‘You Can’t Fool the P Test’: American Science, the Department of Defense, and the Unlikely Invention of the War on Drugs, 1945-1980

By

Justin Wade Hubbard

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Approved:

Arleen M. Tuchman, Ph.D.

Sarah Igo, Ph.D.

Laura Stark, Ph.D.

To my wife, Dr. Nona Lu,

And our lap warmer,

And the 2017 Philadelphia Eagles. Go Birds.

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List of Abbreviations

A
Aberdeen Proving Ground (APG)
Absent Without Leave (AWOL)
Advance Research Project Agency (ARPA; sometimes DARPA)
Adverse Drug Reactions (ADR)
Air Force Base (AFB)
American Medical Association (AMA)
Armed Forces Institute of Pathology (AFIP)
Army Community Services (ACS)
Awareness Counselors (AC)

C
Canine Performance Sciences (CPS)
Civil Operations and Revolutionary Development Support (CORDS)
Community Action Program (CAP)
Continental Command, United States (CONUS)

Criminal Intelligence Division (CID)
Cumberland County Mental Health Center (CCMHC)

D
Department of Defense (DOD)
Drug and Alcohol Abuse Control Program (DAACP)
Drug Abuse and Rehabilitation Team (DART)
Drug Enforcement Administration (DEA)

F
Federal Bureau of Narcotics/Bureau of Narcotics and Dangerous Drugs (FBN)
Free-Radical Assay Test (FRAT)

G
Gas-Liquid Chromatography (GLC)

H
Hamlet Evaluation System (HES)

I
Institute of Medicine (IOM)

J
Journal of the American Medical Association (JAMA)

I
Interagency Bureau of Narcotics (IBN)

L
Law Enforcement Administration Act (LEAA)
Limited Warfare Laboratory (LWL)

M
Military Assistance Command, Vietnam (MACV)
Military Police (MP)
Military Working Dog Center (MWDC)
Monell Chemical Senses Center (MCSC)

N
National Institute of Drug Abuse (NIDA)
National Institutes of Health (NIH)
National Institute of Mental Health (NIMH)
National Science Foundation (NSF)

O
Operation Awareness (OA)
Operation Golden Flow (OGF)
Operation JANUS (OJ)

P
Project THEMIS (PT)
Psychological Operations (PSYOPS)

R
Registry of Tissue Reactions to Drugs (RTRD)
Republic of Vietnam (RVN)

S
Southwest Research Institute (SWRI)
State Bureau of Investigation (SBI)

T
Thin-Layer Chromatography (TLC)

U
United States Public Health Service (USPHS)

V
Veterans' Administration (VA)

W
Walter Reed Army Institute of Research (WRAIR)
Working Dog Center (WDC)

Introduction

A Cup of Pessimism

I've had this patch on my desk for three years now (Illustration 1). The patch is a "Friday patch," a satirical type of military patch that soldiers create and display on more relaxed days in uniform. On the patch are an unauthorized Snoopy in flight goggles and his famous red cape, with a syringe sticking through one of his arms. He's actively urinating into a black receptacle, the stream providing a needed flash of yellow to the patch. Around him are the words "GOLDEN FLOW" and "U-TAPAO THAILAND" — Golden Flow (OGF) being a pejorative nickname for the first drug tests performed on soldiers beginning in 1971, and U-Tapao being a US Air Force (USAF) base in Thailand. None of the details, ostensibly, are random.

Given what we now know, the significance of the event that soldiers commemorated with the patch was not missed at the time. Golden Flow was a gross invasion of privacy, a minor inconvenience, a technological marvel of man, machine, and a lot of body fluids, the first step in the elimination of addiction, the least effective way to help addicts, a model for civilian society, a subject of legal battles in the military's highest court, the product of years of research in toxicology and forensics, drastically understaffed and under-resourced, a sop to chemical-analysis corporations and contractors, the only way to identify drug users, an abject lesson in the limited rights of American soldiers, and the basis for a Friday patch. It was all of these things. And, yet, what appears on the patch are a needle, a bucket, and an arc of urea.

Illustration 1

Miscellaneous Operation Golden Flow patches. Source: http://www.usmilitariaforum.com/forums/index.php?/topic/125509-operation-golden-flow-udorn-thailand/, accessed May 2017.

What follows is a history of how Americans in the armed forces and civilian society responded to a disease in their own time — that of heroin addiction among American soldiers — using the medicine and sciences at hand, inventing others, and only sometimes coming out on the other end successfully. It's a history of technology, drug control, and the technology of drug control, and the limits of faith in science and medicine to resolve social conflicts and improve social conditions. It's a history that provides new insights for how we prepare solutions for opioid abuse in our own age. And, it's a history that explains why soldiers commemorated their routine deposits of urine into plastic cups with a flimsy cotton patch.

The Army Goes Reefer Mad: The Origins of Drug War in Vietnam?

The Army's decision to initiate OGF comprised one piece in a series of interconnected anti-narcotics surveillance, education, and treatment options developed alongside plans at the federal level to remedy heroin abuse. These programs joined in 1971 in the Army as the Drug and Alcohol Abuse Control Program (DAACP). The Army and other service branches' responses to drug abuse before and especially after 1971 have led scholars to depict DAACP and OGF as precursors to and influences on the Nixon administration's evolving positions on policing and treatment, as well as the basis for the militarization of drug enforcement during and after Vietnam. One scholar goes so far as to claim that the Vietnam War and the Army's reactions to drug abuse in that context predicted the strategies, alliances, and motivations of the War on

Drugs.[1] Setting aside the accuracy of that claim, scholars nonetheless ascribe a great deal of significance for the evolution of crime control strategies, the availability of methadone therapy, the massive jump in the US prison population, and the emergence of left-, right-, and consensus-"law-and-order" politics to how the Army arrived at drug control, and how military technology made its way into civilian hands for the purposes of drug enforcement in the same period.

A number of historians have described how OGF flowed from the Nixon administration's evolving drug-control policy positions. Nixon, at least during his first term, proposed joining improved treatment options and more expansive enforcement measures, the carrot and the stick. Earlier federal actions, including the Narcotic Addict Rehabilitation Act in 1966, predisposed federal policy toward emphasizing treatment for narcotics abuse, but Nixon displayed a vigor for establishing medical alternatives to just policing away drug abuse.[2] Once in the White House,

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[1] Jeremy Kuzmarov, *The Myth of the Addicted Army: Vietnam and the Modern War on Drugs* (Amherst: University of Massachusetts Press, 2006). Kuzmarov is unique here, as he provides a convincing account of how even anti-war activists exploited the issue of addicted soldiers to leverage its anti-war claims. Kuzmarov has also published an essay with the titular claim the transition from Vietnam to an ostensibly "post-war" period after 1975 was smoothed with the application of war materiel to counter-narcotics campaigns. See Kuzmarov, "From Counter-Insurgency to Narco-Insurgency: Vietnam and the International War on Drugs", *Journal of Policy History* 20, no. 3, (Jul. 2008), 344-378. David Musto describes the importance of urine testing to Dr. Jerome Jaffe, the first "drug czar" in the US. According to Musto, Jaffe believed that urine testing might "eventually be considered in the same light as chest X-rays for tuberculosis." See Musto, *The American Disease: Origins of Narcotic Control* 3rd edition, (New York: Oxford University Press, 1987), 253. Kathleen Frydl describes soldiers' returns from Vietnam thusly: "Soldiers who fought in one war came home to another." Frydl, *The Drug Wars in America, 1940-1973* (New York: Cambridge University Press, 2013), 412.

[2] David Musto and Pamela Korsmeyer are the best resource for understanding Nixon and his advisors' confusing policy positions as they evolved between his first and second terms. See Musto and Korsmeyer, *The Quest for Drug Control: Politics and Federal Policy in a Period of Increasing Substance Abuse, 1963-1981* (New Haven: Yale University Press, 2002).

Nixon organized the Special Action Office for Drug Abuse Prevention (SAODAP), installing Dr. Jerome Jaffe, a proponent of methadone therapy, to lead the agency. By 1973, Jaffe oversaw the creation of a decentralized network of methadone clinics across the US, and Nixon called to life the Drug Enforcement Administration (DEA) to replace the Bureau of Narcotics and Dangerous Drugs (BNDD). He also bolstered efforts to manage border crossings, most notably in 1969 in Operation Intercept, a temporary border closing and drug sting at the US-Mexico border near San Diego and Tijuana.[3] The early period in Nixon's anti-drug strategies, then, emphasized establishing medical approaches to drug abuse, as well as providing space to expand police actions.[4] Still, before there was methadone and the DEA, there was Vietnam and the Army.

The Vietnam War posed a special problem and opportunity for the Nixon administration vis a vis drug abuse. Due to a combination of opportunistic reporting on soldiers' drug use in country, ie in Vietnam, public dissatisfaction with the Vietnam War, and Nixon's own campaign promises to restore order to US cities after the summer riots of the mid- to late 1960s, his administration instructed military leaders in summer 1971 to address drug abuse amongst members of the Armed Forces.[5]

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[3] On Operation Intercept, see Aileen Teague, "Mexico's Dirty War on Drugs: Source Control and Dissidence in Drug Enforcement", *The Social History of Alcohol and Drugs* 33, no 1 (Spring 2019), 63-87. Teague provides an interesting window, too, onto the anti-drug technology that the US donated to Mexican agents and their military.

[4] Policing and treating are, of course, inadequate words to describe the Nixon administration's confusing relationship to heroin, drug treatment, and drug enforcement. Kuzmarov's focus on the anti-war left's support for the "myth of the addicted soldier" is an interesting take, insofar as it places Nixon's thinking within the context of political showboating.

[5] On opportunistic reporting, see David Musto and Pamela Korsmeyer, *The Quest for Drug Control*, 50; Jeremy Kuzmarov, *Myth*; Kathleen Frydl, *The Drug Wars in America*, 396-397. On the relationship between Nixon, crime, and methadone, see Eric Schneider, *Smack: Heroin and the American City* (Philadelphia: University of Pennsylvania Press, 2008), 165-170.

There were complicating factors, of course, that propelled Nixon's thinking. Novel dangers to soldiers existed in South Vietnam when it came to heroin, namely the country's close proximity to the Golden Triangle, an area in Southeast Asia where a goodly chunk of the world's opium grew, and the relative purity of the heroin available, less cut and more potent than the heroin then for sale in the United States.[6] There were also political factors, as a Senate subcommittee led by Democratic senators had begun hearings as early as 1970 on drug abuse in the services. Following the hearings, and on Nixon's instructions, the Pentagon devised a series of interlocking programs, including providing access to methadone detox services, establishing an amnesty policy for soldiers to seek treatment without punishment, and, most visibly, the installation of mandatory urine tests. The mandatory urinalyses were, to date, the most aggressive and wide-ranging drug-abuse technique that the services writ large adopted.

The Nixon administration's fingerprints were all over the Army's program. Historian David Musto explains that Golden Flow was "the first major task undertaken by SAODAP," a task that resulted from "executive order."[7] Another historian, Kathleen Frydl, calls Jaffe's work helping to coordinate OGF as "his primary contribution to the crisis in Vietnam" prior to his work overseeing methadone clinics in the US.[8] For both historians, as well as for others, Nixon

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[6] On the purity of heroin, and the relationship between the war in Vietnam and the Golden Triangle, see Alfred McCoy, *The Politics of Heroin: CIA Complicity in the Global Drug Trade* Second Revised Edition, (New York: Lawrence Hill Books, 2003). See also, Frydl, *The Drug Wars in America.* Frydl claims that as much as one-fourth to one-third of heroin in the United States in 1972 came from the Golden Triangle, 389-390, 410. Schneider, *Smack*, 160-162.

[7] Musto and Korsmeyer, *The Quest for Drug Control*, 98. On the Department of Defense and drug control under Nixon, see 48-53, 98-11.

[8] Frydl, *The Drug Wars in America*, 404.

ordered the creation of the program, Jaffe administered it, and slowly but surely, Frdyl concludes, "the system of urinalysis testing was perfected."[9]

Similarly, historians have demonstrated the role that American anti-narcotics agencies and anti-drug laws have played in extending the scope of drug control before and after Vietnam. Namely, they have pointed to the Federal Bureau of Narcotics (FBN) and its successor organizations, BNDD and the DEA, related agencies including the Law Enforcement Assistance Administration (LEAA), and new laws such as the Law Enforcement Assistance Act of 1965 and the Omnibus Crime Control Act of 1968.[10] These policies and agencies were responsible, in large part, for connecting the military to civilian policing institutions at the end of the war.

In a sense, then, narcotics provided a way to act together on the world for military and civilian policing agencies to act together on the world, and to think of drug use as both a matter of law and order and national security. Existing work especially helps to make sense of the federal government's extra-territorial, war-related activities in drug control, and the Army's role in the

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[9] Ibid. This is not to say that historians have explained the War on Drugs, the period after 1970 or thereabouts, and the emergence of mass incarceration as solely emanating from Nixon. Indeed, more recent work has charted the left-, right-, and consensus-politics that slowly accrued around drugs and mass incarceration together. For example, on incarceration and land value in the Sunbelt, see Ruth Wilson Gilmore, *Golden Gulag: Prisons, Surplus, Crisis, and Opposition in Globalizing California* (Berkeley: University of California Press, 2007); and Alex Lichtenstein, "Flocatex and the Fiscal Limits of Mass Incarceration: Toward a New Political Economy of the Postwar Carceral State", *Journal of American History* 102, no. 1 (2015), 113-125. On drugs and class politics among Black Los Angelenos, see Donna Murch, "Crack in Los Angeles: Crisis, Militarization, and Black Response to the Late Twentieth-Century War on Drugs", *Journal of American History* 102, no. 1 (Jun. 2015), 162-173. On consumer politics and drug control, see David Herzberg, "Entitled to Addiction?: Pharmaceuticals, Race, and America's First Drug War", *Bulletin of the History of Medicine* 91, no. 3 (Fall 2007), 586-623; and, Matthew R. June, "Protecting Some and Policing Others: Federal Pharmaceutical Regulation and the Foundation of the War on Drugs", Ph.D. dissertation, Northwestern University, (2018).

[10] Frydl, *Drug Wars*. Musto and Korsmeyer, *The Quest for Drug Control*.

same prior to 1971. As early as 1954, with the French surrender to the Viet Minh, FBN agents established posts in Vietnam, especially as it related to source control in the Golden Triangle. During the Vietnam War, agents flew with military pilots on cannabis-eradication sorties, using helicopters to identify and later destroy suspect weeds.[11] BNDD agents trained military and civilian police in drug identification courses in the late 1960s.

Following the war, the military began a fruitful exchange with civilian police forces, adding its materiel, sometimes for the purposes of drug control and sometimes for other purposes. Boosted by the LEAA, the military lent weaponry to civilian agencies. In one of its more concrete manifestations, the Bureau of Customs and the BNDD purchased their first drug-sniffing dogs of the 1970s from Air Force dog-trainers, while employers initiated their own mandatory drug-testing programs.[12] Oddly, scholars have not categorized the proliferation of testing programs as an instance of the militarization of drug control.

When scholars have looked to the science and technology put to use in drug control, they have similarly illustrated the manifold ways that military hardware ended up in the hands of civilian police for the purposes of drug control. One historian, Daniel Weimer, has detailed how federal

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[11] On the helicopter and crop eradication programs, see Kuzmarov, *Myth*. Frydl, *The Drug Wars*. The archival records of the program drive home their descriptions of the program's short, disagreeable existence, as Military Assistance Command, Vietnam sources consistently attest to complaints by military staffers about the manpower and logistical burden of running anti-drug sorties.

[12] This is not to say that these were the first such dogs. In fact, the FBN created a dope-sniffer program in 1946, only to disband it sometime before 1960. Unfortunately, this story exceeds the scope of this introduction and dissertation. On the FBN's first dog program, their story has yet to be written. However, it can be found in RG 472 Records of the United States Forces in Southeast Asia Headquarters, Military Assistance Command Vietnam (MACV) Provost Marshal Office of the Provost Marshal, General Records 1970 Box 2, File 0550-11 "Use of Dogs", Box 147 Entry A19 1940-1949, 1950-1966, Subject Files 1916-1970.

anti-drug agents helped to transport Agent Orange, an herbicide, from Vietnam to Mexico; first being used to expose Viet Cong in the jungle, and then, to eliminate the crops of Mexican poppy and cannabis growers.[13]

The big takeaways from existing histories are obvious. First, because scholars emphasize policy-makers and policing interests, they tend to portray OGF and DAACP as extensions of the the same actors, and pursuits of the same interests, largely some combination of Nixon and the BNDD. Second, the ethics are always already a compromised practice in scientific innovation — that local police forces and the DOD successfully transformed drug control into war.

This is about where the Army's story in existing histories of drug control begins and ends. However, this dissertation makes three claims. First, the military's role in influencing and predicting new trends in drug control goes much earlier and deeper than "Vietnam, 1971," as others have supposed, and that interest was also rooted in a midcentury optimism about the potential of defense research to improve the military and American society writ large. Second, the solutions that the military created and the technologies that its members deployed shaped the imaginations of anti-drug proponents about the boundaries of what was possible or justifiable vis a vis drug control *and* science, and imposed limitations on the same in the years during and following the Vietnam War. Third, despite the promise of new technologies to expand drug war and disparate cases of success therein, social, political, and technical conflicts constantly

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[13] Daniel Weimer, "The Politics of Contamination: Herbicides, Drug Control, and Environmental Law", *Diplomatic History*, 41, no. 5, (Nov. 2017), 847-873. On social science, narcotics, and US interventions in extra-territorial contexts, see also Daniel Weimer, *Seeing Drugs: Modernization, Counterinsurgency, and U.S. Narcotics Control in the Third World, 1969-1976*, (Kent: The Kent State University Press, 2011).

undercut their use, leaving the record of the military's influence on drug control more technically, ethically, and politically ambiguous than we have supposed.

The Cold-War Roots of Vietnam Drug Policy: The Army's Early Interest in the Problems of Narcotics; or, the Army and Narcotics before and after 1971

Where did the drug tests come from? What about the dope-sniffing dogs? The methadone? Surveys? How did the Army come to deploy them, ostensibly, to eliminate drug abuse? How did their inventors and the institutions responsible for producing them imagine their contributions to drug control vis a vis defense research? Just asking these questions suggests a slew of alternative timelines and interpretations for how the Army arrived at drug control, and the consequences of that arrival. In a sense, the War on Drugs was a war of expertise.

This is to say that the technologies that the Army invented and/or implemented to solve the riddles of heroin abuse — drug tests, drug-sniffer dogs, methadone therapy and behavioral conditioning, drug-use surveys — evolved from an array of factors that were unique to the mid-century defense establishment in the US. These factors predisposed the DOD and the Army to take an early interest in the multiple problems associated with narcotics.

Perhaps the most decisive of those factors was the military's post-World War II mandate to fund basic and applied research at the least, or create its own laboratories to stimulate research in both the physical and social sciences. In order to invite scientific innovation, the Department of Defense (DOD) established new research sites, created semi-private, university-affiliated research agencies, including the Human Resources and Research Office (HumRRO) and directly

funded private laboratories.[14] Similarly, researchers and corporate researchers also cultivated DOD contacts, rather than the DOD just picking scientists randomly.[15] The invention of new research institutions, and the fertilizing of existing ones prepared scientists for a new world after World War II.

The DOD responded to new war situations in the Cold War by developing still other research centers. For instance, the Army initiated the Limited Warfare Laboratory (LWL) in 1962 to address guerrilla combatants, and it established the Psychological Operations Division (PSYOPS) in 1952, a program that culled social-scientific research to improve disinformation and propaganda campaigns. It promoted an existing institution, the Armed Forces Institute of Pathology (AFIP), into a designated research site on questions of pathology and toxicology. These sites elevated the application of expertise to American military strategy, and asserted the new role of experts in the postwar military.

One of the upshots of this assumed mandate was that it elevated a belief among the military establishment in the ability of scientists — directly employed by the Army or DOD, or privately employed by a university or corporation — and empirical research to improve the way that the Army prosecuted war. By the time that the United States entered into a more sustained and expanded role in Vietnam after 1966, the Army devised the Hamlet Evaluation System (HES), a

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[14] On the networks of research, see Joy Rohde, *Armed with Expertise: The Militarization of American Social Research during the Cold War*, (Ithaca: Cornell University Press, 2013). On the Cold War and academic research, see Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford*, (New York: Columbia University Press, 1993).

[15] Rohde is great on this point, about how researchers sought out and nurtured DOD contacts. Rohde, *Armed with Expertise*. See also, Mark Solovey, *Shaky Foundations: The Politics-Patronage-Social Science Nexus in Cold-War America*, (New Brunswick: Rutgers University Press, 2013).

series of on-going surveys by PSYOPS and PSYOPS-adjacent groups to assess morale amongst occupied Vietnamese citizens.[16] They experimented with bedbugs at LWL to locate and alert at the sight of enemy troops. Or, as one academic researcher described the relationship between his lab work on dog learning, defensive needs, and practical benefit, the partnership between DOD funders and non-DOD researchers was helping to make, in his case, psychology into a science on par with chemical engineering. The DOD and researchers mutually benefitted one another, as the relationship kept basic research in close proximity to application. As he described it, "contact among the persons working on various aspects of a common problem provides for a great deal of corrective feedback and thus prevents us from drifting too far from the main track."[17] Together, programs including HES and agencies including LWL contributed to a belief amongst researchers in the ability of defense research to improve science and American society writ large. In effect, they created an alternative rubric for assessing the value of defense research.

Among the beneficiaries of the Army's largesse here were university and corporate social scientists. As Joy Rohde and others have demonstrated, the DOD and social scientists faithfully pursued one another after World War II.[18] This was as in the 1960s and 1970s as it had been in the 1950s, as social scientists continued to jockey for funding from the Office of Naval Research,

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[16] On the Hamlet Evalation System, see Dave Young, "Computing War Narratives: The Hamlet Evaluation System", *A Peer-Reviewed Journal About* 6, no. 1, (2017), www.aprja.net.
[17] Robert Berryman, "Towards a Training Technology", in *Conference on Research to Expand the Usefulness of the Military Working Dog*, (Lackland AFB: Air Force Office of Scientific Research, Nov. 1970), 69-77, 71.
[18] Rohde, *Armed with Expertise* and Solovey, *Shaky Foundations*. See also, Mark Solovey and Hamilton Cravens, eds., *Cold War Social Science: Knowledge Production, Liberal Democracy, and Human Nature*, (New York: Palgrave Macmillan, 2012).

the Air Force Office of Scientific Research, and the Army's Research and Development Division. The work of academic psychologist Robert Lubow and his research firm Behavior Systems, Inc., projects, including that of training dogs to perform higher-order scent tracking and scent-discrimination work, brought researchers and the defense department together, each finding mutual satisfactions in the bargain.

Another upshot, was that the mandate tagged the constantly expanding portfolio of military research to other types of goals and motivations. For the executive branch, scientific research became a means of improving the ways that the federal government doled out money to universities, as when the administration of Lyndon Johnson introduced Project THEMIS, a policy to redistribute funds to less-rewarded schools. When other researchers, including Robert Berryman and David Moulton, began their own dog projects, they worked on THEMIS money. These contracts eventually metamorphosed into the Army's first drug-sniffing dogs. Similarly, both LWL and AFIP's missions, according to their directors, were to stimulate industry and encourage innovation in defense research.[19] This more or less framed the way the DOD, researchers, and others contemplated the complex of interests involved — believing that scientific progress and federal subsidies were intertwined.

The commitments that the Army assumed in the years following World War II established an early, much more direct interest in various problems related to narcotics, too. For instance, Henry Beecher, the famed Harvard physician, experimented with methadone, using it as a

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[19] On LWL and industry, see J.E. Mortland, M. Cutler, and E.K. Kaprelian, "Final Project Report — U.S. Army Land Warfare Laboratory Volume 1. Project Report, Appendix A. Documentation & Appendix B. Task Sheets", (National Technical Information Service: Jun. 1974), 1. On AFIP and industry, see Elbert DeCoursey, "Armed Forces Institute of Pathology", *Science* 120, No. 3107, (Jul. 16, 1954), 11A.

painkiller on troops injured during the Korean War.[20] In a more sustained way, researchers at AFIP, including Leo Goldbaum, worked on a process to identify drugs in biological material using gas chromatography. Beginning in 1964, his colleagues built up a database of drug deaths, the Registry of Tissue Reactions to Drugs (RTRD), using dead soldiers as the raw material. Goldbaum was eventually appointed to test run the urinalysis program that went on to become OGF, using the method that he had been devising since the early 1950s. As OGF matured, it also fulfilled the promise of AFIP stimulating industry, as its researchers coordinated testing activities with private labs. In this sense, Goldbaum and AFIP's analytical and engineering work set the imaginations for what was possible in drug control, suggesting how anti-drug technology might shape Nixon's understanding of a viable intervention, rather than Nixon being solely responsible for dictating the politically and technically feasible.

This interpretation of the Army's long-term interests in drug control suggests that various actors within and adjacent to the military utilized diverse research projects to achieve disparate ends, and that they shared an optimism that scientific advance could yield answers to society's messy problems. Further, this interpretation suggests that the Army's interest in drugs was far from sudden, and far from dependent on, Vietnam and 1971. It also suggests something deeper about militarization. The broad mandate that the DOD assumed after World War II prepared members to develop technologies without immediate value as an ends to stimulate industry, to

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[20] Author Unknown, "Methadone: Army Develops Perfect Substitute for Morphine", *Delaware State Medical Journal* 23, (1951), 20, 23. See also, HK Beecher, et al., "Field use of methadone and levo-iso-methadone in a combat zone (Hamhung- Hungnam, North Korea)", *US Armed Forces Medical Journal* 2, No. 9. (1951), 1269-1276.

advance medical and scientific know-how, and to participate in American society as a laboratory for social and scientific improvement.

The World the Machines Made: Defense Technology and the Promises and Limits of Drug Control

The question must arise in any history of technology, as to how well the tools fared. In a colloquial sense, the technology that the DOD and the Army used to prosecute a drug war came with all the baggage, good and bad, that had produced them. And, there were opportunities, technical, institutional and otherwise. With new drug tests, or new prescriptions for methadone, new opportunities for the expansion of drug control came about by first and foremost promising to improve surveillance and treatment capabilities, and second, by establishing new criteria to assess the utility of those tools.

One of the most hopeful prospects of DAACP was that the military might provide treatment that was otherwise unavailable to young, American men. At Fort Bragg, North Carolina, the commanding officer secretly initiated methadone therapy — a program called Operation Awareness (OA) — on the off-chance that he and his staff could make an empirical dent in what they viewed as the unprecedented problem of heroin addiction. By 1972, the Veterans' Administration (VA) medical system assumed control over drug treatment, and officially made methadone available to soldiers for the purposes of detoxing from heroin.[21] To put that in perspective, it had only been since 1964 that physicians Vincent Dole and Marie Nyswander had

[21] This is not a defense of the Army's treatment programs. As I explain later, the programs at individual Posts and at the VA remained chronically understaffed, insufficiently resourced, and often not much different from an actual punishment.

begun to experiment with methadone-assisted therapy in New York, the therapy and its locations being still radical and controversial at the time.[22] These programs, more often than not, though languished to the side while surveillance-oriented programs flourished.[23] Still, the significance here is rich: the military pushed methadone therapies before they were widely available to non-uniformed Americans.

Depending upon your perspective, the most successful programs were those that set out to demilitarize weaponry for the purposes of drug control after Vietnam, and those that had envisioned broader benefits from scientific subsidies. AFIP, the organization behind the planning of OGF, continued to act on drug control, offering its services as a quality control lab for the private labs that continued to take contracts to test urine samples. Still, in the field, as this dissertation shows, the process of testing was littered with staffing issues, logistical failures, and easy means of cheating, leading to a pockmarked control system.

The drug-sniffing dogs were a different, albeit related story. Whereas the drug-dog program had grown up from, in part, the research done by academic and corporate researchers, its planners were victims of the postwar rejection of DOD-funded research.[24] Still, their research firms, as in the case of Robert Lubow's Behavior Systems, Inc., continued after the federal gravy train went off the tracks. Relatedly, agents and federal narcs alike bought their dope dogs from the same people making them for the services at Lackland AFB.

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[22] On methadone, Nyswander, and Dole, see Musto and Korsmeyer, *The Quest for Drug Control*.
[23] On the poor performance of drug-treatment options for soldiers, see Schneider, *Smack*.
[24] On negative reactions to university researchers after Vietnam, see Leslie, *The Cold War*. See also, Thomas Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870-1970* (New York: Viking, 1989).

