurred (DeGrandpre 2006). The long-standing division between “medical” and “nonmedical” obscures the historical dynamic of self-medication among “nonmedical” users.

The social transformation of the addicted population is sometimes attributed to the 1914 passage of the Harrison Act, which abruptly criminalized

physicians' dispensing of narcotics. Narcotic use actually began to decline before that law was enacted (Courtwright 1982/2001). Social and professional learning about the negative organic, social, and economic effects of drug use was primarily responsible for the decrease (Musto 1973/1999). However, patterns of law enforcement and the demographics of criminalization affected how addiction was thought of as a medical and scientific subject. After the constitutionality of the Harrison Act was established in 1919, physicians became reluctant to maintain patients on opiates, for fear of prosecution. Shying from legal entanglements, physicians received little education about how to treat addiction. The medical profession entered a state of sanctioned ignorance during which only a handful of physician-researchers and scientists pursued systematic knowledge about addiction. This book is about those who dedicated scientific careers to studying the neurophysiological processes—but not the social dynamics—of opiate addiction. This chapter characterizes the state of addiction knowledge prior to the mid-1920s, when addiction research was first organized as a cooperative scientific enterprise.

CLINICAL ENTREPRENEURIALISM: THE EARLY STAGES OF A PROTOSCIENCE

Early on, competing theories took root about the underlying neurological and physiological processes of addiction and the disturbing propensity to relapse. The disease concept of addiction goes back to a cultural emphasis on abstinence and temperance that emerged as early as the 1780s. The concept did not really come into clinical and scientific vogue until the late nineteenth century, when animal tests and autopsy studies were first conducted to establish the effects of opiates on tissue. Early clinical studies on intact organisms yielded little scientific consensus. Nineteenth-century and early twentieth-century treatment took the form of clinical techniques for withdrawing individuals, taking up practical questions of whether patients required confinement—or even quarantine—due to the socially contagious nature of their disease. Physicians concerned themselves with the nature of the addicted person's diet; use of proprietary remedies, ranging from hypnotics (e.g., chloral hydrate) to purgatives (e.g., strychnine) to dilute opiates (e.g., laudanum or paregoric); and participation in practical therapies that supplied "an occupation of an absorbing kind" (Terry and Pellens 1928/1970, 546).

Addiction treatment reflected medical sectarianism in microcosm (Starr 1982). Clinical sectarianism was fought out at the level of practical decisions

about whether or not to employ opiates and other “heroics,” how best to regulate dosage, and what the etiology and progression of the disease was supposed to look like. Public debate over the social implications of maintaining addicts on “another poison” arose in the wake of the Harrison Act. Drug maintenance has been the most contested treatment modality, continually constructed not as treatment but as tantamount to condoning drug use or being “soft on addicts.” In the late teens and early 1920s, clinics were briefly established to “maintain” opiate addicts. However, *Webb et al. v. U.S.* (1919) barred physicians from maintaining patients, forcing medical practice to change even faster than it otherwise would have (Courtwright 1982/2001). Changes in clinical practice came out of physicians’ experience with iatrogenic addiction and a culture that did not construct narcotics use as a “public problem” except when it occurred among the so-called dangerous classes. When opiates were one of the few effective weapons in the medical armamentarium, physicians were represented as progenitors of addiction, nefarious purveyors of dope who preyed on innocent, largely female patients. Due to relatively unfettered access to narcotics, medical professionals themselves were subject to addiction at high rates. Developing reliable therapeutic expertise for withdrawing people safely and insisting on abstinence comprised one defense against the charge of iatrogenic addiction leveled at their profession.

Studies in which researchers administered drugs to themselves, family members, or close associates were the primary form of research that supplemented learning from clinical experience during this stage of “clinical entrepreneurialism.”² Clinicians developed a bewildering variety of therapeutic innovations in relatively private settings. Because clinicians themselves were the first “addiction researchers,” many did not take kindly to the emergence of an organized, public research apparatus. One of the first U.S. Public Health Service investigators, Clifton K. Himmelsbach, explained:

Many individual, excellent physicians attempted to do something about [drug addiction], and their approaches had to do with devising new ways of treating withdrawal. They felt that if they could treat withdrawal effectively, that would cure drug addiction. So that was the era of many, many new and weird kinds of treatments of withdrawal, including causing an individual’s skin to blister and withdrawing the blister fluid and injecting that into them. All sorts of weird things were done—purgation, the use of atrophy and alkaloids, etcetera, just some perfectly astounding things, most of which did more positive harm than good so far as the suffering of the individual is concerned during withdrawal.³

Nineteenth-century treatment modalities, nostrums, and techniques were purveyed through private practitioners; franchises, such as the Keeley Institutes; or congregate institutions, such as the New York State Inebriate Asylum in Binghamton, New York (White 1998). These were founded on the notion that alcoholism and opiate addiction were curable, despite there being no systematic technique by which physicians could assess the extent of addiction or predict its degree of curability.

