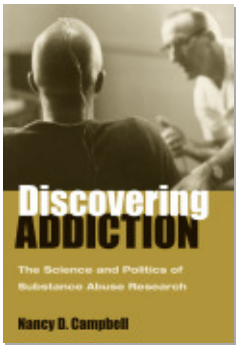
Discovering Addiction

Campbell, Nancy D.



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CHAPTER 7

"The Behavior Is Always Right": Behavioral Pharmacology Comes of Age

Popular drug experimentation in the late 1960s stimulated interest in the difference between use and abuse. Behavioral scientists took up this challenge, placing themselves on the frontier staked out by the once-pioneering ARC. Sites for drug abuse research diversified as the research establishment that had labored for more than thirty years in Lexington lost its virtual monopoly. Through CPDD, established researchers welcomed some newcomers but shut others out. This conflict is best seen not as a generation gap, paradigm shift, or zeitgeist but as a struggle over which hierarchy of credibility would prevail and whose voices would count as the voice of scientific authority. Differences between enunciative communities became evident in the multiple conceptual practices and laboratory logics that developed in the 1960s.[1] Behavioral pharmacologists formed a tiny but cohesive scientific community that moved from margin to center in the economy of drug abuse research.

The newcomers whose pathways I chart in this chapter either had personal drug experience or had witnessed drug use in friends or family. Macrosocial changes increased the range of available legal and illegal drugs while relaxing popular attitudes toward taking them. This fact alone differentiated newcomers from the previous generation, most of whom had never seen an addict before ending up at Lexington. Newcomers had different scientific vocabularies, logics, and techniques at their disposal, most notably those of behaviorism. Long concerned with "habit," behaviorism evolved in the United States in response to urbanization and mechanization (Bakan 1966). Although early behaviorists—such as John Watson, Clark L. Hull, and B. F. Skinner—attrib-

uted psychoanalysis with more cultural authority than it actually exercised, they used the scientific power of behaviorism as a "new psychological claimant" to displace psychoanalysis during a time of considerable change in social relations, sexual mores, and the organization of work in the United States (Bakan 1966, 22).

Asserting the potency of physiological responses while recasting "habit" and "habit strength" as a set of measurable physiological changes, behaviorists carved out a nonjudgmental position in a field they perceived to be dominated by moralism and bias. Ironically, Kolb's K-classification system, an early attempt to place addiction on a scientific footing and so destigmatize it, stood for "prescientific" moralism. Despite Kolb's goal to wrest from U.S. marshals their power over addicts' fates, the founding medical officer in charge of Lexington relied on the psychiatric terminology of the mid-1920s and so became an enduring emblem of what behaviorists were using science to overcome. The orderly framework of behaviorism was well suited to explaining the persistence of the "irrational" behaviors associated with drug abuse.

Behavioral pharmacologists encountered the field with tools, techniques, and, as Roland Griffiths observed, the confidence that "Skinnerian behaviorism seemed to explain everything about the way the world worked." Griffiths continued:

> We came into graduate school and were given an understanding, almost a philosophy of life, the key to the nature of the way behavior occurs. We were radical behaviorists at the time. We wanted to explain everything in terms of stimulus response interactions, partly Skinnerian, but also Pavlovian or classical conditioning and operant reinforcement. It gave us a paradigm and a methodology in which to work. . . . [We were] ready to explain everything about drug behavior interactions using this paradigm and methodology. It gave us the chutzpah to attack problems that might otherwise overwhelm other people that didn't have "the answer."

Radical behaviorists were typically graduate students in experimental psychology or "behavior engineering," who studied what they called "drug-seeking behavior" or "drug self-administration." Griffiths, who saw his personal sense of humility emerging only in retrospect, noted of his cohort: "We knew everything we needed to know about the field. It was with great zeal, but complete naïveté, that we stepped into this" (2005).

Although youthful confidence in the power of behaviorism has waned, many still speak of the explanatory power, conceptual force, strength, and rigor of the concept that drugs are reinforcers, which is central to this epistemic

community.[2] The idea that drugs work to reinforce certain behaviors and extinguish others is no longer restricted to the tightly coupled network of behavioral pharmacologists but has diffused throughout the field. The diffusion process has not gone unremarked by those who enjoyed scientific careers in the heyday of behavioral pharmacology. Behavioral pharmacology remade the laboratory logics of addiction research. Ironically, those who stormed the citadel of the ARC are now one of few sources of collective memory about it.

The behavioral laboratory logic of drug self-administration diverged from the logics of classical pharmacology. Griffiths explained:

> [Behavioral pharmacology was] characterized by intensive single-subject designs and parametric manipulations using the kinds of methods that come straight out of the experimental analysis of behavior. It was a new paradigm. The Lexington folks had been doing drug abuse research for years, but they were using classical clinical pharmacology methods, group designs, statistical analyses, and classical pharmacology approaches that are very powerful—and about which we were completely ignorant. Undoubtedly we thought that our methods were better and much more interesting because we were going to get to the core of the drug abuse problem, so there was [a] sense of glee and naïveté. (2005)

Almost universally, behavioral pharmacologists convey this zeitgeist in their origin stories about the hotbeds of behavioral pharmacology.

Derived from the work of B. F. Skinner, who originated the terms *respondent* and *operant* in 1937 to differentiate between behavior elicited in response to environmental conditions and behavior emitted to operate on the environment (Morris and Smith 2004), operant conditioning offered a new set of laboratory logics that aligned with but was different from Wikler's work on so-called classical (Pavlovian) conditioning. The vocabulary and methodology of operant conditioning, once called "behavioral engineering" or "social engineering," attracted newcomers who were less interested in drugs per se than in using drugs as tools for studying the persistence of behavior despite negative consequences. Substance abuse attracted behavioral pharmacologists because it was a socially pressing problem that had not yielded to previous approaches. Oblivious to the vast accumulation of human and animal data amassed by the ARC, adherents of behavioral pharmacology rarely crossed paths with the existing addiction research network until each social network began using the other's techniques to produce and render data (Brady 2004). As references to "addiction" and "drug dependence" were replaced by references to "drug and

alcohol abuse," behaviorism and pharmacology united to change how sustained drug use was viewed more generally.