The research firms continued, too, as the Army haphazardly gelled its anti-narcotics activities, as did the former federal contract researchers, including HumRRO. Whereas the military cut off HumRRO and other federal contract research centers (FCRC) in 1969 and 1970, the centers continued on as independent research outfits still available for defense contracts.[25] Beginning in 1971, HumRRO began the first of what became annual Drug-Use Surveys, large questionnaires that soldiers completed on the auspices of HumRRO finding and identifying drug-use trends on which military administrators could act. At the same time, these surveys often found themselves in the dustbin, rather than on the planning table. Their utility in person-to-person interventions was consistently ambiguous.

There were other casualties to be sure. First, there was the cancellation of two programs that had propped up the Army's anti-drug efforts, Project THEMIS and the Limited Warfare Laboratory. Having been defensive research programs, both projects' tenures were not long for the world once anti-war opponents successfully challenged their existence at the end of Vietnam, providing tools do so with the so-called Mansfield Amendments.

The treatment options available to soldiers repeatedly failed, often for lack of interest or finances or otherwise support from the military. What this dissertation shows, though, is how difficult success in these programs may have been. Rather than view them as evidence of malfeasance or disinterest, this dissertation raises questions about the substance of addiction as a diagnosis and the barriers to operation that emerged as administrators and commanders attempted to transport addiction into the frame of a disease.

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[25] On FCRCs and the end of Vietnam, see Rohde, *Armed with Expertise*, especially 116-147.

The bittersweet, and often chaotic endings raise uncomfortable questions about the transportation of the detector dogs from counter-insurgency to narcotics enforcement. PSYOPS found a new application for its talents in OA, crafting drug-use surveys, as did the federal contractors responsible for surveying soldiers in ever-larger numbers. This reinforces Kuzmarov's claim that the Army helped to navigate counter-insurgency into the practices of narcotics-control.

While the Army and the Department of Defense envisioned a world more sober through science, the technology developed arrived chaotically, unexpectedly, at the same time that social, political, and technical issues constantly undercut those technologies.

This brings us back to the Operation Golden Flow patch. The drug test was devised in military labs, tested on soldiers, turned on those same soldiers afterwards, and adopted by the private sector as a means of both turning chemistry into industry, and solving a looming public health crisis. What follows is the first history of drug control told as a history of its distinctive technologies.

Roadmap

This dissertation is organized into four chapters and a conclusion.

In the first chapter, I examine the origins of Operation Golden Flow, by tracing the history of the gas chromatograph, the golden standard device for testing urine for traces of opiates, cannabinoids, and other substances. Here, I show how an experimental tool in analytical chemistry and corporate research labs made its way into toxicology and forensics labs on the

back of federal investments in pathology. I also show how under-resourced, under-manned, and over-relied upon the Army's testing program was.

In the second chapter, I explore the Army's first drug-sniffing dogs, looking at dog research at midcentury, and the investigators responsible for the training protocols. As I show, these dogs were the product of years of testing, executive provisions to more justly redistribute research funding around the United States, and from the DOD's mission to stimulate scientific industries in the postwar period. At the end, I discuss how the promising research field in detector-dog research evaporated suddenly, as critics of the Vietnam War successfully cut the purse strings on the research from which the dogs came.

In chapter three, I turn to Operation Awareness, an experimental heroin-treatment program at Fort Bragg, North Carolina. I show how different actors, from the Post's commander, Lieutenant General John Tolson, to the local anti-drug activist and confidante of Tolson, Pat Reese, used the Army's supposed responsibilities to soldiers and host communities to create the Army's first, a heroin treatment center. I also show how conflicts about the diagnosis of addiction, and mismanagement of the center led to its demise.

In the fourth and final chapter, I turn to a bouquet of information technologies — drug-use surveys, crime-data processing, and anti-drug education — and discuss the ways that these technologies evidenced a drug control program that was both novel and innovative, but also coming apart at the seams. These technologies, I argue, remained mired in non-technical, social conflicts that jeopardized their successful implementation.

The conclusion draws connections between the military's technological responses to drug abuse, and the technological responses to opioid misuse in our own time.

Chapter 1

Toxic Correlations: The Armed Forces Institute of Pathology and the Origins of Operation Golden Flow

Introduction: February 1971

Leo Goldbaum, toxicologist at the Armed Forces Institute of Pathology (AFIP), was no stranger to getting his hands dirty. In the weeks and months leading up to February 1971, Goldbaum and a team of investigators prepared to demonstrate the viability of mass drug-testing to an audience that included top military brass, as well as President Nixon and his cabinet. Arguably, this moment was years in the making, going back as far as the early 1950s, when Goldbaum first began publishing his research on chromatography and alkaloid identification. Then, as now, his work involved sifting through the body's effluvia — urine, bile, organ slurries — for evidence of drug use.

What went on to be known, derisively, as Operation Golden Flow (OGF) involved exacting preparations. With 2,500 specimens to collect for the test run, this trial was one of the largest single experiments ever of urine tested by chromatographic analysis, and the first to run on non-hospitalized or arrested GIs. The formal program that began six months later, in August, quickly dwarfed it, but this remained one of the largest experiments of its kind (Illustration 2).

Scholars interested in workplace drug testing have often pointed to Golden Flow as the beginning of workplace drug testing across the United States.[1] This is a story of how Leo

[1] On the Department of Defense's drug-testing efforts and the origins of American workplace drug testing, see for example, Ken D. Tunnel, *Pissing on Demand: Workplace Drug Testing and the Rise of the Detox Industry*, (New York: New York University Press: 2004), 1-20.

Goldbaum turned a test once unique to analytical chemistry into a weapon of mass surveillance. This is the story of the Army's decades-long romance with narcotics.

In this chapter, I examine the origins and use of drug testing in the Army. In order to explain the origins of Operation Golden Flow, the first mass-application of drug testing on a non-institutionalized population, I explore the people, practices, and institutions situated around OGF's principal means of detection, chromatography. How do we explain the use of chromatography as a pillar of Operation Golden Flow? Who was Leo Goldbaum, and what was AFIP's role, if any, in implementing the chromatographic method of drug analysis? What alternatives to chromatography existed for drug assays? And, finally, to what extent did Operation Golden Flow result from Goldbaum and AFIP's actions? It's the claim of this chapter that OGF was not a sudden departure from AFIP's growing authority in medical science, and that AFIP's work in narcotics research and pharmaceutical regulation since the early 1960s predisposed the Army and the DOD to see an easy fix for narcotics in Goldbaum's research on gas chromatography. Put differently, gas chromatography was a *reason* to test, not just a *way* to test.

Goldbaum's influence over the February 1971 experiment, and the broader system of drug testing in the armed forces is inestimable, but determinative. Arguably, and that is, in part, the content of this chapter, there would have been no Golden Flow sans Goldbaum. His work with thin-layer (TLC) and gas-liquid chromatography (GLC) pioneered a new field in drug detection that had, in civilian labs, more or less languished as a tool solely for analytical chemists. The two practices involve mixing a biological sample with a reagent that then separates compounds from a sample, allowing identification through eye tests and computer analysis, respectively.

Due to Goldbaum's research, GLC became, and to this day remains, the golden standard for drug testing.

Prior to Golden Flow, a handful of other methods existed. However, those alternatives were slow, often rudimentary, and suspected of being ineffective at either identifying users or keeping them clean. As Goldbaum worked on his chromatograph, he ran headlong into the existing means of drug testing — largely Nalline and the so-called "spot tests." Goldbaum's research on the detection of narcotics traces differed in substantial ways from Nalline and the spot tests. Three key improvements of Goldbaum's method were that it could, first, detect minute traces of multiple drugs in a single sample, second, reveal the exact amounts of narcotics present in a sample, and third, be performed in massive batches. Previous attempts at inventing drug-detection methods had focused solely on morphine and other opiates, and could only be performed on a few subjects at a time.

But, Golden Flow was not merely the consequence of Goldbaum's work in developing new drug-testing methods, nor was it the endpoint of his and others' work in the physiopathology of drug poisoning. To wit, AFIP — Goldbaum's employer — had steadily increased its role as a research body in American medicine following World War II. It was through AFIP's newly assumed role that Goldbaum was able to carry out years of research on chromatography, helping him to move chromatographs out of experimental laboratories and into potential clinical use. AFIP, then, offered Goldbaum resources and lab space, and Goldbaum offered them a dependable, prolific researcher.

Since its rebirth following World War II, AFIP became a means for the federal government and other parties to stitch together the supposed needs of American medical professionals, the

emerging pharmaceutical industry, and AFIP's own interests in becoming a leading medical authority in the United States, and, even, the world. As part of that unification project, in 1965, AFIP established and institutionalized a lab and registry dedicated to the problem of adverse drug reactions (ADR). Over the next ten years, the Registry of Tissue Reactions to Drugs (RTRD), a registry within AFIP inaugurated in 1965, acquired some 2,700 samples of tissue from cases of suspected drug poisoning. The registry provided Goldbaum and others at AFIP with the opportunity to test his methods of analysis on an even larger data set, and to refine his ability to identify and analyze all manner of drugs. It was, importantly, an early attempt at AFIP to wade into the world of pharmaceutical regulation.[2] RTRD was supposed to do the heavy lifting of elaborating on the physio-pharmacology of drugs, licit or otherwise.

And, that brings us back to the February 1971 trials that eventually transformed into Operation Golden Flow. Goldbaum's team planned the experiment throughout the winter months of 1970 and 1971. Their handwritten notes give a window into their hypotheses — from the number of technicians needed at each laboratory, to the desired chemical thresholds for identifying traces of coca, poppy, and cannabis, to the precise temperatures at which soldiers' urine specimens were to be refrigerated to prevent spoiling and faulty test results.[3] Each of these details, Goldbaum and his team averred, were vital to proving his method. In fact, these trials gave Goldbaum still more raw material for a publishing career, and a conviction that chromatography could live up to its potential.

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[2] On pharmaceutical regulation and its relationship to drug control at midcentury, see Herzberg, "Entitled to Addiction". June, "Protecting Some and Policing Others".

[3] Douglas Beach, "Determination of Barbiturates and Amphetamines", (Jun. 17, 1971), RG 319 —Records of the Army Staff—Internal Medicine Drug Abuse, Folders 19-25, Box. 4, Army Drug Detection Laboratory Plan Worldwide (w/Annexes A through G) Folder 20.

Despite the apparent success of the experiment, skepticism soon followed. Continental Army Posts contracted with private laboratories, while overseas Posts built their own labs, and AFIP took on a role in quality control. As AFIP relinquished direct control over testing, questions emerged about the viability of chromatography outside of supposedly apolitical labs, and the sometimes dubious, non-standardized procedures of private testing facilities. In the end, OGF set up a template for

Histories of the military's initial encounter with drug testing exist, and this chapter will not recycle their narratives. Instead, this chapter does something quite different — it looks at the longer history of drug testing in the military laboratories from which new chromatographic techniques emerged.

Previous scholars have investigated the program known as Operation Golden Flow. As they document, Golden Flow came out of discussions between President Nixon and his drug czar, Jerome Jaffe. Golden Flow, they claim, was a fairly sudden executive decision, a dramatic expression of Nixon's political calculus, even, perhaps, his id.[4] Those scholars have rightly described the psychological toll of testing on drug-using and non-drug-using soldiers, the political utility of drug testing for war hawks and the Nixon administration, the ways that drug-using soldiers successfully evaded identification, and the eventual consequences for civilians of the Army's decision to drug test all of its employees. Still, one historian has described the road

[4] In both Eric Schneider and Kathleen Frydl's versions of Operation Golden Flow, the shift to testing is abrupt. See Eric Schneider. *Smack*, especially 161-165. Kathleen Frydl. *The Drug Wars*, especially 388-410.

to testing as an afterthought — "the system of urinalysis testing was perfected — and this was no easy task."[5]

All of those points are important, valid, and accurate. But, they don't begin to scratch the surface of where Operation Golden Flow came from. Goldbaum's chromatographic method was not the only available drug assay at the time, and the centrality of chromatography to Golden Flow suggests that the program was far from sudden, and very much far from certain. Placed in context, Operation Golden Flow was another chapter in the decades-long evolution of toxicology on the basis of analytical chemistry, the transformation of AFIP into a research and consultation authority in American medicine, and the perceived utility of drug screens themselves.

Per those previous histories, a nearly alchemical equation explains the invention of a mass testing regime, and ignores the much more conflicted battles brewing related to the accuracy and utility of ever-newer toxicological methods, the influence of algorithms and pharmaceutical development on drug testing, and the years of research and consultation centralization that AFIP accomplished following World War II.

Put differently, this chapter shows how a near-sighted focus on Operation Golden Flow misses the far longer influence that the military and military-connected researchers exercised over drug testing. It illustrates the multiple competing modes of drug testing, and how concerns about the accuracy of drug testing persisted well into the period in which Golden Flow began to flush out drug-using soldiers.

If the history of OGF is somewhat opaque, the history of drug regulation is proportionately contentious. Historians have plumbed the origins of the postwar drug-regulation schemes of the

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[5] Frydl, *Drug Wars*, 404.

federal government — extracting stories of how physicians and pharmaceutical manufacturers neutered the potential of greater drug regulation.[6] This chapter exposes readers to how the Department of Defense helped to shape that system.

Relatedly, one historian, David Herzberg, has written convincingly about the relationships between licit and illicit drugs, and the differential modes of governmental regulation therein. Per his findings, Herzberg claims that the origins of a regulatory structure for licit substances developed in tandem with the policing mechanisms developed for illicit substances.[7] Or, stated differently, Herzberg argues that we can't address the supposedly regulatory nature of how the federal government intervenes in the pharmaceutical market, without reflecting on how policing agencies came to assume authority over illicit substances. In his telling, these are two sides of the same coin; related, but ultimately distinguished as regulation versus policing.

The history of chromatography vis a vis AFIP, RTRD, and OGF complicates his distinction. While RTRD developed as a means to study the more general problem of "adverse drug reactions," its research concerned opiates, antimalarials, and all manner of drugs in between. It concerned the interaction of those same drugs in the same body, ie, multi-drug toxicity when

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[6] Dominique Tobbell, *Pills, Power, and Policy: The Struggle for Drug Reform in Cold War America*, (Berkeley: University of California Press, 2011). Scott H. Podolsky, *Antibiotic Era: Reform, Resistance, and the Pursuit of a Rational Therapeutics*, (Baltimore: Johns Hopkins University Press, 2014). Arthur Daemmrich's work is especially relevant here, given the RTRD's role in evaluating "market-phase" drugs. Daemmrich, *Pharmacopolitics: Drug Regulation in the United States and Germany*, (Chapel Hill: University of North Carolina Press: 2005).

[7] Herzberg is the most convincing observer regarding the emergence of two kinds of state responses to drugs. However, he is not the only observer. Frydl has been explicit about what she perceives as the evolution of drug regulation from a regulatory/tax model into a policing model. Herzberg, "Entitled to Addiction". It should be noted that Herzberg is largely concerned with race and differential surveillance strategies. I take his work to be an important intervention in how historians have conceptualized both drug policing and regulation.

chloroquine and morphine joined together in one subject's liver. The methods of RTRD, too, especially with regards to toxicological confirmation, bear a direct link between the invention of a system for regulating licit substances and the policing of others. It was through the research of RTRD that a technique used mainly in analytical chemistry jumped into the world of drug control.

Further, there was no certainty that chromatography would? (might) become the predominant, nearly uncontested method for screening for illegal substances. As this chapter shows, alternative methods of drug testing co-existed with chromatography and more rudimentary spot tests well into the mid-1960s, only to steadily lose ground to the supposedly more accurate, more sophisticated chromatograph.

The distinctions between regulation and policing, licit and illicit drugs, and the methods utilized across contexts blurred under RTRD, and later in OGF. I take this blurring, then, as evidence that the distinctions between regulation and policing remained much murkier and for longer, and that in the 1960s, scientists had not received notice of a new period of differentiation.[8]

I have broken this chapter into four sections, organized largely chronologically. In section one, I detail how AFIP increased its presence in American medical research, and assumed new roles as a central medical authority, from 1950 until 1960. In the second section, I turn to Goldbaum's research in chromatography, and the competing methods of drug testing, from 1945 until 1970.

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[8] Caroline Jean Acker, "Planning and Serendipity in the Search for a Non-Addicting Opiate Analgesic", *The Inside Story of Medicines: A Symposium*, eds. Gregory Higby and Elaine Stroud, (St. Cloud: American Institute of the History of Pharmacy: 1997). Acker's call in this essay that historians more seriously acknowledge chance, luck, contingency, or, as she calls it, "serendipity," is well taken. In part, Acker's emphasis on serendipity reinforces the larger aims of this project around the uncertainty of the various scientific projects underway as part of DAACP.

In the third section, I examine the Registry of Tissue Reactions to Drugs, a period spanning from 1960 to 1975, and Goldbaum's role in the RTRD. In the fourth and final section, I go backwards a bit in order to reflect on February and August 1971, having contextualized Goldbaum's invention in a longer history of AFIP research and authority, and show how criticism and skepticism about the chromatographic method persisted.

"A True Academic Environment for the Future": The Armed Forces Institute of Pathology and the Organization of Medical Innovation in the Cold War, 1950-1960

The Armed Forces Institute of Pathology came about through military order in 1862, in the midst of the American Civil War. According to AFIP historian Robert S. Henry, Surgeon General William Hammond instructed Army physicians and surgeons in 1862 "to collect, and to forward…all specimens of morbid anatomy, surgical or medical, which may be regarded as valuable…in the study of military medicine or surgery."[9] Hammond's directive created the Army Medical Museum, what would become known as AFIP after World War II. The Museum's work at that time revolved largely around collecting and preserving pathological material from illnesses specific to military service, from bacterial infection to Minié-ball wounds. Those specimens ended up in the Museum, as a means of educating military doctors.[10]

As the nineteenth century dissolved into the twentieth, AFIP took to researching injuries that were, in some ways, more indirectly tied to warfare. During the Spanish-American War, AFIP helped to discover the transmission of yellow fever in Cuba, and aided in mosquito eradication. After World War I, AFIP established its first registry, a library of biological specimen which

[9] Robert S. Henry, *The Armed Forces Institute of Pathology: Its First Century, 1862-1962,* (Washington, D.C.: U.S. Government Printing Office, 1964). 12.
[10] Robert S. Henry, *The Armed Forces Institute in Pathology*.

constituent civilian members submitted to augment the Museum's holdings. The invention in 1921 of the civilian-military collaborative registry, the American Registry of Pathology, paralleled similar efforts in May 1922 when the Society of American Bacteriologists donated its bacterial cultures to the Museum.[11] Both efforts signaled the Museum's evolving roles in American medicine, and its stature relative to civilian practitioners and their professional organizations — becoming a site for not only education, but research.

Although the Museum tentatively moved in the direction as a research institution, it stalled partially in the interwar years. World War II was a lightning strike, and its consequences made AFIP's role in American medicine all the more urgent and manifest. The end of WWII precipitated the most substantial revisions to the scale and mission of AFIP. On the one hand, the Museum received an unprecedented number of specimens due to the troop buildup. But, more importantly, in the post-war period, AFIP merged resources with the Veterans Administration, Walter Reed Army Medical Center, the Atomic Energy Commission, the U.S. Public Health Service, and the Food and Drug Administration, among other working federal agencies, reorganizing as the Army Institute of Pathology. In 1947, the new Institute convened its first Scientific Advisory Board, which plotted out the Institute's emerging research priorities.[12]

With the construction of a new building in the early 1950s on the grounds of Walter Reed Army Hospital in Washington, some of the revisions to AFIP were more concrete. Some were actual concrete. One agency pamphlet bragged that the new building was "one of the first institutions

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[11] Henry, *Armed Forces*, 197-202.
[12] Henry, *Armed Forces*, 280-283.

in the Washington, D.C., area designed to resist an A-bomb attack."[13] The building itself was eight stories, and housed research labs, animal-holding rooms, a printing office — all enclosed in "steel-reinforced concrete blast-resistant walls." The building was supposed to bring to bear the latest in medical technology as well, utilizing pneumatic tubes to transmit specimens between labs, and closed-circuit television to communicate between labs, lecture halls, and operating rooms.[14] Throughout the spring of 1955 the Institute's activities shifted to the new building.

In the July 1954 issue of *Science*, a Brigadier General from the Army's Medical Corps and the second director of AFIP, Elbert DeCoursey, announced the other changes afoot at AFIP after World War II. According to DeCoursey, "civilian participation in its activities became increasingly significant," as non-military physicians contributed 10,000 cases a year. Similarly, the Institute was poised to meet the new civilian contributions of the postwar period, moving to a new research site on the Walter Reed campus, the Institute brought to bear on pathology "the conveniences of a larger, modern building, the best in laboratory equipment, an enthusiastic professional staff, supported by expert technical, clerical, and other workers."[15]

The changes in the Institute's methods also included new means for moving the biological stuff of examination. DeCoursey gushed that AFIP's consultative role was to offer "rapid and expert pathologic diagnosis wherever our soldiers, sailors, and airmen are stationed. It is accomplished by the expeditious handling within the AFIP, aided by modern air transportation and

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[13] "The Armed Forces Institute of Pathology", Pamphlet, in *Public Health Reports* 69, No. 2 (Feb. 1954), ii.
[14] "The Armed Forces Institute", Pamphlet.
[15] DeCoursey, "Armed Forces Institute of Pathology".

communication."[16] Such claims were only more evidence of how agents of AFIP imagined its role in postwar medicine, aided by the benefits of jet fuel.

Parallel to the improvement of office and laboratory space, the postwar period also saw an explosion in the Institute's holdings. Agents supplemented the initial American Registry of Pathology with other registries, and nearly doubled the available specimens for inspection. In 1955, the registry of pathological specimens consisted of twenty-two registries and 119,000 cases; by 1962, twenty-seven registries and 200,000 cases.[17]

With the changes to work space and the expansion of its holdings came more immaterial alterations to the scope and function of AFIP. First, AFIP became a premier site for postgraduate training in medical pathology. In part, the new communication devices available at AFIP not only met the new supposed needs of modern scientists, but broadcast a vision of medical leadership reflected in those devices — from the closed-circuit televisions that students could view for the purposes of pathology lectures, to newly installed classrooms and teaching laboratories. Their educational outreach also went global. According to one history of AFIP, in the first six years of residence at its new digs, 1,000 foreign students came to the Institute's new building for instruction in matters of pathology. Eventually, according to one source, the number of courses available at AFIP grew from nineteen to thirty-two between 1970 to 1975, with attendance at 3,127 students.[18]

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[16] DeCoursey, "AFIP".
[17] Henry, "Armed Forces Institute of Pathology".
[18] Paul Stone, *Legacy of Excellence: The Armed Forces Institute of Pathology, 1862-2011*, (Government Printing Office: 2011), 118.

Education and AFIP's leadership therein was not to be confined to students who could attend classes in Washington. Beginning at the end of the 1940s, AFIP began collecting and publishing its *Atlas of Tumor Pathology* — with the first edition coming out in 1949. Not a medical textbook, but rather a series of complementary and updating anatomical atlases, AFIP's *Atlas* became a conveniently readable resource for information about cancer, divided by particular anatomical areas — tumors of the thymus, tumors of the skin, tumors of the peripheral nervous system. What's more, AFIP distributed the *Atlas*, and similar materials, for free to service-linked and federal hospitals, and sold the booklets at cost to private clinics. AFIP was making moves to amplify its influence.

The researchers at AFIP joined forces with local universities — including George Washington University — serving as teaching faculty, in addition to their regular research duties at AFIP. The new building afforded AFIP agents the physical space to carry out laboratory research, without the need to depend on academic benefactors and the whims of university departments. AFIP's sudden emergence as an authority in medical research and pathological discovery might be measured alone on the basis of journal publications. AFIP was no longer just a library of specimens, but an active research site for living and post-mortem pathology research. The growth of the agency's research function was no small feat. For example, the number of articles that AFIP researchers published in scholarly journals jumped from twenty-nine in 1949 to ninety-

one in 1961.[19] These changes in scope and mission set the stage for pathological research and consulting on narcotism and chemical screens.

AFIP, if Brigadier General DeCoursey was to be believed, had metamorphosed into an institution considerably different from its pre-war self — a change that DeCoursey described as "a true academic environment for the future" — complete with air-conditioning.[20] But, whatever had conventionally constituted a "true academic environment" was eroding, yielding to the concrete, air-conditioning, and jet fuel that transformed AFIP from a library into a research center. The postwar period was a period of American state expansion into science and technology; uniting under the banner of human progress the sometimes disparate needs of the Cold War federal government, pharmaceutical industry, and individual researchers.[21] At the dedication of its new building in 1955, no less than President Dwight Eisenhower christened the initiative thusly: "here is one of those typical partnership efforts that bring government, science,

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[19] Henry, *Armed Forces Institute*, 315-334, 354-357. By the early 1960s, AFIP was fulfilling a directive of the Cooney Committee, an Army survey of work at AFIP, "to provide the at the least cost possible a maximum of pathology." Additional research funds were made possible with grants from federal agencies including the Veterans' Administration and the National Aeronautics and Space Administration. 317, 386.

[20] DeCoursey, "AFIP". On air-conditioning, see "AFIP", Pamphlet.

[21] The literature on federal influence over World-War-II-era and postwar academic science is vast. For a sampling, James Capshew, "Engineering Behavior: Project Pigeon, World War II, and the Conditioning of B.F. Skinner", *Technology and Culture* 34, no. 4. (Oct. 1993), 835-857. Stewart Leslie, *The Cold War and American Science: The Military-Industrial Complex and MIT and Stanford*, (New York: Columbia University Press, 1993). On an exemplary study that places city-building alongside defense research, see Michelle Pugh O'Mara, *Cities of Knowledge: Cold War Science and the Search for the Next Silicon Valley*, (Princeton: Princeton University Press, 2004). More recent work has emphasized the connections between the ripple-effect of Cold War-era spending and the soft sciences, see Alice O'Connor, *Poverty Knowledge: Social Science, Social Policy, and the Poor in Twentieth-Century U.S. History*, (Princeton: Princeton University Press, 2001). Mark Solovey and Hamilton Cravens, eds., *Cold War Social Science: Knowledge Production, Liberal Democracy, and Human Nature*, (New York, Palgrave McMillan: 2012).

and industry all together to do a great job for the betterment of humanity."[22] AFIP was suddenly a research center, a spur to industry, and the locus of social improvement.

Through its efforts to modernize its instruments and facilities, AFIP could claim an authoritative role in post-war medicine. DeCoursey trumpeted AFIP's role and echoed Eisenhower's own sentiments: "its traditions encourage industry, assure freedom in research, and support the principle that administration exists to aid scientific progress."[23] As I show in subsequent sections, AFIP's role as an authority in pathology was successful — with Leo Goldbaum being a case in point in this regard. Further, the relationship between AFIP and private business bloomed in the mid-1960s under RTRD.

Still, despite the changes afoot at AFIP, traditions did remain in place. Perhaps the most important tradition that AFIP maintained was its library and museum of pathological specimens. Each year, the military contributed upwards of eighty-percent of the specimens available across the multiple tissue registries. War was important for AFIP, if for no other reason than it tethered the infrastructural resources of pathology research to an institutional highway of dead and dying American soldiers. The road to scientific industry was paved with the draft contracts of enlisted Americans.

Leo Goldbaum and the Instruments of Chemistry: The ROY G. BIV of Toxicology, 1945-1970

It was during the era of AFIP's rising medical authority that both Leo Goldbaum and the chromatographic method of drug identification emerged.

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[22] Eisenhower quoted in John H. King, "Dedication of Armed Forces Institute of Pathology", *American Journal of Ophthalmology* 40, No. 1, (Jul. 1955), 126-127. Quote appears on 127.
[23] DeCoursey, "AFIP".

Goldbaum was, like AFIP, a beneficiary of the wave of interest in pathological research following World War II. As AFIP assumed new research responsibilities, it also inherited Walter Reed Army Hospital's own toxicologist — Leo Goldbaum. Goldbaum, born on November 13, 1913 in Brooklyn, NY, earned a bachelor's degree in 1938, only to serve in World War II as a toxicologist. He returned to his studies after the war, completing his doctorate in pharmacology in 1950, and quickly transitioned to work at Walter Reed Army Hospital in Washington. He moved on to AFIP in 1952.source

In part, Goldbaum's rise up through the ranks at AFIP — eventually becoming Chief Toxicologist for the agency, prior to his retirement in 1979 — was attributable to his insistence on building toxicology in the postwar period on the basis of applying analytical chemistry to the evaluation of specimens. When he came to AFIP in 1952, he had already been a dependable researcher, running experiments and pushing out research papers on topics including the identification of barbiturates in mice livers and the use of spectrophotometry in the identification of barbiturates — more or less, the field in which his life's work poured.

