Kites[1] from Drug Research Rehab

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In August of 1968 a 23 year old graduate student in anthropology from Berkeley packed his new VW Beetle with an ancient writing machine called a "typewriter" and drove to Lexington, Kentucky. He was one of the lucky few during the Vietnam era who had been offered a commission in the U.S. Public Health Service. His orders anointed him the equivalent of a First Lieutenant and assigned him to a new social science research unit at the NIMH Clinical Research Center, locally known as "Narco," a federal center for the treatment of narcotics addicts founded in 1935.

Though he had smoked marijuana, the student had no idea what heroin addiction was about. In fact, he had no idea what an anthropologist was supposed to do working in the U.S., never mind in the strange mix of treatment center, prison and fraternity/sorority house he found when he arrived in Lexington. His senior thesis was based on ethnography in a remote South Indian village, and at Berkeley he had worked with South Asian specialists in preparation for a return trip. That was *real* anthropology. He assumed that he would spend two years at Lexington as a research assistant and help crunch numbers for a sociologist or psychologist, something that he could handle as a recent graduate of NSF's summer seminar in "mathematical anthropology," a phrase that—so went the seminar joke—meant that you numbered your pages.

No such thing. To his surprise and delight Jack O'Donnell, the boss, a sociologist equivalent of a Colonel in rank, told him to go off and do anthropology, whatever that was, and they would evaluate things after three months. He later told him that he'd always wondered what

[1] "Kite" back in the day meant a note sent from one inmate to another.

an anthropologist would do if you had one around and the student offered a cheap experiment to find out.

The experiment continued for decades, on and off. I was that student and, now, as of 2013, I still am, older and not much wiser. The editors of this special issue asked me to write a personal essay with the role of qualitative social research in the foreground. I did that to some extent in a book called *Dope Double Agent: The Naked Emperor on Drugs,* my farewell to the field (Agar 2006), though that was more about research in the context of the disastrous policy called the "War on Drugs."

In this essay, I'll stay focused on research, on the story of a marginal social research epistemology in a sea of positivism and the policy consequences of neglecting the kind of information that it could and did provide.

As soon as I arrived, went through orientation and received my commissioned officer's manual, I wandered around the institution and talked with addicts. Early rumors were that I was undercover with Students for a Democratic Society (younger readers will have to look on the Internet) or a "Fed" posing as a researcher. I'd had no preconceptions about addicts or addict research driving to Lexington. I didn't know enough to have any. But I was learning about being a heroin addict from heroin addicts, like an ethnographer would naturally do. I spent time shooting the breeze in "the unit" where they lived and in the dining hall and the gym. I even checked into the hospital for a couple of weeks to see what life in "the joint" was like.

After awhile I dived into the professional library at the hospital. There were a lot of books and journal articles about addiction, not as many as there soon would be, what with President Nixon's declaration of a war on drugs looming on the horizon. As I read through the literature, though, I was surprised at my reaction. From South India days I was familiar with the

concept of "culture shock." I'd experienced it myself in the village. This new assignment should have been easy by comparison. After all, Lexington was in the same country as Berkeley, sort of. Most everyone spoke English. What was the problem?

Instead of culture shock, I thought of what I was going through as "library shock." Once I started reading about addicts as behavioral/social scientists and clinicians described and explained them, it got confusing. The two versions of "addict"—the one I was hearing *from* them and the one I was reading *about* them—had very little to do with each other. That was the library shock.

Library Shock Was the Tip of a Two Century Old Iceberg

I had led a sheltered life in anthropology departments as an undergraduate and then a first year grad. The Lexington library dunked me into a two hundred year old argument about what a human social science should be, an argument I hadn't really heard much about in my anthro-cocoon. That argument is summarized in *The Lively Science: Remodeling Human Social Research*, which I shamelessly recommend (Agar 2013).

In the 19th century, as August Comte and John Stuart Mill laid the foundations for a human social science, not everyone agreed they were doing it right, especially in Austria and Germany. The heart of the Teutonic criticism was both simple and profound. The phenomenon of the new human/social sciences—humans in society—had characteristics that the phenomena of the natural sciences had never taken into account, logically enough, because their phenomenon didn't have those characteristics. That was bad enough. But in addition to that major problem, the scientist him or herself was also an example of the phenomenon of the science, setting up all the twists and turns of self-reference that were to come in the 20th century. Human social science was indeed a science, argued the Germans and Austrians, but it had to be

different in many important ways compared with the experimental laboratory tradition of physics and chemistry.