MAKING THE WORLD OF DRUG ABUSE A PLACE WHERE BEHAVIORAL SCIENCE WORKS

As quoted at the beginning of chapter 2 of this book, sociologist Howard S. Becker observed: "Science works when you make the world into the kind of place where that kind of science will work. That's the purpose of creating laboratories."[3] The two primary origins of behavioral pharmacology were Pavlov's Institute for Experimental Medicine in Saint Petersburg, Russia, and the Psychobiology Laboratory at Harvard Medical School, where B. F. Skinner joined the faculty in 1948 and British pharmacologist Peter Dews joined shortly thereafter.[4] Previous laboratory stirrings occurred at the University of Minnesota, where Skinner was from 1936 to 1945, during which time he trained Kenneth MacCorquodale; at the University of Chicago when Joseph V. Brady was in graduate school; and in the "military industrial academic complex at Walter Reed, the University of Maryland, and at Johns Hopkins University," to which Brady emigrated (Brady 2004). Individuals central to the history of behavioral pharmacology share a delightfully self-reflexive streak, using the field's lexicon to describe their own behaviors and the powerful reinforcements afforded by their scientific method. Brady wrote: "[T]wo laboratories (Harvard and University of Maryland) potentiated the methodological and conceptual interplay by increasing the baseline rate at which drug behavior experiments were undertaken in laboratory settings. Clearly they raised the operant level" (2004). At Maryland, Brady trained Travis Thompson and Charles R. (Bob) Schuster, who cowrote the field's first textbook after Thompson departed for the University of Minnesota and Schuster was lured to the University of Michigan by Maurice Seevers. Once in Ann Arbor, Schuster pioneered primate self-administration using the platform built by Jim Weeks and Tomoji Yanagita described in chapter 2. The striking observation that nonhuman primates could be induced to self-administer the same drugs that human primates use to modulate their emotional and physiological states undergirded the institutionalization of behavioral pharmacology in industry and academia.

Drug self-administration marked both a turning point and a zenith in the long history of the behavioral enclave, which enjoyed ascendant status in the 1970s. Although they knew relatively little about addiction (having been trained

primarily as experimental psychologists and incidentally as pharmacologists), behavioral pharmacologists experienced their entry into the field as an intellectual and technical insurgency that displaced and invalidated all previous approaches to substance abuse. The problems of substance abuse—including techniques to measure drug abuse liability—became one of the main arenas in which behaviorism enjoyed enduring preeminence. Like all seeming scientific revolutions, this one came about gradually and was preceded by acolytes who were not accepted by more established scientists. Behavioral pharmacologists' feelings of marginalization were so intense that they formed a CPDD satellite, named the International Study Group for Investigating Drugs as Reinforcers, in the early 1970s.

Behavioral pharmacology emerged as both experimental and observational. Over time, behavioral researchers played an increasingly large role in pharmacology, as evidenced by their growing presence in the American Society for Pharmacology and Experimental Therapeutics. Alexandra Rutherford (2003) argues that behaviorism was institutionalized by the research undertaken by B. F. Skinner and Peter B. Dews at the Harvard Psychobiological Laboratory and by Brady's consultations. The framing of drug abuse as a social problem that behavioral pharmacology might help control led to expanded funding and institutional support for conducting abuse liability assessment on behavioral terms. In turn, evaluation of pharmacological effects helped make human (and animal) behavior accessible to the research techniques developed by behaviorists, turning the attention of pharmaceutical companies to side effects.

The founding fathers of the first generation of behavioral pharmacologists, Dews and Brady, credited the 1954 discovery of chlorpromazine (CPZ) as the triggering event that led to the differentiation of behavioral pharmacology from behavioral analysis and pharmacology.[5] They disputed the view that serendipity led to the discovery of CPZ/Thorazine, credited with creating the conditions for deinstitutionalization in the United States (Caldwell 1970; Healy 2002).[6] Assuring readers that he did not mean to discredit the pioneering work of Macht, Skinner, and others who worked the field prior to 1954, Dews foregrounded the significance of CPZ, which he called "one of the half dozen most important drugs in the history of mankind, . . . achieved by the systematic use of methods of behavioral pharmacology: not by serendipity or by molecular biology" (1985, 3).[7] This statement, made at a conference in 1984, indicated the extent to which behavioral pharmacologists already felt pushed aside by psychologists, pharmacologists, and, especially, molecular biologists. Only in the halls of NIMH,

Dews wrote, were behavioral pharmacologists appreciated and rewarded. To drive home his point, Dews used an extended parenting metaphor, in which behavioral pharmacology was the "offspring of pharmacology and psychology with the paternal genes of pharmacology predominating."

> Unfortunately when behavioral pharmacology was born, father was beginning a long infatuation with molecular biology which still continues, in spite of the fact that most of the major achievements in pharmacology since that time . . . are by no means achievements of molecular biology. Behavioral pharmacology has tended to be judged by its molecular relevance, which has been, and still is, modest to say the least. Psychology has been an even more unsympathetic parent. Living in halls of ornate theory, psychology has asked what behavioral pharmacology had to offer in the way of additional embellishment. Behavioral pharmacology is close to earthy reality, so the answer has been again, precious little. Indeed heavy-footed behavioral pharmacology has caused tremors that have jeopardized the whole filmy fabric of theories. (1985, 4)

Acknowledging that behavioral pharmacology was not yet a mature science by the mid-1980s, Dews urged his colleagues not to wait for neurobiologists or molecular biologists to provide the impetus for further discoveries (1985, 5). Although Dews's reproductive metaphor was especially colorful, behavioral pharmacology is often depicted as a flat-footed, descriptive science in contrast to other, more fanciful sciences.