While he would become important in the translation of chromatography to clinical and clinical-adjacent settings, he displayed an interest in identifying the effects of all manner of drugs on the body. He was not faithful to chromatography either, but was instead, willing to experiment with other methods, including spectrophotometry. And, for good reason, Goldbaum was unfaithful to chromatography.

As early as 1946, Goldbaum pursued answers to the vexing issue of drug physiology, or, the body's predictable responses to known quantities of drugs. His work attested to the ubiquity of

new pharmaceuticals. While still in the Army, he assisted others in this research — designing a process for assessing both physiological reactions to sodium pentothal, and a means for identifying the drug from a tissue or bodily fluids.[24] Using two soldiers being treated at Walter Reed Army Hospital, and the organ tissue of an unstated number of rabbits, Goldbaum and a principal investigator began testing the ability of spectrophotometry to assist in accurately identifying sodium pentothal. The justification for their experiment, they claimed, was that "in recent years sodium pentothal has become important in anesthesia," but that "very little is known about its physiologic distribution in the body and…the greatest difficulty has been the lack of a specific method for the determination of the drug."[25] His life's work would go on to be a series of interconnected experiments to develop those specific methods.

He published subsequent experiments throughout the late 1940s and early 1950s all detailing the advances he had made in drug identification. These projects not only continued his interest in the identification of barbiturates, but also the handful of other methods that he experimented with to identify those drugs.[26]

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[24] Joseph Jailer and Leo Goldbaum, "Studies on the Plasma Concentration and Tissue Distribution of Sodium Pentothal (Sodium Ethyl (1-Methylbutyl) Thiobarbiturate", *The Journal of Laboratory and Clinical Medicine* 31, no. 12, (Dec. 1946), 1344-1349.
[25] Jailer and Goldbaum, "Studies", 1344.
[26] Albert Dorfman and Leo Goldbaum, "Detoxification of Barbiturates", *The Journal of Pharmacology and Experimental Therapeutics* 90, (4), (Aug. 1947), 330-337. Leo Goldbaum, "An Ultraviolet Spectrophotometric Procedure for the Determination of Barbiturates", *The Journal of Pharmacology and Experimental Therapeutics* 94, (1), (Sep. 1948), 68-75. Leo Goldbaum, "Determination of Barbiturates: Ultraviolet Spectrophotometric Method with Differentiation of Several Barbiturates", *Analytical Chemistry* 24, (10), (1952), 1604-1607. Raymond Goldblum, Leo Goldbaum, and William Piper, "Barbiturate Concentrations in the Skin and Hair of Guinea Pigs", *The Journal of Investigative Dermatology* 22, (2), (Feb. 1954). 121-128. Quote on 121.

What these earlier experiments perhaps signaled to Goldbaum, too, was the distance between his experiments, and their use on human subjects. In these earlier experiments, Goldbaum had a wealth of technical resources at his disposal — the spectrofluorometers, steady access to the drugs in question, and lab space to perform the studies. He also benefitted from abundance of non-human research animals — white mice, albino rabbits, and guinea pigs mostly — to supply the organ tissue on which to assess the success of his tools.

What he lacked most was human subjects. For example, in his 1948 paper on spectrophotometrics, he used an unstated number of three-kilogram rabbits, ten specimens from morbid cases of drug poisoning, and a single human who had received phenobarbital. To mimic the finding of phenobarbital in human tissue, Goldbaum's team extracted blood from the hearts of rabbits, then systematically killed them by air embolism.[27] Similarly, in a 1954 study on barbiturate concentrations and time duration, Goldbaum's described his process thusly: "the guinea pigs were sacrificed at vary intervals of time by fracturing the cervical spine. Three to 5 ml. of blood were drawn by intracardiac [sic] puncture."[28] These earlier studies had to make due with a limited supply of humans, and these animals ostensibly represented later human subjects. Goldbaum more or less broke off his use of animals, and turned decisively to chromatography by the late 1950s.

Goldbaum's infidelity toward chromatography reflected the crowded marketplace of identification techniques available. But, it also signaled the varied understandings of how to use a drug test, and in what contexts. The earliest drug tests — referred to as "spot tests" in the

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[27] Goldbaum, "Ultraviolet", 72.
[28] Goldblum, Goldbaum, and Piper, "Barbiturate Concentrations", 121.

present — resembled chromatography in key ways. Known collectively as "color tests," the Froehde, Mecke, and Marquis Tests involved spiking given samples with a mixture of chemicals that reacted with alkaloids present in opiates, producing, in the case of the Froehde Test, a violet color when it encountered morphine. Each of these tests, it should be noted, were entering elder age by the time that Goldbaum began his research in the 1950s: the Froehde Test had existed since 1866, while Mecke came about in 1899, and Marquis in 1901.[29] Still, even by the mid-1960s, these color tests remained standard practices to forensic work.

Froehde, Mecke, and Marquis remained, largely, the provenance of chemists, pharmacologists, and forensic examiners. Their utility, then, was not in surveilling living people, but revealing the mysteries of unknown substances in a sample.

Some of the first drug tests intended for use with human subjects were not even for narcotics, but, instead, for alcohol. Take, for example, Bogen's Test, first publicized in 1928, which was a latex balloon designed by a Cincinnati-based pathologist, Emil Bogen. Bogen's Test, like other eventual rival assays, utilized a color-marking process to detect traces of alcohol in a subject's breath. Bogen's Test involved a subject breathing into a latex balloon that had been filled with a reagent — called Antsie's reagent — that changed colors when it encountered traces of alcohol.[30] It is unclear to me, though, how extensive was the use of Bogen's method of alcohol detection.

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[29] On Froehde, see Albert B. Prescott, "Note on Froehde's Reagent as a Test for Morphia", *American Journal of Pharmacy*, (Feb. 1876), 59. On Mecke, see "A New Alkaloidal Reagent", *American Journal of Pharmacy*, (Oct. 1900), 489. On Marquis, see H.J.H. Fenton, "Organic chemistry — aliphatic division", *Annual Reports on the Progress of Chemistry* Vol. 2. (1905), 77.
[30] Emil Bogen, "Drunkeness. A Quantitative Study of Acute Alcoholic Intoxication", *The American Journal of the Medical Sciences*, (Aug. 1928), 153-167.

Still, identifying narcotics remained a prime objective for physicians and pharmacologists who worked with recovering drug users. The first widespread assay for drug use in living subjects came, accidentally, from the United States Public Health Service's Lexington Narcotic Hospital. First publicized in 1953, the assay was an accidental discovery in the course of research on non-addicting opiates, a larger research interest at Lexington in producing a non-addicting painkiller.[31] The chemical — also known as Nalorphine, Nalline, or, by its chemical name, N-allynomorphine — acts in some ways like an opiate antagonist.[32] Opiate antagonists, theoretically, react to opiates and reverse the symptoms of opiate intoxication, while opiate agonists, instead, mimic the effects of opiates and opioids.[33] Nalorphine was more or less a failure as an analgesic, but it revolutionized drug testing.

And, it was precisely Nalline's presumed status as an antagonist that fostered its rise in testing circles. According to reports, Nalline exhibited a strange quality — in morphine users who had recently taken the narcotic, Nalline generated symptoms similar to opiate withdrawal. They might sweat, shake, feel nauseated, but, most importantly, their pupils dilated.

The steps involved in a Nalline Test were fairly straightforward. First, a physician measured a subject's pupils using a card illustrated with black circles of varying diameters. Next, the physician injected a subject with N-allynomorphine. Following some fifteen or so minutes, the physician re-measured the pupils. If the pupils had increased in size, then the subject had

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[31] Caroline Jean Acker, "Planning and Serendipity".
[32] Acker claims that Nalorphine was the "first presumed morphine antagonist." According to her, Nalorphine exhibits qualities of both an agonist and an antagonist. See Acker, "Planning and Serendipity", 150.
[33] Acker, "Planning and Serendipity", 3.

recently used an opiate. If there was no change or a contraction, then the subject had tested negative for opiates.

Nalline's use largely occurred in penal and clinical settings. Its appearances in experimental laboratories was often more the result of comparing it against other tests.[34] Indeed, its usage was less a means of detecting crime than surveilling prisoners and patients. According to one proponent of the method, a physician in California's prison system, the utility of the test was that it "was designed to be and has been used as a club over the head of the addict whom no one should believe, or as a trap for the addict whom a few could believe. But in not a few cases it has served instead as a declaration of independence for the conscientious addict who is trying to stay 'clean.'"[35]

As the physician might have claimed, Nalline's utility was not accuracy. Far from it. Its utility was, instead, in convincing drug users that any relapse would be caught through the blanket effects of intentionally-induced withdrawal. The test would have, given the context, been of no use to analytical chemists or toxicologists. Why not? Wasn't its ability to determine whether morphine was in someone's system useful?

But, enthusiasm for the test consolidated quickly after its discovery in 1953 and subsequent promotion. As one metric of the test's popularity, California's penal system had conducted

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[34] E. Leong Way, Henry W. Elliott, and Norman Nomof, "Comparison of Chemical Tests with the Pupillary Method for the Diagnosis of Narcotic Use", in *Bulletin on Narcotics* no. 1, (1963), 29-33.

[35] Charles T. Hurley, "Anti-Narcotic Testing: A Physician's Point of View", *Federal Probation* 27, no. 32, (1963), 32-38. Oakland California Police Department, *Narcotics Addiction and Nalline: A New Approach to Detection of Narcotics Addiction*, (Oakland: 1963), 34.

17,000 such tests by 1960, and was regularly performing up to 10,000 tests a month in 1963.[36] The Oakland Police Department had adopted the test as early as the year of its discovery, and promoted its use in 1963 as "a major advance in police science."[37] Oakland looks to have led the rest of the state in the implementation of the test within a year of its discovery, churning out 239 tests in 1955 alone, compared to the combined 215 tests by the State of California Parole Office (75), the Santa Rita Rehabilitation Center (41), the Alameda County Probation Office (58), the state Bureau of Narcotic Enforcement (4), and the Federal Narcotic Bureau (1) among other agencies.[38] The conclusion, then, might be that the test's implementation was slow, despite its potential to aid investigators and clinicians. Further, its use was largely turned toward rehabilitation, and not the identification of chemicals alone.

The assay was not without its critics. Its limitations were manifold. Nalline could only help detect morphine and other opiates, thus it was useless in the detection of cannabis, cocaine, or amphetamines. Nalline required a physician, and could only be performed in small numbers. And, as some critics alleged, the test was hardly accurate, based as it was around pupil measurement. Social scientists assailed Nalline for other reasons, as well.

Attesting to the staying power of Nalline and the entrenched status of drug testing into the 1970s, sociologist Virginia Lewis decried the methodological architecture of what she and others perceived to be an echo chamber. Comparing Nalline and the eventual introduction of

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[36] Way, Elliot, and Nomof, "Comparison". Henry W. Elliott, et al., "Comparison of the Nalorphine Test and Urinary Analysis in the Detection of Narcotic Use", *Clinical Pharmacology and Therapeutics* 5, no. 4, 405-413.

[37] Oakland California Police Department, *Narcotics Addiction and Nalline,* 1. Quoted in Carey and Platt, "The Nalline Clinic", 224.

[38] James G. Terry and Fred L. Braumoeller, "Nalline: An Aid in Detecting Narcotic Users", *California Medicine* 85, no. 5, (Nov. 1956), 299-301.

chromatographic methods, Lewis claimed that "there is no independent method (excluding the nalline test) assessing test detection reliability."[39] Even more damningly, Lewis claimed that "the drug tests have become certified more by testimony than by hard evidence…the findings from [experimental literature] are far more sketchy and uneven than the supporters of drug testing suggest."[40]

Still, and it can't be overemphasized, Nalline remained a test largely for clinical and penal use, not manifestly useable for laboratory researchers outside of being a topic of research itself. Alongside the development of Nalline, Goldbaum was still producing research on mice and rabbits. His long tenure at AFIP looks to be the first chances in which he translated his determination techniques to humans. In that sense, Goldbaum's move to AFIP hinged on the double uncertainty on which much of this study rests: first, the pioneering of new investigative and detective techniques for drugs, and second, the pioneering of a new site from which the federal government and the DOD hoped to shape American scientific innovation.

In one of his first research papers as a member of AFIP, Goldbaum described what he perceived to be the issues facing the practicing toxicologist. From his perspective, the existing method available to toxicologists "involves many steps, is time consuming and results in a loss of considerable fraction of the drugs. The final extract … also contains relatively large amounts of interfering normal tissue extractives…[that] interfere with the chemical and physical tests."[41] In

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[39] Virginia S. Lewis, et al., "Nalline and Urine Tests in Narcotics Detection: A Critical Overview", *The International Journal of the Addictions* 8, no. 1 (1971), 163-171. Quote on 168.
[40] Virginia S. Lewis, et al, "Nalline and Urine Tests", 164.
[41] Leo Goldbaum and Melvin Williams, "Procedure for the Rapid Isolation of Basic Drugs from Tissue and Their Subsequent Purifaction and Identification", *Journal of Forensic Sciences* 4, no. 1 (Jan. 1959), 144-152. Quote on144.

that study, Goldbaum used paper ionophoresis — a process of electrically charging a paper smeared with a biologic sample, and spraying with a reagent to reveal colored bands corresponding to known drugs. In this study, as with others, Goldbaum's interest was twofold: the correct identification of a drug, and an interest in consistently shaving off time from the process.

Still, starting around the mid-1950s, Goldbaum had moved on near exclusively to using chromatography to investigate drug reactions. Much of the value of such a method, Goldbaum repeatedly insisted, was that "the quantity of sample is always limited" and "the chemical agents [in a sample] are in low concentrations."[42] Further, chromatography could not only pull out the minute traces of a drug, it could identify multiple drugs simultaneously — with some individual alterations for individual drugs. Still another benefit of the chromatograph, Goldbaum claimed, was that it was a method for "rapid" screening, a claim that, as I show later, he often returned to.[43]

It's possible that Goldbaum's initial experiments with other methods of drug identification reflected the imperatives of working with animals and, even, the diversity of drug screens available. It also reflected the specific qualities of AFIP's collection of specimens. AFIP's supply of specimens was expansive, albeit finite. Goldbaum aimed to craft a method that made the best use of small tissue samples, but presented enough of the drug to perform "conclusive

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[42] Leo Goldbaum, Eugene Schloegel, and Abel Dominguez. "Application of Gas Chromatography to Toxicology", *Progress in Chemical Toxicology*, (1963), 11-52. Quote on 11.
[43] For example, see Leo Goldbaum, T.J. Domanski, and Eugene Schloegel, "Analysis of Biological Specimens for Volatile Compounds by Gas Chromatography", *Journal of Forensic Sciences* 9, no. 1, (1963), 63-71.

identification" if necessary. Or, stated differently, Goldbaum was trying to navigate a path between too much and too little effluvia, done in the shortest time.

For these reasons, AFIP and Leo Goldbaum turned out to be a match ordained in the stars. For AFIP, they inherited a researcher who could further push the envelope in pathological research. For Goldbaum, he inherited the wealth of tissue specimens available at AFIP; a genuine opportunity to realize his program of drug identification with actual human samples.

Adverse Drug Reactions, the Registry of Tissue Reactions to Drugs, and the Chromatographic Regulation of Pharmaceuticals, 1960-1975

Overdoses, it must be said, provided a new window through which to assess drug tests and the scope of drug use more generally. It was during this period that drug overdoses became another point of evidence for describing the impact of illegal drug use. But, it wasn't overdoses on illicit narcotics that alarmed physicians and researchers. It was the explosion of pharmaceuticals that hit American medicine following World War II. Looking back from 1974, Goldbaum described how, "a great number of individuals are taking drugs that are being ingested singly or in combination with other drugs and alcohol. This has resulted in a marked increase in the number of samples submitted to the toxicology laboratory."[44]

The supposedly increasing problem of "adverse drug reactions" (ADR) — the toxic physiological reactions that resulted from licit and illicit drug use — enjoined federal regulators to step in. ADR encapsulated a world in which, on the one hand, following World War II, the

[44] Leo Goldbaum and Abel Dominguez, "A System for the Toxicological Analysis of Drugs in Biological Specimen", *Progress in Chemical Toxicology* Vol. 5. (1974), 101-149. Quote on 101.

supply of heroin stabilized, and potentially increased.[45] The war had interrupted the familiar and reliable transit routes of heroin smugglers, but more importantly, had opened a highway of pharmaceutical development in the United States. In a world in which the number of pharmaceuticals available to consumers mushroomed, the potential for toxic reactions increased. ADR was, in the early 1960s, a novel term. Whereas the war had introduced Americans to German pharmaceutical advances — including methadone and amphetamines — so, too, did it expose Americans to the potentially fatal side effects of sometimes untested medicines.

Americans responded with a handful of solutions to ADR. Professional groups and the federal government set up a handful of safeguards — first, the American Medical Association's Council on Drugs established in 1954 a registry for blood dyscrasias, and the Food and Drug Administration initiated a reporting program involving at least 200 hospitals across the United States. Similarly, the Kefauver-Harris Amendments in 1962 set up new protocols for pharmaceutical products. New limitations placed on pharmaceutical corporations signaled the emergence of a new way of producing understandings of drug efficacy. The modifications adopted in 1962, fostered the growth of randomized-control trials, and a new industry and administrative standard, comparative efficacy.[46] Perhaps, another significant effect of the amendments, though, was a new emphasis on toxicological detection.

The creation of the Registry of Tissue Reactions to Drugs in 1965 subsumed the earlier blood registry, and supplemented the FDA's reporting program and the Kefauver-Harris Amendments.

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[45] Schneider, *Smack*, 35-50.

[46] On Kefauver-Harris and comparative efficacy, see Jeremy Greene and Scott Podolsky, "Reform, Regulation, and Pharmaceuticals — The Kefauver-Harris Amendments at 50", *New England Journal of Medicine* 367, no. 16, (Oct. 2012), 1481-1483.

The purpose of the RTRD was straightforward: to collect and research cases of suspected ADR. The Registry, according to Nelson Irey, RTRD's director, was to institutionalize in one place "the evaluation of iatrogenic diseases related to drugs" that had already entered the "market phase" — having passed clinical trials, but still not definitely known (Irey 1971, 346). RTRD, similar to the other changes at AFIP after World War II, combined cutting-edge technology and the Institute's own traditions — coding by computer a combination of patient histories, toxicological reports, histology slides, and biological specimens fix sentence.

Between 1965 and 1975, RTRD acquired over 2,700 samples of tissue for its library. Nelson Irey, a pathologist, led Florabel Mullick and Claude Delage to collect, organize, and preserve a database of tissues with accompanying information coded to complement the tissues using the then-novel contributions of medical computing to assist their work. In fact, it was the use of computers that Irey found to be central to RTRD, as he claimed that being able to compare massive quantities of tissues via computer would allow them to construct "a more complete clinicopathological profile than would otherwise be possible."[47] As he put it in one essay, his teams distilled 800 "descriptor terms" into twenty to forty-five terms, which, with the assistance of the computers, "[bring] out correlations that would otherwise not be detected."[48] Modern drugs needed modern technology.

RTRD drew its specimens from the exacerbating situation in Vietnam, the Veteran Administration's clinics, and from the nearby Washington, D.C. coroner's office. The

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[47] Nelson S. Irey, "Adverse Reactions to Drugs and Chemicals: A Resumé and Progress Report", *Journal of the American Medical Association* 230, no. 4 (Oct. 1974), 596-598. Quotation on 598.
[48] Nelson S. Irey, "Registry of Tissue Reactions to Drugs", *Military Medicine* 136, no. 4, (Apr. 1971), 346-348. Quote on 347.

Department of Defense supplied RTRD with the lion's share of its collections. According to one estimate by Irey, some thirty percent of the Registry's acquisitions came from civilian labs in Washington, D.C. and elsewhere. But, beyond those civilian contributions, sixty percent of additions came from deceased servicemen. The final ten percent of specimens came from autopsies performed at the Veteran's Administration.[49]

Perhaps most importantly, RTRD fulfilled the post-war mission of AFIP — to leverage its component registries to stimulate industry and know-how, and to use AFIP to spread the benefits During RTRD's first full year in operation, in 1966, the program ran on a $100,000 budget borne by the American Medical Association (AMA), the Food and Drug Administration, and the Pharmaceutical Manufacturers' Association (PMA).[50] RTRD was, then, the unique realization of Cold War-era science — a collaboration between private and public entities supposedly for the public good.

Both and the AMA and PMA's enthusiasm for the registry revealed the appeal of the program. Decrying a state of affairs in which "hysterical and questionably valid overemphasis on the potential risk of a drug" existed, physician and member of the AMA's Council on Drugs Jean Weston claimed that, "factors which can improve validity are large numbers of suspected drug-associated reactions…carefully analyzed and documented at the source and transmitted to a

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[49] On the composition of RTRD specimens, see Nelson Irey and Richard Froede, "Evaluation of Deaths from Drug Overdose: A Clinicopathologic Study", *American Journal of Clinical Pathology* 61, no. 6, (1974), 778-784. Numbers on 779.

[50] National Academy of Sciences. *Annual Report — 1965-1966*, (U.S. Government Printing Office: 1968), 150. It is this program that concerns me vis a vis both Tobbell and Podolsky. The RTRD adds another venue through which federal researchers, professional organizations, and pharmaceutical manufacturers united to regulate pharmaceuticals.

common repository for evaluation and ready retrieval…[by] electronic data-processing equipment."[51]

Weston, to be sure, was invariably a reliable booster for the Registry, and a mouthpiece for pharmaceutical firms' lobbying power. Within just a few years, notably, Weston had taken on a position at the National Pharmaceutical Council, Inc. In one editorial submitted to the *Journal of the American Medical Association* (JAMA), Weston laid out the purpose of such a registry — an age of "therapeutic nihilism" in which "this whole question of adverse experience to drug may not be completely out of relation to reality."[52] In another op-ed in JAMA, he championed a similar pilot program run out of Tufts University and the Lemuel Shattuck Hospital in Boston, Massachusetts.[53] As with RTRD, PMA provided a seed grant to initiate the Tufts program.

With RTRD up and running, there was another venue through which Goldbaum and AFIP could realize the potential of toxicological analyses of dead soldiers and civilians. Although Goldbaum was not the lead investigator of RTRD, he and the other toxicologists acted as gatekeepers to the Registry through their work advising examiners in mysterious autopsies. In their consultation responsibilities, they could translate RTRD findings into the stuff of clinico-pathological advice.

In part, pathologists outside of AFIP treated the toxicological findings of AFIP? as conclusive evidence, relative to the other kinds of evidence before them. While their tests most certainly could identify the presence of drugs in a corpse, they also became the basis for explanations of

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[51] Jean Weston, "Adverse Drug Reaction Reporting and the Related Establishment of a Registry of Tissue Reactions to Drugs", *Medical Annals of the District of Columbia* 34, no. 8, (1965), 380-382. Quote on 380.
[52] Jean Weston, "The Present Status of Adverse Drug Reaction Reporting", *Journal of the American Medical Association* 203, no. 1, (Jan. 1968), 89-91. Quote on 90.
[53] Jean Weston, "Surveillance of Drug Therapy", *Journal of the American Medical Association* 209, no. 13, (Sep. 1969), 2049-2050. Quote on 2049.

the cause of death. In one case of apparent suicide, having described the levels of barbiturates in a soldier's blood and urine, the examiner concluded that "these amounts [of barbiturates] present at the time of death suggests that the individual may have been mentally confused, disoriented, and delusional."[54] In another autopsy, the same examiner noted, as if emphasizing that the toxicology results were the determinative evidence, "death due to drug overdose (Morphine — 2.4 mg% in bile; 1.7 mg% in urine; Amobarbital — 2 mcg/ml. in blood)."[55] To be sure, he didn't list the anatomical findings next to his diagnosis.

In another case of suspected suicide, examiners reiterated the toxicology findings as if they could reconstruct the victim's actions. "The latin american [sic] male sustained an alleged self-inflicted gunshot wound to the head…toxicology studies revealed the presence of alcohol and morphine which probably contributed to this individual's actions leading to his demise."[56] In still another case of suspected suicide, the examiner concluded that while the victim had ostensibly exploded an improvised bomb on himself, his urine suggested that "the blast injury caused death. In my opinion, drugs were contributory in the sense that bizarre behavior can follow from heroin use in Vietnam."[57]

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[54] Lester Legaspi, "Examination Report", 6 July 1970, RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 11-17, Box 3, Drug Abuse: Post-Mortem Protocols: RVN I 1970, Folder 12 (1 of 2).

[55] Lester Legaspi, "Examination Report", 25 Dec. 1970, RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 11-17, Box 3, Drug Abuse: Post-Mortem Protocols: RVN I 1970, Folder 12 (1 of 2).

[56] R. Londen, "Examination Report". 29 June 1971. RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 4-10, Box 2, Drug Abuse: Post-Mortem Protocols: RVN II 1971, Folder 10.

[57] Major MC T.A. Gaffey, "Examination Report", 22 Oct. 1970. RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Box 3 Folders 11-17, Drug Abuse: Post-Mortem Protocols: RVN I 1970, Folder 12 (1 of 2).

Each of these examples suggest that pathologists outside AFIP held a special reverence for the insights that the measurement of bodily fluids and tissues could disclose. Further, in each of the above cases, these reports reinforced AFIP's status as a medical authority in matters of drug-related deaths.

Through these autopsies and similar consultation duties, AFIP toxicologists interacted with a potentially steady supply of additions to the Registry. Often times, these additions made it in with the simple description of "interesting nature" — with no more explanation added to the report.[58] The requests to outside examiners were more specific about the kinds of data that they wanted than their justifications for requesting it — frozen tissues, photographs of the autopsy, and patient histories.

One potential explanation for what AFIP toxicologists considered worthy cases for admission to the Registry is that pharmaceuticals were more valuable than illicit drugs. To wit, in one case, the chief of AFIP's environmental pathology division described his hesitance to conclude that a soldier had died from heroin poisoning.[59] According to that report, the Registry's own holdings of eighty propoxyphene cases, plus research by Goldbaum and another AFIP researcher had helped to show that the soldier had died of propoxyphene poisoning rather than heroin, propoxyphene being an analgesic similar to methadone. A month later, Goldbaum performed his own examination and wrote to the original pathologist. As he explained in the accession request,

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[58] "Receipt and Report of Pathology Material", Oct. 1 1971, RG 319—Records of the Army Staff —Internal Medicine Drug Abuse, Folders 11-17, Box 3, Drug Abuse: Post-Mortem Protocols: RVN I 1970, Folder 12 (1 of 2).

[59] Charles Stahl, "Examination Report", 16 Feb. 1971, RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 11-17, Box 3, Drug Abuse: Post-Mortem Protocols: RVN I 1970, Folder 12 (2 of 2).

"your cooperation in sending frozen tissues to the AFIP is greatly appreciated. Tissue distribution studies following the exposure to propoxyphene are of great interest to us."[60]

Another potential explanation is that the tools for toxicological study set boundaries between normal and "interesting" cases. For example, when an otherwise obvious heroin overdose case came before him, Charles Stahl, the chief of Environmental Pathology at AFIP, informed the examiner of the "interesting nature of this case." But what had caught his attention? Having analyzed the bile and urine, Stahl discovered that the bile contained 5.2 mg% of morphine. This concentration of morphine in the bile pushed the upper limits of the levels of narcotic poisoning that examiners encountered. A case a year later reinforces the possibility that the chromatograph measuring the concentration of drugs set the boundaries of interesting and normal cases. When one soldier appeared to have died in his bed, the AFIP coroner noted the relatively high concentration of morphine in the victim's bile. Whereas "a series of fatalities at the AFIP had levels ranging from .5-5.6 mg% in the bile," this particular decedent had produced 6.8 mg% in their bile.[61] The latter case suggests that machine-assisted measurements could not only provide information in individual cases, but set new understandings of normal and abnormal cases.

Alongside the Registry, Goldbaum still produced research on chromatographic methods for drug identification using dummy samples. When, in 1968, he published a process for identifying minute traces of morphine, he more or less revisited a process that he should have firmly

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[60] Leo Goldbaum, "Request" 12 May 1971, RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 11-17, Box 3. Drug Abuse: Post-Mortem Protocols: RVN I 1970, Folder 12 (2 of 2).