Should modern readers doubt that a vast amount of empirical research was conducted on drug addiction in nineteenth-century and early twentieth-century clinical settings, even brief perusal of *The Opium Problem* (Terry and Pellens 1928/1970) should set such doubts to rest. This controversial compendium, produced in 1928 by the Bureau of Social Hygiene in New York City, summarized over four thousand treatises in more than a thousand pages. Aptly characterized as "diffuse" (Acker 2002, 59), *The Opium Problem* attests to the sheer multiplicity of uncoordinated and individualistic efforts to cast drug addiction as a matter for systematic clinical and scientific investigation. After the bureau had supported addiction research for three years, the Rockefeller Foundation took responsibility for the systematic study of drug addiction, before courting the National Research Council (NRC). The NRC, an organ of the National Academy of Sciences, was initiated to coordinate war-related research during World War I. In 1929, the NRC merged its Committee on Pharmacological Research with the New York bureau's Committee on Drug Addictions (Acker 2002, 77).

The resulting NRC Committee on Drug Addiction was constituted to take an "increasingly extended intellectual approach and a corresponding decrease in emotional, commercial, and other interests," in order to provide a sound and well-founded basis for national and international drug control. The authors of *The Opium Problem* concluded that until policy was placed on scientific footing, "we should look with disfavor upon dogmatic statements and arbitrary and unscientific rulings relating to either groups or individuals, while seeking in the experiences of all earnest and intelligent workers such elements of fact as may be uncovered and utilizing them in the gradually evolving plans for prevention and control that unquestionably will develop with an increased knowledge and eventually supersede the chaos of contradictory opinion that marks present-day activities" (Terry and Pellens 1928/1970, 928). The newly incarnated NRC committee adopted a singularly pharmacological research agenda: developing a nonaddicting analgesic as a "technological fix," as Caro-

line Acker aptly termed it, for the opium problem. However, to achieve that narrow yet still elusive goal of medicinal chemistry, useful knowledge of how addiction worked had to be produced.

Like other Progressive disease typologies,⁴ early twentieth-century scientific investigations were directed at determining whether or not there were physiological or psychological markers useful for distinguishing addicts from nonaddicts (Acker 2002, 42, 50). By 1925, there was a consensus that there were no distinguishing physiological attributes that provided clinicians a reliable guide to identifying addicts (Acker 2002, 50). Lawrence Kolb advanced convincing explanations based on underlying psychopathology, which were implemented in the 1930s in the treatment regimen of the Public Health Service narcotics farms. Disease models of addiction came into clear relief against competing constructions of addiction as a moral problem (i.e., a vice, sin, or crime)—notions that Kolb undertook to dispel. In 1925, he published three papers based on work at the federal government’s Hygienic Laboratory in Washington, D.C., in which he set out diagnostic criteria—called the K-classification scheme—to assess the degree of psychopathology exhibited by drug addicts (Kolb 1925a, 1925b, 1925c).⁵ Used to categorize addicts for decades, the Kolb classification system was based on state-of-the-art psychiatric diagnostic categories that were modified with the passage of time (Kolb 1925c).

Kolb’s goal was to dispel popular associations between drugs and crime. He argued that opiates inhibited aggression and demonstrated that changes in law enforcement—not an increase in violent crime—had increased the number of addicts behind bars. He divided addicts into two primary categories: (1) those accidentally addicted via medical treatment but otherwise normal and (2) those predisposed by psychopathology to the “vicious” pleasures and consequent “deteriorations” of narcotic drugs. Assuming that the Harrison Act was making occupants of the first category an endangered species, Kolb argued that occupants of the second category, suffering from mental disease, be treated humanely. He adopted a language of psychopathology to protest punitive criminalization. His theory that psychoneurotic deficits predisposed individuals to addiction aligned with those of his contemporaries who attributed addiction to “constitutional psychic defects” (Acker, 2002, 152). Kolb’s construction of drug addiction as stemming from psychiatric problems was a strategic construction absent compelling physiological data. Production of that data and techniques and metrics for producing it would become the mainstay of the Addiction Research Center, which was housed at a thousand-acre congressionally mandated narcotics farm (nicknamed “Narco”) in Lexington, Kentucky,

where Kolb served as medical officer in charge from its opening in 1935 until the beginning of World War II.