Enlightenment science, which won the argument and guided most of what I read in the Lexington library, required experimental simplification and scientist control to meet the "gold standard" set by John Stuart Mill. But, according to the critics, a "science" of humans in their social worlds is exactly what that kind of science was *not*, because it didn't engage the phenomena—real people leading real lives—and it didn't handle the fact that the science applied as much to the observer as it did to the observed

The model of human/social science that John Stuart Mill built on the natural science tradition did not deal with the contexts and meanings and practices of subjects, the world of their lived experience as it evolved over time. It didn't bother to learn their intentionality from their perspective—their beliefs, emotions, desires and purposes—without which an accurate description and explanation weren't possible. It didn't recognize that the scientists themselves were subjects, dominating the encounter by defining everything in terms of their own intentionality and lived experience. Human subjects, it turned out, were not gas molecules behaving according to laws, and scientists weren't passive recorders of data on the observable behavior of non-sentient objects. The human/social science of the German speakers was an encounter between subjects, "intersubjective," neither objective nor subjective in any simple way.

No matter. At that critical19th century fork in the road, mainstream human/social science was declared to be just another example of Enlightenment science. The more it looked like a laboratory experiment, the better. "Science" meant Galileo and Newton. Who could argue with success?

The "literature" about addicts—the way most of it was written I couldn't figure out why they called it that—reported tests of what the scientists already thought they knew, in this case, that addicts were social-psychological failures. They tested a small number of variables—things they thought might be possible examples of that failure. Subjects were brought into a world simplified, designed and controlled by a scientist. The problem was, they didn't know much about addicts before the research started, and they did the science in their own terms without knowing what if any sense it made to their subjects. The results, looking back from 2013? A lot of books and articles that, for the most part, didn't help much to understand or solve the "drug problem."

What was required here, as one of the German critics, Wilhelm Dilthey, had written, was a *different kind of science*. But with the force of history behind it, the human social science that came down to us was based on the experimental laboratory of natural science. Any science requires *evidence* organized according to some *logic* in an argument that can be *challenged*. No one disagreed with that, including the Austrian and German critics. But the version of human/social science that dominated denied the fact that the starting and ending point of the science had to be the world of humans in society, not an event constructed and controlled by a scientist. It denied that the "objects" of the science were—like the scientists—"subjects" with beliefs, values, feelings and goals, not to mention the social influences they exerted on each other. The subjects brought to the moment a history and lived experiences. Not only was the scientist unaware of that, or at best only aware superficially. Scientific control, under the epistemological flag of "objectivity," allowed them to impose a framework that probably distorted those worlds. And the scientists themselves were exempt from the influences that they

claimed to study. They worked under the delusion that, being "objective," they had nothing to do with research process and research results.

My acute library shock had a pedigree that I was unaware of. Much of what I heard from addicts in Lexington wasn't in the published research I was reading, because there was no way it could have appeared. There were a couple of exceptions, like a book about addiction by Alfred Lindesmith (Lindesmith 1947), someone who had obviously listened to some addicts talking about their lives in their own terms and learning from what they said. I found an odd piece by a former jazz musician named Howard Becker that described how you had to learn to get high from smoking marijuana (Becker 1953). And there were a few autobiographies around that offered some compelling insights into real addict worlds (Brown 1965).

The few examples to the contrary notwithstanding, most addiction research described a physiological problem with negative psychosocial causes, and those causes had to do with explaining what both science and popular discourse viewed as an "escape" from the more desirable "normal" world in which we ordinary non-addicted mortals lived. The main question the behavioral/social scientists wanted to answer was, what makes those dope fiends[2] want to escape? It had to be an escape, since "getting high" could never be a positive thing in the vocabulary of the researchers, clinicians, and—most importantly of all—the politicians of the times. Unless, of course, they were drinking.

Dope fiends were more than the dope

Dope fiends "escaped from reality" with heroin—Lexington's specialty—until they had used it often enough and long enough to become addicted. They did it either because they were

[2] I use "dope fiend" in this article in honor of all the addicts I've worked with who preferred street terms rather than jargon or euphemisms when talking with non-addicted audiences.

psychologically disturbed or socially deprived. Those were the main kinds of variables that the researchers tested.