[61] Lester Legaspi, "Examination Report", 1 Mar. 1971. RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 4-10, Box 2, Drug Abuse: Post-Mortem Protocols: RVN II 1971, Folder 10.

established at the beginning of the same decade. But, in his description of the experiment's methods, it becomes clearer that the kinds of proving methodologies of his own past — the use of surrogate-for-human specimens, especially — remained a point of contention. In the 1968 study, Goldbaum resorted to drug-free urine that he then spiked with morphine to evaluate his assay. He also utilized what he called "tissue blanks and recoveries" to which he added morphine. This experiment suggests that Goldbaum was still left to an unreliable supply of drug-users to design his assays.

RTRD may have rejiggered the foundations of Goldbaum's work. By 1969, he helped to publish a report that drew exclusively from the Registry. Using thirty cases of what the authors claimed were "sudden death in narcotic addicts," Goldbaum and his team described how to "determine very low concentrations of morphine in biologic samples."[62] Yet, what the team discovered was that while their method showed morphine in bile and urine, the two fluids "[were] not a good indicator of a recent intravenous injection."[63] Further, the team found that kidney slurries retained a far higher concentration of morphine.

There was a potential setback here. Did the higher concentrations of morphine in kidneys relative to bile and urine mean that Goldbaum's methodology would languish either in experimental labs or coroners' offices? To be sure, kidneys were only accessible after death, whereas urine could be obtained from a living subject. Perhaps more importantly, was

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[62] Edward Johnston, Leo Goldbaum, and Richard Whelton, "Investigation of Sudden Death in Addicts: With Emphasis on the Toxicologic Findings in Thirty Cases", *Medical Annals of the District of Columbia* 38, no. 7, (Jul. 1969), 375-380. Quote on 375.
[63] Johnston, Goldbaum, and Whelton, "Investigation", 380.

Goldbaum unintentionally segregating his work to the identification of illicit and semi-licit drugs like heroin and morphine?

The Registry's director, Nelson Irey, could have answered those questions, as he potentially envisioned the ways that Goldbaum and the Registry's research dovetailed. He described in 1971 his vision of the Registry's potential: "evaluation of clinical cases as they occur in the marketing phase of a drug's history represents the ultimate testing ground for its effects on patients," adding that "extrapolation of results of animal testing to the probable effect on humans in uncertain, and clinical trials are carried out on a relatively few patients."[64] Insofar as Goldbaum could not only substitute samples from dead drug users for animal surrogates and "tissue blanks," he was similarly evaluating the drug tests as much as the pharmaceuticals under study.

RTRD accomplished a handful of objectives. It assisted AFIP in suturing together the worlds of government regulation and private pharmaceutical research, and the regulation of pharmaceuticals as well as the policing of narcotics.[65] It introduced toxicologists, even Leo Goldbaum himself, to larger data sets from which they might further correlate drug quantities and physiological reactions. And, it empowered the toxicologists as gatekeepers who could use toxicological analysis to differentiate between supposedly normal and "interesting" cases. Still, even with RTRD firmly in place, the legitimacy of the chromatographic method for drug analysis remained questionable.

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[64] Nelson Irey, "Registry of Tissue Reactions to Drugs". Quote on 347.
[65] Again, the later studies helped to show the interconnected development of drug surveillance, in both its regulatory and policing functions.

Operation Golden Flow and Its Discontents: February to August, 1971 and Beyond

Six years after the creation of RTRD, in February 1971, Goldbaum embarked on proving the efficacy of chromatographic methods in mass drug-testing efforts. Taken in light of AFIP's growth as a leading medical authority in the postwar years, Goldbaum's emergence as a proponent of toxicological analysis, and the RTRD's steady supply of specimen research, OGF was the culmination of Goldbaum's translation of the chromatograph into a tool of surveillance.

Goldbaum and AFIP's role was not a certainty, despite the years of work that the agency had put into drug testing and the field of adverse drug reactions. In a proposal to the U.S. Army surgeon general, the assistant secretary of defense, Dr. Louis M. Rousselot, requested that the acting surgeon general appoint AFIP to lead a study in response to executive interest in drug testing.[66] Rousselot's support for AFIP suggested a twofold conclusion. First, that drug use was a problem which could conceivably be addressed by technological engineering. Second, that AFIP's research, and especially Goldbaum's research, had projected a vision of certainty — the study was to make use of "[techniques] developed at and in use by the AFIP Toxicology Branch."[67] The experiment's planning began shortly thereafter.

The project's investigators, Commander Charles Stahl, Lt. Col. Abel Dominguez, and Goldbaum, as well as an epidemiologist, and nine technicians rounded out the team. Stahl, Dominguez, and Goldbaum's presence on the team was no mere coincidence. Instead, their appointment signaled that their collective research on chromatographic methods of alkaloid

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[66] Louis M. Rousselot, "Pilot Study of Drug Excretion in the Urine of Military Separatees", 21 Jan. 1971, RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 38-46, Box 6, Congressional Correspondence Re: Drug Testing Contracts, Folder 38.
[67] Rousselot, "Pilot Study", 4.

identification was getting the chance to leap from experimental and research contexts into practical utility. The pre-OGF experiment was an opportunity to test the viability of chromatography beyond heavily-controlled laboratory environments.

Because of the relatively small size of the team, the experiment revolved around collecting and evaluating 2,500 urine samples from service members stationed at U.S. military installations. Stahl, Dominguez, and the epidemiologist visited the collection sites in mid-January to inspect the available laboratory space, and the techs collected the urine samples throughout February. From February through March, AFIP lab techs processed up to 100 samples per day.

The analysis portion of the experiment translated AFIP's chromatographic research into a usable form for the purposes of a massive screening project. Unlike the color tests and related spot tests that posed the greatest potential for massive screening, "analytical techniques will be those developed at and in use by the AFIP Toxicology Branch, using the techniques of gas chromatography, UV spectroscopy and mass spectrography as necessary."[68]

The experiment was adjudged a success. By June, military planners put together a booklet describing the process of establishing a testing program, based on Goldbaum's conclusions in regards to chromatography. Testing facilities went up at Long Binh and Cam Ranh Bay, both US military posts in Vietnam. (Illustrations 2 and 3)

Following the initial experiment, AFIP's role in screening efforts changed. Neither Goldbaum nor AFIP were responsible for directly testing the urine samples. Instead, AFIP joined the permanent testing efforts as a quality control lab, when in 1972 the DOD initiated its Drug Detection Quality Control Laboratory, which one author described as "provid[ing] worldwide

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[68] Rousselot, "Pilot Study".

Illustration 2

The results of thin-layer chromatography heroin screens, Long Binh, Vietnam. Source, RG 112: Records of the Office of the Surgeon General (Army), 1775-1994, Photographs of Army Medical Activities and Military and Civilian Life in Southeast Asia, 1965-1982 112-AIR, Folder J14-J438.

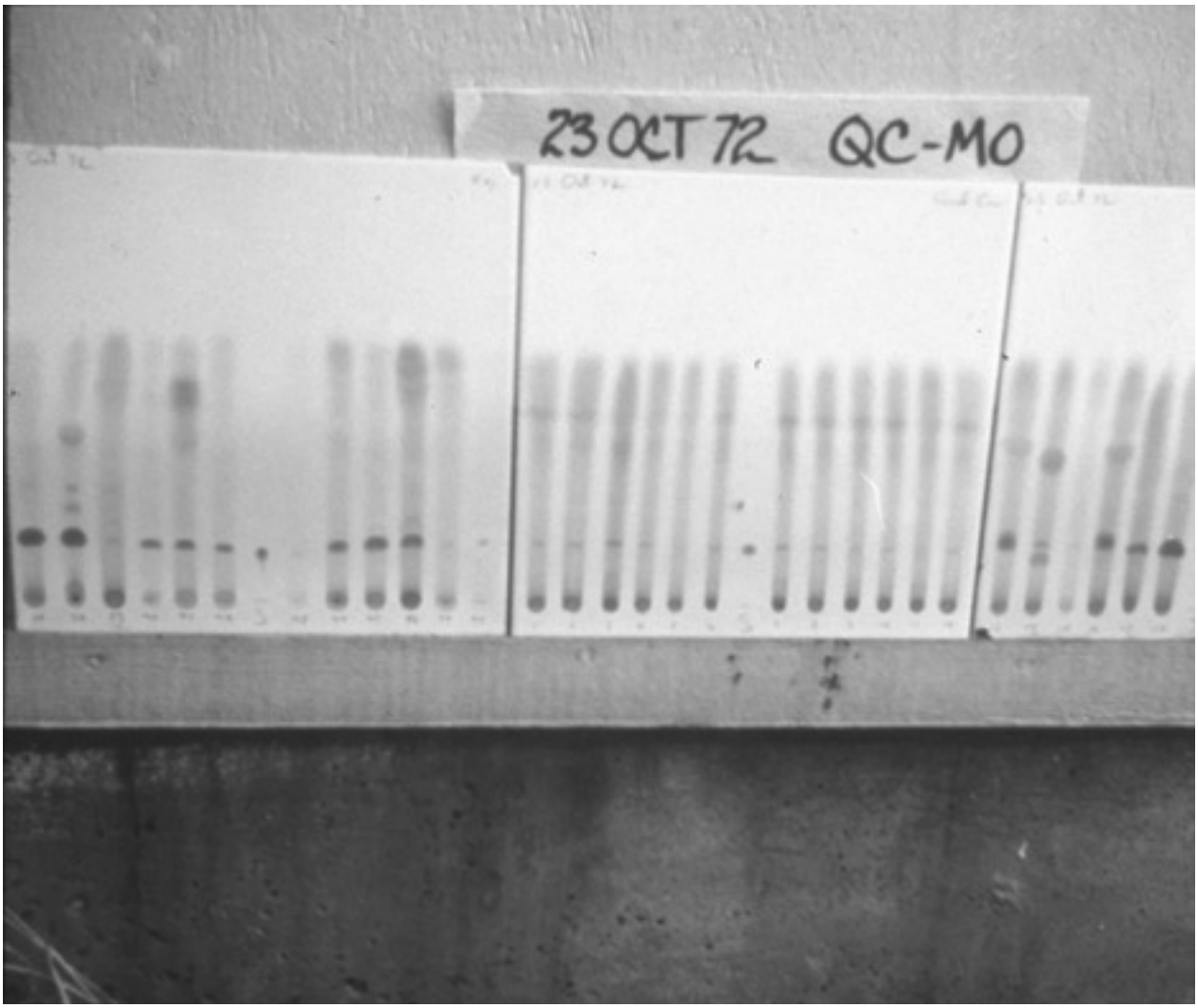

Illustration 3

The Pee House of the August Moon, Long Binh, Vietnam. Source, RG 112: Records of the Office of the Surgeon General (Army), 1775-1994, Photographs of Army Medical Activities and Military and Civilian Life in Southeast Asia, 1965-1982 112-AIR, Folder J14-J438.

supervision of quality control for military drug abuse detection laboratories." Further, in 1972, the quality control (QC) lab introduced a "comprehensive revision of the computerized system," further linking AFIP to its constituent clients.[69] In large part, AFIP spiked samples of urine with a known narcotic, and sent the spiked samples to service-based labs and private contractors to evaluate the precision of their procedures.

Despite the apparent success of the experiment, translating the experiment into an operable system involved no small amount of heavy lifting. The translation similarly opened up new questions about the viability of the program. In the U.S., the process of inventing a system was somewhat easier than in foreign countries. For installations in the continental United States, there were contracts to be made with private labs. On the one hand, contracting civilian laboratories eased the workload on the military's own laboratories. A number of them had already offered drug testing to consumers, however, in no way approaching the scale of OGF. Their main drawbacks however, were, first the scale of operation involved, and second, a relative lack of standardization across labs. That's to say that the United States and Europe offered readily available labs, but with less obvious direction into how to coordinate testing procedures across the continent.[70]

If the U.S. suffered from a surfeit of labs, albeit without a standardized testing procedure, extra-continental conditions presented their own problems. At posts, especially those in Southeast

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[69] Armed Forces Institute of Pathology, "Annual Report: Calendar Year 1975", (Date Unknown), RG 319 Records of the Army Staff, U.S. Army Center of Military History Medical History Division 1970-1979, CONUS General Hospitals to CONUS Institutes Box 39, Institutes — Armed Forces Pathology 1970-1979. Quote on 26.

[70] "Annex B: Capabilities of the Army System", (Date Unknown), RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 19-25, Box 4, Army Drug Detection Laboratory Plan Worldwide (w/Annexes A through G) Folder 20, (Date Unknown), B-2.

Asia, there was a dearth of testing laboratories. So, the DOD had to invent them. There was *a* benefit to initiating these labs — the Army, for example, could establish its own procedures and regulate them internally. Still, the cost of the decision to build working laboratories is staggering. For example, planners expected that in order to create a functional testing lab, the testing machine cost $26,000, the gas chromatograph cost $10,000, and a fume hood at $2,290. Further, staffing alone would cost over $200,000 per year.[71] And that was just for the initial screening.

Officials also estimated the cost of keeping AFIP as a quality control and confirmatory lab. Per their figures, the cost of a GC-MS?? system alone was, at the time, $52,000! Then there was the question of the other instruments and resources that AFIP agents believed to be integral to a functional QC lab: the cost of perishable supplies, including dry ice and reagents was $34,000 annually. To put the cost of such a venture in perspective, planners estimated that the annual salary of a single lab tech employed in one of the labs was anywhere from $8,000 to $12,000.[72]

The problems with the experiment were more than just fiscal. Goldbaum's initial experiment may have been quite an inexact, if not an outright inappropriate method for testing the accuracy of drug testing, and the chromatographic methods involved. According to one military official, "thin-layer chromatography is not a simple technique. It is a standard laboratory technique… under laboratory conditions by well trained technicians." The author continued, explaining that

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[71] "Laboratory Costs to Examine a Single Urine Specimen for Drugs", 30 Nov. 1971, RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 11-17, Box 3, Subcommittee on Laboratory Methodology (Meeting Notes) Folder 17.

[72] Enclosures 1 and 5, "Subject Requirements for Tri-Service Drug Detection Quality Control Programs", 19 Nov. 1971, RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 11-17, Box 3, Subcommittee on Laboratory Methodology (Meeting Notes) Folder 17.

"TLC is dependent on many variables? and may be too hairy for setting it up…under relatively primitive conditions."[73] Here, the author reflected on one of the persistent dilemmas of Goldbaum's sleight of hand — the belief that chromatography was a fish out of water when it fell into the hands of non-researchers.

Indeed, by the next year, in May 1972, problems bubbled over. One military physician stationed in Thailand described some of the difficulties involved in maintaining a testing system:

> We are not taking advantage of the policy that permits screening only 10% of samples…we have screened 100% of the urine specimens submitted to us for barbiturates and amphetamines. What we have said is that we do not routinely report observations developed from that screening for we have no confirmatory test. There is no confirmatory test since our GLC machine is inoperable primarily because of the lack of essential supplies.[74]

Another physician echoed similar frustration, grousing that "we have one functioning FRAT [free-radical assay test] machine. The other instrument has been out of order for six months, and we have no basis for predicting when it will be returned to service."[75] Another Army physician described the problems confronting the proposed system more colorfully: "procedures in the Laboratory, have been severely hampered by an intolerably inept supply system in the United States. This problem continues to dwarf all others."[76] If the program depended on a single

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[73] Milton Cutler, "Detection of Heroin in Urine", (Jun. 7, 1971), RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 11-17, Box 3, Land Warfare Laboratory Folder 16.

[74] Unknown author, "Letter to Col. Robert Joy", (May 2, 1972), RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 38-46, Box 6. Trip Report Bangkok Beach Folder 40.

[75] Leslie B. Altstatt, "Letter to Col. Edward L. Buescher", (Apr. 25, 1972), RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 38-46, Box 6, Trip Report Bangkok Beach Folder 40.

[76] Walter Noll, "Progress Report, Drug Testing Laboratory: Organization and Initial Operating Experience", (Mar. 15, 1972), RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 38-46, Box 6, Trip Report Bangkok Beach Folder 40. Quote on p. 7.

machine — the gas chromatograph or FRAT — then any interruption in service bottlenecked the smooth processing of urine sample, and threatened the accuracy of the entire project outright. Indeed, it was the extra-laboratory concerns that troubled these staffers.

Similarly, Douglas Beach, the head of the Biochemistry Division at AFIP and author of later screening planning memos, worried that the test procedure could easily fail to produce the desired results. In his planning notes, Beach mandated that of the enlisted soldiers who made up the urine collection teams, "should be rotated within RVN on a six-month basis to prevent compromise." Planners' paranoia about the potential social dangers of the testing process added still more conditions: members of the command and control group, which were in charge of the entire process from collection to analysis, were "messed in close proximity," while "detection teams should be segregated and quartered and messed as a unit."[77]

But, it perhaps wasn't exactly unwarranted paranoia motivating Beach. As a letter from 1972 described some of the problems involved in running a detection program, "the protection… against the daily petty harrassment [sic] of our client military activities has been welcome…yet in its final result, the screen imposed by the Administration around the Laboratory has fostered as many problems as it has prevented."[78]

Beach must have expected the worst, that all soldiers in Vietnam were potential liabilities in an otherwise scientific process. He claimed that "there must be the highest assurance that team members are <u>drug-free</u> [his emphasis] and remain so during the period of their association with the detection teams."[79] The soldiers who made up the detection team, in addition, were subjected

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[77] Douglas Beach, "Determination of Barbiturates and Amphetamines", 6.
[78] Noll, "Progress Report". Quote on 8.
[79] Douglas Beach,"Determination of Barbiturates and Amphetamines", 4-1.

to daily urine screens themselves. Finally, Beach called for "maximum security" around the collection and analysis sites, surrounding the latter with sandbags and barbed-wire fences. In social and spatial terms, the only solution to potential flaws in a laboratory process were to sanitize and segregate, to attempt to make a laboratory that operated in a vacuum.

Finally, two related, but unforeseen existential issues grew out of Goldbaum's initial proving experiment, and the subsequent testing program. First, that thresholds for detection had to be lowered. As one Army Criminal Investigation Division memo requested, researchers should take steps to lower the threshold of detection such that drug users might be caught as late as thirty-six hours after ingestion.[80] Second, and related to the second concern, was a near obsessive fixation on false negatives, otherwise presumed to be missing identifications.

Indeed, the obsession with false negative inspired an arms race of sorts. Put differently, multiple parties weighed in on the future of drug testing citing their preference for greater sensitivity to increasingly minute traces of alkaloids. This is, to be sure, distinct from the problem of false positives. There are numerous reports of false positives, but, perhaps more important to investigators, researchers, and physicians was the supposed problem of soldiers with minute quantities of illicit drugs in their bodies escaping detection. There were critics, to be sure, of the arms race in sensitivity. As a report from September 1972 at the Aberdeen Proving Grounds laboratory explained the problem, "it is apparent that research and development efforts directed solely to improvement of the sensitivity of a detection technique may be far less

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[80] Henry Tufts, "Rapid Determination of Heroin", RG 319—Records of the Army Staff—Internal Medicine Drug Abuse, Folders 11-17, Box 3, Land Warfare Laboratory Folder 16.

profitable than those directed at improvements in detector application."[81] For that team of researchers, the potentially intractable problem of ever-decreasing chemical thresholds outweighed the potential benefits of continuously lowering the thresholds.

As a bow on the present that he had gifted American soldiers the world round, Goldbaum began publishing his efforts to develop a mass drug-testing procedure in 1972. Referencing his old hobbyhorses, Goldbaum described the "rapid" and "simple" procedure that he and AFIP had invented for the initial experiment.[82] He produced a sequel to the same report in 1974, pronouncing such an effort as eminently feasible.[83] Unlike the more skeptical observers, Goldbaum did not reference the costs of testing, the wide variability in testing procedures across laboratories, or the potential of faulty chromatographs to gum up the processing. Goldbaum remained, as always, convinced of his invention.

Conclusion

Since the late 1940s, AFIP's role as an authority in pathological research and education multiplied. It was from AFIP's newfound status as a premier research site — the "true academic environment for the future" as Elbert DeCoursey had put it — that Leo Goldbaum emerged. While other modes of drug testing existed before, during, and after Goldbaum's chromatographic

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[81] Donald O. Egner and Donald Campbell, "Significance of Detector Sensitivity in Detection of Drug Abusers": Technical Report No. 72-07. (Sep. 1972). 6, RG 319 — Records of the Army Staff — Internal Medicine Drug Abuse, Folders 11-17, Box 3, Land Warfare Laboratory Folder 16.

[82] Leo Goldbaum, Philip Santinga, and Abel Dominguez, "A Procedure for the Rapid Analysis of Large Numbers of Urine Samples for Drugs", *Clinical Toxicology* 5, no. 3, (1972), 369-379.

[83] P.E Winter, et al., "Drug Excretion in the Urine of Military Separatees: A Pilot Study", *Journal of Forensic Sciences* 19, no. 2, (Apr. 1974), 317-324.

process, their utility stalled in the mid-1960s as pharmaceutical interests tethered their fortunes to AFIP's. Goldbaum's work at AFIP, and his access to the biological samples through the RTRD offered an unparalleled resource for translating chromatography into clinical and clinical-adjacent use. Competing methods of drug identification did not have such luxuries. And, despite the seeming success of the February 1971 experiment, questions, if not outright skepticism remained about the feasibility of such a testing program.

This chapter has shown how Nixon's decision in 1971 to begin drug testing U.S. soldiers leaving Vietnam, and eventually across the globe hinged on a combination of concerns and aspirations about the efficacy of drug-testing, AFIP's growing role as a medical authority, and the emergence of new research into the adverse effects of drugs. Golden Flow was yet another chapter in the political economy of postwar pathology research, as much as it fulfilled the calculations of a single presidential administration. That such a program would emerge and succeed in its objectives remained a mixed record. In contradiction to descriptions of the program as "perfected," the assaying procedure remained mired in irresolvable contradictions related to the social pressures, costs, and proper environment of chromatographic testing.

Further, this chapter has also shown how chromatography stitched together the regulation of pharmaceuticals and the surveillance of narcotics. One journal paper, one specimen bag, and one chromatograph reading at a time — these were the means of developing a modern evaluating system for pharmaceuticals, and of translating a tool once unique to analytical chemists into the means of curbing illicit drug use. These developments went hand-in-hand.

Chromatography was more than an accidental or coincidental choice for drug testing, it was *the* means of drug testing, warts and all.

As this chapter has begun to illustrate, the Army's decision to pursue drug control in the Vietnam War was less a single decision at a specific moment. This, ultimately, reinforces Eric Schneider's description of the services' response to drug abuse being "piecemeal."[84] Instead, it had pursued specific areas of research that eventually became the technologies of a general campaign against narcotics. Uncertainty about both the topics of inquiry, and the appropriate support of discoveries propelled the Army into more expansive and invasive forms of drug control by the early 1970s.

[84] Schneider, *Smack*, 163.

Chapter 2

Kennels of Control: Military-Science-Industry Collaboration, the Invention of Drug-Sniffing Dogs, and the Brittle Narco-State, 1962-1973

Introduction: Making Scents of the Past

John Romba's job led him to rural Aberdeen, Maryland in the 1960s. A psychologist by training, Romba ended up at the Limited Warfare Laboratory (LWL) evaluating various kinds of animals for all manner of counter-insurgency tasks. He had gotten to train pigeons to radio-transmit locations of enemies, and bed bugs to detect elusive guerrilla troops; both failed. So, when Romba and other researchers began to assess the suitability of canines for narcotics detection in 1971, the move sounded like an addition to the highlight reel of failed experiments.

A journalist once colorfully described the work of Romba's lab: "L.W.L. produce diabolical devices that rival the output of the men from U.N.C.L.E.," a reference to the gadgets-and-spies television show of the same name.[1] LWL had a reputation for novel, albeit outlandish experimental solutions to pressing military problems. They had recently contracted a feasibility project to Behavior Systems, Inc., a private firm, using dogs to detect mines just a few years earlier, and had turned that project into a working company of mine-detection dogs and handlers in Vietnam.[2] So, when Military Assistance Command, Vietnam (MACV) requested the feasibility study from Romba in 1971, the dice were in the air.

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[1] William Beecher, "Way-Out Weapons", *The New York Times*, (Mar. 24, 1968), 49, 52. Quote on 49.

[2] Robert Lubow, *The War Animals: The Training and Use of Animals as Weapons of War*, (New York: Doubleday, 1977). On mine-detector dogs, see 173-202. On narcotics-detector dogs, see 209-215.

Instead of failing, Romba seemingly demonstrated that his test cases, just two Labrador dogs, *could* be trained to detect heroin. His experiment precipitated further tests. Within a year of Romba's experiment, the Army and Air Force contracted similar feasibility studies, using private research firms to evaluate the sensory capacities of dogs for narcotics detection. Romba's conclusions transformed into training principles at the Military Working Dog Center (MWDC) at Lackland Air Force Base, which had itself recently begun to organize research on military working dogs. By the next year, the Bureau of Customs and the Bureau of Narcotics and Dangerous Drugs placed orders to MWDC for their own dope dogs.[3]

The burst of interest in narcotics-detector dogs and research on them at the DOD was short-lived, however. Beginning as early as 1969, Congressional opponents of the unique system of postwar, military-funded research — the same system that underpinned detector-dog research — emerged in 1973 temporarily victorious, as they imposed new limits on defense-research spending. The effect chilled subsequent defense research on narcotics-detector dogs for the foreseeable future.

This chapter shows how a collaborative system of sensory research unique to the postwar United States pushed Romba toward studying narcotics-detector dogs, how that research fulfilled the varied interests of DOD project managers, laboratory scientists, and research institutions, and how, despite significant, newfound interest in narcotics control in 1971, critics of that collaborative system brought down the developing research track in using dogs for narcotics control. In doing so, it elaborates on the central claim of this dissertation by showing how

[3] "U.S. Will Sic Dogs on Pot-Loaded Mail", *Chicago Tribune*, (Sep. 3, 1970), 1.

uncertain was the infrastructure surrounding the military's response to heroin use — what I term here the brittle narco-state.

This chapter surveys the military agencies, policies, individual researchers, and research institutions responsible for building up a research track in narcotics detection, and the critics who ultimately brought the system to its knees. Along the way, it addresses the language that supporters of sensory research used to frame their work, and the consequences of collaborative labor. In this sense, this chapter is both an attempt at describing the political economy of narcotics-detector-dog research, as well as an examination of the subjective experience of that political economy.

The narcotics-detector dog and olfactory research were creatures of the Cold War, and their mutual history is a tale of two overlapping themes — that of the fate of the postwar working arrangement between the DOD, American universities, and private research firms, and that of the search for scientific legitimacy in social-scientific research, and especially sensory research therein.

In the half century since the turn of the twentieth century, the federal government had largely left scientific research to private bodies. Beginning in World War II and continuing through the early Cold War, the federal government undertook a massively enhanced role in supporting research in science, technology, engineering, and math.[4] This resulted in projects including the

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[4] This is not to say that the federal government's interest in supporting various kinds of scientific research prior to World War II. There are shades of such investments going back at least to the demonstration farms and land-grant universities of the late nineteenth century, if not earlier. However, World War II began the fertilizing of STEM-style research for the purposes of defense. On American science during the Cold War, see Stuart W. Leslie, *The Cold War and American Science*. Rebecca S. Lowen, *Creating the Cold War University: The Transformation of Stanford*. (Berkeley: University of California Press, 1997). Mark Solovey. *Shaky Foundations*.

Manhattan Project and the atom bomb, and the National Aeronautics and Space Administration and the first American satellites and trips to the moon. Both projects illustrate not only how involved the federal government became with regard to experimental research, but also how the federal government's influence blurred the line between defensive research and non-defensive research.

Detector dogs didn't, however, come from such an ambiguous origin. Instead, they were direct products of the Limited Warfare Laboratory, itself a product of the post-Cold War boom that the Army staked out in the 1960s. Conceived in 1962, its planners employed over 100 scientists to experiment, evaluate, and invent anti-guerrilla tactics. Still, despite its Cold-War origins, the LWL was not in a vacuum. Rather, its products satisfied other ambitions on the part of the federal government, including fostering collaboration between its agencies, American universities, and private research firms and non-profits. As much as the federal government's subsidies of STEM research fit plans to build up the U.S. arsenal, they also satisfied other needs,

especially that of producing a "science" industry.[5] This led to Project THEMIS, a program which funded early detector-dog research at the University of Mississippi. It also funded and exchanged findings with private firms, including Behavior Systems, Inc., each cribbing from the other. Detector dogs, first mine and tunnel dogs, then narcotics-detector dogs, were the direct products of the ongoing exchange of research between these parties.

