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Discovering Addiction

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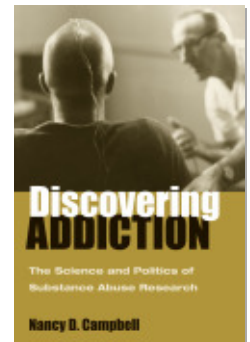
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Introduction

Popular beliefs about drugs and drug addiction are increasingly pitched in the language of science, which has become part of the stew of assumptions and stories we serve each other in everyday life. Drug users speak of being overtaken by cravings or uncontrollable urges, a way of speaking drawn from notions of appetite, habit, and craving that come not from unmediated drug experiences but from psychoanalysis. When relapse is attributed to “cues” or “triggers,” patients and providers draw on a vocabulary of operant conditioning introduced by experimental psychologists and behavioral analysts. When drug users speak of feeling “chemically imbalanced,” they rely on a late twentieth-century vocabulary of endorphins, neurotransmitters, opiate receptors, and brain chemistry drawn from neuroscience. Science offers specialized vocabularies that fuse with popular vernaculars—“the fix,” “the rush,” “getting high,” “hitting bottom,” or “kicking the habit”—through which people describe their innermost sensations. Expressive argots recursively feed into science: scientific theories affect how people interpret drug experiences, and users’ reports in turn become research material. The frames used in science are consequential, for the production of scientific knowledge is a social privilege inextricably bound to questions of social justice.

Scientific knowledge about drug addiction is believed to reveal the inner workings of brains, minds, and bodies.¹ Addiction knows none of the social distinctions imposed by policy regimes that have repeatedly constructed drug use as a crime or as a disease in the United States.² Calling something a “disease” appeals to scientific conventions and clinical vocabularies but generates a

cascade of questions: Is it curable or incurable? Does it mark its victims? Is it a metabolic disease, an infectious disease, a brain disease, a social contagion, a biochemical imbalance, a disease of the will, a disease of desire, a disease of stress? Is it chronic, lifelong, or episodic? Is it more like diabetes or allergy? Is it genetic? Addiction researchers have taken up all these possibilities at one time or another as they struggle to explain individual variation in vulnerability to addiction and relapse.

Discovering Addiction reveals that scientific efforts to explicate addiction have each answered the preceding questions differently and that none have stabilized any one set of answers for long. Scientific categories are also social distinctions, for habits of mind and body promote psychoactive drugs to positions of social and cultural significance. Access to drugs deemed problematic is restricted through formal laws, informal policies, and social norms; access to drugs deemed useful or medicinal is not only encouraged but prescribed. Some drugs are taken to cause problems; others to solve them. “Bad” drugs are shunned; “good” ones are promoted. Whether legal or illegal, recreation or medication, lubricant or barrier, drugs structure identities and relationships, social routines of work and play, and local and global economies.

BRINGING TO LIFE THE HISTORY OF ADDICTION RESEARCH

Scientific inquiry has substantially influenced the shifting lexicons and logics through which drug use is understood. This book is about the scientific communities that contribute to the confluence of scientific research, clinical practice, and social policy through which addiction has been addressed. Substance abuse researchers have grappled with what “addiction” means in the human and animal laboratories through which they have brought their science to life. *Discovering Addiction* reenacts the technical and ethical dilemmas that pervade the political, ethical, economic, and policy contexts in which this branch of science originated and is situated today.

Addiction researchers comprised one of the first multidisciplinary collaborative clinical research teams assembled expressly to mount a collective assault on a scientific frontier that was simultaneously considered a social problem (Walsh 1973a, 1229). The historical origins of addiction research lie in the Addiction Research Center (ARC), a laboratory that was once part of a federal prison-hospital in Lexington, Kentucky, and in the “monkey colony” at the University of Michigan at Ann Arbor. Characterizing addiction research as an “extraordinarily closed world,” a *Science* magazine reporter described domi-

nant personalities who exerted a “virtual monopoly” over knowledge production at the time (Walsh 1973a).³ I analyze the making and unmaking of that closed world, mapping how the field morphed into a bewildering array of intersecting social worlds, each with its own vocabularies, hierarchies of credibility, and laboratory logics.⁴ In writing the history of substance abuse research, I have struggled to come to terms with the scruples of those who conducted experiments on animals and human beings in these two sites. How did they construct, maintain, preserve, and defend their self-identity as ethical scientists, as physicians who were taught the precept, “first do no harm,” and as scientific authorities who worked within an “illegitimate” or “stigmatized” research enterprise?