Behaviorism and pharmacology were coconstitutive, a quality projected through the parenting and marriage metaphors of its participant-historians. The field's first textbook, *Behavioral Pharmacology* (1968) by Thompson and Schuster, offered a companionate marriage metaphor: "As is true in any marriage, the two partners, while sharing common conceptions and goals, must settle certain differences before a harmonious working relationship can be established" (ix). Steering clear of neural, mental, or emotional events in favor of observable changes in behavior, Thompson and Schuster cited a debt to B. F. Skinner for the assumptions, technologies, and techniques on which the new science rested (x). Laying bare the fundamentals of the two disciplines, Thompson and Schuster did not promise a "new synthesis" but, rather, mapped the "remaining chasm separating the two domains" (6). They also conveyed a felt sense of the "limited popularity" of the tactics and methods to which they subscribed, which they attributed to their empirical emphasis: "The overall advantage of the descriptive approach lies in the empirical soundness of the entire structure of scientific knowledge, from the microstructural foundation to the molar behavioral superstructure" (7). This degree of integration was

unprecedented, despite the best efforts of the collaborative team approach tried at Lexington.

When Smith Kline and French Laboratories bought the rights to CPZ from Rhône Poulenc in France, a new set of behavioral laboratory logics for finding specific uses for drugs through behavioral techniques was elaborated in the U.S. pharmaceutical industry, which built an infrastructure of psychopharmacology laboratories in hopes of finding another CPZ-like drug. This was reverse engineering, the systematic use of behavioral responses to predict drug effects. Rather than agreeing that drugs affect behavior, behavioral pharmacologists see the behavior itself as "a predeterminant of the quality of the drug effect" (Cook 1991, 2).

Despite their commitment to what seems like an applied science, Thompson and Schuster conceptualized behavioral pharmacology as a basic science of behavior that utilized drugs as tools. Differentiated from clinical research, their work used behavior to study the mechanisms of drug action. Criticizing those who studied drug effects on "unlearned reflexes," they focused on learning as a form of "conditioned response" (1968, 2). Although they cited Wikler (1953), they carved out a different practical terrain by using behavioral techniques to screen clinically desirable compounds and describe effects by drug class. Seeing such popular categories as "tranquilizers," "psychic energizers," or "antidepressants" as false or misleading labels, Thompson and Schuster set out to place classification on a more systematic footing and criticized the ARC's physiological and psychological instruments as unrefined (1968, 5). They extended the study of drug effects beyond the central nervous system, in contrast to pharmacologists, who tended to see the "brain [a]s the primary site of action of most behaviorally active drugs" (1968, 32). Behavioral pharmacologists recognized that drug effects went beyond the central nervous system because behavior had to be understood in intact organisms (Thompson and Schuster 1968, 33; Iversen and Iversen 1981, 52). Contra depictions of behaviorism as simplistic relative to cognitive science, they saw it as a nonreductive science.

Operant conditioning along Skinnerian lines became the cultural currency among psychologists who adopted the vocabulary and techniques of behaviorists. Susan and Leslie Iversen explain:

> The demands of behavioral pharmacology are more consistent with Skinner's approach to behavior [than cognitive theories], which is basically a description of the variables influencing behavior in a particular situation, with no recourse to explanation. This is not to say there are no underlying reasons for behavior, but simply that if behavior can be defined, described, and consistently manipulated, these reasons are irrelevant. (Iversen and Iversen 1981, 12–13)

Behavioral pharmacologists saw the interpretive vocabulary of psychology—such terms as *motivation, emotion, anxiety,* and *neurosis* or attempts to name internal drive states—as misleading (Iversen and Iversen 1981, 12–13, 35–36). "From fish to men," as the Iversens put it, Skinnerians found that schedules of reinforcement were predictive: "There is every reason to think that man's behavior is controlled by the same basic contingencies as is behavior in the pigeon" (1981, 32). This fundamentalist refusal of anthropomorphism sparked antipathy toward behaviorists, who viewed themselves as moving psychology away from softheaded early psychopharmacology to hard-core science. Moving from the soft science of subjective effects into a hard science of objective observables was embraced so fully by converts to the field that it had the quality of a revolutionary call to arms (cf. Fleck 1979, 43).

Second-generation acolytes of behaviorism, such as Thompson and Schuster, emerged as staunch defenders of their science. In their 1968 textbook, Thompson and Schuster concluded: "Critics of this descriptive approach to behavioral effects of drugs find it superficial, 'know nothing,' and grossly oversimplified." They conceded that behavioral pharmacology was "superficial in that it deals exclusively with observables; 'know nothing' to the extent that it does not claim to know anything that can't be replicated by independent observers; and . . . oversimplified to the extent that the world is simple." Calling theirs a "modest approach," they asserted that they were "content to add descriptive links to the body of knowledge relating drugs to behavior" (229). This potent combination of modesty and technique catapulted behavioral pharmacology to the center stage of the drug abuse research enterprise, while molecular, neurobiological, and genetic approaches waited patiently in the wings. The proud but distracted parent disciplines watched the performance. Earlier in their textbook, Thompson and Schuster presented a very different picture of behavioral pharmacology as a "young and complex" science requiring a great deal of imagination and opposed to "oversimplified and premature judgments about the behavioral actions of drugs" (157). Citing numerous paradoxes of action and effect, they made ingenious attempts to control for the complexities and nuances imparted by contingencies in the environment.