This system also produced unique criteria for evaluating sensory research on animals, especially on dogs. Researchers and planners alike described the benefit of their work and its collaborative nature as its ability to stimulate the free exchange of discoveries and other information, in its entertainment value, its ability to nurture creativity in the laboratory, and, finally, in its ability to

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[5] On reassessing the "Cold War-ness" of social science in the Cold War, see David C. Engerman, "Social Science in the Cold War", *Isis* 101, no. 2, (Jun. 2010), 393-400. On science's institutional and economic value in the Cold War, see O'Mara, *Cities of Knowledge*. This chapter addresses, too, an interest of Elizabeth Popp Berman, who has written about the "economization" of university science. Berman focuses on patents for university research, whereas this chapter shows one research firm, Behavior Systems, that an academic researcher owned. See Berman, "Why Did Universities Start Patenting? Institution-Building and the Road to the Bayh-Dole Act", *Social Studies of Science* 38, no. 6, (Dec. 2008), 835-871. See also Berman, "Not Just Neoliberalism: Economization in US Science and Technology Policy", *Science, Technology, and Human Values* 39, no. 3, (May 2014), 397-431. Davarian L. Baldwin, "The '800-Pound Gargoyle': The Long History of Higher Education and Urban Development on Chicago's South Side", *American Quarterly* 67, no. 1, (Mar. 2015), 81-103. Alex Sayf Cummings, "'Brain Magnet': Research Triangle Park and the Origins of the Creative City, 1953-1965", *Journal of Urban History* 43, no. 3, (May 2017), 470-492. For a more recent work that makes sense of defense tech and business in pre-Silicon-Valley Palo Alto, California, see Stephen B. Adams, "Arc of Empire: The Federal Telegraph Company, the U.S. Navy, and the Beginnings of Silicon Valley", *Business History Review* 91, no. 2, (Summer 2017), 329-359. Hunter Crowther-Heyck provides a novel account of patronage in the period, claiming that there were two periods — with the second focusing on technological specialization. See Crowther-Heyck, "Patrons of the Revolution: Ideals and Institutions in Postwar Behavioral Science", *Isis* 97, no. 3, (Sep. 2006), 420-446. On cooperative research in American medicine and the associative state of the 1920s, see Harry M. Marks, *The Progress of Experiment: Science and Therapeutic Reform in the United States, 1900-1990*, (New York: Cambridge University Press, 1997). See especially 42-70.

make permanent the scientific status of social scientific research. This rounds out our understandings of how scientists responded to their encounters with the DOD in the postwar period; detailing the linkages between social scientists, scientism, and DOD patronage, and showing the metrics on which scientists valued their research for the DOD.[6]

The invention of narcotics-detector dogs spans the heyday of that collaboration between 1945 and 1969, as well as its partial dismantling at the end of the Vietnam War. The dismantling of this system aligned with the American withdrawal from Vietnam, and Richard Nixon's overall plan of Vietnamization. The first challenges to the system that ultimately produced the detector dog came in 1969, when Mike Mansfield, a Democratic senator from Montana, passed the first of two "Mansfield Amendments," so named for their author. The first amendment cancelled Project THEMIS, while just four years later the second Mansfield Amendment passed, limiting the projects at the Advanced Research Projects Agency to ones with ambiguously defined "direct military relation," cutting out the remaining leg of the detector-dog research program.

Parallel to Vietnamization, Nixon announced in June 1971 the first "war on drugs," declaring that heroin was American enemy number one. Nixon's declaration may have been responsible for the American command in Vietnam requesting that the LWL assess the feasibility of training narcotics-detector dogs. This more or less confirms a chronology that other historians have identified.(sources) Whatever the reason, despite a quick burst of interest, the program caved-in on itself with the passing of the second Mansfield Amendment. The last of the detector-dog

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[6] This chapter takes Mark Solovey's direction to "follow the money" in both directions, attempting to balance the perspectives of diverse stakeholders. This chapter also supports his claim that social scientists and the DOD highly valued "scientism," especially as it regarded quantification of previously unquantified subjects. See Mark Solovey. *Shaky Foundations.*

research programs submitted their final report in 1976. Despite sudden interest from the White House in the problem of heroin abuse and its control, what was once a promising research area fizzled out shortly after sparking. In the sense that narcotics were an appealing justification for the research given the context, detector dogs should have survived the new era in defense-funding battles. Instead, even the sudden allure of narcotics control could not save the research field from extinction. While the research program failed, the detector dogs remain a mainstay of anti-drug operations.

The narcotics-detector dog research program disappeared because it depended on a system insufficiently stable to protect it. The brittle narco-state that developed out of existing programs could not survive even in a fertile environment.

This chapter proceeds in three sections and a conclusion. In section one, I survey the landscape of military defense research and funding projects on canines, including LWL, where the DOD's first experiments with detector dogs and an eventual research system emerged. I also discuss the implications of Project THEMIS, a policy that helped to fund at least one dog researcher. Both created conditions in which a narcotics-detector dog project might appeal to researchers. In section two, I turn to three researchers funded by the DOD, Robert Berryman, David Moulton, and Robert Lubow, and discuss how their academic institution-building coalesced with their research on canine learning and olfaction. I discuss in more detail how their careers interacted with the creation of a detector dog experiment. In the third, and final section, I present the critics and challengers to the system that produced the narcotics detector dogs. Throughout, I

acknowledge the language that researchers and administrators used to evaluate sensory research with regards to detector-dogs.[7]

Finding a Home for Detector Dogs: Sensory Research on Canines in the Age of Counterinsurgency, 1960-1965

This section focuses on two institutions, the Limited Warfare Laboratory and the Military Working Dog Center, and one policy, Project THEMIS. Each of these pieces composed the foundation of the federal government's interest in detector dog research. This section shows how planners attempted to accomplish multiple Cold War-era objectives, from fighting guerrilla soldiers to redistributing DOD research money, and worked extensively with corporate and academic researchers to invent a detector-dog program. It also shows how project managers and scientists that the DOD employed came to value using dogs for new war applications.

The cooling of the Cold War between the Soviet Union and the United States post-Cuban Missile Crisis produced one of the stranger quirks of the period: the extrapolation of the Cold War to the developing world.[8] As the focus of American intervention shifted to supporting

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[7] I have a few qualifications for this chapter. First, it is not intended as a study of the entirety of the Military Working Dog training program. Second, it is not a history of the specific experiments that researchers ran, although those are themselves important. Third, it is not meant to be a history of the entirety of detector dogs, or even military detector dogs. Instead, this is a history of the origins of narcotics-detector dogs in academic and defensive laboratories. Fourth, and finally, this is not a history of the constitutional quagmires that such dogs invite. Although constitutional issues surrounding detector dogs, police searches, and privacy are worthwhile, this study explores the political economy of their invention.

[8] This is not to say that conflicts didn't already exist. On replacing a bipolar conception of the Cold War, i.e., the United States versus the Soviet Union, with a multilateral version, i.e. the influence of smaller, developing states on foreign and domestic policy in the First and Second World, see Odd Arne Westadt, *Global Cold War: Third World Interventions and the Making of Our Times*, (New York: Cambridge University Press, 2005). Marilyn Young, *The Vietnam Wars, 1945-1990*, (New York: Harper Collins, 1991).

contests between communists and their opponents in the developing world, the American military confronted a dangerous gap in knowledge. Prior military planning had near exclusively prepared the U.S. military to fight against large, regular armies using heavy artillery, and, more recently, the atom bomb. The focus of war-tech research up to that point, too, had emphasized facing off against discernible enemies.

The geographical re-emphasis of American military engagements to Asia and further invented a new opportunity for patrons and researchers. As the United States military discovered upon their first forays into Vietnam, defeating communists in the developing world involved tactics distinct from those devised for a context in which the US fought large, standing, regular armies. The small-scale strategies that guerrilla forces eventually adopted in, for example Vietnam, including pungee sticks, field mines, and ambushes challenged the United States's dependence on tanks, atom bombs, and formal engagements.

Enter the Limited Warfare Laboratory. LWL was a powerful contrast to the big-budget projects responsible for U-2 spy planes and nuclear reactors. The LWL was one of the first organized responses to the supposed problem of guerrilla warfare. Located in Aberdeen, Maryland — northeast of Baltimore on the Chesapeake Bay — the LWL illustrated the ways that the Cold War made canine soldiers more appealing. When administrators activated the laboratory in 1962, the proving grounds were already a demonstration and development site for untested weaponry — an ordnance research site known as Aberdeen Proving Ground (ABP). ABP itself had come about during World War I as a means for evaluating the cutting edge of heavy artillery.[9] But,

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[9] "Aberdeen Proving Ground", 2001, *The Oxford Essential Dictionary of the U.S. Military*. Online Edition, (2002).

perhaps as some analysts claimed, those proving grounds were like the tactics developed therein, dependent on a form of possibly extinct warfare.

The institutional experiment, as one analyst reflected on LWL after its creation, was a response to "warfare of a third dimension, viz. guerrilla insurgency, [that] loomed increasingly large as a threat to world peace." According to that same analyst, American war research had been confined to "being prepared to fight in Northern Europe."[10] The potentially radical consequences of guerrilla warfare, then, became yet another reason to pursue research uniquely suited to irregular fighting.

Whereas U.N.C.L.E. had its emphasis on super spy gadgets intended for use on other spies, LWL was responsible for a handful of research projects targeting peasant combatants. The research and development site cum think tank employed between forty-five and seventy scientists at any given time — including but not limited to chemical and sanitary engineers, a botanist and zoologist, and a physicist and anthropologist. In terms of personnel, planners strove to acquire experts from "virtually every" scientific research field.[11] This point is more important upon deeper reflection. That a military laboratory focused on improving fighting capacities sought technical and scientific experts is hardly surprising. However, the diversity of experts suggests that LWL's planners perceived of limited warfare in fairly expansive terms — giving them discretion to pursue technologies and techniques that differed significantly from the emphasis in the past on large-scale operations and deployments of soldiers and materiel.

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[10] Mortland, Cutler, and Kaprelian. "Final Project Report", 1.
[11] Mortland, Cutler, and Kaprelian, "Final Project Report", 47.

Some of the lab's early projects were both mundane and more eyebrow raising. Their work included miniaturized first-aid kids, the use of bedbugs and frog belly-skin as *biosensors* — essentially using a living organism as a surveillance device — to detect enemy troops, and continued innovation in the creation of what was called "the people sniffer," basically an electronic sniffing device positioned on a helicopter that ferreted out the ammonia in human sweat.[12] The projects suggest a handful of qualities of the LWL — that it was a creative venture that utilized unconventional phyla for warfare, as in the bedbug experiments. It also suggests that the lab's researchers were insistent on transforming animals into higher-order defense applications.

As much as LWL was an experiment in defense tech, it was also an experiment in how to produce defense research. According to one glowing article in 1967, the weapons that LWL devised "are the result of a unique coalition. For the first time, civilian scientists and engineers…have traded the comfort of desks for the sweat and grime of the battlefield." Further, civilian scientists "team up…with fighting troops to learn from firsthand experience what the problems are. Then, they return to solve them in the laboratory."[13]

LWL's technical director, Edward Kaprelian, claimed that one of the advantages of LWL was that it decreased the time between idea and application, from years to months. A journalist emphasized the swashbuckling needed to combine research for limited wars with a desire to

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[12] William Beecher, "Way-Out". On his experiments with frog skin, see Alfred T. Kornfield and Maxx Krauss, "Technical Memorandum 65-02: Biosensing Technics for Human Detection II. The Frog Skin Transducer: A Continuous Flow System for Making Critical Measurements", (Aug. 1965), *Defense Technical Information Center*, http://app.dtic.mil. It's worth noting, too, that even the frog-skin experiment involved a private lab, Biosearch Company.

[13] Mort Schultz, "Wild New Weapons for Vietnam", *Popular Mechanics*, (Jan.1967), 94-98, 214. Quotes on 97.

produce quickly new weaponry, "by cutting red bureaucratic red tape and by hiring men with a maximum of initiative and giving them a free hand."[14] In a sense, Kaprelian and others believed that LWL removed whatever middleman may have impeded translating lab solutions to battlefield problems. Their semi-maverick, semi-laissez faire view of scientific research evidenced itself elsewhere.

For the scientists who came to LWL, including John Romba, there was still more appeal. While the journalist described LWL's employees as "inventive, offbeat scientists," Kaprelian claimed that scientists came to Aberdeen because they "were attracted by our higher civil service grades. Others like the autonomy, the constant cultivating of ideas, the prospect of actually being able to see the results of their efforts."[15] Kaprelian, and others repeatedly described the work in terms of its free exchange of ideas, as if the nuts and bolts of killing needed a veneer of respectable curiosity within the scientific community.

Still, as much as Kaprelian and others even went so far as to note the niche role that LWL occupied, it was part of a broader constellation of research. Instead of acting solely as a laboratory in its own right, it also "provid[ed] a point of contact with industry and the other Services."[16] Beginning as early as 1963, LWL used a private lab, Biosearch Company, Inc., to conduct its initial experiments on the ability of biosensors to pick up human sebum — essentially, the materials that compose sweat. Eventually, LWL's relationship with private labs extended to the frog belly experiments, and, later, to using dogs as biosensors for mines, tunnels, and narcotics.

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[14] William Beecher, "Way-Out Weapons", 49.
[15] Edward Kaprelian quoted in Beecher, "Way-Out", p. 52.
[16] Mortland, Cutler, and Kaprelian, "Final Project Report". p. 7.

Both the flexibility of the research, and LWL's insistence on putting animals to work created the foundations from which LWL's eventual detector-dog project came. It wasn't long after the activation of LWL in 1962 that Dr. Max Krauss, the head of the laboratory's biological sciences division, began studying the capabilities of radio-tracked dogs in scouting and reconnaissance missions; an entire directive to put animals to work.[17] Krauss, like Kaprelian, described the difficulties of his work at LWL as follows: "[F]inding problems is not the trouble. The trick is to solve them, and we'll use any means to do that."[18] Indeed, the abundance of research opportunities may have been the reason that Krauss ended up at LWL in the first place, echoing Kaprelian's own description of the lab as the freedom to pursue research questions, as if the utility of the innovations had less influence over the research.

In 1963, Krauss began his initial experiments with the use of unleashed reconnaissance dogs. Effectively, starting in 1963, Krauss began LWL's nearly decade-long push to use dogs in ways novel to the military's existing uses, which amounted to guarding missile silos, leading soldiers from the front, and attacking enemies when the opportunity arose. Krauss's experiment, instead, paired dogs with a radio transmitter, allowing dogs to roam freely, but relay the movement of troops through the transmitter.

There was very recent precedent for using dogs in warfare, notably in terms of research on dogs as detectors — so the idea was not exactly new. To wit, during World War II, David Moulton,

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[17] On other then-contemporary attempts to "wire" animals during the Cold War, see Etienne Benson, *Wired Wilderness: Technologies of Tracking and the Making of Modern Wildlife*, (Baltimore: The Johns Hopkins University Press: 2010), see especially 5-51. As Benson shows, other military research agencies, including the Office of Naval Research, the Naval Research Laboratory, and the U.S. Naval Medical Research Institute, took an interest in studying animal migration patterns by using tracking devices.

[18] Dr. Max Krauss quoted in Schultz, "Wild New Weapons", 98.

whom you will meet later in this chapter, investigated mine-detection dogs for the Royal Army Veterinary Corps. Similarly, the U.S. Army Engineers Research and Development Laboratories assembled a team consisting of an anatomist, biochemist, and a parapsychologist following World War II to investigate their own mine-detection dogs.[19] Still, there was little precedent for actually translating that research into working military materiel.

Krauss's research on detector dogs necessarily stitched together private and public, corporate and martial functions. For example, in 1971, Krauss ran a series of experiments first at the University of Mississippi, then at ABG. Funded by a grant from the then-recently created Law Enforcement Assistance Administration (LEAA), a policing reform introduced following the urban riots of the mid- to late 1960s, Krauss tested five dogs for their ability to detect explosives. Upon completion of the trials, Krauss sent two of the dogs to LEAA, and trained additional handlers from the Army and the New York Police Department.[20] The jurisdiction of war dogs was expanding.

If LWL was a creative response to supposedly novel enemy tactics, then the creation of the Military Working Dog Center was a new claim on the newfound relevance of maintaining a standing army. Like LWL, MWDC moved into a coordinating role to manage multi-sited research on military working dogs.

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[19] See See Robert G.W. Kirk, "In Dogs We Trust? Intersubjectivity, Response-Able Relations, and the Making of Mine Detector Dogs", *Journal of the History of the Behavioral Sciences* 50, no. 1. (Winter 2014), 1-36. It's very much worth noting here, too, that the Federal Bureau of Narcotics, the U.S.'s predecessor to the Drug Enforcement Administration, initiated its own drug-detective dog program sometime around 1946. The agency disbanded the project sometime in the late 1950s.

[20] Max Krauss, "Explosives Detecting Dogs", Technical Report No. 71-11. (Sep. 1971).

During the first two World Wars, various service branches used dogs to fight. In World War II, the Army created the K-9 Corps, which operated five training schools from 1942 until the end of the war. Following the war, the Army deactivated the K-9 Corps. The DOD's disorganized and impermanent relationship to military dogs, however, changed at the beginning of the Cold War. Beginning in the early 1950s, the Air Force made permanent a sentry dog school to provide air bases with security around missile silos and plane hangars. With so much emphasis on nuclear arms in the early phases of the Cold War, the interest in dogs that could patrol the same materiel is obvious. And, in 1958, the Air Force created its Patrol/Sentry Dog Training division. The creation of the latter school also signaled a new mode of using dogs for war in the U.S., namely its centralization at Lackland.

MWDC was a two-fold response to the Cold War. First, there was the Center, responsible for acquiring all dogs for the DOD, and coordinating dog-research contracts awarded by the Air Force. Second, there was the Air Force Patrol Dog Training School, the largest training kennel in the DOD. The school, importantly, was where all military dogs, regardless of service branch, received their initial obedience and command training. The centralization of services at Lackland meant that the size of Lackland accounted for its multiple uses. In 1970, Lackland could house up to 900 dogs, and averaged between 450 and 600 dogs, and up to 200 dog-handler teams in training at any given time.[21]

The Cold War produced a qualitatively different army vis a vis the U.S.'s pre-World War II military. One of the largest changes following 1945 was the maintenance of a large standing

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[21] Roland C. Olson, "Veterinary Aspect of Military Working Dogs", *Conference to Expand the Usefulness of Military Working Dogs*, (1970), 39-50.

Army. The effect on the dogs was just as important, as the creation of MWDC signaled that planners saw working dogs as central to a functioning military. Still, MWDC was not simply a vehicle for keeping a standing kennel of working dogs, but also a means for DOD patrons to coordinate and develop research that would supposedly improve the functions of working dogs writ large.

Like its Maryland peers at LWL, the utility of the dogs blurred between the collaborative effort as an end in itself, and the objective of training dogs for war.

MWDC's efforts at coordinating research on military dogs culminated in a conference in 1970 at Lackland AFB. Here, project managers laid out what they saw as the objectives of MWDC's existence. According to the conference's printed proceedings, the importance of MWDC's work was that working dogs "are so highly useful both within the military and elsewhere" and that they also provided an opportunity to "[seek] to provide answers for questions which haven't officially been asked."[22] Like their counterparts at LWL, MWDC was leading research into unknown places. But, it was also exposing profitable ventures for scientists, as "confidence in the long range benefits of research for the military dog program," would lead inevitably to "promising opportunities for research to aid in the selection and training processes."[23] In this sense, the dogs had value not only as a means of accomplishing the obvious military tasks for which they would be trained, but that research on them was its own potentially profitable commodity.

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[22] "Foreword", *Conference to Expand the Usefulness of Military Working Dogs*, (1970), i-iii. See also Charles Fuller, "Introductory Remarks", *Conference to Expand the Usefulness of Military Working Dogs*, (1970), 1-2. Quote on 2.

[23] "Foreword", *Conference to Expand the Usefulness of Military Working Dogs*, ii, i.

Still, what managers repeatedly harped on at the conference was the symbiosis between the military and research scientists. Lt. Col. Charles Fuller's view of the import of MWDC was that it could break down barriers to discoveries. "It is essential for all of us here, and many who aren't here," he claimed, "to act together, to communicate freely, and to establish research requirements which will encourage research and development."[24] Fuller described the Air Force's role in MWD vis a vis research to "support…the university community, non- and not-for-profit organizations, and industry."[25] The composition of the conference members is revealing in this regard, in terms of how planners envisioned the problems of detector dogs. With the obvious inclusion of numerous soldiers, the other attendees included university researchers, private research firms, and the researchers at otherwise non-research companies. For example, the only other researcher at the conference working on olfaction came from a scientist at Honeywell Corporation, an air-conditioning and refrigeration company. Was MWDC, like LWL, to be a contact point between research, industry, and defense?

Regardless, Fuller was even more optimistic. Indeed, he claimed that the utility of coordinating working dog research was that "we hope to create new and open channels of communication and a realistic understanding by the research scientists here of problems faced by the Air Force. We also hope that Air Force representatives here will better understand the capabilities, goals and objectives of the research scientists."[26] Fuller's claims, nonetheless, suggest that he suspected that in the DOD's and researchers' enthusiasm for collaborating, they had never learned to communicate in the first place. Insofar as MWDC was an attempt to solve the particular puzzles

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[24] Fuller, "Introductory Remarks", 1.
[25] Fuller, "Introductory Remarks", 2.
[26] Fuller, "Introductory Remarks", 1, 2.

that interested dog trainers and others, it was also an attempt to sustain a research ecosystem that supposedly operated on the energy of synthesis.

In addition to the formal research institutions at LWL and MWDC, one mid-1960s policy worked in the opposite direction of the push at MWDC. Initially known as Project MINERVA, Project THEMIS worked to decentralize university research that the DOD funded, focusing on the collaborative production of dog research between DOD managers and outside researchers.

Project THEMIS, according to one observer, was an attempt by President Lyndon Johnson's administration to "direct federal funds to improve research capabilities of institutions not heavily engaged in programs relevant to the mission of [the DOD], thus affording improvement of high quality research and education."[27] In effect, THEMIS was supposed to be a mode of distributing the bounty of defense research more widely than it had been, having been holed up in the Northeast corridor. THEMIS, importantly, funded projects dealing with the largest of materiel down to, for our purposes, detector dogs.

The goals of THEMIS aligned with and adopted the language of other Johnson-era programs, namely his War on Poverty. The same observer of THEMIS borrowed language conflating THEMIS with the redistribution of federal money — either upwards or downwards depending upon your perspective:

> [Project THEMIS] is now underway—a new untested program, designed to strengthen the university community, broaden the base of scientific endeavor, and provide new centers of excellence. It could be the basic step toward the development of much more than what appears at first blush. It could be an incentive to the true 'have not' colleges and universities to develop capabilities from whence they may leap to project THEMIS programs [sic], and may thus result in a spreading of centers of excellence away from the

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[27] Vern D. Calloway, Jr, "PROJECT THEMIS", *JAG Law Review* 10, no. 29, (Mar.-Apr. 1968), 29-32. Quote on 29.

> centralized selfperpetuating [sic] larger university complexes toward a much more geographically even distribution of high quality centers.[28]

The thinking went that if universities and the DOD could align the interests of individual researchers, university bureaucrats, and military funders, then the federal government could stake a leading role in the expansion of scientific inquiries, and bend research toward its applications to defense preparation.

Universities and potential project managers responded positively to the calls of THEMIS for new research plans. Upon the announcement of available funding through THEMIS, the DOD received 479 applications from 171 institutions.[29]

Two Project THEMIS grants eventually made their way to Mississippi and Pennsylvania, where Robert Berryman and David Moulton, respectively, began their initial studies of dogs for the DOD.

Taken together, LWL, MWDC, and THEMIS created an atmosphere ripe for the exploitation of canine labor for the purposes of narcotics control. Between the early 1960s and the middle of that decade, LWL and MWDC familiarized the military with the use of canines for irregular warfare, while all three showed the ways that the federal government sought to pursue technological innovation via collaborative research. It also shows how various parties attached subjective value to the work, as Joy Rohde has described of social scientists, through its abilities to stimulate creativity and exchange ideas. According to Rohde, "behind social-scientific rhetoric and creative institutional configurations" lay a means for scientists to "expound [on]

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[28] Calloway, "Project", 32.
[29] Calloway, "Project", 31.

counterinsurgency methods as peaceful techniques" and to "recast…the militarization of social knowledge as an antidote to Cold War militarism."[30]

"We Are Built to Predict and Control": Researchers, the DOD, and the Institutionalization of Experimental Sensory Research, 1945-1975

In this section, I explore the backgrounds of three researchers related to the detector dog programs, Robert Berryman, David Moulton, and Robert Lubow. Berryman, contracted through Project THEMIS and the MWDC, Moulton, the recipient of a Project THEMIS grant, and Lubow, contracted through the LWL, contributed in bigger and smaller ways to the detector dog programs. In addition to exploring their diverse paths to participating in the DOD's plans, this section reveals that these researchers, especially the social scientists, like their federal patrons, valued the utility of their projects as much for their ability to institutionalize collaborative research and make their work more seemingly scientific.

In addition to the invention and demonstration of biosensors by physiologists at LWL, the DOD and other parts of the federal government increasingly depended on the work of social scientists.[31] The trend of military funding for social scientists was not a novelty to the Cold War U.S, but began during World War II.[32] The historian Mark Solovey has claimed that the military became the largest patron of psychology during the Cold War. More to the point, the Cold War

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[30] Rohde, *Armed with Expertise*, 11.

[31] See especially Paul Erickson, et al., *How Reason Almost Lost Its Mind: The Strange Career of Cold War Rationality*, (Chicago: University of Chicago Press, 2013).

[32] Donald S. Napoli, "The Mobilization of American Psychologists, 1938-1941", *Military Affairs* 42, no. 1, (Feb. 1978), 32-36. On B.F. Skinner's attempts to contribute to the U.S. war effort, see James Capshew, "Engineering Behavior". See also Rebecca Lemov, *World as Laboratory: Experiments with Mice, Mazes, and Men*, (New York: Hill and Wang, 2005).

remained an optimistic time for scientific researchers, especially psychologists. During World War II, the military had employed 1,000 of the U.S.'s estimated 4,500 psychologists.[33] Despite an initial moment of worry about the elimination of defense patronage of psychology following the 1945 armistice, the war bubble-wrapped federal support for subsequent psychological research.

Robert Berryman was one of those psychologists. He had grown up partly in South America owing to his father's work as an engineer, and served in the Navy Air Corps from 1942 until 1946. After the war, he returned to school, graduating from Columbia University in 1947 with a bachelor's degree in philosophy, and, in 1954 he graduated from New York University with a doctorate degree in psychology, having worked on a project about the extinction of behaviors. By 1961, he held three concurrent faculty positions at Adelphi University, Hunter College, and Columbia; all in New York City.

Even in his earliest work, Berryman's interests belied his fascination with empiricism and research design. When he died in 1982, one of Berryman's former students described him glowingly as "a sorcerer of a kind…who seemed to gain energy from the very demons that the rest of us denied — the metaphysics of scientific materialism and of radical behaviorism."[34] His work already displayed an interest in quantifying and evidencing the supposedly scientific findings of his experimental work.

Berryman, too, was known to others as something of a jokester, drawing entertainment from his research. According to one memorable anecdote about his time at Hunter University, he had

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[33] Solovey. *Shaky Foundations*. Both the quote and the estimate appear on 59.
[34]RL Thompson and JA Nevin, "Remembering Robert Berryman (1921-1982)", *Journal of the Experimental Analysis of Behavior* 40, no. 3, (Nov. 1983), 215-216. Quote on 215.

taught a mynah bird to speak. From Berryman, the bird learned two phrases — "I am an organism" and "how do you do," but the parrot proved to be a sharp student. Soon, one of the lab members' wives and the Irish security guard added to the bird's vocabulary "go man, go!" and "some Gaelic invective."[35] The parrot, according to the anecdote's author, very much became a fixture of, if not a mascot for that Hunter College laboratory.[36] In this sense, the parrot was the most public-facing of Berryman's work up to that point.

Importantly, Berryman managed labs at Columbia and later Hunter using government money, or as one source put it, "at Columbia…the government paid the bills."[37] Even in this earlier period, Berryman proved to be an able candidate for securing federal patronage for behaviorism — largely patronage from the National Institutes of Health, the National Science Foundation, and the U.S. Public Health Service. At no point in his early career did DOD contracts find their way into his lab.

He was also, seemingly, tied to New York City's psychology universe. There were obvious benefits to the way that Berryman produced his research. First, he proved an able, successful applicant for federal funding. His research was not only important to the field of experimental psychology and behaviorism, but important in terms of securing funding. Second, Berryman's success in securing funding suggests something even more important — that the federal government and behaviorists had productively cross-pollinated in the years during and after World War II.