Settlement of the physiological question by 1925 did not propel greater knowledge of addiction. Indeed, Caroline Acker shows that it acted as a limit against which proponents of criminalization asserted themselves. Treatment institutions founded on the notion that addiction was a treatable medical condition declined, leading to a decline in private addiction research (Acker 2002, 62–63). In her account researchers found themselves stymied, and addiction research stalled to become the backwater science that it would remain through much of the twentieth century. Acker claims that “no front in physiological research seemed to offer promising leads for understanding addiction” (2002, 63). Not until the opiate receptor research of the 1970s and even later use of medical imaging technologies for studying the effects of opiates on the brain, she argues, was there a promising physiological front. My story differs in that I argue that the early neurophysiological research established the knowledge base necessary for neuroscientific approaches ascendant today.

Addiction research had to become a highly interdisciplinary knowledge formation to get around technical obstacles, the obduracy of addiction as a social problem, and the politicization of that problem. Ongoing attempts to integrate physiological with psychological conceptualizations of addiction might be seen as “failures” but also stand as an early example of problem-oriented research and the attempt to base treatment and the politics of drug control on science. Scientific knowledge production about addiction took multiple forms during this period of seeming neglect. Enthusiasm for the multiple modes of clinical entrepreneurialism documented in *The Opium Problem* was dampened by the emergence of the national coordinating committee headed by the NRC. *The Opium Problem* concluded by raising the epistemological problem of policy making on the basis of uncertain and contradictory data. None of the fundamentals of the opium problem were sufficiently well established in 1928 as to be beyond doubt, according to the NRC committee. After reviewing a vast amount of data, the committee stated that the apparent honesty and intelligence of those who advocated contradictory positions convinced it to accept some “partial truth” from each of the statements, studies, opinions, and unproved theories that formed the data set (Terry and Pellens 1928/1970, 926).

Setting out this pluralist and constructivist account of the scientific knowledge of “chronic opium intoxication,” the committee advocated the “most elastic administrative measures possible” given the “default of exact knowl-

edge” (Terry and Pellens 1928/1970, 926). The authors of *The Opium Problem*, physician Charles Terry and his wife, Mildred Pellens, were sympathetic toward opiate addicts and had a sense of urgency about the declining availability of treatment in the wake of the Harrison Act. They believed that an opportunity for a more systematic knowledge of addiction was disappearing. Noting that maintenance clinics offered opportunities for further scientific study of the kind that might place policy on a more rational footing, Terry and Pellens recommended the development of a research program by qualified individuals to apply existing knowledge in the clinic (1928/1970, 927).

Physiological models of addiction were multiple, converging only on the hope of a cure. For example, autoimmune theories based on the idea that drugs are “toxins” were advanced independently in the 1910s by George E. Pettey (Terry and Pellens 1928/1970, 548–52) and Ernest S. Bishop (Acker 2002, 39–40). These theories owed much to bacteriological models underlying the germ theory of disease.⁶ Proponents of autoimmune theory considered addiction the direct correspondent of infectious disease. One of them wrote in 1910 that “morphinism is a disease as really [*sic*] as is typhoid fever or pneumonia” (quoted in Terry and Pellens 1928/1970, 145). Proprietary “antitoxic” treatments arose, including Narcosan, a mixture of lipoids, nonspecific proteins, and vitamins that was responsible for a number of deaths. Based on the theory that narcotics call forth protective or neutralizing substances when introduced into the body, Narcosan was thought to protect against these unidentified substances in the blood and reduce the discomfort of withdrawal. One critic of Narcosan, George S. Johnson, found that the mixture added no value to the withdrawal process. Another criticism of Narcosan was based on the theory that addiction was an endocrine disturbance or hormonal imbalance. Proponents of this theory advised injections of testicular and ovarian “proteals,” which were inexpensive and easily manufactured since they were not patented.⁷ The Narcosan debate illustrates how unregulated competition sometimes caused grave public health problems during the era of clinical entrepreneurialism.

Hoping to identify the underlying structural pathology of addiction, clinicians unsuccessfully tried to locate an essential “toxemia” or even brain lesions, which many claimed existed but none could prove. Agonistic contestation over the merits of various therapeutic approaches and etiological theories led Terry and Pellens to complain: “The most apparent conclusion to be reached from the material reviewed is that, for the most part, treatment of this condition has not emerged from the stage of empiricism” (Terry and Pellens 1928/1970, 428).