Drug abuse research offered an ideal arena in which to work out the basic mechanisms of behavior in complex environments. Behavioral pharmacologists considered the designation "drug-behavior interaction" a misnomer, because they recognized that intact organisms interact not with drugs but with environments. They criticized research that failed to account for environmental factors, arguing that "drug-behavior interaction" should be thought of as "drug-environment interaction" (Thompson and Schuster 1968, 158). Not

unlike Beecher, they believed that physiological states and environmental contingencies modified drug effects.[8] However, once complex transactions between drugs, behavior, and environment were recognized, behavioral pharmacologists acted to "strip away uncontrolled conditions" and reveal the lawfulness of behavior and its roots in "causally determined events" (Glick and Goldfarb 1976, 1–2). Steve Goldberg wrote: "Drug addiction is a complex type of behavioral disorder that depends in part on specific biochemical and physiological mechanisms of drug action, as well as the present and past behavior of the individual addict and the environmental conditions under which the behavior occurs." Studies conducted prior to use of operant-conditioning techniques were dismissed by Goldberg as merely biochemical or physiological or as misguided attempts to identify "metabolic aberrations" of addicts or potential addicts, while he defined operant behavior "simply as behavior that is controlled by its consequences" (1976, 283). For a consequence to act as a reinforcer, it must follow the response immediately so as to increase its frequency. For behaviorists, reinforcement depended not only on intrinsic, pharmacological properties of drugs but on individual behavior and environment.

Behavioral pharmacologists walk a thin line between their recognitions of complexity and multiplicity, on the one hand, and their straightforward, hardcore scientific ambitions, on the other. This ambiguity showed up in their assessments of prior work. For instance, Goldberg dismissed "wrongheaded" claims that "psychological disorders" caused addiction, crediting several early works (Tatum, Collins, and Seevers 1929; DuMez and Kolb 1931; Kolb and Himmelsbach 1938; and Seevers 1936b) with establishing the biological basis of physiological dependence. Describing how behavior leading up to morphine injection could be interpreted as "escape behavior" designed to terminate or avert abstinence symptoms, Goldberg traced a genealogy by which addiction researchers realized that physical dependence was "neither a necessary nor sufficient condition for addiction." "Drug dependence," he stated, evolved away from "addiction," "habituation," "abuse," "pleasure," "euphoria," and "craving," due to difficulties involved in quantifying and operationalizing such states (1976, 284). Recalling his forebears' dissatisfaction with terminology, it is difficult not to see behavioral pharmacology as amnesiac. Offering neutral terms, such as *drug self-administration, drug seeking,* or *drug taking,* behavioral scientists sought to operationalize similar concepts to those Abraham Wikler had decades earlier sought to place on the firm ground of public science.

Propelled by the rise of behavioral pharmacology, laboratory sites for evaluating drug effects proliferated and decentralized. The events recounted in the

previous chapter of this book created a vacuum into which behavioral pharmacologists were prepared to step. As Chris-Ellyn Johanson put it (personal communication with the author, March 22, 2005), the ARC's hold over human research loosened just as behaviorists invented and adapted the apparatus and recording devices that made their science visible. Pharmacologists had long performed animal research and toxicity testing; they were more experienced in animal care than were behavior analysts (Rowan 1984). The convergence between behaviorism and pharmacology occurred simultaneously at multiple sites.[9] Like other interdisciplinary fields, behavioral pharmacology "maintain[ed] itself through regular and purposeful interaction with other fields and other domains" (Frickel 2004, 5). Yet it also consolidated its own lexicon and laboratory logics, which came to predominate in the field of addiction research by the late 1970s.

Behavioral pharmacology emerged with unusually tight connections between basic and applied research modes. Those who rely on its techniques today to assess the abuse potential of chemical compounds have been deeply involved in developing policy-relevant approaches to drug dependence or addiction. The institutional core of behavioral pharmacology did not initially lie in the federal science system—an "upstart" science issuing outside the ARC, it was perceived to be part of a competing approach. However, in the 1970s, the field of "substance abuse research," as it increasingly was called, became a reliable funding stream for behavioral pharmacologists. Despite Wikler's own previous preoccupation with classical and operant conditioning and his longstanding interest in Pavlovian approaches, behavioral pharmacology was heralded as a new experimental paradigm. Something about it smacked of an assault on the citadel. Behavioral pharmacologists saw the knowledge they produced as relevant not only for understanding drug abuse but also for treating or even preventing it. Going beyond basic research, behavioral pharmacologists threw themselves into the political fray, taking responsibility for policy, treatment, and prevention in ways their forebears had not. An unprecedented chance to affect national drug policy and federal research priorities arose during the Nixon administration. Drug abuse researchers gained influence over drug policy in the early 1970s, when the political opportunity structure enabled them to control the setting of research priorities and to translate basic knowledge into wider use.

Substance abuse offered an opportunity for behavioral pharmacologists to demonstrate innovative animal self-administration and drug-discrimination techniques (Lasagna 1969, 23). Such major figures as Henry K. Beecher under-

stood that bridges between behavioral science and psychopharmacology were being built in the 1950s—despite subscribing to the sense that psychopharmacology was an inescapably "subjective" science (1970, 60). Previous work had focused solely on chemical compounds that had visible, measurable effects on the bodies of monkeys and men.[10] Now behavioral pharmacologists saw drugs as reinforcers of behavior, learning patterns, and conditioned responses. This insight allowed them to invent precision techniques, protocols, and laboratory logics for producing new knowledge about drug self-administration in humans and animals. To discern the patterned sociality of this particular community of scientific practice, the next section of this chapter examines the career trajectories of behavioral pharmacologists who worked out the new laboratory logics.