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[35] Thompson and Nevin, "Robert Berryman", 215.
[36] Thompson and Nevin, "Robert Berryman", 215.
[37] Thompson and Nevin. "Robert Berryman", 215.

Berryman's traveling itch again emerged in 1964, when he removed himself to Brazil. He accepted an offer to join the faculty of a new Brazilian university research program at the University of Brasilia, a fledgling university founded in 1961. Under the direction of Fred Keller, one of Berryman's former colleagues at Columbia, Berryman joined other New York-based experimental psychologists, including Carolina Bori.[38] The initiative was intended to bring behavioralism and behavioral analysis to the fledgling university in Brasilia, the city itself founded only a year earlier in 1960. According to one source, Berryman and Keller's arrival brought B.F. Skinner to Brazil.[39]

The novel program at Brasilia ran into problems early on. Brazil's military coup in spring 1964 eventually resulted in the gutting of the University of Brasilia's psychology department. Following the university faculty's opposition to a series of arrests of other faculty members, Keller's wonder-team of psychologists dispersed. Except Robert Berryman.[40] He remained at the University of Brasilia until 1970, when he returned to the United States.

Berryman's publishing career effectively ended in the mid-1960s, when he left for Brazil. While he assisted in and managed in other published work, he no longer assumed the status of principal investigator. His time in Brazil was significant, if for no other reason than it shook

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[38] On Fred Keller, behaviorism, and Brazil, see Atsushi Akera, "Bringing radical behaviorism to revolutionary Brazil and back: Fred Keller's Personalized System of Instruction and Cold War engineering education", *Journal of the History of the Behavioral Sciences* 53, no. 4, (Autumn 2017), 364-382.

[39] Angela M.B. Biaggio, "Brazil", Kazdin, A.E. (Ed.). *Encyclopedia of Psychology, Vol. 1*, (Washington, D.C.: American Psychological Association: 2000), 464-466. See also Joao Claudio Todorov, "Behavior Analysis in Brazil", *Avances en Psicologia Latinoamericana* 24, (2006), 29-36.

[40] Todorov, "Behavior Analysis in Brazil".

Berryman out of a mold that he had inhabited since graduate school in New York. It also took him out of New York for the first time in his professional life.

Although Berryman scurried from one place to the next throughout much of his post-1964 life, his research suggested a remarkable amount of continuity. In large part, his work examined the basis of learned and unlearned behaviors; basically the psychological theory of "extinction." Much of that research, too, came by way of teaching pigeons to alert when it recognized patterns. Regardless of the merit of his research, it still remained mostly a series of laboratory tricks or demonstrable behaviors with no clear application.

Upon his return to the United States, Berryman bypassed New York, taking up shop at the University of Mississippi. Importantly, this return to the United States, and Berryman's position at Mississippi, entangled Berryman and newer efforts to improve the Military Working Dog program and stimulate innovation in drug detection. Berryman was, in Mississippi, living off of the largesse of Project THEMIS.

It's worth noting here that all of Berryman's research had, until his tenure at Mississippi, run on government contracts. Federal and federal-adjacent sponsors, including the National Institutes of Health, U.S. Public Health Service, and the National Science Foundation, subsidized his research on olfaction and scent discrimination. This DOD contract was a novelty for Berryman.

Berryman's landing in Mississippi is curious. For so much of his life in the twentieth century, he had been a New Yorker. And, why not? New York City provided multiple opportunities for someone of his fame in the scientific community broadly, and within the microcosm of New York's university biome. The sudden interruption to his publishing career in the mid-1960s coincided with his departure to Brazil. It is also important to remember here that part of the

objectives of his time at the University of Brasilia was to establish a psychology department on the basis of behavioral research. So, how to explain how this near lifelong New Yorker ended up in Oxford, Mississippi?

Again, the military's role in subsidizing sensory research on dogs is helpful insofar as it helps to establish a pattern of work that fit multiple motivations, not just drug control. Further, Berryman's adventure at Brasilia indicates that he valued the opportunity to create and manage departments such that behaviorism was not just an idea, but the basis of academic departments and research labs.

It is also worth reflecting on the fact that of the research completed at the University of Mississippi under the biosensor contract to Berryman was, instead, the work of junior scholars. Put more simply, Berryman was the figurehead for research that supposedly pushed his own research interests forward. Together, Berryman's near abandonment of research publication, as well as his mission in Brazil, suggest that it is possible that Berryman had gone to Mississippi with a mission similar to that of his time in Brazil — to invent and train a psychology department using his status and experience in creating new departments, and his prowess in securing federal patrons.

It remains unclear how Berryman came into contact with MWD. It is possible, however, that Berryman's contracts with federal entities back in the 1950s had made him a familiar name around the offices of military-based researchers and administrators.

Still, Berryman's initial work for the DOD ended up at the MWD conference at Lackland in 1970. There, he set forth some of the more aspirational rather than realistic opportunities ahead. According to Berryman, "the principles [psychologists] have established in carefully controlled

laboratory work are bringing about some revolutionary changes in our understanding of the problems of human education." Further, Berryman indirectly tied his work to past work in behaviorism: "largely due to the pioneering work for Professor B.F. Skinner and his school, we are beginning to see the emergence of a true technology of education in which principles derived from controlled experiments are put to use in the solution of practical problems."[41]

At least in front of this audience, Berryman claimed an even more stable basis for his work. In the same breath as his claims about the principles of psychology, he also proposed that, "we may think of 'behavioral' engineering…in the same way as we speak of chemical engineering when chemical principles are used in the production of a given compound or substance." Still, Berryman cautiously advised his audience that the translation of principles to technologies often lagged. Instead, when it came to training military working dogs, Berryman lauded the enterprise. Speaking on the payoff of interdisciplinary research, he claimed "all of these considerations show the need for and importance of a conference such as this one, where contact among the persons working on various aspects of a common problem provides for a great deal of corrective feedback."[42] Even in the context of psychology's supposedly more stable role as an engineering science, Berryman harkened to the collaborative efforts that surrounded such empiricism.

Despite the profusion of cash for this project, it actually evolved in order to fit the MWD program. In an experimental report from 1972, Berryman reflected on his journey to researching working dogs. Having found that a planned experiment was unfeasible, Berryman instead

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[41] Berryman, "Toward a Training Technology for the Military Working Dog", *Conference to Expand the Usefulness of Military Working Dogs*, (1970), 69-77. Quote on 69.
[42] Berryman, "Toward a Training Technology", 71.

decided to "shift the focus of research to studies directly relevant to…the military working dog in stimulus detection and recognition tasks."[43] Berryman's admission here is all the more important when considered against the past work done at LWL with scout and detector dogs earlier in the same decade. On the one hand, it illustrates all the more how researchers looked to dogs as a kind of swiss-army knife for new war applications. On the other hand, it raises an interesting problem regarding the substance of social-scientific research.

Berryman retrofit his research to experiment with working dogs. This was not his initial intention. Instead, when he had first applied for what became the dog project, he was working with birds, "using birds as pattern detecting [sic] components in automated machine systems."[44] Yet, as he continued, the nature of dog work suggested something permanent — "work is also under way on an automated procedures [sic] for training dogs to search for, detect, and report odorous substances of *military importance*."[45] The category of military importance was, to be sure, a different value than discovering principles of psychology or transitioning psychology to the status of a lab science. As I show later, "military importance" became a mysteriously powerful cudgel against this system.

The case of David G. Moulton reinforces and contradicts some of Berryman's paths into detector-dog research. Moulton was a physiologist working on olfaction at the borderlands between physiology and psychology. Like Berryman, Moulton was equally peripatetic: born in 1928 in Bombay, India, he grew up in Portugal and Scotland. He earned his bachelors of science

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[43] Robert Berryman, "Final Report: Sensory Capacity of the Military Working Dog", (National Technical Information Service: 1972). 1.
[44] Berryman, "Final Report", 1.
[45] Berryman, "Final Report", 2. Emphasis mine.

from the University of Glasgow in 1954, and immediately began research on mine-detector dogs through the Royal Army. In 1956, he moved to Birmingham, England to begin his doctorate in anatomy. In 1958, Moulton arrived in the United States by way of Stanford University, subsequently spending time at both Florida State University and Clark University, in Tallahassee, Florida and Worcester, Massachusetts respectively. In 1969, he became a professor of physiology at the University of Pennsylvania, in Philadelphia, Pennsylvania.[46] He stayed at Penn until his death in January 1981.

Prior to his emigration from the United Kingdom, Moulton had been involved in canine physiology research as early as the early 1950s in his home country. His work began with proving the abilities of mine-detector dogs, using government funding to accomplish his work for the British counterpart to MWD, known as the Royal Army Veterinary Corps' War Dog School. While part of the effort to demonstrate canine olfaction, he worked at the University of Glasgow's zoology department.

Moulton's purse strings at Birmingham, and later at Stanford and Florida State in the early 1960s, were not attached to defense preparation. At Florida State, his main benefactor remained the Public Health Service.[47] The focus of his research in this period was on industrial research, for the area of sensory research remained largely the pursuit of industrial chemists and corporations with interests in products as diverse as, for example, air conditioning and perfume.

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[46] A.L.C. Moulton, "David Moulton - A Human Perspective", *Chemical Senses* 6, no. 4, (Jan. 1981), 237-239. Moulton has already been the subject of some research on the training of detector dogs. See Robert G.W. Kirk, "In Dogs We Trust? Intersubjectivity, Response-Able Relations, and the Making of Mine Detector Dogs", *Journal of the History of the Behavioral Sciences* 50, no. 1, (Winter 2014), 1-36.

[47] D.G. Moulton and D. Tucker, "Electrophysiology of the Olfactory System", *Annals of the the New York Academy of Sciences* 116, no. 2, (Jul. 1964), 380-428.

It was at Clark University that Moulton received his first Project THEMIS grant for a project intended to study both the regeneration of olfactory cells, as well as to estimate "the minimum number of molecules necessary to stimulate a single receptor."[48] "Until recently," Moulton claimed in his report, "few reliable quantitative data have been available (for species other than man) that would allow a reappraisal of this question of species differences in detection thresholds." It was the second aspect of this study, the quantification of smell reception, for which Moulton claimed that "we shall briefly outline the techniques involved [in measuring smell reception], since they are especially critical in assessing the validity of olfactory data."[49] Basically, Moulton and his team set up chambers in which rabbits sniffed for amyl acetate and interrupted a laser when they crossed it. Adding a gas chromatograph and implanted electrodes, his team measured air concentration and the supposed response to stimulation of the rabbits' olfactory bulb. As if presenting their findings on a silver platter to the DOD, their report proposed that, "one variable particularly significant in determining the level of human olfactory performance…as well as that of other species, is the amount of training received."[50]

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[48] D.G. Moulton, R. Celebi, and R.P. Fink, "Olfaction in Mammals — Two Aspects: Proliferation of Cells in the Olfactory Epithelium and Sensitivity to Odours", Reprinted from *Ciba Foundation Symposium on Taste and Smell in Vertebrates*, 1970, 227-250. Quote on 227. RG 0342 U.S. Air Force Commands, Activities, and Organizations Air Research and Development Command/Air Force Office of Scientific Research, Scientific Research Studies 1952-1974, AFOSR-TR-72-0622/Univ. of California/Aerospace and Mechanical Engineering, 1971 THRU AFOSR-TR-72-0680/Harvard University/Physics, 1971— Box 296, AFOSR-TR-72-0641/Clark Univ./Olfaction of Animals, 1970, OLFACTION IN MAMMALS—TWO ASPECTS: PROLIFERATION OF CELLS IN THE OLFACTORY EPITHELIUM AND SENSITIVITY TO ODOURS.

[49] Moulton, Celebi, and Fink, "Olfaction", 238.

[50] Moulton, Celebi and Fink, "Olfaction", 242.

From the perspective of the DOD, Moulton's work looks to have been valued under their research category of "detection, surveillance, navigation, and control." Moulton's work, then, existed alongside projects investigating lasers and automatic navigation.[51]

Moulton multiplied this research into, at the least, four publications, ending up at the University of Pennsylvania in 1969. Moulton's immigration to the United States, and especially his new residence at Penn were fortuitous in other ways. In the same year of Moulton's move to Philadelphia, physiologist Morley Kare became the first director of a recently formed philanthropic research institute, Monell Chemical Senses Center (MCSC). MCSC was, at the time, an oddity. The center enjoyed an early stimulus, in 1967, when the Ambrose Monell Foundation — source of MCSC's name — contributed $1 million to Kare to initiate research into the chemical basis of human senses, including vision, hearing, and smelling. In their own self-published history of MCSC, the institute grew out of agents from the National Science Foundation and the Veteran Affairs Administration encouraging Kare to continue his research in food choice — ie, what is sweetness? what is saltiness? Further, the initiative was a novelty at the time, as a history of the institute quotes Kare as claiming that "Monell itself is a scientific experiment."[52] This is all to say that Monell grew up quite suddenly at the time that other researchers looked to the psychology of animal learning.

MCSC also connected various parties interested in sensory research. Those parties included, but were not limited to industrial scientists, academic researchers, as well as military bureaucrats. In this sense, Monell mirrored other public-private-academic models happening elsewhere in

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[51] Calloway, "Project", 32.
[52] Unknown author. "History", Monell Center, unknown date. www.monell.org/about/history.

drug control research, and in American research more generally. In fact, when Monell moved from its initial building to its home since 1971, the institution ended up in the geographical manifestation of such public-private-academic ventures — a neighborhood in West Philadelphia at the time recently renamed "University City."[53] One historian has described the park-like environment around Monell as intended to be "a place where scientific innovation literally existed next door to the commercial application of technology."[54] MCSC was one of the proud benefactors of the neighborhood's renovation efforts.

The University of Pennsylvania also benefitted from Moulton's tenure in Philadelphia. In addition to Monell, the University of Pennsylvania was not only active in supporting Penn faculty with its own development grants, it also actively courted a working relationship with the federal government. One the one hand, Monell's scientific staff had homes in various departments of the University of Pennsylvania's School of Medicine. Monell continued Penn's earlier mission to establish what became University City. In addition, as historian Margaret O'Mara describes the situation facing university officials in the early 1960s, "Penn was beginning to more actively explore the ways it could use its reputation as a research institution and as a significant federal grantee to create closer and mutually profitable ties with industry." The appeal of Monell to Penn, then, might be summed up as "a semidetached, private entity that would be able to carry on applied research without conflicting with the interests of academic departments, but would provide an outlet for university researchers and other scientists to apply their findings commercially."[55]

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[53] Margaret Pugh O'Mara, *Cities*, 166-172.
[54] O'Mara, *Cities*, 168.
[55] O'Mara, *Cities*, 167-168.

Like Berryman, Moulton benefitted from the grants available through Project THEMIS. But, he also benefitted from his connection to the University of Pennsylvania and the Monell Chemical Senses Center. Monell and Penn provided workspace for Moulton's experiments — dedicated lab space in which he could not only research the physiological aspects of animal senses, but then apply them to animal training. Moulton's work with canine olfaction pushed forward his particular method of inquiry — itself a slowly congealing consensus centered on the gas chromatograph as integral to an olfactometer.

Unlike Berryman, Moulton was not a psychologist, but rather a physiologist. His participation in the projects that produced the drug dogs suggests a powerful similarity with Berryman, though, the insistence on using the DOD to ply researchers with the resources necessary to their experiments. Further, unlike Berryman, Moulton never displayed as much of a chip-on-his-shoulder attitude about the empirical basis of his work.

Around the same time as Moulton's experiments, other MCSC researchers were pursuing research on the federal government's dime. Their work represented the diverse array of federal patrons available for seduction. Marilyn Getchell surveyed frogs and their olfactory receptivity for the Air Force. Robert Cagan investigated the sensation of taste for the National Institutes of Health, while D.A. Marshall experimented with a subminiature bio-telemetry transmitter for the same organization. Gary Beauchamp examined small rodents' attraction to urine for the U.S. Public Health Service. Clifton Baile and his team of researchers — attached to both MCSC and the private Smith Kline and French corporation — ran studies on animal feeding and adrenaline

for the National Science Foundation.[56] Private organizations, including the Rockefeller Foundation, rounded out the funders. Further, the University of Pennsylvania developed research initiatives with in-house funding — with a number of researchers at Penn receiving money from the University of Pennsylvania Plan to Develop Scientists in Medical Research.[57] Still, as the funding for other researchers suggest, MCSC went hand-in-hand with federal subsidies. MCSC was both a physical site for performing research on olfaction, but also an institutional bridge linking researchers with federal patrons.

Moulton's participation shows how private organizations, and whole universities, attempted to wrangle support from the DOD. In effect, dogs were a great binder between human and institutional actors.

When Robert Lubow joined L.W.L. research on detector dogs, he had already translated his academic career into a science firm, Behavior Systems, Inc. His career reveals the linkages between psychology and the research industry. As early as 1959, Lubow, a doctoral graduate of Cornell University, arrived at General Electric in Ithaca, New York, performing behavioral

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[56] Marilyn Levisohn Getchell and Robert C. Gesteland, "The Chemistry of Olfactory Reception: Stimulus-Specific Protection from Sulhydryl Reagent Inhibition", *Proceedings of the National Academy of Sciences of the United States of America* 69, no. 6, (Jun. 1972), 1494-1498. Robert H. Cagan, "Biochemical Studies of Taste Sensation I. Binding of ^{14}C-labeled Sugar to Bovine Taste Papillae", *Biochimica et Biophysica Acta* 252, no. 1, (Oct. 1971), 199-206. D.A. Marshall and G. Celebi, "A Tunable Subminiature Bio-Telemetry Transmitter", *Physiology and Behavior* 5, no. 6, (Jun. 1970), 709-712. Gary K. Beauchamp, "Attraction of Male Guinea Pigs to Conspecific Urine", *Physiology and Behavior* 10, no. 3, (Mar. 1973), 589-594. Clifton A. Baile, et al., "Adrenergic Agonists and Antagonists and Feeding in Sheep and Cattle", *Life Sciences* 11, no. 14, Part 1, (Jul. 1972), 661-668.

[57] Chai-Ho Lo and Tony Ma, "The Plasma Membranes of Bovine Taste Pappillae: Polyacrylamide Gel Electrophoresis of Circumvallate Membrane Proteins", *Biochimica et Biophysica Acta* 307, no. 2, (May 1973), 343-352.

research on mice.[58] He moved on to North Carolina State University in the 1960s, teaching at N.C. State between 1963 and 1970. In 1971, Lubow ended up in Israel, teaching psychology at Tel-Aviv University. His appointment at Tel-Aviv as a professor of experimental psychology marked the first decade of the program's existence, as it had only come into being in 1961. He eventually accepted contracts from the Israel Defense Force after his immigration to Israel, where he performed research on bomb-detecting dogs.

Lubow's move south was auspicious. First, he took a position in academia after just a few years in industrial research. Leaving industry, he potentially saw himself as returning to a world in which the scientist could freely pursue his interests. He once described his own labors in similar terms as LWL researchers; "the experimental psychologist as a human-problem solver, sharing the concerns for continuing and improving our human condition even as he experiments with animals for war".[59] Second, he had successfully translated his lab work into a self-sustaining business; Behavior Systems, Inc., a business he started in 1966 with Dr. Gene Bernard. Third, the location of Lubow's work in Raleigh was no less auspicious, as the small city at the time was experiencing a boom in scientific research of its own. In the roughly two decades prior to Lubow's move, Raleigh was being pulled into the orbit of the geographical novelty, the Research Triangle.[60]

Keeping with their emphasis on novel inventions, the L.W.L. rewarded Behavior Systems, Inc. in 1968 with a contract to develop "a dog-based system" for military operations. For their part,

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[58] Robert E. Lubow, "A Spatial Gradient for Exploratory Research", *Psychological Reports* 5, no. 3, (1959), 293-296.
[59] Lubow, *The War Animals*, 19.
[60] O'Mara, *Cities*, 216-217.

Lubow and Bernard "were trying to convince the Air Force that if they were going to seriously pursue the use of organic systems for reconnaissance and intelligence…then a generalized, broad-scoped development program would have to be developed."[61] Lubow regarded the eventual dog contract with some temerity, though. According to his own memoirs of his time training animals for war, "I had never even had a dog for a pet. The only furry animal with which I had more than a casual acquaintance was the white rat. My partner…was in no better shape, although he claimed to have once had his picture taken while sitting on a horse." The strange pairing of Lubow and a dog-based application of his research was, however, more grave than his levity suggests. Lubow notes in those same memoirs that "our only military-animal experience had been with pigeons, and even that was in the laboratory or was part of a paper-and-pencil exercise."[62]

Like Berryman, he was quick to assert the scientific basis of his research. Lubow published his own account of his experiments less than half-a-decade after the experiments, in which he described his time working on DOD contracts. While he claimed that his work was imminently applicable to "real-world problems," he also defensively framed psychology vis a vis the hard sciences. "Although most scientists ask questions about the inanimate world," Berryman continued, "there is also the science of psychology, which concerns itself with empirical investigations of behavior. Not mind, not soul, but behavior."[63] His emphases here are important. On the one hand, this was a defensive move in an era when parapsychology was

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[61] Lubow, *The War Animals*, 87.
[62] Lubow, *The War Animals*, 176-177.
[63] Lubow, *The War Animals*, 2.

losing its luster but still tied to psychology departments, and it also repeats psychologists' claims to empiricism.

It's hard to disentangle Lubow's claims about empiricism and application from his role as the owner of a research firm. Unlike Berryman and Moulton, Lubow owned his own research firm. The need to stress application to military situations was as much a business decision as one of reason, experimentation, and discoveries. Application was, perhaps, the defining feature of research firms vis a vis universities. It's also worth noting here that Lubow's time at N.C. State and at Behavior Systems, Inc. overlapped. Is it possible that Lubow leveraged his academic appointment to earn a grant from LWL? In what ways did his academic career influence his business career? Answers to these questions, unfortunately, are not as forthcoming.

Lubow, unlike Berryman and Moulton, helps to place the ways that private corporations leveraged university appointments to funnel research to themselves. It also shows how dependent the DOD had become upon utilizing outside researchers to augment their materiel, and how researchers viewed the benefits of their relationship with the DOD.

Up in Smoke: The Army's First Dope Dogs and the Elusive Military-Science-Industry Complex, 1969-1973

Some three years passed in between when Berryman, Moulton, and Lubow first conducted their dog projects, and the translation of that research into a feasibility project for narcotics-detection dogs. Following John Romba's successful demonstration case in 1971, subsequent research followed, continuing in the mould of the collaborative system that had propped up the dogs, while other federal agencies recognized the dogs as valuable assets.

Despite initial enthusiasm for the research project, it quickly withered on the vine. The application of animals to war, especially a drug war, did not result in automatic approval. Instead, a steady chorus of detractors of the postwar research system had been active since 1968, as Congress passed its first limitations on research explicitly tied to warfare. Indeed, some politicians directly acknowledged the animals in polemics directed at the DOD. As I also show in this section, opponents of that system utilized a language of "military importance" and "direct military application" that challenged, directly and indirectly, the claims of researchers including Berryman and Lubow about the translation of their research into working military practice.

This section describes the beginning of DOD research explicitly concerning narcotics-detector dogs, and the formation of an opposition to the system of research that produced drug dogs.

John Romba's experiments with two Labrador puppies took place in late 1971. Following a request from the Military Assistance Command Vietnam (MACV), the U.S.'s commanding authority in Vietnam, Romba began the study to evaluate first, whether canines could distinguish the smell of heroin, and whether or not they could be trained to routinely identify it on a search team. This is to say that the proximate cause of Romba's study was the sudden panic over heroin among US troops stationed in Vietnam.

Considerable momentum pushed Romba's experiment forward, in at least three ways. First, there was newfound interest from the White House in the larger issue of heroin use and trafficking, and the particular problems of drug control as a series of interlocking practices. Nixon declared just months earlier the beginning of a war on drugs. Second, there was the existing preference at LWL and among sensory researchers for translating experiments on dogs into working canine military teams. Third, researchers and project managers had long nurtured a

belief that the free exchange of information yielded measurable, quantifiable, and practical results.

The basis of the study, as Romba claimed, was "largely the product of unreported exploratory work in odor discrimination and behavior control techniques…at [LWL]." According to Robert Lubow's memoirs, LWL's decision to initiate a project on narcotics detection came about because of its easy translation from existing research programs in mine detection. He explicitly claims as much, explaining that "the success of the mine-detecting dogs prompted the? U.S. Army to request information from [LWL] on how to train dogs to detect heroin." [64] Two factors for which neither acknowledged or admitted influenced this exchange. First was the publication of all the work that had preceded the project; that work had been out in the open for some time, most recently as July 1971.[65] Second was the ping-pong'ing of information between researchers and institutions. That exchange was not, as Lubow might have hoped, simply one-way, but rather based on the complex of researchers and research after World War II.

Romba used two dogs, both Labradors, to investigate the feasibility of training dogs for drug detection. Romba's study covered, in large part, how to create a training program for the dogs, and studied which reinforcements best taught dogs to discriminate the peculiar scent of processed heroin. Marshaling the scientistic language that the other dog social scientists, both Berryman and Lubow used, Romba explained that "an attempt was made to determine

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[64] Lubow. *The War Animals*, 209.
[65] R. R. Haney, John A. Bedford, and Robert Berryman, "Schedule control in the White-Necked Raven, *Corvus cryptoleucus*", *Psychonomic Science* 23, no. 1, (Jul. 1971), 104-105.

empirically whether dogs could detect heroin and, to a limited extent, if good search performance for heroin could be obtained."[66] He judged the experiments a success.

That Romba judged his experiment a success paralleled enthusiasm for narcotics-detector dogs elsewhere. Enthusiasm, if the phenomenon can be accurately reduced to enthusiasm, multiplied quickly for such a project. (Illustration 4) In the same year, the U.S. Bureau of Customs placed orders to MWDC to purchase and train their own drug detector dogs. Drug-dog handlers even put on a performance of the dogs' abilities to President Nixon, performing a demonstration in the Rose Garden.[67]

Closer to home, Romba's seeming success resulted in additional contracts to assess still further the conclusions of the project. This time, however, the contracts went to the Southwest Research Institute (SWRI), a private research firm formed by a former oilman outside of San Antonio, Texas.[68] SWRI had existed since 1947, when Thomas Baker Slick, Jr., an established oilman, decided to put his money behind scientific, engineering, and technological innovation. Slick's investments also included the Texas Biomedical Research Institute, also in San Antonio. The company carried out experiments and data analyses for projects involving modular home building for the Housing and Home Finance Agency to non-destructive missile and rocket testing for NASA. Like Biosearch Company and Behavior Systems before it, SWRI continued along the

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[66] John J. Romba, "Training Dog for Heroin Detection", Interim Report, (Sep. 1971). My emphasis. *Defense Technical Information Center*. http://app.dtic.mil.

[67] On Customs, the DOD, and drug dogs, see "U.S. Will Sic Dogs on Pot-Loaded Mail", *Chicago Tribune*, (Sep. 3, 1970), p .1. On the Rose Garden demonstration, see William B. Mead, "'Pot' Sniffer Shows Stuff", *The Atlanta Constitution*, (Oct. 15, 1970), 1A.

[68] On SWRI's contracts see E.E. Dean, "Training Dogs for Narcotic Detection", Final Report. (Jul. 1972). C. William Hall, "Narcotic-Explosive Detector Dogs", Final Report, (Jan. 1973), *Defense Technical Information Center*, http://app.dtic.mil.

Illustration 4

Cannabis-sniffing dog. Source: Rich Groscost, in *Leatherneck* 55, no. 3 (Mar. 1972), 40-43. Image on 40.

JOINT-SMOKER'S NEMESIS

Story by LCpl Bob Weir
Photos by SSgt Rich Groscost

SERGEANT Lloyd Barchers and his 80-pound, black and tan German shepherd dog, "Duke," entered the barracks where packages of marijuana covered with burlap had been hidden hours before.

As the team approached the wall locker in the corner of the room, the dog got excited. Then, the dog leaped and began tearing at the metal door and barked ferociously. Barchers opened the locker door, and concealed in the corner was an olive drab packet containing dope. It had been masked with perfume, but the dog

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path that research firms and university scientists had carved out in the near decade that had passed since the first dog contracts to private firms. The situation appeared primed to satisfy one of the goals of the earlier MWDC conference, that of producing new research niches through detector-dog research.

But, despite the momentum that surrounded this and similar dog projects at LWL and MWDC, and even in a political climate supposedly welcoming to more effective, if not creative solutions to drug control, the final reports on detector dogs, and specifically narcotics-detector dogs dropped off near immediately after 1973. Subsequent research on narcotics-detector dogs all but disappeared, while research on mine-detection dogs more slowly dissolved. By 1975, the research track, and the Limited Warfare Laboratory disappeared. MWDC remained, but the focus of training working dogs shifted almost completely to explosives detection.