After a thorough review of the extant literature, they found that few proponents of treatment procedures offered rationales for measures they urged others to adopt. Like most Progressives, Terry and Pellens evidenced more faith in the research enterprise than its findings warranted, and they failed to locate sufficient proof to warrant the unqualified acceptance of any theory. Thus they urged more research, including laboratory experiments designed to yield generalizable conclusions.

Scientific rationalists saw themselves as particularly opposed to moralists, for whom addiction was simply a bad habit or a vice. Judgments about whose views were most scientific mapped onto positions regarding which treatments worked best. Pettey wrote scathingly that those who held addiction to be a "perversion of the will" advocated coercive, abrupt withdrawal "notwithstanding the fact that others who have tried it have wound up with a maniac or a corpse to testify to the success of their efforts" (quoted in Terry and Pellens 1928/1970, 146). Those who constructed themselves as scientists scolded their unscientific brethren for holding the position that addiction resulted from emotional or psychological defects: "When will it be realized that all the derivatives of opium, whatever they are, have the same objections, the same dangers, and lead to the same addiction, and that the only way of curing an addict is not to give him another poison, but to remove the one he is taking?"⁸ Most, if not all, early studies recognized both "psychic" and physiological components at work in the production of addiction.

A bewildering welter of theories, definitions, and models of addiction arose. Attempts to define the pathology, symptomatology, and prognosis of the disease were so heterodox that they were easily displaced by the set of newly dominant ideas that became the "official model" of the 1930s (Acker 2002, 32, 38). The official model was a mosaic crafted from an array of conceptual approaches and clinical phenomena, consisting of both physiological pieces (e.g., the phenomena of tolerance and withdrawal) and psychological pieces (e.g., explanations based on psychopathy or personality deficit). The dynamic proliferation of theories about addiction as a disease process meant that researchers could pursue their own ideas without having to integrate or account for those of others (Caroline Acker, personal communication with the author, August 18, 2006). However, the physiological model put forward by the particular thought collective described in chapter 3 was able to define the parameters within which more research would take place (Fleck 1979).

The search for a technological fix proceeded in an opportunistic, uncoordinated, and heterodox fashion prior to the advance of the "official model" of the

1930s. Just as Terry and Pellens feared, an opportunity for systematic clinical study was lost with the abrupt closure of the maintenance clinics in the early 1920s. The decay and dismantling of the treatment infrastructure and the deskilling of physicians meant that clinicians emerged from the era of clinical entrepreneurialism less knowledgeable about addiction, treatment, and pain management than they had been before the Harrison Act.

PSYCHOANALYSIS, “NARCOTIC BONDAGE,” AND REPETITION COMPULSION

Coexisting alongside clinical entrepreneurialism was a second important inquiry into addiction, that of psychoanalysis. No longer considered scientific (and disdained by many scientists), psychoanalysis has just about disappeared from drug historians’ gaze and might be considered unworthy of attention in a book like this. In its day, however, psychoanalytic interpretations garnered more attention than did prescientific, empirical studies of addiction. Psychoanalysis could be considered to have been the primary professional domain for the study of drug addiction in the first several decades of the twentieth century.⁹ The legacy of psychoanalysis lingers, which is not to say that the lineage is straightforward or simple. Today, the powerful drive to repeat seemingly irrational acts is couched in the unassailably naturalized neurobiological and behavioral vocabularies of putative “pleasure genes”—not in terms of Freud’s pleasure principle. Drug users now enjoy endorphin rushes, which require no recourse to the concept of libido. Operant conditioning explains repetition but not, perhaps, compulsion. There are nevertheless common structures of belief between now-discredited pseudoscience and neurobehavioral and genetic accounts: (1) addicts are inexorably driven to repeat unintelligible acts by internal and external forces beyond their control; (2) these acts have undeniably negative consequences; (3) the underlying internal mechanisms take psychopathological form; and (4) addicts are not masters of themselves but are instead in thrall to the external forces of suggestion, substance, and impulse. These beliefs persist even in scientific accounts cleansed of the language of psychoanalysis. Despite the declining cultural salience of psychoanalysis, it has provided a wellspring of beliefs about addiction as an artificial or unnatural state of dominance by the id and dissolution of the ego.

Through psychodynamics, an American branch of psychoanalysis with medical ambitions and scientific pretenses, some of these beliefs still have traction today. The persistent vestiges of psychoanalysis can best be explained by

recourse to Ludwik Fleck's idea of thought collectives, each of which form thought styles, defined as a "readiness for directed perception, with corresponding mental and objective assimilation of what has been so perceived." These thought styles undergo social reinforcement, a feature of all social structures, which then constrain the individual by "determining 'what can be thought in no other way'" (Fleck 1979, 99). According to Fleck, whole eras are ruled by particular thought constraints, which later leave "remnants" in the form of multiple ties or historical connections to thought styles that dominated previous eras (1979, 100). The history of addiction science can thus be narrated as a succession of thought styles that displace one another yet conserve some aspects of the previous structure of social and cultural constraints on cognition.