COMING OF AGE: BEHAVIORISM AS THE KEY TO SOLVING SOCIAL PROBLEMS

Coming of age in Camden, New Jersey, during World War II, Charles R. (Bob) Schuster played jazz underage in local bars and nightclubs, hanging out at the WCAU studios in Philadelphia, where Red Rodney was the featured soloist for a big band that played live every afternoon.[11] Schuster's first close encounter with heroin addiction occurred in 1947, when Rodney, an African American musician who played with Charlie Parker and was a few years older than Schuster, started to use heroin. In a 2004 interview, Schuster said: "I watched Red and others of his friends shoot up heroin. . . . At that period of time I smoked marijuana, jazz musicians did that. I thought, gee, if I'm a jazz musician, that's part of the game."[12] Schuster recounted getting fired for getting high while playing, after others noticed the distorting effect of marijuana on his sense of timing: "Once I got up and played about six notes and sat back down because I thought I had played too long; my friend, who was playing saxophone, realized why and started laughing. We both got fired for that. But I was too neurotic at that point in my life to think about putting a needle in my arm, . . . I was too anxious. Fortunately, I never experimented with it, because, of course, opiates are very good anxiolytics. I was definitely at risk."

Confrontations between drug users and the law shaped his political commitments. Schuster recalled that the "jazz musician's lawyer," Charles Royceman, would invariably be called when Billie Holiday checked into Philadelphia hotel rooms, and the police found "stuff" up in the ceiling or behind the lamp:

"They wouldn't arrest her, they just didn't want her in town. . . . That was the other thing that got me out of the nightclubs. I was always underage, so I always had false ID. You had to have special permits for working in night clubs . . . I saw that police didn't have much regard for jazz musicians, and I was frightened. I just couldn't see myself at age forty being subject to being pushed around by them." His familiarity on the jazz scene put Schuster in the position to witness racial segregation, police harassment, and red-baiting at a young age: "Philadelphia was segregated at that time. I went to a segregated school as a child in Camden, New Jersey. Black kids went to school fifteen minutes before or after we did, so we wouldn't be on the street at the same time." After moving to Albuquerque, New Mexico, where he frequented a South Fourth Street bar called the Chicken Shack during his junior year of college, Schuster witnessed the arrest of an African American friend for selling marijuana: "The police just couldn't understand why I would go to this place on South Fourth Street if it wasn't for drugs. And it wasn't for drugs. It was because there was good music there" (2004).

Most people imagine a world of difference between the jazz clubs of Philadelphia and the animal models of behavioral pharmacologists, but Schuster drew connections between research, personal observations, and his political commitments: "Back when I was a jazz musician, I had seen people who would sit around and play with needles, even injecting themselves. They were so-called 'needle freaks.' [Y]ou could make a monkey a 'needle freak,' too, by associating stimuli with the drug. After a while those stimuli became conditioned reinforcers. So I looked at 'needle freaks' as an instance of individuals who were engaging in a behavior that was frequently associated with drug use, and therefore the whole act of taking the drug, cooking it up and so forth, had some conditioned reinforcing properties. I was also able to demonstrate that when they went into withdrawal they would work to a much greater extent for opiates. I was surprised that the drugs that animals would take were by and large the same drugs that some humans got into trouble with. It was amazing, the concordance" (2004). Schuster would have been less likely to make such connections working solely in the laboratory. The insights he garnered from social relationships with active drug users lay at the basis of the kind of scientific knowledge he pursued.

Social proximity between the researcher and the research subject is important because researchers are constituted in part by the social process of research, the form of work that is situated within the social context of the laboratory. Schuster said,

> First of all, I have always felt a great deal of compassion for individuals who have become addicted to drugs because I witnessed the transformation from people who were just playing around with drugs to becoming truly addicted and unable to stop despite the fact that they were quite aware of the fact that there were huge deleterious consequences to their continued use. Secondly, I guess I also came out of it with the feeling that to deal with it as a problem of morality didn't make any sense to me either. I didn't think of these people as being immoral, perhaps because I knew them prior to the time they became involved with drugs. By and large most of them were pretty decent individuals who for a variety of reasons became involved with drugs and became addicted.

For this reason, Schuster found it made sense to think of drug abuse as a behavioral disorder in which drugs functioned to reinforce social learning. Setting out to induce animals to self-administer drugs of abuse, Schuster anticipated he would have to "trick" animals into taking drugs. He found that he did not have to make animals physically dependent, however, in order for opiates to serve as positive reinforcers: "All I had to do was make them available and I found very, very few animals who would not learn to emit some sort of operant response when you used drugs as a consequence for that" (2004).

Marching rapidly through the known pharmacopeia, behavioral pharmacologists cataloged drug effects in animals and drew up tables of concordance between human and animal responses. Schuster explained: "Drugs that are aversive in humans, such as phenothiazines—animals would actually learn to avoid injection of them so they served as negative reinforcers. The concordance was striking. I don't know of any other animal model in any area of psychiatry that has both the face validity and construct validity that animal self-administration studies do." Preoccupied with establishing concordance and responding to critics who saw behaviorists as reductionists, behavioral pharmacologists eventually got around to exploring the existing knowledge of the field into which they had blundered so enthusiastically. Now in graduate school at the University of Maryland, Thompson and Schuster made a visit to Nathan Eddy, who they recognized as the leader of the NRC Committee on Drug Addiction and Narcotics. Schuster said: "He was at NIH in this funny building off campus that had to have structural supports because there were so many books in it. He was essentially blind. He invited us to a CPDD meeting in Ann Arbor, Michigan" (2004).[13]

After reporting on his animal self-administration work with Thompson at the 1963 CDAN meeting, Schuster was lured by Seevers to the University of Michigan. Seevers imported behavioral approaches and the devices on which they depended. Because of his early interest in desire for stimulants (see chap.

2 of the present book), Seevers was predisposed to accept the behavioral tenet that drugs acted as positive reinforcers, and he believed that desire signaled something beyond physiological need.[14] Animal self-administration differed from previous drug screening done at the monkey colony, where emphasis lay on describing the toxic consequences of drugs at doses that animals would voluntarily take. Schuster said, "Whereas I was interested in behavior, [they were] interested in describing what happened to animals when allowed free access to drugs. [They were] not necessarily interested so much in their behavior as [their physiology]."[15] The work of the Applied Psychology Laboratory, as Schuster's laboratory at the University of Michigan was called, emphasized behavior and reinforcement. Schuster explained that in the social world of behavioral pharmacology, "the behavior is always right," and "the behavior is the reality" (2004). The work accomplished in the brief time that Schuster worked at Michigan with James Woods—who was hired as a lab assistant but worked his way up to head the lab—is often pointed to as one of the chief origins of the drug self-administration breakthrough.