How did a promising research track, especially one tagged to a newly urgent social concern — heroin abuse, disappear? What killed off the dog program? Here, I describe the opposition to the programs that had produced narcotics-detector dogs, and the motivations behind that opposition.

The first rumblings that narcotics-detector-dog research might be imperiled came two years earlier, in 1969, when the Senate amended the Military Authorization Act of 1970. Senator Mike Mansfield of Montana had led the charge, but the effects of the opposition wouldn't be felt for some years to come.

The Mansfield Amendments were two adjustments to the Military Authorization Acts of 1969 and 1974. Essentially, Congress signs an annual authorization bill, permitting the executive branch to pursue war as it sees fit. Senator Mike Mansfield, a Democrat representing Montana,

authored the bill. The initial bill implemented in 1969 set a new standard for evaluating the merits of a research contract, a seemingly obvious categorization of "direct or apparent relationship to a specific military function or operation."[69]

Critics of Mansfield's amendment quickly forecasted the effects that the bill would have on academic research. As one editorial in *Analytical Chemistry* wailed, "an amendment by Sen. Mansfield which passed with virtually no advance public notice has since caused apprehension, confusion, and secondary effects of a magnitude that still eludes estimation." The editorialist extrapolated still further, predicting a future in which what he called the "Mansfield effect" — an all-out assault on all government-funded research, regardless of relationship to the military — would rob the academy of useful work and training. "What is needed is a strong and continuing commitment to basic research in support of all mission-oriented technology, both in relation to research training at pre- and postdoctoral levels and in relation to research output. Anything less will waste the talents of a substantial group of skilled workers and will compromise our technological future."[70]

The effects of Mansfield's amendment also hit closer to home. Robert Lubow called out what he believed led to the sea change in the research system, decrying the "political hedonists, who, when faced with problems for which science had not yet provided solutions, attacked further expenditures in the theoretical sciences as being wasteful and frivolous." He also reserved special ire for what he saw as the "New Humanists" in psychology, whom he blamed for "their

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[69] Philip M. Boffey, "Mansfield Amendment Curb on Basic Research May Spread", *Science* 167, no. 3924, (Mar. 13, 1970), 1473.
[70] Herbert Laitinen, "Reverberations from the Mansfield Amendment", *Analytical Chemistry* 42, no. 7, (Jun .1970), 689.

efforts to vilify the experimental psychologist, to make science and human values antithetical, and in particular to evoke specters of fascism when discussing issues of prediction and control in psychology."[71]

However, in attempting to add the amendment to the 1971 appropriations bills, a House-Senate committee changed the wording to such that "unless such project or study has, in the opinion of the Secretary of Defense, a potential relationship to a military function or operation." Mansfield assailed the change, claiming that "it affirmatively states that the Department of Defense will solely determine what research is beneficial to it." Similarly, another amendment aimed at curbing DOD patrons from funding industrial research failed.[72]

Yet, Mansfield pressed forward with his slate of antiwar resolutions.

President Nixon's science advisor explained that the biggest consequences of the act were for more resource-intensive programs, during his meeting a Senate sub-committee meeting. During statements in the Senate regarding science policy in summer 1970, Lee DuBridge, the presidential advisor, claimed that:

> if DOD had been working only under budgetary restrictions…it would have cut out what it believed to be the less productive, the less valuable of the research activities that it was supporting. But with the Mansfield amendment it had to cut out some of our most productive and most valuable projects to the country because they could not prove their relevance to specific military purposes.[73]

DuBridge's strange admission, that the DOD was continuing to fund relatively insignificant projects rather than integral ones because of the Mansfield Amendment didn't go unnoticed.

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[71] Lubow, *The War Animals*, 6, 17.
[72] Philip M. Boffey, "Mansfield Amendment Not Yet Dead", *Science* 170, no. 3958, (Nov. 6, 1970), 613.
[73] Lee DuBridge quoted in "DuBridge and His Critics", Philip M. Boffey, *Science* 169, no. 3943, (Jul. 24, 1970), 356-357. Quote on 356.

Mansfield personally responded to DuBridge's claim, explaining the irony here, that DuBridge was admitting that the DOD funded cruddy projects.

The amendments were not, as critics including DuBridge later alluded, anti-technology or anti-science bills. In fact, a 1976 field report by Mansfield on Southeast Asia suggests something else entirely. According to that report, despite U.S. aid to Thailand decreasing as the U.S. left Vietnam, the U.S. would continue to fund population control aid and, importantly, "antidrug programs."[74] Even in that report, Mansfield pressed his concerns about the drawbacks of mixing American anti-drug tech, foreign enforcement, and the blurred boundaries between policing and military operations in Thailand. According to Mansfield's report, "after an investment of $8.5 million in equipment and advisers, plus the cost of an additional $2.6 million annually for regional U.S. Drug Enforcement Administration operations, there is little to show in Thailand for the American investment." The issue? According to Mansfield, the Thai police's apathy toward the heroin trade doomed any American interventions to expand narcotics control. Mansfield's searing critique, though, touched specifically on the materiel used: "until there is a much greater commitment [by the Thai government] to deal with the problem, putting more millions of American money into buying helicopters, radios, jeeps, and other fancy equipment for the Thai antinarcotics [sic] police will not have the desired effect."[75] If anything, Mansfield became a deficit hawk around what he perceived to be a wasteful system of war tech.

Mansfield was not the only politician critical of the various programs that the DOD had subsidized in the postwar period. During a testy exchange on the floor of the Senate in 1969,

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[74] Mike Mansfield, "Postwar Southeast Asia: A Search for Neutrality and Independence", Congressional Report, (Washington, D.C.: U.S. Government Printing Office, 1976), 3.
[75] Mansfield, "Postwar Southeast Asia", both quotes on 6.

William Fulbright challenged the defense research edifice head on. He claimed to have just read an article about research on birds of war, funded by THEMIS, in the *Washington Post*. Describing the project, Fulbright fulminated that "I might note that the only project listed for the University of Mississippi is one for 'Biocontrol Systems,' discreet terminology for work to turn birds into killers." Fulbright's emphasis in this discussion turned on what he saw as a preposterous experiment with birds. Having ridiculed the experiment's birds, Fulbright fumed "this contract illustrates quite clearly how far afield the Department of Defense has gone in financing research programs which have no direct relationship to legitimate defense needs."[76]

With Fulbright's ire burning red hot, the Department of Defense was in unfamiliar territory, a defensive position with regard to research and national security. Nearly two decades of staunch support for the research-military-industrial complex was crumbling. Dr. John S. Foster, Jr., the DOD's Director of Defense Research and Engineering, responded to Fulbright's fulminations. "Historically, animals have been used to perform a variety of tasks…dogs have been trained to perform a variety of tasks that have proved helpful to our military personnel in Vietnam. Dogs can detect buried and above ground mines, booby traps and trip wires and warn of the presence of these devices…It is our purpose in this research to extend and exploit [animals' capabilities] for use when our forces are exposed to situations such as those they now face." Indeed, the future of the military and military-funded science seemed to hinge on an antiquated device — animals — but put to work in new ways. Foster ended his appeal by waving the bloody rag:

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[76] J. William Fulbright, "Birds Alerted for War", Oct. 8, 1969, *Congressional Record*, Congress Session 91-1, 29139-29142.

"we believe that the potential of utilizing the unique capabilities of certain animals and birds should be investigated if by doing so human lives may be saved."[77]

The second Mansfield Amendment passed in 1973, cutting out the remaining legs of the program.

Insofar as detector dogs were an object of concern for legislators, their quick proliferation suggests a couple of happy endings, depending upon perspective. First, that the DOD and researchers including Lubow and Moulton successfully formalized research programs to fit the needs of narcotics detection. And second, that the roles for dogs that they had exposed — largely, higher-order sniffing and scent discrimination — outlasted the research programs themselves.

1973 represented something of a hard brake on narcotics dog research. Between 1966 and 1972, select dog researchers found a place for their work in defense research. However, in 1969, Congress passed the first of two Mansfield Amendments, adding a prohibition and reduction of DOD support for American universities. One casualty of the first Mansfield Amendment, as the amendment has come to be known, was Project THEMIS. The Mansfield Amendment effectively cancelled Project THEMIS. The second such amendment, in 1973, limited DARPA/ ARPA spending only to projects with direct military application. Over a decade later, in 1986, the namesake of the original amendments, Mike Mansfield, helped to amend the Foreign Assistance Act, explicitly targeting research funding for drug interdiction.

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[77] John S. Foster, "Response to J.W. Fulbright", Nov. 3, 1969, *Congressional Record*, Congress Session 91-1, 35723-35724.

Conclusion

The DOD's postwar collaborative research system spurred enthusiasm about using dogs in war in new ways. The DOD maintained the research system that had developed out of World War II long into the Vietnam War. On the one hand, the DOD created institutions meant to respond to changing enemies abroad. On the other hand, the Johnson administration refreshed those funding pipelines for new ends, including social redistribution. Further, as I've shown in this chapter, agents at LWL regarded their research as important in a number of ways, not the least of which were the supposed free exchange of ideas and the pioneering spirit of their work.

Berryman, Moulton, and Lubow represent multiple strands of the collaborative system that developed alongside and through experiments on canine olfaction and learning. They are also examples of how scientists understood the importance of their sensory research, of attaching significance to an experimental infrastructure, and foisting a definite economic value to that work. For Robert Berryman, the collaborative complex was responsible for helping to secure psychology as a science, and for making fundamental discoveries about behavior and sensory perception. Meanwhile, for, Robert Lubow, his work was, while perhaps superficially disagreeable, necessary and multifaceted in its consequences. The ongoing interest in detector dogs, and the novel interest in narcotics control projected that similar dog research would continue indefinitely.

Even the sudden interest in drug control couldn't protect the dog research program. Despite enthusiasm not only for using dogs in narcotics detection and existing momentum to continue the research pipeline, critics of that system temporarily eliminated the foundations of that research track. In the case of Robert Lubow, criticism of that system itself even stalked his own

laboratories, where his own staff registered their displeasure at research contracts signed with the DOD.

Extrapolating from the individual case of detector dogs to the larger, overarching edifice of the military's DAACP, the detector dogs reinforce this study's claim that the assumption of a broad-based drug control apparatus depended upon the novel and often tenuous linkages between private researchers and the DOD, the fresh fertilization of new and unconventional academic research in the 1960s, and the creation of new means of prosecuting war in the context of non-traditional military strategies.

This, too, reinforces all the more the sudden-ness of the DAACP and a more widely decentralized, but coordinated offensive against drug use in the early 1970s. The DOD and researchers alike appropriated existing opportunities — the Military Working Dog program, ongoing studies in dog learning and olfaction — to manufacture dogs that they considered appropriate for narcotics control.

Further, the transformative allure of narcotics control was never as transformative as it could have been, and was, arguably, incomplete. The potential funding pipelines that drug control opened never completely translated into technologies new or distinct to drug control, but were, in fact, only of use insofar as they could also translate into bomb and mine detection. Drug control never supplanted other tasks, though it remained a reason for maintaining dog training programs well after Vietnam.

The implications of this chapter have been that the edifice of drug control was brittle, and was always susceptible to outside interference. In the broader context of this project, the unique conditions that had made a detector dog program possible — the military-university-industry

complex, an emphasis on sensory work, and researchers' claims to scientific-ness and applicability — also contributed to the experimental program's demise.

Chapter 3

'You Just Puke and Earn Points': Treating Heroin Addicts at Fort Bragg, North Carolina, 1970-1971

Introduction: On the Origins of Alternative Medicines

Lieutenant General John Tolson effused in a 1970 interview with the *Washington Post*. He described what he saw as a unique opportunity posed by creating a secret, experimental heroin rehab unit at Fort Bragg, North Carolina: "civilian society…isn't equipped to take care of [heroin addiction and treatment]. The soldier is here and there's a big tax-payer investment in him. Rather than send him outside, let's take that man so he can perform."[1] Throughout the autumn and winter of 1970, print and television journalists applauded Tolson's experiment with military-based rehab, publicizing one of the Army's first attempts at responding sympathetically to heroin addicts through medicine. By November of the same year, Tolson appeared in front of a Senate subcommittee in Washington, D.C., repeating his motivations for treating addicted soldiers to American senators. The program — codenamed "Operation Awareness" (OA) — offered a treatment option to soldiers dealing with drug abuse, a treatment not yet acknowledged or allowed by the Department of Defense. In fact, OA was secret because military code — AR 645-212 of the Uniform Code of Military Justice — specifically prohibited such treatment, so Tolson kept it secret *from* the Pentagon. Until mid-1970, narcotics addiction was a reason to

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[1] Bernard Nossiter, "'Operation Awareness' at Ft. Bragg: Army Trying to Reclaim GI Addicts", *Washington Post*, Sep. 21, 1970, A1, A3. Quote on A3. There's a queer moment in social medicine history here: a head of the institution whose role in creating many of the analytical and practical problems of modern social medicine, positions said institution as the only actor capable of practicing social medicine with regards to drug treatment.

prevent drug-users from entering the Army, as well as a reason to discharge them. Tolson turned that relationship on its head: addiction might well be a reason to *keep* drug-users in the Army.

In this chapter, I examine the rise and fall of Operation Awareness, which existed from mid-1970 until mid-1971. Using testimony from commanders, local activists, related clinical staff, my own oral histories with staff, and the statements of OA's own patients, this chapter shows that local mental health activists and recent military social policies pushed Lt. Gen. Tolson to break from the military status quo and invent a treatment program.[2] However, conflict over the legitimacy of the medical diagnosis, on the one hand, and concerns about the military's social responsibilities, on the other hand, chastened the possibilities for the program's success. The rise and fall of OA were always bound to these twin uncertainties, at once institutional and administrative, and simultaneously diagnostic and therapeutic.

Lieutenant General John Tolson, the commanding officer at Fort Bragg, initiated the program in March 1970, following extensive meetings with local anti-drug activists. Tolson described the initiative both as an experimental opportunity, and a chance to heal soldiers whom society had abandoned. He drafted the Harvard-trained head of psychiatry and neurology at Ft. Bragg, Major Richard Crews, to design an in-patient rehab program. Crews tossed in some ipecac, fake heroin shoot-ups, and operant conditioning for good measure. (Illustration 5) Tolson's successor, General John Hay, was less invested in in-house addiction treatment at Bragg, and shuttered the program in June 1971. In the same month, June 1971, the Veterans Administration assumed general responsibilities for detoxing and rehabbing soldiers stateside. So, despite the short-lived

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[2] Chip Chapados, "Interview by Justin Hubbard", phone, (Mar. 30, 2016). Transcripts in possession of author.

Illustration 5

Simulated heroin injection, Operation Awareness, 1970. Source: Photographer not identified, *Los Angeles Times*, (Sep. 28, 1971), C7.

The Army copes with drug addiction in "Operation Awareness." A white-coated sergeant supervises a simulated heroin injection by two drug patients.

nature of the Fort Bragg program, it presaged a new moment: military medicine suddenly approached addiction through the clinic, rather than solely through military courts. Yesterday's criminals were today's sick people; deviants were now potential patients. The Army was suddenly intervening on addicted soldiers because they were sick, not because they were deviant.

The following chapter explores the invention of addiction as an illness in the Army, and is divided into four sections and a conclusion. Each section is oriented around specific actors and moments in the history of OA. In the first section, I discuss the background of Gen. John Tolson's decision to initiate OA, as told through Tolson's perspective and that of local mental-health activist Pat Reese. In the second section, I turn to the psychiatrist responsible for designing the program, Maj. Richard Crews, as well as some of the staff and staffing issues involved. In section three, I examine the available evidence of how soldiers experienced their time as patients in the clinic. In the fourth section, I turn to outsiders' reactions to Operation Awareness, as well as the end of Fort Bragg's experiment in in-patient rehab.

A "Sudden, Fantastic Increase": The Social Setting of Medical Alternatives; or, Tolson's Dilemma

Depending on one's perspective, Lt. Gen. John Tolson's decision to invent a treatment program in March 1970 can be blamed on or credited to Pat Reese.[3] Reese, a local investigative reporter

[3] I am intentionally using the verb "invent." Similar to Charles Rosenberg's claim in 1964, I treat diseases as a complex of biological state, social relations, medical thinking, and technological interventions, among other things. Rosenberg, *Cholera Years: The United States in 1832, 1849, and 1866*, 2nd Edition (Chicago: University of Chicago Press, 1987). See also, Charles Rosenberg, "Framing Disease: Illness, Society, and History", in *Framing Disease: Studies in Cultural History*, Charles Rosenberg and Janet Golden, eds, (New Brunswick: Rutgers University Press, 1992), xiii-xxvi. Charles Rosenberg, "What Is Disease?: In Memory of Oswei Temkin", *Bulletin of the History of Medicine* 77, no. 3, (Feb. 2003), 491-505.

for the *Fayetteville Observer*, was a self-described recovering drunk and addict himself, and a local activist for expanding Fayetteville's mental health and addiction treatment options. Following repeated listening sessions between the two, Tolson concluded that Reese's compelling descriptions of the suffering associated with both addiction and the absence of treatment options in the civilian and military communities, necessitated action. Tolson once described the Post's necessary working relationship with Fayetteville locals thusly: "I emphasize [OA] is a civilian-military effort," continuing that "there is no point of thinking you can solve this thing at Fort Bragg, within the post, and not to do something about assisting outside, and vice versa."[4] Tolson responded to Reese's requests due to some sense of empathy for strung-out soldiers, regard for the reputation of the Army and Fort Bragg, and a sincere belief in the Army as social leveler.

Was there a heroin problem at Fort Bragg, and how big was it? Were there precedents for the Army to get behind treatment? Whom did Tolson believe was the target patient population, and how did he come to believe that the Army should treat addicts? This section addresses the origins of OA by describing early anti-drug initiatives in the Fort Bragg community, Tolson's perception of the Army's relationship to its soldiers, and some of the political constellations orbiting Fort Bragg's drug abuse program.

Tolson only arrived at Fort Bragg in 1969, following a tour of duty in Vietnam. His career began in 1937 when he was a paratrooper stationed in Hawaii, and ended with his retirement as a deputy general in 1971, having been reassigned and appointed to the U.S. Continental Command

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[4] John Tolson, "Statement of Gen. John Tolson", in "Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics", Nov. 17 and 18, and Dec. 2 and 3, 1970, 91st Congress, Second Session, (Washington: 1971). 19.

(CONUS). Were it not for Tolson's decision to initiate OA, he may have only been remembered for what his obituary writer described as his stature as a pioneer of aviation. In fact, he was appointed to the Army Aviation Hall of Fame in 1975 for his accomplishments in air battle.[5] While stationed in Vietnam, he commanded helicopter units from the First Cavalry at both Khe Sanh and in the Tet Offensive. Interestingly, Tolson claimed that during his tour in Vietnam, neither heroin nor marijuana abuse had come to his attention. Drug problems, he averred, were just not an issue in his command. However, upon arriving at Bragg, Tolson fell face-first into an Army Post that locals and GIs alike claimed was flooded with all manner of narcotics and lesser drugs.[6] North Carolina, not Vietnam, made visible to Tolson the metastisizing problem of addicted soldiers. Even if drug use was not apparent to him in Vietnam, locals made sure that he recognized it at Fort Bragg.

Fort Bragg is, arguably, the least likely setting for a story about the Army's Vietnam-era heroin epidemic, in part, because so much of scholars' focus has been on Vietnam.[7] It shouldn't be,

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[5] Bruce Lambert, "Gen. John J. Tolson, 76, Dies; Pioneered Army's Helicopter Use", *The New York Times*, (Dec. 6, 1991), D21.

[6] Tolson's claim flies in the face of many of his contemporaries who claimed that Vietnam produced a cohort of heroin addicts. His claim also flies in the face of later scholars who have, arguably, overemphasized the Army's pursuit of drug use in the Vietnam theatre. But might pilots have had lower rates of addiction?

[7] A quick scan of the literature reveals that the majority of scholars interest in drugs and the military has largely focused on Vietnam. See Schneider, *Smack*. Kuzmarov, *Myth*. Frydl, *The Drug Wars*.

however.[8] Bragg is the largest Army Post in the United States. Its influence on the surrounding community is measurable. Bragg sits northwest of Fayetteville, a mid-sized city; the two held in umbilical suspension by Bragg Boulevard. Since its creation in World War I, Fort Bragg has been a permanent part of the Fayetteville landscape and social scene, a generator for immigration and emigration into and from the city and its surrounding counties. To wit, during the Vietnam War, the surge of troops — the number of soldiers at Fort Bragg compared to citizens in Fayetteville was almost one to one — earned Fayetteville the pejorative moniker "Fayette-nam." The effect wasn't just demographic. One estimate from 1970 regarding federal spending in the community reveals the extent of military influence: out of a total $409 million dollars that the community received from the federal government, $344 million came from defense funds! Further, locals reportedly estimated that upwards of sixty percent of the local economy depended on military pay rolls.[9] The economics of a military town blurred the boundaries between Post and city.

Just as the licit local economy depended on Fort Bragg, so did its illicit and illicit-adjacent economies. During World War II, the troop buildup at Fort Bragg resulted in a concomitant rise

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[8] To wit, many of the most celebrated studies of drug use and drug control in the U.S. have emphasized the drug markets and addict populations of New York City, Chicago, and Washington, D.C. Some other scholars, Dave Musto and David Courtwright, in particular, have discussed the importance of lesser-known sites in the history of narcotics; the morphine clinic in Alexandria, Louisiana, for example. However, the prevailing thrust is limited to big cities. David Musto, *The American Disease*. David Courtwright, *Dark Paradise: A History of Opiate Addiction in America*, (Cambridge: Harvard University Press, 1982).

[9] Doran Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", "Hearings on Drug and Alcohol Abuse in the Military", 215-239. 263.

in vice crimes.[10] Bragg earned an early reputation for debauchery, a reputation whose referent one anthropologist has described as Fort Bragg's perverse "carnival before death."[11] Whether you were a billiards hall owner or a heroin dealer or a pimp, soldiers meant steady business. To be sure, the Post had, historically, stitched together these neighboring vice markets. There are examples of soldiers and veterans connected to Fort Bragg being snagged in federal narcotics stings as early as the mid-1950s.[12] This is to say that there were precedents to show how Fayetteville's drug market further entangled Bragg and the city.

Still, a more engrained fear of this connection had to wait. By the mid-1960s, local civilian and military police found themselves arresting soldiers and civilians alike for lysergic acid and psilocybin mushrooms, and increasingly heroin. Doran Berry, a lawyer, local anti-drug/pro-treatment activist, and frequent collaborator with Pat Reese, claimed that the Dominican Civil War precipitated what he called "a sudden, fantastic increase" in the number of narcotics arrests. One estimate, put together in 1970 by Berry and Reese's military-civilian anti-drug committee

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[10] Doran Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", in "Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics", 215-239. World War II discussed on 200.

[11] Catherine Lutz, *Homefront: A Military City and the American 20th Century*, (Boston: Beacon Press, 2001), 133.

[12] Unknown author, "'Tea Nest' Raids Uncover 'Pushers' in Virginia and N.C.", *Pittsburgh Courier*, (Jun. 16, 1956), 31. In the article, the author claims that "the contact zone for marijuana in North Carolina has been in the vicinity of Fayetteville since the last two wars". "Dope Trial Will Open Here Today", *The Atlanta Constitution*, (Jan. 3, 1958). 37. Unknown author, "Ft. Bragg dope ring is smashed; four arrested", *Afro-American*, (Aug. 16, 1958), 19. Author unknown, "N.Y. Man Held in N.C. On Narcotics Charges", *New Journal and Guide*, (Aug. 1, 1959), 15. Author Unknown, "Charge GI's Sold 'Reefers' at Army Post", *The Chicago Defender*, (Feb. 18, 1961), 1. Soon after the February arrest, two more soldiers were busted for selling marijuana. Author Unknown, "Two Soldiers Charged as Dope-Peddlers". *New Journal and Guide*, (Dec. 30, 1961), 11. Author unknown, "Arrest Korean in Dope Haul", *The Chicago Daily Defender*, (Mar. 1, 1961), 3.

threw into relief the change that just five years had made: in 1965, there were seven total arrests for narcotics, whereas, in August 1970, police had already made 192 narcotics arrests. According to statistics that the State Department of Corrections gave to Reese and Berry, as of July 13, 1970, fifty-eight of the 143 prisoners in the state system for narcotics convictions had been sentenced in Cumberland County, in which Fayetteville lies. Further, the committee identified that soldiers overwhelmingly made up the suspect population of those arrests to the tune of 102 of the first 177 drug arrests in 1970 alone. Finally, they estimated that up to fifteen-percent of arrestees had an affiliation with the Army, either as dependents of soldiers or as veterans who remained in the city.

A situation one might credibly explain as "Mayberry enters the 1960s," locals described in more ominous terms. The "sudden, fantastic increase" was simultaneously an "onslaught on the pre-1966 cultures or frameworks."[13] Reese and Berry's committee referred to the increase more dramatically, explaining how the life-ways of young drug users might affect their own: "in effect, the American Indians suffered their plight because their cultures or frameworks for living failed under the onslaught of the white man."[14] The origins of an entrenched drug scene was not just a looming public-health emergency, it was cultural genocide made possible by heroin, the committee claimed.

Perhaps, surprisingly, Reese and Berry's committee didn't conclude that Vietnam was to blame for the sudden jump in narcotics arrests. Instead, they claimed that the U.S.'s brief intervention

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[13] Doran Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", "Hearings on Drug and Alcohol Abuse in the Military", 219.
[14]Doran Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", "Hearings on Drug and Alcohol Abuse in the Military", 217.

in 1965 in the Dominican Civil War had precipitated a rise in drug crimes.[15] What's more, they contended, the vast majority of the opiates came to Fort Bragg and Fayetteville from within the United States, especially Atlanta, Georgia, Richmond, Virginia, Washington, D.C. and New York City.[16] For members of this committee, the military was the primary generator of drug traffic in Fayetteville, importing big-city drug problems into small-ish town North Carolina.

It's debatable the extent to which these arrest statistics and estimates reveal an epidemic, or an actual increase in drug-use patterns. Indeed, as some criminologists and sociologists have argued, drug arrest rates — and arrest rates, generally — are more revealing of policing and recording practices than certifiable increases in the number of crimes or criminals.[17] What is less debatable, is the effect of these statistics. Locals began to see Fort Bragg as a port of entry for big-city drug problems. Reese and Berry's committee described fatalistically the effect of Bragg's proximity on Fayetteville's illicit economy: "it would be little more than a semantical

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[15] I think that how the committee explained the origins of this particular outbreak is surprising, insofar as commentators then and scholars now have emphasized the Army's response to American drug users in Vietnam.

[16] Pat Reese and Doran Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry".

[17] In fact, to complicate matters further, IBN's first full year of operation in 1969 correlated with "more drug arrests in Cumberland County [where Fayetteville is located] than in the rest of the state." Howard Covington, "Coordinated Campaign — A War on Drugs Is Paying Off", *Charlotte Observer*, (Mar. 8, 1970). Excerpted in Pat Reese and Doran Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", 212. For a primer on sociological and criminological debates about the relationship between arrest statistics and the number of crimes, see Colin Loftin and David McDowall, "The Use of Official Records to Measure Crime and Delinquency", *Journal of Quantitative Criminology* 26, no. 4, (Dec. 2010), 527-532. I don't mean to suggest that there is consensus about using arrest statistics, but, instead, that using such data is rife with controversy.

exercise to debate whether or not we would be in this trouble if Fort Bragg was not part of the community."[18]

In 1990, *The Night of the Living Dead* came to the U.S. silver screen. Somewhere in the background of the film, you'll see an elderly Pat Reese, Fayetteville's one-time junkie whisperer, mourning. Reese, it must be said, was a man of many overlapping talents. Born in 1927 in Hendersonville, in the western part of North Carolina, Reese served in the Navy before ending up in Fayetteville in the 1950s. In 1957, he joined the staff of *The Fayetteville Observer* as a reporter.[19] In 1961, he became the director of a local halfway home for recovering alcoholics. During the 1960s, Reese helped found the Fayetteville Little Theatre, where he was known for regularly performing as a particularly ghastly version of Dracula. He was also active with the Young Democrats and the Cumberland County Democratic Executive Committee, and served on the board of the North Carolina Mental Health Association. Suffice to say that in the 1960s, he was known around Fayetteville for his numerous hats — reporter, actor, educator, counselor, and confidant — and boundless energy.