Despite attempts to clear the air, a whiff of stigma surrounds not only the objects and subjects of drug research but researchers themselves (Becker 1963; Clarke 1998; Goffman 1963). They differ on how to define and pose the problems they study. Many eschew the term *addiction*, already a dirty word in “drug dependence” research by the early 1950s. Since then, an array of substitutes has been offered—from “drug abuse” research in the 1970s, “substance abuse” or “chemical dependency” research in the 1980s, and research on “chronic, relapsing brain disorders” in the 1990s (White 2004). Today the phenomena under study have mutated into matters of “disrupted volition” (Volkow 2006) and “neurobiology gone awry” (Volkow and Li 2004), and the term *addiction* is in the process of returning to vogue (O’Brien, Volkow, and Li 2006). Each generation reconfigures the lexicon. Semantic shifts signal conceptual and technological changes in how science is done, as well as the changing social contexts, material conditions, and institutions within which scientific knowledge is made.

Discovering Addiction provides a history of the changing status of drug users as objects and subjects of knowledge. Terms like *paradigm shift* or *zeitgeist* are somewhat mystical explanations for how change happens in scientific communities. Instead of using such terminology, this book is organized around the focal concept of “laboratory logics,” the pattern of beliefs that shape practical reasoning in science. Practitioners within epistemic communities trade in relatively narrow and coherent structures of belief that enable and constrain the lexicons, techniques, technologies, and practices that are considered legitimate ways to speak authoritatively about drugs in a given domain. Laboratory logics connect cultural beliefs and prior commitments to the actual practices enacted in the lab. Some laboratory logics incite research subjects to speak; others require their silence. Despite my emphasis on the laboratory, I believe that

drug users themselves know quite a bit about how drugs work. This book tells the story of how they have come to play a lesser role in producing scientific and clinical knowledge than they once did.

In the 1950s, scientific researchers argued that drug addiction should be treated as a public health problem. They won few adherents beyond a handful of lawyers and sociologists opposed to framing addiction as either a crime or a disease (Becker 1963; Lindesmith 1946, 1947). No alliance formed between those who sought to reframe addiction as a public health matter. When I first encountered the scientific voices of the mid-twentieth century, I wondered who these “science guys” were, where they came from, and what authority led them to stand up to resist a law enforcement apparatus that was bent on prosecuting physicians and scorned scientific knowledge. Where did their knowledge come from? What were their relationships with addicted persons? Did they exploit the bodies and minds of their subjects—human or animal—in the name of science? Their research subjects, too, testified that addicts should be treated as if they were sick persons rather than criminals (Campbell 2000). What did these drug users think about those who put them under a microscope? Did they feel used, like the “human guinea pigs” the popular press made them out to be? Archives are largely silent on such questions. Interrogating the ethos that the addiction research culture carried into its work required bringing back to life long-dead laboratories where conversations arose about the ethics of experimental work with human and animal subjects. Long before clinical researchers confronted today’s compulsory ethical questions, addiction researchers debated whether “treatment-naive populations” were preferable to “knowledgeable subjects” who had experiences with drugs behind them. Taking a fresh look at how addiction researchers coped with their predicaments casts today’s unresolved dilemmas into sharper relief and puts historical flesh on bioethical bones.

LOOKING INSIDE THE SOCIAL WORLDS OF ADDICTION RESEARCH

While no science speaks with a unified voice, substance abuse researchers comprise a remarkably disunified chorus. Listening closely to substance abuse researchers can tax even the most scientifically literate scholars and the most well-informed policy makers. Although this cauldron of conceptual confusion can be a barrier to understanding, multiplicity in the sciences can also be a source of strength rather than a failing (Mol 2002). Multiple literacies are less a

problem to manage than valuable tools for mapping boundaries between different communities of practice and attending to what it means to cross them. Doing oral history interviews forced me to cross boundaries between science and policy that I would once have left intact. I have become accustomed to being scolded about using terminology that issues from a social domain, scientific approach, or historical moment different from those of my interviewee. Thus my favorite definition of a drug has become the most prosaic one: “a substance that, when injected into a rat, produces a scientific paper” (Leake 1975, 30).