I remember I kept thinking of the line in *West Side Story* when one of the gang members imitates a psychiatrist, "Juvenile delinquency is a social disease." True enough, most Lexington "patients"—they were "sick," so that's what they were usually called by the clinicians—came from backgrounds of poverty. Addicts with money seldom showed up at Lexington. And most addicts whose stories I heard didn't grow up in what I would then have called "Leave It To Beaver" families, either, But then again, it's a pretty good guess that most people who grew up poor in less than perfect families never became heroin addicts. Even by traditional Enlightenment standards, this was already bad science. If most people *similar to* addicts were *not* addicts, then how do you explain that? John Stuart Mill 101: You can't explain addicts only by studying only those who are addicted.

From day one, listening to dope fiends, I knew there was more to them than that. How to put what I was learning into words? Shortly after I arrived, I got some help. I discovered their folklore (Agar 1971). As usual in the kind of human/social science that the Germans and Austrians argued for, most of what counts in the research is only *learned* by the researcher *after* the research is well underway. Some of the folklore I discovered—an example in a moment—did reflect the social problems and physical dependency that the literature was obsessed with. There was some truth to it. But there was also another side to the story.

Here's how the widely known *Honky Tonk Bud* started out:

> Honky Tonk Bud, the hip cat stud, stood diggin' a game of pool
> Though his bags were draggin' Bud wasn't braggin'
> He knew he was real cool.
> He was choked up tight with a white-on-white
> Had on a cocoa front that was down
> Sported a hand-painted tie that hung down to his fly

And had on a gold dust crown.

This was *not* the image of a social failure with psychological problems. It is an introduction to a long rhyming story, a "toast" they called it, that described what addicts then called a "righteous dope fiend," a phrase that I later learned had been used as the title of an article by Alan Sutter in 1966 (Sutter 1966). It wasn't in the Lexington library.

Then, not too long after I found out about Honky Tonk Bud, an article appeared in a mainstream drug journal called "Taking Care of Business: The Heroin User's Life in the Street," the lead author an anthropologist named Ed Preble (Preble and Casey 1969). Ed would later become a hero and mentor. You can see from the title that this, like Honky Tonk Bud, is not the image of a dope fiend with a "social disease" nodding off and drooling and escaping from a middle/upper-middle class world.

There were also toasts describing a down side to heroin addiction. The image of social-psychological down-and-out in the literature wasn't all wrong; it was just, at best, partial, the part that the non-addicted world had decided must be the entire story. For example, another toast that Lexington addicts knew about was called "King Heroin," later recorded as a song in 1972 by James Brown. It described how a heroin habit forced a person out of socially desirable mainstream roles. Heroin made a "schoolboy neglect his books" and a "world famous beauty neglect her looks."

And one day I asked a Lexington patient/musician named Rick to sing Lou Reed's song "Heroin," a song that expresses both sides of the story at the same time. It starts like this:

> I don't know just where I'm going
> But I'm goin' to try for the kingdom if I can
> 'Cause it makes me feel like I'm a man
> When I put a spike into my vein
> Then I tell you things aren't quite the same

The other addicts who were listening? They went into physical symptoms of early withdrawal, even though they'd been at Lexington for awhile and therefore were "detoxed," as they called it. Their bodies remembered the feeling of heroin. Physical addiction really happened, and once a person crossed that boundary it was a powerful force of nature, even after acute withdrawal was over and done with.

Even though there was a down side, I'd learned about other parts of who they were that didn't fit the image that filled the books and articles of the professional drug field in 1968. There was more to addiction than King Heroin, and the "more" had a lot to do with explaining how they became addicts and how they might change.

Here is one example of why it mattered. Lexington "patients" went home and said it was like they'd never been in the institution, so different were the two worlds of the joint and the street. The truth of this showed up because so many of them came back quickly under the federal civil commitment program of the time. Well, "civil" commitment. I learned that it usually meant that you got busted by city or state cops and then they said, walk over to the federal building and commit yourself and we'll *nol pros* your case and avoid a lot of paperwork.

Lexington dealt with the "dope" part of "dope fiend" by assuming that it was caused by King Heroin type variables, though this doubled down on the failure theme. Failure caused the addiction and then the addiction caused the failure, a feedback loop that the usual linear equations didn't handle. But then Honky Tonk Bud the righteous dope fiend went home to take care of business, clean and in good health, thinking that that first fix would feel really, really good. Lexington couldn't deal with Honky Tonky Bud because, according to Lexington, Honky Tonk Bud didn't exist.