As early as 1897, Freud regarded drug addiction as a substitute for masturbation, the "primary addiction" (Rosenfeld 1965). Believing there was a chemical basis for psychosexual behavior, Freud posited that endogenous or "toxic" mood alterations freed individuals from the "compulsion of logic" and made them enter "suggestible" states over which the pleasure principle no longer presided. He argued that repetition compulsion, now independent of the pleasure principle, gave the appearance of a "hint of possession by some 'daemonic' power" (Freud 1961, 26–30). In *Three Essays on the Theory of Sexuality* (originally published in 1905), Freud sketched clinical similarity between neurotic disorders of sexual life and the "phenomena of intoxication and abstinence that arise from the habitual use of toxic, pleasure-producing substances" (Freud 1975, 82). Freud's followers categorized addiction as a disorder of desire; a psychosis or neurosis; a borderline state; a perversion or fetish; an impulse neurosis; a form of narcissism, mania, or melancholia; or an outcome of castration fears, "dismemberment motives," paranoia, schizophrenia, sadism, and masochism. One of the most coherent psychoanalytic explorations of addiction can be found in the corpus of Sandor Rado, an orthodox Freudian who advanced an influential account of the "pharmacogenic orgasm" prior to emigrating to the United States from Germany (Rado 1926, 1933). After a decade at the New York Psychoanalytic Institute, Rado's ambivalence toward Freud led him to found the Columbia Psychoanalytic Clinic for Education and Research, the first U.S. graduate psychoanalysis center that was under medical auspices. His writings illustrate the discursive shift between the psychoanalytic vocabulary of desire and a psychodynamic vocabulary more resonant with biomedicine.

Finding an inexact science in the pharmacology of the 1920s, Rado advanced a "metaerotic" theory to penetrate the "obscure region" of morbid cravings (1926, 396). He suggested that the artificial technology of the hypoder-

mic syringe displaced “natural” libidinal discharge by short-circuiting addicts’ biological sexual apparatus. The “potent poisons” they ingested forced the natural genital organization to surrender to an unnatural form of erotic gratification, the “pharmacotoxic” or “pharmacogenic” orgasm, equivalent to the diffuse, pregenital alimentary orgasm of the baby at the breast.¹⁰ A siege metaphor pervades Rado’s writings on the subject, in which the id conquers the ego and erects an “absolute monarchy” (1926, 405). Drug addiction was framed as the outcome of a battle between a weak ego and an imperialist id that deranged the dependable regulation of pleasure and pain.

In his classic 1933 paper, Rado argued that drugs subsumed addicts’ reality and allowed an artificial “pharmacothymic regime” to triumph, causing altered libido, mental atrophy, and sexual impotence (68–70). He retained Freud’s insight that addiction substituted for masturbation, claiming that the pharmacothymic regime “initiates an artificial sexual organization which is autoerotic and modeled on infantile masturbation” (71). Those who used drugs in an auxiliary or “prosthetic” manner were excused from full-blown pathology: for instance, normal coffee drinkers and smokers exhibited “abortive forms of this illness” (79). Among the first to propose that drug addiction was a functional (if maladaptive) attempt to self-regulate “tense depression,” Rado viewed drug use as the libido’s attempt to take what it could get in the absence of actual gratification. He maintained that the problem with the “spurious pleasures” of drugs and alcohol was their false inflation of the action self, which managed self-awareness and hedonic control in contrast to the “dark unexplored continent” of the physiological processes of bodily pleasure (Rado 1969, 24). A theory of “narcotic bondage” provided Rado a touchstone throughout his lengthy career. His dedication stood in marked contrast to his contemporaries, most of whom considered the subject uninteresting.

Addiction interested Rado because he was part of a movement to place psychoanalysis on an experimental, hypothesis-driven—or “basic” science—footing (Rado and Daniels 1956). A leading proponent of the movement called “adaptational psychodynamics,”¹¹ Rado emphasized that the self-awareness and organismal integrity of the “action self” was propped up by physiological mechanisms or “neural activating systems” (1969, 52). “According to my hypothesis,” he wrote, “the action self is the organism’s systemic picture of itself, derived from the information it receives about its own activities by means of its sensory equipment” (1969, 49). He argued that the action self was directly affected by the pleasures of consumption: “If a man cannot derive a kick out of what he does, if he cannot feel happy and satisfied through his suc-

cessful actions, he has nothing to feed his action self with because the action self is nourished upon the pleasures experiences derived from successful activity. Factors that go into developing this certainly include genetics and past experience" (1969, 50).