Given the centrality of drugs in social life, it is surprising that the history of scientific research on addiction has not yet been mapped in the social studies of scientific knowledge. Though *Science* magazine decades ago cast narcotics research as ripe for the sociology of science (Walsh 1973a), addiction research has yet to attract its share of sociologists, historians, or cultural theorists of science, medicine, and technology.⁵ Griffith Edwards, longtime editor of *Addictions*, has observed, “Despite the growing interest in building a cadre of specialists devoted to the study, management, and prevention of addiction, there is still virtually no systematic information, much less an organized body of knowledge, about the individual and social factors that contribute to the evolution of a specialist field like addiction studies” (2002, 383). Historian Virginia Berridge has lamented the lack of analytic history of addiction despite increased availability of primary source materials such as oral histories and visual material (2006). *Discovering Addiction* replies to these lacunae by providing a historical account of the social organization of scientific knowledge production about drug addiction, a history of the ethics of human and animal experimentation, and a historical sociological account of the structure of a public science that was organized in the face of a proprietary pharmaceutical industry.

Listening to insider accounts by lifelong inhabitants of the addiction research enterprise is instructive for discerning the social structure of the scientific communities involved in “discovering” or constructing addiction. My work is indebted to “participant-historians” who have written accounts of their scientific careers or conducted interviews with one another. These sources informed my own interviews with key figures. I am grateful as well to scientists and surviving family members who granted me access to primary materials moldering in basements and filing cabinets. I have used all of these materials as well as the sparse scholarly literature to understand the social structures and material conditions that shaped the beliefs, commitments, and practices I here describe.⁶ The chapters of this book cohere around the figures of prominent

scientists not because of their individual genius or acumen but because they were what Ludwik Fleck called “standard-bearers in discovery” (1979, 42), voicing new laboratory logics and embodying emerging configurations in ways that have periodically remade the social worlds of substance abuse research.

Participant-historians were invaluable in distinguishing scientific subgroups, establishing patterns of interaction within social networks, and documenting accelerations in the pace of innovation or geographic mobility, formation of institutional infrastructures, and growth of coherent identity and shared history. For example, behavioral pharmacologist Joseph V. Brady’s (2004) “revisionist” history of the field argues that the conceptual interplay between behavioral analysis and pharmacology came about not because of individual genius but from the “gifts of a fortuitous environment”—a widening pharmacopeia, increased federal support, and growth of in-house pharmaceutical industry testing. He attributed research pharmacologists’ interest in drug abuse to regulatory efforts to assess the abuse liability of compounds not yet on the market, a point that insiders consider obvious but that an outsider might miss by emphasizing another facet of the addiction research enterprise, such as the search for less addictive painkillers or more effective addiction treatments. No one facet should be conflated with the others; no one facet is sufficient to understand the whole. No one definition of addiction has integrated enough levels of analysis to achieve universal acceptance or even to survive very long.

Insider accounts also offer invaluable clues to the thought styles at play in a scientific collective (Fleck 1979), but they are of course always political reconstructions of events (Clarke 1998, 5). Thus “internal history” must be handled with care so as to avoid overreliance on major figures at the expense of social-structural factors. Griffith Edwards has acknowledged: “The heroic view of history as something constructed by the deeds of great men and women is today not too fashionable. The historian in this case will rightly insist that what has happened over these decades in the drug and alcohol field must be understood in terms of technical innovations, the larger background social processes, and the great play of ideas” (1991, xiv). My focus on the conceptual interplay between the social organization of science and the discoveries of individual laboratories spurred the turn to oral history, which offers data on the shifting repertoire of lexicons and logics through which addiction has been theorized. Oral history plays a contested role in the history of science and medicine because it is often identified with internalist history. Pharmacologist David Healy, who has published interviews with his colleagues, has been taken to task

for basing his analysis on “reminiscences, oral histories, and interviews made by the author with his fellow psychopharmacologists” (Rasmussen 2003). Yet interview transcripts tap into a reservoir of material on the social organization of the research enterprise and reveal how scientists perform their everyday ethical subjectivity.

Oral history interviews offer empirical pointers toward what begs for analysis. Research ethics were typically broached by those whose careers began at the Addiction Research Center in Lexington, Kentucky, many of whom assumed my interests lay primarily in the treatment of human subjects rather than the sociology of knowledge. Jerome H. Jaffe has explained that the ARC “stood virtually alone as [an] island of scholarship in a sea of general indifference to questions about the nature and treatment of alcohol and drug problems” and that “the few scholars who did become interested in these backwaters of science were either affiliated with th[is] center or dutifully made voyages to [it]” (2002, 101). Today’s addiction research enterprise can be traced to these “backwaters,” from which an impressive number of key figures sailed into prominent roles in government, academia, or industry.