Here's an example of how this difference came to life in therapy. Dope fiend life in the streets was not conducted in a stable upper middle class environment with polite people who said "excuse me" a lot. Professional staff, who had grown up in and lived in just that kind of environment, heard talk of caution, mistrust, betrayal, and suspicious motives. The clinically oriented thought in terms of "paranoid tendencies." Dope fiends regarded the same "pathology" as something else. They called it "street smart."

"Addict professional discourse," to take Foucault to places he would know all too well, assumed heroin addiction was only about social-psychological failure, both cause and consequence. The researchers put addict subjects in simplified situations of the scientists' design. Only King Heroin questions were asked, and only King Heroin answers were possible. And it should be said again, there is truth to that image of addiction from a dope fiend's point of view as well. But then there was also truth to Honky Tonk Bud. Questions about that image were beyond what the research imagination would permit, and the required top-down control of Enlightenment science guaranteed that they would never come up.

The research results suited the chemical scapegoating role that "drugs" played in American ideology of the time. Nixon's war on drugs was motivated politically by the use of "drugs" to explain college protests, failure in Vietnam, and rising crime rates in the cities. Things are screwed up? It's the dope. Animals, said Levi-Strauss writing of totemism, are good to think with (LeviStrauss 1968). Drugs, I used to tell colleagues in the drug field, are good to blame with.

At Lexington in the late 1960s, the epistemology I had learned in anthropology, the one featured in this special issue, was viewed as somewhere between pseudo-science and "mere journalism" by colleagues at the hospital. It was neither of those. Like any science, it made a

case based on evidence, logic and falsification. The research I read in the Lexington library, in contrast, built walls of laboratory control around the possible ways a dope fiend could be described and explained by "science" even before a project started. Listening and learning about a Lexington "patient's" own intentionality and lived experience showed how the results of most behavioral/social science of the time—results that had been shaped before the research started—was far from a full description and explanation of heroin addicts. Instead, the results aimed directly at the part of being an addict that served the politically useful portrait of dope fiends that U.S. drug policy needed to justify the "war" to come.

My library shock was born of the different results produced by two 19th century human social science epistemologies applied to the same people, heroin addicts in the urban U.S. Addicts were more complicated than we wanted to believe they were. Their sense of who they were and their relationship to heroin shifted and changed, with context and over time. In Lexington, with treatment staff, they were usually King Heroin, either because they felt that way in that context or because they were gaming the system to get an early release date. On the unit where they lived with each other, Honky Tonk Bud made frequent appearances. On the streets, as I heard then and would later see in living color, they cycled between the images, depending on circumstances.

But one thing was clear. Library shock was real. Honky Tonk Bud wasn't in the literature, hardly at all, not then. But he was alive and well in the conversations in the unit, in the gym, and in the dining hall, and most importantly for the issue of "relapse," in the streets when he or she went back home. Nothing in policy or treatment was dealing with that.

During the two years at Lexington, it felt pretty lonely, talking about how dope fiends were about more than just the dope. My friend Dick Stephens kept me sane, and much later, in

1991, he put a more comprehensive image of addicts into a much bigger picture with his book *The Street Addict Role* (Stephens 1991). Cracks in the monolithic hooked looser image appeared before then, though, after I had left Lexington to return to grad school in 1970. David Musto, another two year Commissioned Corps wonder, published a history, *The American Disease*, in 1973 (Musto 1987). The book described how dope fiends had changed, from middle class women around the turn of the 20th century, to white immigrant men living in cities in the 1920s, to blacks and Hispanics in the 1960s. Same drug, different historical conditions, different kinds of people got addicted. Then a psychiatrist. Lee Robbins, published an article with her colleagues in 1975 (Robins, Helzer and Davis 1975). She interviewed Vietnam vets who'd become addicted in that country. It turned out that the ones who weren't addicted *before* they left by and large quit when they got home.

Narcotics addiction looks different now than it did in the late 1960s, just like it looked different in the 60s compared to when it took shape in the 1920s. And thanks to the kinds of human/social scientists writing in this special issue, the alternative science is now more present than it was then. Nevertheless, I still encounter, most of the time in work I've done over the last decade, the old time religion, that human social science *is* the test of prior hypotheses rather than the learning of new ones, and that the more the test looks like a laboratory experiment the more credible it is.