Behavioral pharmacologists embraced single-subject designs and were skeptical of statistical analysis in ways that led them to focus closely on individual subjects. From his faculty position at the University of Minnesota, Travis Thompson trained individuals who became prominent in the field—among them George Bigelow, Thomas Crowley, Roland Griffiths, and Roy Pickens—before becoming interested in behaviors of self-injury and self-harm. Bigelow and Griffiths migrated to Johns Hopkins University, spending their careers in behavioral biology and neuroscience. Their long-standing scientific collaboration began in the unlikely halls of Faribault State Hospital in Faribault, Minnesota, where they tried to use behavior modification techniques with severely mentally disabled individuals. Griffiths described this first collaboration as follows:

> Here were these grossly deteriorated institutions with profoundly and severely retarded people just being housed under what would have to be described as inhumane conditions. There was no sense that you could do anything for them. They were cleaned up and then they sat in these bare dayrooms and rocked back and forth or were put in restraining rooms when they misbehaved. This whole technology of behavioral control (and this is the power of the experimental analysis of behavior) says, wait a second, these people don't have to sit there, let's teach them something. We have the technology for teaching them something. It's a question of contingencies. Take a look at what contingencies exist in this situation, and no wonder they run around hitting each other. The only time that anyone gets any attention from the staff is if they soil themselves

> or if they get aggressive. Then the staff come over and start interacting with them. From a behavioral paradigm, it was just backwards. (2005)

A palpable enthusiasm for radical behaviorism still shines through in the early work of Bigelow and Griffiths but has been tempered with the awareness of naïveté and with the discovery of the limits of behavioral approaches.

Another early collaboration between Bigelow and Griffiths, a study on a single chronic alcoholic at the Baltimore City Hospital, illustrates an important aspect of research design in behavioral approaches (Bigelow and Griffiths 1973). This study resembled Wikler's experimental readdiction paper and focused on establishing a baseline of how much a chronic alcoholic would drink when allowed to do so. Then Bigelow, Griffiths, and Pickens stabilized the individual and tailored a set of contingencies to which they felt he would respond. This approach presaged today's "individual treatment plans" and derived from a sense of the human costs of a widespread phenomenon. Yet behavioral pharmacologists confronted a broader pharmacological research enterprise that was in the process of becoming hostile to within-subject designs.

Single-subject design contrasts to large-scale clinical trials. "[Behavioral science] is not interested in gathering data that are then described by using inductive or deductive statistical measurements. The interest is in the individual case rather than in the mean of a sample. . . . Much of modern behaviorism also concentrates on detailed work on one individual in preference to the descriptions of groups, on the grounds that an effect, once properly demonstrated by the unique individual, will be found to be true of all" (Candland 1993, 356).[16] The belief in the power of single-subject designs placed behavioral techniques and knowledge at odds with today's drift toward large sample sizes, statistical analysis, and population-wide assessments. Those who entered the field when single-subject designs were still possible lament a loss of precision: "People don't get the power of single-subject analysis anymore. It is the anomalous finding that is most interesting" (Griffiths 2005). Coming of age brought behavioral pharmacology into a more realistic integration with other currents of the addiction research enterprise.

THE USES OF PROXIMITY: PUTTING BEHAVIOR INTO ADDICTION RESEARCH

Establishing cross-species concordance between animal models and findings in humans was an impressive reinforcer for behavioral scientists. It required

either proximity between animal and human laboratories that produced data under similar conditions or access to cumulative data sets produced by other researchers. Eventually, behavioral pharmacologists turned to the only existing human data set—that produced by the ARC. They did not do so until they were ready to trust the validity of the ARC's results. This happened only after they matured beyond the enthusiastic, almost religious fervor of the 1960s. That readiness did not emerge until after the ARC's relocation to Johns Hopkins University, a stronghold of behavioral pharmacology. Two members of the Hopkins Behavioral Pharmacology Research Unit, George Bigelow and Roland Griffiths, visited Lexington to talk over the possible move.

> The real issue for the Lexington people was not that we were looking at them but that they wanted information from us about the prospects of their being able to prosper in Baltimore. They were interested in the fact that we had experience doing human drug self-administration research and working with an IRB [institutional review board] for getting approval and could describe the environmental context of being able to recruit substance abuser volunteers from the community. That's something they were unfamiliar with and very afraid of. For forty years they had been dependent on a literally captive population for doing their work. They had a lot of doubts and concerns about whether their methodologies could be transferred over to working with a truly volunteer population who could walk out and choose not to have anything to do with them. The main purpose of [our] mission on that visit was for them to get somewhat reassured by us and from us that you can do human clinical pharmacology work in Baltimore, in this institution and in this urban setting, and both get the regulatory approvals and the volunteer participation and cooperation. (Bigelow 2005)

As members of the behavioral pharmacology enclave, Bigelow and Griffiths were initially oblivious to the work of the ARC. Griffiths remembered giving a paper in the 1960s after which Bill Martin had chided him for not acknowledging work done at Lexington. There were deep conceptual and technical divisions between the classical pharmacology of the ARC—which relied on group design, subjective ratings, and classical (Pavlovian) conditioning—and the operant (Skinnerian) conditioning of the new guard. Griffiths admitted that such divisions mattered: "It didn't occur to me that there was any value in asking people how they felt. I continue to have huge skepticism about what people say" (2005). Divisions of lexicon and training were consequential for research design, development and deployment of instruments, and interpretation of results.