Adaptational psychodynamics attempted not only to cleanse psychoanalysis of its dependence on Freud but to incorporate biochemical and behavioral elements of systems theory. Elucidating basic laws of behavior in biobehavioral systems, Rado went so far as to construct a taxonomic table of behavioral disorders along the lines of organic chemistry (1969, 173–75). His general theory of "narcotic bondage" was a search for the essential pathology underlying all "drug dependence," a term he and others substituted for "addiction" in the 1950s. Rado maintained that self-medication was "a malignant form of miscarried repair artificially induced by the patient himself" (1957, 165). Self-government, he proposed, was an evolutionary framework that culminated in mature human adults fit for the levels of cultural cooperation exacted by democratic systems (1957, 167). In adaptational psychodynamics, "the organism [was viewed] as a biological system operating under hedonic control" (Rado 1957, 167). Similar to his contemporary Roy S. Grinker, who espoused a theory of functional anxiety, Rado thought that pain, stress, and deprivation were necessary to stimulate growth. He considered addiction almost a "natural experiment," because it interfered with hedonic regulation, a cumulative process modified by culturally specific patterns of reward and punishment: for example, sophisticated individuals could accept "delayed reward in lieu of immediate reward," but the less sophisticated could not (Rado 1957, 167). Both pleasure and pain took more culturally specific form in adaptational psychodynamics than in the experimental approaches of behavioral analysis (see chap. 7). Reward, however, indicated successful performance in both lexicons.

Rado explained that narcotic bondage resulted from the failure of gratification to travel the normalized pathways of reward in systems where "effort and performance are spurred by the pains of deprivation and are directed, facilitated, and rewarded by a variety of pleasures" (1957, 167). He maintained that conduct within regimes of hedonic control required the predication of education on "reward and punishment, that is, offering pleasure and inflicting pain" (1963, 164) and that reward varied between individual and species: at the physiological level, the "alimentary orgasm" signaled nutritional satisfaction; at the reproductive level, the sexual orgasm signaled insemination (1957, 167; 1963, 164). Rado argued that narcotic bondage threatened the species because it represented a failure of the entire regime of hedonic control. He

observed that absent “standard (hetero)sexual union,” masculine failures, including impotence and homosexuality, appeared in addicts (1963, 163). Rado explained that in the “adaptive struggle for existence” waged between a primordial desiring-self and a reality-tested self, the reality-tested self normally triumphed (1957, 166); and he noted that the “superpleasure” of narcotic bondage deposed the reality-tested self and enthroned the grandiose desiring self, a process that reduced highly evolved individuals to infantile states. In Rado’s view, social and legal regulation could not control this failure; indeed, law enforcement, policing of affect, or imposition of punitive policies produced bitterness, defiance, and rage in addicts. Rado maintained that prohibition produced and incited the very behaviors it sought to repress.

Relating narcotic bondage to new research on the physiology of pleasure emerging from behavioral and experimental psychiatry, Rado claimed that psychodynamics foreshadowed the search for physiological mechanisms, pathways, and “pleasure centers” in the brain. He corroborated his theory with the work of James Olds, who made the well-publicized discovery that rats would electrically stimulate the “pleasure centers” in their brains up to two thousand times per hour and were highly motivated to work for such “brain-stimulation rewards.”¹² Optimistic that a biochemical immunization would be developed to guard against the dangers of superpleasure (Rado 1957, 169), Rado believed that only psychodynamics reached the underlying disorders that made subjects susceptible to narcotic bondage. Rado observed that these currently remained inaccessible to biochemistry, which lacked a method for the “human biological study of mental life” (1963, 160). Rado’s writing took on an increasingly strident tone as he witnessed the exclusion of psychodynamics from the “medical core” of addiction studies in the 1960s. Lamenting Freud’s mystical romanticism as a dangerous “revival of ancient animistic speculations” (Rado 1963, 161), Rado undertook a lifelong effort to make an honest biomedical science of psychoanalysis.