Methadone is More Than a Medication

Here's one more example of the epistemology difference from the old days. After a year back in graduate school and a couple of years as an assistant professor, the state of New York decided, as Lexington had earlier, that it needed a center for behavioral and social science as part of its "war on drugs," known in the Big Apple as the Rockefeller Laws. Since the total number of

drug experts at the time could dance on the head of a pin with room left over to park a couple of cars, I was offered a job and, having learned that academic politics made drug policy look positively rational, I took a position as research scientist and moved to the Upper West Side in 1973.

The drug war had escalated, just like Vietnam had. I stepped back into drugworld, a little surprised at my own return, but comfortable in my discovery that I was a natural New Yorker, assuming the worst, not being surprised when that assumption turned out to be optimistic, and then turning the disparity into dark humor.

Methadone was the news of the day when I arrived in 1973. I've written about this elsewhere and won't repeat it all here (Agar 1977). At the time, it was *the* new magic tool to weed out the "drug problem" and toss it into the Hudson. One of the founders of this approach to heroin treatment, Elizabeth Nyswander, become a personal hero. She'd worked in Lexington, concluded that, whatever they were doing, it wasn't working worth a damn, and figured the best way to treat dope fiends was with dope. I heard stories in New York of how she actually answered addict telephone calls personally (Hentoff 1968).

Methadone, ironically enough, was a Nazi invention, something that I'd heard referred to as "dolophine" by addicts in Lexington, street name "dollies." The "doloph" part was named after guess who? "Adolph." Methadone was a synthetic narcotic, same family as heroin, but with a wave of the magical medical wand it became a "medication." It fit into Nixon's war on drugs. The plan was, dry up the importation of heroin, but make treatment more available so that addicts would be forced into it when they couldn't find heroin in the streets. Nyswander, and her colleague Vince Dole, argued that addiction was a physiological imbalance and that narcotics

were simply necessary for some people to even things out and make the "patient"—the two of them were still, in the end, doctors—normal. Like insulin for diabetics, that kind of thing.

So one fine day as I was working my way into the streets via neighborhood-based methadone clinics, I was chatting with an "outpatient" who had just picked up his "medication." A friend of his, also an addict, came by and the three of us engaged in drug-based pleasantries, like discussing what particular psychoactive item was hot that week in the Union Square market. The friend then excused himself, saying that he had to leave because he had to go to the clinic to "cop."

In the kind of human/social research that the German critics of John Stuart Mill articulated, when something like this happens, it is not "error variance." It is a surprise that hits one on the head with a signal of a different point of view, a new angle on things that needs to be explored. I called such moments "rich points" in earlier writings about ethnography. "Cop," needless to say, is not the term that physicians use when they write a prescription. It is the word that dope fiends use when they go to their dealer to buy. To "cop" methadone meant to go to your dealer—i.e. the clinic—and get your stuff.

The language shift signaled a context shift, a point of view change, at least for that person. The "patient" had placed what the straight world thought of as "medication" into the street category of "dope." Thinking back on the previous section of this article, you could think of it, Hollywood screenplay style, as Honky Tonky Bud meets methadone.

I started a program of research, more than I can fully describe here, to show how "meth"—it then meant "methadone," not "methamphetamine" like it does today—became a street narcotic, how "meth clinics" sprouted all over the metropolitan area where the only requirement for admission was a believable story. There was also good news, even though it was

not what the programs intended. The large number of small dealers who sold a part of their clinic dose, known in the medical literature as "non-compliant patients" kept the street price low. And, for the first time in decades, a narcotic was offered as part of addiction treatment. It meant that the street/treatment boundary was made easier to cross than it ever had been. Methadone did, and does, help a lot of people, in more ways than the clinics imagined at the time. It helped many a dope fiend in a King Heroin state of mind to buy some time to leave "the life."

The chiefs at the state office where I worked were not happy with this research. Showing how meth had turned into a hot street commodity was not on the political agenda. In a moment of biographical irony some twenty-five years later, I was asked to help evaluate a brand new drug called Buprenorphine. It was meant to fulfill all the tarnished hopes and dreams of the methadone advocates, a new medication for narcotics addicts that the medical profession could control. The full story is too long to add to this already overburdened article. It is enough to say that three colleagues and I wrote an article using a concept we called "field trials," a play on the notion of "clinical trials" (Agar et.al. 2001).