UNSETTLING ASSUMPTIONS: EXPERIMENTATION ON HUMAN BEINGS

Bringing to life the hidden history of how clinical trials of analgesics were once conducted raises unsettling questions about governance. Regulatory requirements set in place by the Food and Drug Administration (FDA) in 1962 caused growing demand for human subjects, so pharmaceutical companies turned to prisons to find them (Adams and Cowan 1971). Certain events (recounted in chapter 6 in this book) forced them toward alternative populations in the mid-1970s. Thirty years later, prison research is being reconsidered due to a shortage of willing participants eligible for clinical trials (Urbina 2006). Because the U.S. population is “treatment saturated,” private clinical research organizations now seek “treatment-naïve” subjects the world over or turn to poor communities within the United States (Fisher 2005; Petryna 2006; Shah 2006). Clinical trials remain class- and race-stratified exercises that unevenly distribute risk and benefit.

What would it take to achieve socially responsible clinical trials? Research typically involves interaction between two classes of individuals. One group, who is usually white, middle-class, possessed of advanced degrees, and enfranchised within the social worlds of U.S. biomedicine, experiments on individu-

als from another group, who are usually less educated, less enfranchised, and living in poorer communities. The latter group is racially and ethnically more diverse and less experienced with the technoscientific aspects of biomedicine but is more experienced with directly adverse social impacts of science and extractive technologies. The increasing social distance between researchers and subjects forecloses public participation and leads to clashes between scientists and nonscientists.⁷ Although these conflicts are often attributed to lack of scientific literacy or to inadequate public understanding of science, American publics have proved willing to bear some of the burdens and costs of scientific research (Lederer 1995, 137). Acceptable risk, however, is “in the eye of the beholder,” according to a study by Ubel et al. (2006) showing that “the more distant the patient, the more likely people were to recommend [taking perceived risks]” (Bakalar 2006). Such results indicate how social distance enters into the human calculus of risk and benefit: the more distant the decision maker is, the more likely it is that there will be a recommendation to engage in risky practices.

The postaddicts described in this book were human subjects who were in far closer social proximity to the scientists who studied them than are many subjects of clinical trials today. Although postaddicts had their own reasons to submit their minds and bodies to science, they did so knowingly and without false hopes that participating in a study would somehow cure their personal dependency on addictive substances. Today, the political and economic climate of pharmaceutical research and promotion works against realizing the ideal of socially responsible clinical trials—by increasing social distance between the researcher and the researched and holding out hopes that cures will come about because of individual participation. Compounding these problems is the fact that “unbought scientific opinion is increasingly hard to find” (Le Carre 2001, 267). As *Discovering Addiction* reveals, research environments can and should be structured to guard against commercial interests colonizing the research agenda. Restraining commercial entities takes more than token representation or stepped-up attention to informed consent. Looking back to a time when researchers were in closer social proximity to their research subjects, this book tells the story of a committee of the National Research Council that directed the addiction research trajectory while buffering researchers from commercial interests. Centralized coordination by the National Academy of Sciences had great advantages but also created a “closed world” that relied on a mode of expertise that prevailed in a small enclave and was not readily translated beyond it. One might have expected misunder-

ings between science and its publics to result from the practice of walling off scientific venues from public scrutiny.

By revealing how the social organization of the addiction research enterprise has worked since the 1920s, I hope to stimulate more-informed responses not only to drug problems but to pharmaceutical research. By showing how scientific lexicons, logics, and commitments move between the microsocial worlds of addicts, scientists, clinicians, and policy makers, I hope to offer fresh insight into the beliefs and assumptions that underlie concepts of drug addiction and dependence, as well as the ethics of drug research. Both research and drug policy would look different if advocacy from the perspectives of the communities most affected by the War on Drugs were taken into account. Science is not a very effective weapon in the War on Drugs, but it could be an enterprise that effectively calls wars on drugs into question and reduces the toll they take on the most vulnerable among us.

STUDYING UP: SITUATED KNOWLEDGES, UNSEEN AUDIENCES, AND IMPLICATED ACTORS

Anyone studying the rapid multiplication of the conceptual and experimental approaches through which science is made encounters challenges when navigating the incestuous social networks thick with unseen kinships that comprise scientific social worlds. External scrutiny enables outsiders to discern connections not apparent to those who inhabit scientific networks. Some of my connections, lexicons, and conclusions are counterintuitive for inhabitants of the social worlds I study. My work takes place in a different “universe of mutual discourse”—the social studies of science and technology. The historical sociology of scientific knowledge offers conceptual resources—hierarchies of credibility, epistemic cultures, and enunciative communities—suited for making sense of the social networks and practical discourses that form in “fortuitous environments” and for studying the political economy that structures such environments and shapes the cultures within them.