Marginalized and delegitimated by the mid-twentieth century, psychoanalytic constructions of addiction are puzzling in their ubiquity and tenacity. Psychoanalysts constructed addiction as a libidinal relation—a craving or overpowering desire—between persons and substances. Even where there was only a skeptical embrace of psychoanalytic concepts, craving, euphoria, and repetition compulsion were still called on to explain anomalous, intangible, obscure, or problematic relations between persons and their objects of desire. In the United States in the mid-twentieth century, psychoanalysis was also called on in courts of law, the popular press, and policy hearings, to explain otherwise unintelligible acts. When psychoanalysts incorporated into their thought a

vocabulary of stress informed by cybernetic systems theory and advances in endocrinology, they essentially smuggled psychoanalytic discourse into behaviorist, biological, and sociological explanations for the behavior of drug addicts—explanations that became dominant even as the cultural authority and persuasive power of psychoanalysis waned.

Constructions of addiction as a disorder of desire—resulting from disinhibition, regression, and the substitution of artificial pleasures for real ones—inherited not only in therapeutic discourse and cultural parlance but in global and domestic policy instruments. Official antipathy toward psychological constructions of addiction was well established. Yet the nation's first "drug czar," Harry J. Anslinger, chief of the Federal Bureau of Narcotics from 1930 to 1963, sounded like Freud when he wrote: "[Addicts'] intense overexcitement of the nerves and emotions leads to incontrollable irritability and irresponsible acts due to irresistible impulses of suggestive origin. . . . In the earliest stages of intoxication the willpower is destroyed and inhibitions and restraints are released; the moral barricades are broken down and often debauchery and sexuality results" (Anslinger and Tompkins 1953, 21). Even outspoken critics of psychoanalysis adopted the assumption that addictions are rooted in early childhood experiences that predispose some individuals to lives of violent crime, constitutional mental instability, or misdirected aggression.

Psychoanalysis could account for the force of repetition compulsion and explain why addictions were so notoriously difficult to overcome depending on personality type: "An egotist will enjoy delusions of grandeur, the timid individual will suffer anxiety, and the aggressive one will resort to acts of violence and crime. Dormant tendencies are released and while the subject may know what is happening, he has become powerless to prevent it. . . . The drug has a corroding effect on the body and on the mind, weakening the entire physical system and often leading to insanity after prolonged use" (Anslinger and Tompkins 1953, 22–23). Trying to contain a "potent weapon of aggression" wielded by amoral nations seeking to spread addiction to the "free people of the world," narcotics law enforcers placed themselves in the heroic position of defending democracy (Anslinger and Tompkins 1953, 10–11, 25). By contrast, those who could not deny themselves drugs were considered weak links in the modern project of globalizing democratic freedom through the cultivation of individual self-mastery.

Moral deterioration was the major threat that drug addiction was said to hold for democracy. Its sources were those of which Rado—and Freud before him—spoke: habitual substitution of chemical satisfactions for real ones. Psy-

choanalysis was losing its grip over the ownership of the problem; it was cast as “crude” in contrast to the new conceptual framework of experimental psychology (Lindemann and Clarke 1952). For American ego psychologists, “satisfaction of basic drives or tension states” underlay the motivation for using addictive drugs (Lindemann and Clarke 1952). They maintained that addicts simply did not have the proper tools for integrating perceptions, memories, and expectations; that the more realistic and mature an organism was, the more functional it would be, and the less likely it would use drugs to “depart from the plane of reality” or regress to “primitive forms” (Lindemann and Clarke 1952). According to the psychologists, how well individuals defended against emotional pressure depended on whether the ego had access to an arsenal of social skills: if pressures exceeded individual capacities, the result might be imbalance, regression, or drug use; addiction had become the failure to solve problems with proper tools or social skills. Recurring portrayals of drugs as technologies mediating between artificial and natural states, normal and abnormal desires, and self and other represent the continued presence of a psychoanalytic past that was deeply interred in addiction science.

In the psychoanalytic literature, dependency was figured as a survival technique—dating from infant experiences—in which adults magically responded to the infant’s simultaneous states of helplessness and omnipotence. The magical state of dependence was preempted by self-sufficiency, organization, and independence in most, but not all, adults. Maladaptive dependencies signaled the unconscious retention of infantile states. Dependent adults were considered problematic in a competitive society that equated masculinity with strength, dominance, and superiority, while equating femininity with weakness, submission, passivity, and inferiority.¹³ These pathological dependency formations were not just gendered but racialized by such psychiatrists as Joel P. Fort (1954), who described addiction as a perverse link between dependency and masculinity that formed in the “matriarchal circumstances” of most male African American heroin users. He concluded that pathological mother-son relations and father absence produced addiction, especially in urban African Americans. In the mid-1950s Fort was a psychiatry resident at the U.S. Public Health Service narcotics hospital in Lexington, Kentucky (1997). There he encountered a growing population of black and Puerto Rican patients and founded a chapter of the American Civil Liberties Union in Lexington.