We looked at data from the lab and from other countries and concluded that Buprenorphine, too, is a narcotic, and it would probably have a future in the streets. Once again, as with the earlier methadone work, the funders suggested maybe this wasn't such a good thing to publish, only this time they were a little more insistent. I waited a year, out of respect for a dead policy, and then published it anyway. As it turned out, the scenario we imagined came to pass. Buprenorphine, under its various names such as Suboxone, found its way into the streets and flourished. You just can't create a narcotic substitute and expect it not to substitute for a narcotic in the street as well as in the clinic. And the fact that it does substitute in the street is by and large a good thing even if it does look like a bunch of "noncompliant patients."

If you're getting the feeling that annoying the ideological command post in the war on drugs was a sign you were doing something right, you're getting the feeling of what it was like to work in the early days of the war on drugs. And if you're getting the idea that one of the reasons the Enlightenment view of behavioral/social research was favored was because it allowed for preservation of those ideological premises before the research started, you have (re)discovered the foundational arguments of the "Frankfurt School," another group of Germans with a critique of science as preserving ideology through the epistemology of top-down scientist-controlled simplification (McCarthy 1978).

Methadone, and later Buprenorphine, were more, much more, than just "medications." No one considered that possibility when they were first proposed by the drug policy wonks and medical professionals and pharmaceutical companies. No one wondered what they might look like from different points of view, like the point of view of someone who was addicted to narcotics. As a matter of fact, in the early 2000's, dope fiends still sold the "medications" in the streets, in Baltimore where I was working at the time. Treatment slots weren't available, or an addict wanted to clean up without marching to the demands of a program bureaucracy.

And of course no one ever entertained the notion that methadone was actually a convoluted way of legalizing and regulating narcotics. Except maybe the cops, who knew what was going on in the streets. Dope fiend life requires dealing with an illegal market. Make it easier and cheaper to get the dope outside those traditional market controls and the effects will be positive in the short term, possibly leading to an easier exit from dependency in the longer term.

But that's not because a new "medicine" has arrived; it's because the new narcotic profoundly impacts the underground market in ways that would make Milton Friedman proud. He's not exactly my favorite economist, but he did in fact say that most of the harm that comes

from drugs is because they are illegal. I don't favor simple legalization—another topic beyond the scope of this article—but I think that, though Friedman's advice messed up Latin America, he was right about narcotics.

An Epidemic is More Than a Disease

There are many more stories to tell. At this point the editors, and probably the readers, are sorry they started in on this shaggy drug story. I want to add one more, though, the last study I did as a drug researcher. This one is different because it moved up in scale from ethnography of a kind of person/group. It used the same epistemology at a higher level to understand historical dynamics that, like a rip-tide, pulled in large numbers of different kinds of people at different points in time in a similar way.

In the early 1980s I was—with some pride—politely ejected from a committee of the National Research Council, a story told in *Dope Double Agent: The Naked Emperor on Drugs.* In a Luci-Desi comedy hour episode in 1957, Lucy told guest star Talulah Bankhead, "I've been thrown out of better places than this!" Talulah replied, "You have never *been* in better places than this." But I'm telling you, it was Foucault in living color, drug war ideology gripping the committee in its tight fist and working very hard to strangle it if it started to say the wrong thing. I left the field for a decade or so in the 1980s.

But then, one day in the early 1990s, I did some work for a Hopkins public health program in Baltimore run by Carl Latkin that made sense to me (Latkin, Sherman and Knowlton 2003). I went back into the streets and felt angry and depressed that, after years of the war on drugs and I don't know how many billions of dollars, things were even worse than they'd been in the 1970s. The crack cocaine scene looked more destructive than anything I'd seen before. Why

does this keep happening, I kept asking myself? Why hadn't illegal drug epidemiology answered its core question—*Why these people in this place at this time*?

This question wasn't only about learning the lived experience of heroin addicts. This was, why do narcotics epidemics happen at all? But the research used the same epistemology of the alternative human/social science as the earlier work. No hypothesis, just a researcher in learning mode, looking for a pattern rather than a variable, using whatever information he could find, changing the research at time T depending on what was learned at time T – 1, letting the world speak to him in its own terms rather than hammering it into the closest imitation of a laboratory he could make up. And, in the end, still aiming for an argument based on evidence and logic that could be challenged, just like any science would.

What was an "epidemic" anyway? It was and is a public health word inherited from medicine, a term that came into the drug field along with its dominant medical language, the same language that explains earlier sections of this essay on the "dope" and the dope fiend "patient" and on methadone as "medication."