Multiple cognitive and interpretive resources are necessary to characterize shared beliefs, commitments, logics, material conditions, and institutional arrangements in which a research enterprise is situated (Haraway 1988; Harding 1991; Longino 1990, 2002). This book draws on the feminist sociology of knowledge despite the fact that I do not focus on women or gender. Drug-using women did not participate in the research related here. Nevertheless, all scientific work takes place through forms of expertise that are grounded in

specific social situations and institutions that are gendered in ways that affect the research culture of laboratories just as surely as they pattern the microsocial dynamics of hospitals and prisons. Instead of centering explicit gender dynamics as I did in my previous book *Using Women: Gender, Drug Policy, and Social Justice* (Campbell 2000), here I am using feminist situational analysis to attend to power differentials and map connections between social worlds (Clarke 1998, 2000, 2005).

Each chapter of this book shows how “drugs” and “drug users” are constituted as objects and subjects of knowledge in a given domain; which techniques, technologies, and material practices are obligatory in that space; and which conceptual and practical logics are considered legitimate for making knowledge in that social world. Situating knowledge within the material constraints of its production also requires paying attention to the subjects whose lives, brains, and bodies are under study. Whether called “addicts,” “patients,” “users,” “IVDUs” (intravenous drug users), “research subjects,” “informants,” “inmates,” “participants,” or “clients,” they are “implicated actors” whose behavior and beliefs are targeted for change in all of the social worlds of substance abuse research (Clarke 1998, 16–17; Clarke and Montini 1993; Clarke 2005, 46–48). They are largely silent subjects who are often “only discursively present—constructed by others for their own purposes” (Clarke 2005, 46). In most addiction studies, addicts were not actively involved in negotiating their identities or the meanings of addiction but were instead acted on, “neither invited by those in greater power to participate nor to represent themselves on their own terms” (Clarke 2005, 46). Although users’ voices appear rarely, they haunted the pages of this book just as they haunted the corridors of the laboratories that depended on them in the process of making science.

Addiction is a complex social phenomenon that exceeds the grasp of every explanatory account. Social questions have been largely foreclosed by the laboratory logics of most laboratory-based substance abuse research, which repeatedly disqualifies or “controls for” the social. Yet the meanings attributed to drugs differ among social contexts, which partly shape the experience of drug effects and the interpretation of seemingly physiological sensations. Although scientific lexicons are incorporated into the vernaculars through which people narrate their inner sensations, drug experiences obviously are not simply the stuff of science, nor do they occur entirely within the brain. Current neuroimaging studies attempt to visualize craving by invoking social cues, such as paraphernalia or ritualistic aspects of preparation to which addicts are conditioned. These return to the perennial question of how individual desire and

experience is shaped by social setting and cultural context and to the puzzling variations in how individuals respond to drugs.

Yet the substance abuse research enterprise has become fully and even forcefully differentiated from social settings where drug use actually occurs. Even preclinical and clinical research programs now take place at considerable distance from each other. Pathways into the field have changed radically as neurobiological and molecular pharmacological routes supersede all others to focus at the cellular and subcellular level. Widening gaps between “science” and “service,” “research” and “treatment,” and “basic” and “applied” research have developed alongside an increasingly specialized vocabulary for naming, lamenting, closing, or bridging these gaps. The research effort coordinated by the U.S. government centered on addiction as an object of inquiry that required multiple approaches, but it never yielded completely to their integration.

Dynamic shifts in institutional structures and laboratory logics have required inhabitants of this research arena to retool often to keep pace with the evolving techniques used to study substance abuse. The ongoing quest to identify markers of addiction has been refracted through many different concepts, logics, and techniques, each linked to different political stances, ethical interpretations, and policy regimes. This book chronicles what scientific researchers from myriad disciplinary cultures have said and done to bring scientific knowledge about drug addiction into being. Its purpose is to show how researchers’ commitments to particular beliefs and ways of speaking are predicated on a set of laboratory logics that structure how they enact their science. Specific laboratory logics depend on the social location of laboratories and field sites, and they change when cross-fertilizations with other sites develop. Writing a history of science that traces the intersections of conceptual logics is difficult. These intersections take the “natural course of an excited conversation among several persons all speaking simultaneously among themselves and each clamoring to make himself heard, yet which nevertheless permitted a consensus to crystallize” (Fleck 1979, 15). Such a conversation also permits consensus to refract. The coming chapters alternate between crystallization and refraction.