The figure of a domineering mother was thought to cause heroin addiction by weakening masculine identification and creating within the child a dependency that was later transferred to drugs. In his therapeutic encounters with heroin-addicted men and boys, Fort saw a strong “desire to revolt against a

feminine identification," which he sought to release in the therapeutic encounter. This often opened tirades of invective against domineering mothers: "The resulting engram of masculinity in their minds was usually one of hostile, evil identification. The mothers often fostered a considerable sort of dependency which later on increased the young man's guilt over being a man. As these patients grew up, they tended to have to cover up their basic lack of masculine identification and dependency with violent aggression" (Fort 1954, 255). Fort maintained that heroin took the edge off fear, guilt, and doubt by providing a "phantasmagoria of psychic effects" that allowed subjects to achieve their desired goal, orgiastic pleasure; he argued that gang activities offered them a compensatory but "false" masculinity that supported individual gang members' "doubtful decision to be a man" (1954, 257). Fort saw the pharmacological orgasm as an erotic discharge of guilt, aggression, and "frankly incestuous interests": "The drug becomes an object in itself, and ultimately the only desired object" (1954, 253). Surprisingly, the idea that addictive substances subsume all other forms of gratification became a means through which psychoanalytic interpretations were extended into explanations of addiction that centered the role of social stratification in producing and sustaining patterns of drug use.

Multiple social worlds contended for problem ownership of drug addiction. The core group of addiction researchers repudiated psychoanalysis, as illustrated by a 1958 symposium of the National Institutes of Mental Health, where prominent researchers from the Addiction Research Center in Lexington, Kentucky, vented their frustration with psychoanalysis. They favored neurophysiological explanations that centered on the brain, disdaining "toxic theories," clinical confusion, and public hysteria.

[T]he brain is the instrument governing social as well as individual physiological integration. We need to know particularly about the limits and opportunities of an addicted person's behavior, his internal value system of appetites, rewards and punishments relating to narcotic drug abuse, the predisposing factors, the relationship of addiction to his past experiences and future prospects, the internal and external lures and deterrents as seen from his point of view. (Livingston 1958, 185)

Psychoanalysis was chief among the "toxic theories" to which they referred. Yet when addiction was cast as a neurological matter rendered through the behavioral language of reward and punishment, there remained a residual psychoanalytic emphasis on appetites, childhood experiences, and predisposition. Objectively measurable degrees of drug dependence gradually displaced the

earlier lexicon of psychopathology denoted by subjective states of consciousness—craving or pica, narcotic bondage or habituation. Yet the historian of science Ludwik Fleck reminds us:

[W]e can never sever our links with the past, complete with its errors. It survives in accepted concepts, in the presentation of problems, in the syllabus of formal education, in everyday life, as well as in language and institutions. Concepts are not spontaneously created but are determined by their “ancestors.” That which has occurred in the past is a greater cause of insecurity—rather, it only *becomes* a cause of insecurity—when our ties with it remain unconscious and unknown. (1979, 20)

The repression of psychoanalysis was the necessary backdrop against which addiction research constituted itself as “scientific.” To those who had dedicated their careers to the study of addiction, basic brain research appeared to offer a clear pathway out of a muddled arena.

Narcotic addiction researchers emphasized the scientific nature of their inquiries because they were operating from marginal social locations. Except for a toehold in applied departments of pharmacology, such as the Department of Materia Medica at the University of Michigan (which became the Department of Pharmacology discussed in the next chapter), they were unrepresented in any medical school curriculum or department of psychology. Addiction research was not an academic enterprise but a governmental one, borne out of the official response to the “opium problem.” The NRC Committee on Drug Addiction induced alkaloid chemists and pharmacologists to look closely at the chemical structure of morphine, separate what produced addiction from what relieved pain, and develop compounds that promised reduced abuse liability and could be tested in animals and humans. Psychoanalysis was the backdrop against which addiction research moved onto the experimental stage and into the social worlds of laboratory science. Although neurophysiology and pharmacology dominated twentieth-century addiction research, vestiges of psychoanalysis stuck in scientific as well as popular constructions of the concept of addiction or drug dependence. Even as addiction research became the experimental science described in the chapters to come, psychoanalytic and psychodynamic explanations arose to account for why only some individuals who are exposed to drugs become addicted to them. Such accounts shaded into older moralistic constructs of alcoholism and addiction as “diseases of the will” (Sedgwick 1993; Valverde 1998). Addiction researchers set out to refute previous explanations of addiction they considered “unscientific,” which included both moral and psychopathological accounts of individual variation.