Epidemiology has its roots in infectious disease, as in the classic SIR model that classified populations into changing ratios of "susceptible," "infected," and "recovered." With drugs, this translated into looking at "peer groups" as representing the kind of contact that could "infect" other members, or looking at dealers as "vectors," or speaking in terms of "incidence" (rate of new users/addicts per some time interval) and "point prevalence" (proportion of users/addicts at some point in time). There are other variations on both these terms. One of my favorites was a concept from Dutch colleagues at a conference, "last night prevalence," as in, "did you get stoned last night?

A drug epidemic obviously isn't a disease like whooping cough. "Epidemic" is just the name for a mathematical curve that you can find in lots of other places, like the take-off of a consumer product, or a sudden increase of any kind. In fact, Malcolm Gladwell grabbed the ordinary English language expression "tipping point" and got rich off the curve (Gladwell 2000). Gabriel Tarde used it in his 19th century sociology and, later, Rogers made it a basis of his theory of diffusion of innovation (Rogers 1995).

In drug world, "epidemic" just meant that there was a little bit of drug use rumbling along over time, and then it seemed like, out of nowhere, all of a sudden, a lot of people were using it. "Epidemic" meant an illegal drug tipping point, a curve, for example, that resembled the takeoff of iPad. Or, another example, when I went off to India to do my traditional anthropology in 1965 everyone in college was guzzling beer. When I came back in 1966, one academic year later, many students were smoking marijuana instead. It certainly made the conversations more interesting. Epidemic, tipping point, "logistic growth curve" if you want some mathematical sex appeal.

For drug epidemics, no one had been able to predict them very well. Everyone was surprised when they happened. Everyone had their favorite variable to explain them, but a little inductive logic showed that their offerings on the altar of science were neither necessary nor sufficient.

I worked on this project for years, until 2005, thanks to a grant from NIDA, together with colleagues like Heather Reisinger and James Peterson and Alejandra Colom. We wrote a lot and there's too many stories to tell, though the book I keep plugging in this article, *Dope Double Agent*, will get you going if you'd like to see more. The bottom line was, illegal drug epidemics

are really a story best told as economic history, a history that unfolds in ways better formalized by the then new sciences of chaos and complexity than by any of the old linear models.

An illegal drug epidemic was poorly served by a metaphor of one person catching a disease from another. Epidemics had to do with an organizational crisis that changed drug production, with an international migration stream that provided couriers, and with a segment of a population who were rapidly and unexpectedly slammed by a historical change of either rapid decline or undelivered promises. The different shapes narcotic epidemics had taken in American history had already been described in Musto's history, cited earlier in this article. In fact, we built a complexity computer model of a heroin epidemic that was going on even as we did the historical research, based on the lived experience and intentionality of the people who drove the epidemic curve as it took off. That work showed that a business model better served to show how epidemics worked than did a disease-based metaphor.

Why did this matter? One article we wrote described the crack epidemic using the model we developed (Agar 2003). The article was used, along with a lot of other material, to change the discriminatory crack laws that required longer sentences for crack arrests than it did for powder cocaine, there being a racial correlation there. That article, written about an epidemic that occurred when I wasn't paying much attention to the drug field, was the most useful thing I ever did. The model made the disparity visible. I guess it just took a few decades to learn how to get it right.

Another example: The research showed other interesting things. The epidemic curve actually flattened out *before* public policy reacted to it. Why? Because feedback mechanisms that we learned about worked in the world of people who were experimenting with the "new" drug to

brake the curve before anyone thought to start new policies and programs. This led us to suggest all kinds of possibilities for intervention.

For example, we did some drug education about heroin during the project and talked about how groups could be led into discussions that amplified the negative feedback in their small group, something that occurred naturally in more diffuse ways in their social worlds. But then this strategy meant the users would also need to talk about the positive effects of experimenting as well. For early use of heroin, many people report exceptionally positive experiences. No way we could do that in the U.S. drug field.

Then we came up with a model for early intervention, a "monitoring" system based on information from front-line people in drug treatment and counseling together with a simple computer model to track developments and make informed decisions about where to invest resources and gain further knowledge about what a potential epidemic curve was doing. We asked around, but the research people said it was too applied and the applied people said it was too research-like. The exceptions were those front-line people who helped us conceptualize the system. They loved the idea.