As Griffiths observed, "getting animal and human laboratories to talk to each other" requires material conditions and institutional circumstances that

enable such cross talk (2005). Such circumstances align in few places, and the ARC had been one when at Lexington. Intensified scrutiny and the loss of Lexington increased "pressure for methodological adaptations that would permit similar scientific evaluations to be done in other settings and with other [nonprisoner] populations" (Bigelow 1991, 1617–18). A new set of alliances was enabled when the Behavioral Pharmacology Research Unit at Johns Hopkins University gained access to the knowledge base produced by the ARC. Describing his initial exposure to the history of the field while at the University of Minnesota, Bigelow noted: "We had some familiarity with the methods of the Addiction Research Center, but I think it was a pretty superficial understanding. Frankly, their methods were viewed somewhat skeptically by people from a rigorous operant background. They were giving drugs to people and asking them what they thought, not using any operant principles. Not until later, certainly after I moved to Hopkins, did I really develop an appreciation for the quality of that work and the orderliness of those types of measures and that methodology for assessing drug effects" (2005). The relationship between the ARC and the Behavioral Pharmacology Research Unit provides a perfect example of what Ludwik Fleck maintained occurred when "a large group exists long enough [that its] thought style becomes fixed and formal in structure": "Practical performance then dominates over creative mood, which is reduced to a certain fixed level that is disciplined, uniform, and discreet" (1979, 103). As a dominant thought collective, the ARC had reached a position where its ways of going about things were formalized to the point of being stylized. As a stable, specialized group, the ARC was an exclusive enclave that can in some sense be said to have "discovered addiction"—and came to occupy the same space as a thought collective of "true believers."

Behavioral pharmacologists constituted "addiction" anew, rendering it subject to the method of operant conditioning. Bigelow reported that not until operant conditioners ran up against limitations did they "largely but not completely" abandon "the most rigorous operant methods" and adopt "self-report measures as an index of drug effects in humans to a substantial degree" (2005). He explained that prior to the advent of computers in the laboratory, it was cumbersome to collect, store, manage, and analyze qualitative data generated from self-reports or questionnaires.

> In the early seventies when Roland [Griffiths] and I were working together at Baltimore City Hospitals, doing our first human operant work, we started off trying to be rigorous operant conditioners. We had people pull levers and

> ignored, to a large extent, opportunities to ask people to provide self-report information. We didn't have confidence in self-report as a useful measure. When we did try to do things like that, we quickly found ourselves overwhelmed with more pieces of paper than we could conveniently look at and analyze. With the advent of personal computers, all the questionnaires could be put on the computer. Instead of having to sort through and score things, you'd get a nice, orderly spreadsheet of summary data at the end. It suddenly became a domain of data that could be really conveniently collected and managed and analyzed and appreciated. (2005)

Despite instruments designed to elicit and compare the subjective effects of drugs, not until computational resources became commonplace in laboratory settings could the subjective domain really be analyzed and understood.

Surveying the standardization of methodology and instruments for measuring abuse potential, Bigelow credited the ARC with nearly six decades of work forming the "primary foundation for virtually all of our currently available methods for clinical drug abuse liability assessment" (1991, 1615). Crediting the ARC with foundational, seminal, or pioneering contributions to the field is commonplace in the publications of behavioral pharmacologists to a far greater degree than in the other arenas of addiction research. This may be in part a "cohort effect," where today's senior behavioral pharmacologists once found themselves relying on the ARC data as a way to establish concordance and back up their assertions concerning the power of their methodologies. Yet the Addiction Research Center Inventory (ARCI)—consisting of five hundred true-false statements concerning the subject's emotions and perceptions and administered in conjunction with observation and physiological tests (e.g., of pupil diameter) while the subject is on drugs—is far more subjective than behavioral pharmacologists generally get. The ARCI was many years in the making at Lexington, where psychometricians Harris Hill, Richard Belleville, and Charles A. Haertzen developed it in response to Harris Isbell's request for a sophisticated way to measure the subjective effects of drugs and distinguish their profiles of effects from one another. While Isbell intended the project to meet short-term needs, the ARCI became a complex, multiscalar inventory still used to distinguish the subjective profile of each major drug category.

Efforts to standardize the study of subjective effects relied on the use of drug-experienced human subjects—the very postaddicts described in previous chapters—as bioassays. Coupled with intensified scrutiny of human subjects experimentation and the absorption of the ARC into NIDA, the loss of Lexington increased "pressure for methodological adaptations that would permit

similar scientific evaluations to be done in other settings and with other [nonprisoner] populations" (Bigelow 1991, 1617–18). Among the collaborations undertaken between the relocated ARC and the Hopkins unit were animal and clinical studies of buprenorphine, a drug approved in 2003 for treatment of opiate addiction. When they moved from Lexington to Baltimore, ARC researchers brought both the drug itself and the idea to use it in this manner.

By the late 1960s, behavioral pharmacologists had invented a new lexicon, in which the process of addiction was one of conditioning, learning, and motivation. As experimentalists, they saw pharmacology as a means to an end for studying the principles by which drugs act as peculiarly powerful reinforcers of behavior. They were critics of physiological functionalism and psychoanalytic constructs alike. From their earliest days, however, some believed that neurobiology would one day be reintegrated with the study of behavior (MacCorquodale and Meehle 1948). Behavioral pharmacologists formed an enunciative community that occupied fertile ground between, on the one hand, such antecedents as Wikler's long-standing interest in classical conditioning and, on the other, harbingers of change in the material, social, and technological conditions of work in the laboratory. When behavioral pharmacologists entered the social worlds of addiction research, a gradual but wholesale transition was under way. This transition was not unlike what the field of substance abuse research is undergoing with the current shift to neuroscience and genetics and the eclipse of behavioral pharmacology. Rather than experience this transition as a negative event, many of my interviewees in behavioral pharmacology used what Renee C. Fox has called "ritualized optimism" to cope with change (1959/1998). They pointed out how integrated and holistic their enterprise was relative to that of reductive, molecular or subcellular approaches. According to Schuster, reductionistic approaches "can only be so successful," because unexplained phenomena emerge at each level of integration. Others argue behavioral pharmacology served as a gateway for neurobiology to enter the field.[17] General acceptance of behavioral pharmacology paved the way for the appropriation of its specific vocabulary and precise concepts—since, as Schuster argued, "everybody thinks they're an expert in behavior" and uses the language of drugs as reinforcers. Schuster explained:

> A lot of the techniques that are used by neurobiologists have been taken from behavioral pharmacologists. Now that we are getting into subcellular events, it's extremely exciting, it's absolutely marvelous. Ultimately, however, I believe that not just in this science [but] in all sciences, . . . a reductionistic approach can only be so successful. As one moves up levels of integration, at these higher

> levels of integration, new phenomena emerge that are not reducible to those that are below. Ultimately, no matter how well we understand the enzymatic and protein pathways of the cell, we've got to explain the behavior of the intact, integrated organism. The behavior is always right. It is ultimately the job of the biologist to be able to predict that behavior because the behavior is the reality. The behavioral effects of drugs are the reality. Understanding them at different levels is fine, and I'm excited about it, but we cannot forget that the end product is that we're trying to change people's behavior. (2004)

Statements such as "the behavior is always right" and "the behavior is the reality" mark both the specificity of behavioral approaches and their singularity. These statements impart the flavor of a mantra that still conveys some of the earlier, almost religious fervor with which behaviorists approached "addiction."

Behavioral pharmacologists themselves go to great lengths to differentiate the range of reinforcers with which they work and have become increasingly refined at doing so. However, they have also been criticized for being "biological determinists" or believing in "universal reinforcers." Both charges obscure the degree to which behavioral pharmacologists try to analyze social complexity, the working of memory in environmental cues, and the divergence between what happens in animals and what happens in humans. A science that shows that subjects being conditioned to experience drugs classed as "stimulants" as depressing, or vice versa, can hardly be accused of making universalist or biologically determinist assumptions. Behaviorists in fact reject universalism and biological determinism, favoring direct observation and objective measurement, in contrast to views that drugs provide inherent biological rewards.

Arising to counter what they viewed as unjust stigmatization of addiction and mental illness, Skinnerian behaviorists applied the principles of operant conditioning as an alternative to moral judgment. They were not particularly modest witnesses—indeed, many were cocksure until the limits of their own philosophy and techniques became clearer over time. They remain an interesting species of practitioners of the experimental life, having been chastened by realities but still successful at translating their science into practical knowledge through the evidence-based practice called "contingency management." Because of them, it is now generally accepted that drugs act as reinforcers and that social or adaptive learning sustains the social structures and neural substrates of addiction. They offered a useful explanatory model of how addiction works that accounted well for a wide variety of phenomena, including the idea that drug addiction can be an inflexible response to negative consequences.

Behavioral pharmacology enjoyed what Helen Longino called the "episte-

mological success of content" (Longino 2002, 114–21). By experimentally isolating behavior, behavioral pharmacologists recognized social complexity, but their Skinnerian upbringing led them to set up experimental situations according to the laboratory logics of behavioral reasoning. Their laboratory logics depend on observation, a socially organized form of perception: "Observation is not simple sense perception (whatever that might be) but an organized sensory encounter that registers what is perceived in relation to categories, concepts, and classes that are socially produced. Both ordering and organization are (dependent on) social processes" (Longino 2002, 100). Behavioral researchers are more cognizant than many scientists that they slip between what is actually happening and the conceptual categories through which they make sense of what is actually happening. Their discourse is replete with casual and systematic acknowledgment of that gap. However, as sociologists of science show, science depends on producing "intersubjective invariance of observation" by narrowing what counts as a plausible way to produce credible findings and claims (Longino 2002, 103).

Behavioral pharmacologists have built tightly interwoven social networks that enable ongoing social interaction with similarly situated others in order to share tacit and formal knowledge in ways that reduce intersubjective variance. Longino wrote:

> Cognitive processes have a social dimension. Of course, while the sociality of cognitive processes is part of what grants them their warranting status, this social dimension can be a source of difficulty. For example, the invisibility of many background assumptions as assumptions . . . means that a closed community will not be able to exhibit those assumptions for critical scrutiny. (Longino 2002, 107)

The founders of the behavioral pharmacological research enclave set forth its underlying principles and tenets as if they were, in fact, revolutionary.

Typically, the underlying assumptions of a particular mode of knowledge production become most available for scrutiny during shifts in dominant modality. The rise of neuroscience in the mid-1990s pressured behavioral pharmacologists to incorporate new findings concerning the neural substrates of behavior (Blank 1999, 81). Behavioral pharmacologists came to occupy a subordinate position relative to the neuroscientist newcomers, who the behaviorally oriented had once tried to attract to the field in hopes of creating a more socially situated neurobiology. We might analyze this story as one of social succession, paradigm shifts, scientific revolutions, or zeitgeist. The narrative illus-

trates the social process by which scientists "transform the subjective into the objective, not by canonizing one subjectivity over others, but by assuring that what is ratified as knowledge has survived criticism from multiple points of view" (Longino 2002, 129). By the mid-1970s, there was considerable heterogeneity in drug abuse research, which included the fields of neuroscience and genetics—the second of which had rarely, if ever, been mentioned at Lexington—and even a few scientists beginning to study drug taking as the result of metabolic defects. Far from waiting for the visualization of the opiate receptors in the 1970s for promising leads (Acker 2002, 63), behavioral research had earlier pointed toward the neurochemical and molecular basis of repetitive behaviors, as well as to the social environments in which they occurred and were interpreted to be "addictions." Multiple thought styles were becoming a way of life in the social worlds of the addiction research enterprise.