At that point, in 2005, the grant ran out and so did the almost 40 year run of that anthropology grad student who landed at Lexington in 1968. Since then he has moved on and continues to work on new hopeless issues, like water in the Southwest. But he hopes that this article serves the purpose of the editors who invited him to write it, namely, to open up human/social science so that it can expand beyond the unrealistic 19th century straitjacket that it created for itself and attend to its phenomenon, us, in our ordinary social world, doing whatever it is we do to make a life work. The epistemology for this alternative science is more intellectually interesting, the work is more exciting, and the results are more useful.

Implementing its results in ideologically charged territory like the U.S. drug field has always been a problem, because such fields then have to give up control over what they think they already knew and change as a result. But that's a topic for another day.

Back to the Library

This article reproduces a very old argument in a recent and specific context. The fact that the old argument still requires a "special issue" rather than being a normal part of any human/social research journal is testimony to the hegemony of an epistemology with an important but limited range of application in our efforts to understand human social life. The conversation about different epistemologies is easier to have now than it was forty years ago, no question about that. But even it misses the heart of it all, in my view. "Qualitative" and "quantitative," the form that the conversation often takes, leads us astray because the only clear meaning of the terms is a kind of data—numerical and propositional—and both kinds of data are relevant to any human/social science epistemology. The phrase "mixed methods," popular in recent times, seems vacuous to me. At one level, the message of using more than one source of information to explore a research question sounds too obvious to be anything more than a baby step forward. On the other hand, the phrase often translates—in academic and organizational projects I have worked on in the last decade—to "add in a focus group to support the real science."

An advance will come when education incorporates a curriculum that shows how science in general, and human/social science in particular in this case, has a history of many varieties, that "science" means more than a laboratory experiment to test a prior hypothesis. Perhaps that education has already become part of K-12, where it needs to begin. Still, though, in recent projects I have done in the so-called real world, people who invited me in because "the numbers"

were neither identifying nor solving their organizational problem were fascinated with and skeptical of what I was doing. They had no idea that there was an alternative way of doing a human/social science that could produce a claim based on evidence and logic that could be challenged.

In many ways the situation is the fault of those of us who pursue the alternative epistemology. Sometimes it has been presented as "anti-science" rather than "different science." In my day in university we only heard the 19th century advocates of a human/social science as footnotes. We were sent off to take a statistics course in the psyc or soc department with no context for what it was, when it might be useful, and why it didn't serve the epistemology as well as other forms of mathematics might.

I hope this article is part of a final push to get past this two-century history of privileging only one epistemology as the way to do what we usually call behavioral/social science. I hope the day comes soon when any human/social science journal will contain articles of many types, based only on whether or not they contain a well-crafted argument that can be challenged, rather than based on the degree to which the research imitates an experiment in a natural science laboratory.

It's just Science 101. When the phenomenon of research are human social worlds, then the characteristics of the phenomenon have to be part of the science. Human social worlds—the argument returns us to the 19th century debate—have among their characteristics intentionality, lived experience, and biographies interacting with histories. And in this kind of science, the research itself is a human social world, because the researcher is also an example of the phenomenon. Absent those characteristics, human social research will inadequately describe and, therefore, poorly explain who we are and why we do what we do. Sad to say, most mainstream

behavioral/social science chose to leave out those critical characteristics in the rush to emulate the revolution in science brought about by Galileo, Newton, and what followed in the European Enlightenment. The science they created was and is fascinating and useful, but it lacks the epistemological horsepower to adequately handle human social phenomena.

Let me close with a recent story to end on a positive note. Though I left the drug field in the mid-2000s, I was invited to attend a conference in El Paso/Juarez in September of 2009. Those two cities decided to team up and hold a conference "celebrating" forty years of failure of the War on Drugs. They recruited all the old-timers they could find from research, law enforcement, policy, and treatment to come and say, over and over again, how the "war" didn't just miss the point; it caused more problems than it solved, and we needed new ways to think about drug issues. Media coverage was great, that being the main purpose. I received a compliment from the *Economist* blog when it concluded that my presentation had argued to "let the world be your laboratory" (http://www.economist.com/blogs/democracyinamerica/2009/09/the_folk_pathways_of_preventio). But reading it was the definition of mixed emotions. On the one hand, it was an honor to hear that summary after all these years. On the other, he still had to use "laboratory" as the only possible metaphor that science was in play. The conference, though, was a breath of fresh air, age and experience saying we got it wrong, one voice after another saying, the world is telling us so, so let's listen and learn and maybe a new generation of researchers can get it right next time.

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