Reese managed to gain the ears and hearts of enlistees and commanders, heads and lifers, and local and federal officials alike. He helped sow together Fayetteville and Fort Bragg's response to heroin, convincing Tolson that a drug-treatment program was a necessity. As a result, Reese played a large role in using the military to support Fayetteville's mental health options for civilian drug-users. How had Reese insinuated himself into the military milieu? How did locals begin to address what they perceived as Fort Bragg and Fayetteville's drug problem?

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[18] Doran Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", "Hearings on Drug and Alcohol Abuse in the Military", 215.
[19] Author unknown, "Pat Reese Obituary", *The Fayetteville Observer*, (Oct. 31, 2000), 1B.

Pat Reese constituted the city's first responder dispatched to heroin addiction. Reese had been involved in building resources for recovering alcoholics, and beginning in 1966, almost at the same time that the "sudden, fantastic increase" began, Reese started a drug lecture series with civilians and local police. Working alongside a local psychiatrist, Reese initiated weekly lectures targeted to teenagers already using narcotics. The local mental health center, the Cumberland County Mental Health Center (CCMHC) expanded their operations, joining with Fayetteville Technical Institute to train police officers, teachers, and various community agency directors in the basics of what they called "community mental health." As Reese and Berry's committee noted, Reese's lecture series constituted "the first school on narcotics" for local police.[20] I turn in more detail to local policing efforts in chapter four. For a time, Reese's lectures seemed to serve their purpose.

Reese did more to improve medical resources for addicts. In November 1969, Reese helped local mental health workers at CCMHC apply for a grant for expanded federal services under the Community Action Program (CAP). According to one historian, CAP "channeled federal antipoverty funds directly to local community action agencies, bypassing changing the usual structures of state and local government in order to create change at the grassroots level."[21] CCMHC was just such a plan that fast-tracked federal social welfare funding toward grassroots efforts. As part of their plan to increase services for drug abusers, the authors of their application explained that a four- to eight-bed inpatient unit, complete with a methadone detox program, was

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[20] Doran Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", "Hearings on Drug and Alcohol Abuse in the Military", 229-231.

[21] Elizabeth Rose, "Poverty and Parenting: Transforming Early Education's Legacy in the 1960s", *History of Education Quarterly* 49, no. 2, (May 2009), 222-234. 228.

in order, in addition to more room and staff. For Reese and a committee of mental health activists, the expansion of CCMHC was both a short-term solution to heroin abuse, and a long-term basis for improving the mental health infrastructure of the city.

Reese influenced a broader transformation of the existing mental health options in Fayetteville. His work with the CCMHC intensified the relationship between the Army and civilian resources. In their CAP application, the authors claimed that the initiative was positioned to succeed because Army physicians and psychiatrists had pledged to moonlight at CCMHC, that the constant presence of Army wives supplied a trained albeit inactive nursing population, and that a since-anonymous Army psychiatrist consulted them on developing a methadone detox program. This latter point was especially important, especially after OA opened. In fact, Fort Bragg was the only site in North Carolina in which a patient could access methadone legally.[22]

This is all to say that Tolson didn't invent a working relationship with Fort Bragg, though his efforts in OA did intensify them. Instead, locals Berry and Reese turned to the Army's drug abuse problem as the launching site for more collaboration between city leaders and Post commanders. Regardless of their success in stemming drug use, they fostered a working relationship that civilians and military officials believed was fundamental to success. This was the milieu in which Tolson arrived in 1969. The campaign to treat addicted soldiers emerged from the same milieu.

"It is like when you first start blowing smoke. You sit around at rap sessions in the pads. There is a lot of talk, you know, about the war. There is also a lot of talk about the

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[22] Author unknown, "Report of Cumberland County Mental Health Center", in "Hearings on Drug and Alcohol Abuse in the Military", 254-306.

establishment…after you start doing hard drugs, you don't talk about anything but one thing," Pat Reese continued, "that is scag [heroin]. You don't discuss the world problems or anything else."[23] In Reese's social etiology, hard drugs emerged in the informal sites of counter-cultural politics, but sapped the political energy of those same counter-cultural actors.

Reese's comments about the social etiology of heroin addiction are illuminating. There were, to be sure, a handful of shibboleths in Reese's comments about the trajectory of addiction. Rap sessions, pads, the establishment, scag. Each of these phrases suggests a host of meanings. First, Reese adjusted his pitch to match the language of the population about which he was discussing. Second, these words suggest that Reese conceptualized a discrete population about which there existed a shared vocabulary and series of social experiences. Third, these words came out of a youth vocabulary unique to the post-war generation of Baby Boomers who were most likely to compose the enlisted and drafted populations of the military writ large.

More importantly, Reese's etiology is the same that he brought to Bragg. Somehow Reese managed to find time between acting, reporting, counseling, and consulting in the civilian community to begin lecturing military audiences about the perils of narcotics abuse. In 1969, Tolson invited Reese to educate commanders in the 12th Support Brigade, the Special Forces, and the 82nd Airborne about drug use and abuse — maybe as a result of his work in educating teenagers, teachers, and police about drugs. In a pattern that would become familiar to most any enlistee, Reese would have young soldiers sit down in "rap groups" — informal talking and

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[23] Pat Reese, "Statements of Pat Reese, Reporter for the Fayetteville Observer; and, Doran Berry, Esq., Chairman, Cumberland County Commission on Narcotics and Dangerous Drugs, Fayetteville, N.C.", in "Hearings on Drug and Alcohol Abuse in the Military", 194-206. Quote on 206.

listening sessions — in which he and attendees discussed narcotics. He seems to have been successful, or, at the least, successful in making military officials believe his speaking engagements mattered, because he was eventually invited to speak at nearby Pope Air Force Base and Camp Lejeune, respectively an Air Force base and Marines encampment.[24]

Reese, Berry, and the Fayetteville posse of anti-drug and pro-treatment activists were not solely responsible for OA. To be sure, Tolson had invited Reese to Ft. Bragg. Why? In part, a claim of this chapter is that new military social welfare policies had influenced Tolson's perception of the Army's responsibilities. New military policies intended to domesticate the Cold-War standing Army developed in parallel to Reese and Berry. Prior to the Cold War, World War II had seen the largest military amassed in American history. Despite drawdowns after the war, and partly after the Korean War, Cold-War tensions in the 1950s and 1960s had led to a situation in which the U.S. Army, for the first time in history, maintained an immense standing Army. The development of a standing military had lasting consequences for how the Army approached the social lives of servicemen and servicewomen.[25]

One of the consequences of the Cold War-era buildup was the Army's formal creation of the Army Community Services (ACS) division in 1964, which, when it began, was also known as a "family services program." ACS and its own army of social workers were a bold continuation

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[24] Pat Reese and Doran Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", 231-234.

[25] On the Army, welfare benefits, and social privileges see Jennifer Mittelstadt, *The Rise of the Military Welfare State*, (New York: Cambridge, 2015).

and intensification of social work resources available to service members and their dependents.[26] In a 1966 essay from Army about the newly formed ACS, author Lt. Gen. James Woolnough described the purpose of the program. According to Woolnough, ACS was formed in July 1965 as "a centrally located service which would provide information, assistance, and guidance to members of the Army community in meeting their personal and family problems."[27] Among the services provided to deal with personal and family issues, ACS advised soldiers on financial assistance, housing, transportation, medical care, and legal aid. But, their work was also more granular, as Army wives wrote to ACS to laud the program's help in relocating, describing their relocations as "even travelling [sic] with my husband, I've never had it so good" and "they do everything for you except make the actual trip."[28] In a few words, Woolnaugh claimed that service was "truly a program with a heart."[29] It's worth noting that a social worker attached to OA, Lieutenant Colonel Raymond M. Marsh, was part of a mission codenamed Operation Tomahawk, where he flew to Tomahawk, Kentucky to present a soldier's wife with an identification card to earn service-related benefits.[30] The rise of ACS suggests that Tolson's paternalism reflected more recent benefit evolutions for a large, standing army.

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[26] Mittelstadt, *The Rise of the Military Welfare State*, 126-127. Here, Mittelstadt discusses the overlap in content/mission between officers' wives clubs and ACS. Also on ACS, see Thomas Herold, "The Evolution of Dependent Medical Care in the U.S. Army", *Military Medicine* 176, no. 10, (2011), 1133-1137. Jesse Harris and Stacey Berry, "A Brief History of the Military Training of the Enlisted Mental Health Worker", *Journal of Human Behavior in the Social Environment* 23, no. 6, (2013), 800-811.

[27] James K. Woolnough, "Army Community Service: Help from the Heart", *Army* 16, no. 10, (Nov. 1966), 41-42, 84. Quote on 41.

[28] Anonymous wives quoted in Woolnaugh, "Army Community Service", 42.

[29] Woolnough, "Army Community Service", 84.

[30] Jessica Marie Ryan, "ACS Enhances Military Lives for 50 Years", (Jun. 22, 2015), www.army.mil.

In addition to ACS, the Army initiated other programs during the Vietnam period that tangled social welfare with military service. In August 1966, Defense Secretary Robert McNamara announced Project 100,000. The initiative relaxed entrance standards for educational, intelligence, and psychological fitness — what commentators at the time described as "slow-learners" -- in order to funnel more troops into the war effort in Vietnam. The result was a new category of inductees, Category IV, or troops who had previously been rejected from induction. According to President Lyndon Johnson, Project 100,000 accomplished three existential social goals: first, the program offered more troops for service; second, the military could supply technical training to the US's underclass; and third, the program would more equally spread the federal government's support to citizens who had been disqualified from service. President Johnson described the program thusly: "Project 100,000 extends the responsibilities of citizenship and the benefits of military training."[31] In one speech to Congress, Johnson claimed of Project 100,000, "with intensive instructions, practical on-the-job-training and corrective medical measures, these young men can become good soldiers." Johnson continued his description of the need for Project 100,000, claiming in support for college deferments, "[deferments] are not unfair — however manifestly unfair are the conditions of life which permit? some to go to college while others cannot."[32] The Army, in Johnson's fantasy, would address the facets of American life where inequality persisted.

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[31] Lyndon B. Johnson, "A Summary of Accomplishments for the Welfare of Veterans and Servicemen, Provisions of the 1969 Veterans' Budget, Extending the Basic Benefits, and Furthering Job Opportunities through Project 100,000 and Project Transition", *Speech to Congress*, Jan. 30, 1968, House of Representatives Doc. No. 245. Quote on 4.
[32] Lyndon B. Johnson, "Message to Congress on the Selective Service System", *New York Times*. (Mar. 7, 1967), 32.

ACS and Project 100,000 complemented other DOD initiatives that entangled military service and private sector employment. Alongside Project 100,000, the Johnson administration simultaneously announced a related program, Project Transition. The latter program advised soldiers on their job opportunities in the private sector as they neared the end of their enlistments and tours of service. These programs together — Project 100,000, ACS, and Project Transition — suggest that Tolson's commitment to using the military to supplement civilian social improvement programs was as much a new institutional imperative, as they were any innate sense of responsibility to soldiers, fear of the breakdown of military discipline and authority, and commitments to local civilians. Don't you mean the reverse?

Tolson, it must be said, comes off throughout as a progressive. As he had claimed to reporters, the Army had a duty to soldiers, as well as to the communities from which they came to Ft. Bragg. OA was, in its way, a Great Society program for drug-using soldiers. Indeed, when Tolson died in 1991, his obituary described him thusly: "[after his tour in Vietnam and his appointment to Fort Bragg] he continued to be an innovator. Publicly acknowledging illicit drugs as a growing problem, he granted amnesty and provided treatment for soldiers…a departure from the usual practice of discharging officers."[33] More than perhaps even this author realized, Tolson was boldly suggesting ways that the Army might stand in as a welfare state for soldiers.

This has all been to say that, since at least 1968, the resonance of narcotics in and around Bragg was double-edged. Yes, soldiers stationed at Bragg were often part and parcel of the local drug scene. But, the silver lining was perhaps just as significant: the military participated in a

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[33] Bruce Lambert, "Gen. John J. Tolson, 76, Dies."

growing centralization of resources and organization of drug policing and treatment. The long-standing commitments between the Post and Fayetteville had made it possible for someone like Pat Reese to assume the role of something of a drug czar to Tolson. Really interesting. When you revise for publication, flesh this out.

In doing so, this section reveals that a combination of local anti-drug activism and then-recent military social welfare policies influenced how Tolson understood the drug problem at Fort Bragg, his responsibilities for treating heroin addicts, the difficulties of heroin treatment, and how best to establish an effective medical response. His response, as I show, illustrates how a far-reaching military paternalism made the space for an experimental program to emerge.

Taken together — the activism of mental health and substance abuse providers, and growing domestication of military services — pushed Tolson to act. By March 1970, Tolson decided that more extensive policing strategies and the Army's own potted drug education lessons were too feeble to address the heroin problem. After weeks of planning, the clinic opened in early summer 1970.

"Crews's Pad": The Work of the Clinic

The existence of Operation Awareness was always a tenuous proposition. On the one hand, it mirrored and reaffirmed Tolson's optimism about the Army's role in American life, and, on the other hand, it suggested the unlikely political constellations that had to fall into place to make military-based drug treatment a reality. Still, the clinic opened, welcoming its first patients on May 4, 1970.

This section focuses on how two staff members, Richard Crews and Chip Chapados, perceived the legitimacy of the project, the viability of medical treatment for drug use, and the potential for patients involved in the program to succeed. In large part, this section is based off of my own interview with Chapados. Staff remained skeptical about the accuracy of labeling addiction as a disease, optimistic about the potential efficacy of the treatment methods, and pessimistic about whether patients could be healed.

When Specialist James "Chip" Chapados volunteered in 1970 to join the staff at Operation Awareness, he got lost. Chapados's commander had directed the Army medic, recently returned from Vietnam, to Womack Hospital. He wandered around Womack Hospital, a shiny, new monument to the influx of war dollars into the sandy soil of south-central North Carolina. Wherever he looked, he could not find the unit. The staff at Womack looked puzzled when he quizzed them about the location of OA. Operation Awareness? "Never heard of it," they said. Chapados was new to Bragg, but his commander looked serious when he suggested that Chapados join the initiative as a staff member. Was this all a mirage? Was there actually a rehab unit at Fort Bragg? All signs pointed to Chapados's commander having hoodwinked him, taken advantage of Chapados's sincere interest in helping junked-out soldiers.

Chapados finally stumbled onto a collection of ancient, single-story, wooden barracks adjacent to Womack Hospital. He was standing in front *Old* Womack Hospital, an artifact from Fort Bragg's first heyday during World War II. Chapados approached Ward 30, the hospital barracks in which OA operated. As he edged closer to the ward, he remembers seeing men openly smoking joints outside the building. Like Dorothy arriving at the gates of an Emerald City gone to seed, Chapados rapped his knuckles on the door. Out stepped a round-faced helicopter pilot in

charge of the day-to-day work of the unit, Captain Richard Elmore. After introducing himself to Elmore and explaining why he was at the building, Elmore asked Chapados a strange question.

That question, as Chapados would find out, framed his tenure in the Army's first heroin rehab unit. In an effort to discern his usefulness to the patients and his stomach for the extreme, Elmore offered Chapados the following proposition: which option might Chapados choose to treat addicts — to place them in group therapy if only twenty percent would be healed, or to hang patients by their toes to a tree for hours if eighty percent would be healed? Chapados picked the latter option, and Elmore invited him into the building.

As Chapados made his way through the dark hallway that bisected the building, his surroundings confirmed that OA was a detour from accepted medical thinking and conventional Army discipline. Inside the facility sat a number of rooms, some dedicated to housing up to twelve patients at a time, some to group therapy, and another, labelled "Crews's Pad." The hallway burst with color and cartoons, as patients were allowed, and even encouraged, to decorate the clinic as they saw fit. Posters and hand-painted slogans festooned the walls declaring "No Hope without Dope!," "No Thrills without Pills," and "Will Power — Dig It!" One military journalist described the hospital ward thusly: "it is alive with garish painting — a gay, unbalanced, hippieland [sic]."[34]

The designer behind this abnormal clinic was Major Richard Crews. Lt. Gen. Tolson enlisted Crews, a psychiatrist, to flesh out and bring to life his initiative; to make concrete Tolson's commitment to a new form of social medicine. Crews is an enigmatic character — after he discharged from the Army, he became a critic of allopathy, and was later implicated for his role

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[34] Fred H. Bost, "Ft. Bragg Style", *Army* (Mar. 1971), 23-27. 23.

in presiding over a fake university in California — who has left few archival traces. Prior to being stationed at Fort Bragg, Crews attended Harvard Medical School, and then performed his residency at Letterman Army Hospital in San Francisco. Despite his latter-day journey toward biomedical heterodoxy, his educational background suggests that his status as a highly-credentialed doctor in 1970 lent some legitimacy to the experiment, and to the notion that addicts were "sick."

The initial patients would have come to an atypical therapeutic community, a long-term in-patient treatment modality that involved addicts as experts to help other addicts.[35] They would enlist in the program, receive conventional therapy sessions, and, the thinking went, come out after some number of weeks ready to live a sober life. However, OA was not destined to be just any treatment modality. Instead, as one ACS staffer put it, "after approximately 1 month of operating a rather traditional closed psychiatric ward, it was found that the patients required a more clearly outlined, closely supervised set of constructive and organized expectations that could be administered in a fair, objective, and positive way."[36] OA was destined to be more than just an experiment in administrative and diagnostic creativity.

In breaking the conventional mold of drug treatment, Crews leaned on behavioral conditioning, a field of research psychology that claims that behaviors dictate cognition, and that human

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[35] For a better description of therapeutic communities, their emergence in the 1950s, and their abstinence ideology, see Claire D. Clark, *The Recovery Revolution: The Battle Over Addiction Treatment in the United States*, (New York: Columbia University Press, 2017). See especially 17-51.

[36] Raymond Marsh, "Statement of Raymond M. Marsh, Chief, Social Work Services, Womack army Hospital Specialized Treatment Center, Fort Bragg, N.C.", "Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics", Nov. 17 and 18, and Dec. 2 and 3, 1970, 91st Congress, Second Session, (Washington: 1971), 23-27. Quote on 25.

cognition can be manipulated by rewarding and punishing given behaviors. Its most radical adherents went a step further, arguing that there was no such thing as cognition, only behavior. Still, behavioral conditioning, sometimes referred to as behaviorism, had its heyday in the 1950s through the 1970s. Much of behaviorism is, accurately or not, associated with the Harvard-based researcher, B.F. Skinner. It bears mentioning, too, that Crews attended Harvard at the same time that Skinner maintained a lab there, so it's plausible that he may have even taken a course taught by Skinner. Regardless, by the mid-1960s, around the same time that Crews would have been in medical school and residency, operant conditioning had made its way into psychiatric practice, and enlivened debates about how psychiatrists might address mental illnesses with novel therapies. According to a 1966 review of *Pavlovian Psychiatry: A New Synthesis*, a tome on the benefits of behaviorism to psychiatric practice, "this volume is another in a shower of books dealing with the recent advances in the application of conditioning techniques to psychiatric problems."[37]

As other historians have illustrated, behaviorism enjoyed something of a renaissance after World War II. Earlier proponents of behaviorism, notably Jacques Loeb and John B. Watson, had spent their careers in research labs and, in Watson's latter days, advertising.[38] In short, behaviorism was very much a product of universities, and to a lesser extent the world of business. During World War II, behaviorists openly courted the attention and patronage of the military and federal government, as Yale researchers lent their Cross-Cultural Survey (later renamed the Human Relations Area Files) for use in the war effort. B.F. Skinner even worked to

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[37] Author unknown, "Book Review — Pavlovian Psychiatry", *The Psychiatric Quarterly*, 40, 1-4 (Dec. 1966), 777.
[38] Lemov, *World as Laboratory*, 11-45.

train pigeons to pilot missiles! According to historian Rebecca Lemov, during the course of the War, "the navy, army, air force, and Central Intelligence Agency gave $50,000 each for research…to help administer geographical hot spots." This new infusion of cash fostered, Lemov continues, the "elevat[ion of behaviorism] to the status of science at the same time they were celebrated for their 'potentially great practical value."[39] Both behaviorists and the federal government continued to fertilize these research fields throughout the postwar period, organizing anthropological studies during the nuclear tests in the South Pacific, and running the CIA-funded lysergic acid-mind control tests that came to be known as MK-ULTRA.[40]

Despite the pedigree associated with conditioning, and the growing interest among psychiatrists in its potential benefits for medical therapies, there were few templates for how physicians could translate behavioralist hypotheses into psychiatric therapies for drug abuse. To wit, one psychology researcher claimed in 1973, two years after the end of OA, that "while behavioral approaches have been applied with some success to addictive behaviors including alcoholism… and obesity…they apparently have received only limited utilization in the treatment of drug abuse."[41] In this sense, Crews had a dearth of precedents from which to stage an intervention. However, what Crews lacked in precedents, he made up for in the opportunity and abundance of test subjects. This is good story telling.

Crews anchored OA on two principles. The first, operant conditioning, refers to positive reinforcements to court wanted behaviors. Crews attempted to reinforce desired behaviors —

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[39] Lemov, *World as Laboratory*, 160-161.
[40] Lemov, *World as Laboratory*, 170-221.
[41] Peter M. Miller, "Behavioral Treatment of Drug Addiction: A Review", *The International Journal of the Addictions* 8, no. 3, (1973), 511-519. Quotation on 519-520.

from making beds to cleaning the clinic — through the use of a points-based token economy. Basically, patients worked to gain 20,000 points, after which time they were considered healed. Infractions of the rules, including going AWOL or relapsing, resulted in the loss of points. These points were no small issue either: patients could redeem their points for privileges, such as the opportunity to leave the clinic without an escort. Points equaled autonomy.

Second, Crews looked to aversive conditioning, which entails negative reinforcement to discourage unwanted behaviors. Aversive conditioning is also the source from which OA earned some of its notoriety. Crews believed that addicts were as stimulated by and addicted to the ritual of the shoot-up — known as "the needle fix" — as to the opiates themselves. When patients achieved 20,000 points, they could elect to participate in a procedure based on aversive conditioning: a fake heroin shoot-up.

Patients and staff would gather inside "Crews's Pad," where they lounged on mattresses scattered on the floor. One source claims that, as part of the procedure, soldiers smoked fake joints as well, however I have not been able to confirm that.[42] Nonetheless, once gathered, staff and patients began to recreate a shoot-up. With music blaring in the background, a staff person would tie off the patient's arm to expose a vein. They then injected a mild barbiturate mixed with water, followed by a cup of either Coca-Cola or Hawaiian Punch that had been laced with ipecac. In theory, ipecac produced the aversive result for which Crews had planned — soldiers would quickly experience stomach aches, nausea, and vomiting, which they would supposedly then associate with the injection, thus breaking them of their "needle fix." If this procedure

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[42] Given Chip Chapados's recollection of his first visit to the clinic, where he encountered soldiers conspicuously smoking joints, it's entirely plausible that the joints referenced in this article were, in fact, actual joints.

sounds intense, its patients were quick to confirm that. I will turn to patients' perspectives in the next section, but for now, it's worth mentioning that when quizzed about the efficacy of the treatment, one patient stated "'dynamite—tough, but worth it.'"[43]

In addition to the token economy and the shoot-up ritual, there were more recognizable, but equally promising techniques. While Crews opposed the use of methadone for maintaining users, he was invested in its utility as a means of detoxing patients. Methadone is an opioid analgesic that physicians can prescribe as a substitute for heroin, among other uses. There were ongoing debates about the efficacy of the drug, where critics claimed that methadone therapy was simply a substitute for heroin use; swapping one addiction for another addiction.[44] Crews, instead, offered patients methadone during the detox process, when withdrawals from opiates exact their painful symptoms, which include nausea, sweating, and other flu-like symptoms. Methadone staves off those symptoms, without supposedly offering the euphoric joyride that is a heroin high. Methadone at OA was, then, a time-sensitive pain killer, not a long-term solution to addiction.

As part of the broader psychiatric clinical toolkit of the early 1970s, methadone was also the stuff of research careers. Starting in 1964, Vincent Dole and Marie Nyswander began trials with methadone at Rockefeller Hospital in New York. Outside of New York and the Public Health Service's narcotic hospitals, methadone was a rarity in 1970. The exoticness of methadone waned as the 1970s dragged on and municipal governments attempted to create public

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[43] Anonymous soldier quoted in John Noble Wilford, "Army Post Gives Addicts Help Instead of Censure", in *The New York Times*, (Nov. 23, 1970), 1, 30. Quote on 30.

[44] For a roundup of the different positions on methadone, and their differences, see Author unknown, "Methadone Maintenance for Heroin Addicts", *The Yale Law Journal* 78, no. 7, (Jun. 1969), 1175-1211.

methadone clinics. But, in 1970 North Carolina, methadone was not only hard to come by, it was illegal.

From a more optimistic point of view, Crews brought experimental therapies with a lot of upside, and even less downside. If he tried and failed, patients might be a bit more nauseated, but no lasting harm. If he succeeded, then Crews would have not only invented a groundbreaking therapy, but he would reinforce and justify Tolson's commitment to the Army-as-social-leveller. Others would actually duplicate Crews's behavioral instincts in more recognizable research sites, as when a physician at the University of Oregon devised an electro-shock syringe in 1971 to address the needle fix, and in 1970 and 1971 when researchers at a rehab clinic in New York City tested out a token economy with civilly-committed patients.[45] It's the claim of this chapter that the experimentalism of the initiative, the sheer acknowledgement of the unknown, helped to foster an optimism, however weak, about the "coulds and shoulds" of the moment. Should the Army treat addicts, and how might they do so? Even, the question of whether or not addiction counted as a disease was a live one, which opened the space for new therapies. Crews was operating in a moment in which methadone therapy and behavioral conditioning set new horizons for what was possible in terms of care to narcotics users.

From a more skeptical perspective, Crews's experiment projected onto patients a series of unproven therapies with unknown consequences. When it came to the graduation shoot-up, the ethics of Operation Awareness are even more ambiguous. Critics of aversive conditioning noted

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[45] On the electroshock syringe, see P.H. Blachly, "An 'Electric Needle' for Aversive Conditioning of the Needle Ritual", *International Journal of the Addictions* 6, no. 2, (1971), 327-328. On the token economy, see Michael Glicksman, Gennaro Ottomanelli, and Robert Cutler, "The Earn-Your-Way Credit System: Use of a Token Economy in Narcotic Rehabilitation", *International Journal of the Addictions* 6, no. 3, (1971), 525-531.

that negative reinforcement was a method very familiar to prisons and jails. That is to say that aversive conditioning was not exactly a therapy for willing, consenting patients.

It's unclear whether Crews believed in his treatment method, or even in the new administrative status afforded to addiction. Crews had come to the project with some trepidation, viewing his work as an experiment in psychology rather than a known treatment in psychiatry. Further, he was incredulous that his new patients suffered from a condition actually considered a disease. Crews viewed addiction, instead, as the consequence of an immature mind unable to comprehend the responsibilities of adulthood. Crews stated that "it is most important for children and adults to have clear limits. You make their world safe for them with limits. There is nothing compulsory in this. But we do make clear the costs and rewards for acts."[46] To that end, he believed that his experiment helped soldiers grow up. Just how successful his experiment in maturing young addicts was remained open.

More to the point, Crews's perception of his patients is worth more digestion. First, that Crews believed that his patients' addiction resulted from a developmental hiccup suggests that he wasn't convinced that addiction amounted to schizophrenia or other discrete mental illnesses. Put differently, they needed to develop, not get healthy. Second, his belief that his patients were not compelled to enter rehab suggests that he viewed them as "deserving" sick people. This trope parallels the tropes that Tolson himself had broadcast: soldiers can seek treatment if they like, or they can choose to refuse it. Those who seek treatment are deserving of sympathy and intervention. They are the truly sick. Those who don't seek treatment, and thus don't accept the

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[46] Dr. Richard Crews, quoted in Bernard Nossiter, "'Operation Awareness'", A3.

label of addict, would continue to be treated as criminals. And, because no one was compelled to be in OA, then they weren't criminals.

Of course, the distinctions between compulsion and consent get blurry when set against the backdrop of administrative punishments. These complications gum up easy differentiation between the labels "sick" and "healthy." It's not inconceivable that sick people refuse treatment. Nonetheless, illness is performative.[47] And, "addict" operated in OA simultaneously as a diagnosis — an umbrella of shared behaviors — as well as an administrative category — amnesty from punishment. Accepting the label of addict entailed, at least on paper, freedom from the discoveries and punishments meted out by military investigators, commanders, and judges. It also entitled its holder access to medical procedures that Crews and others called treatment.

Designing OA was one thing, but implementing Crews's designs was quite another thing. From the start, a series of funding and staffing difficulties knee-capped the program. Financially speaking, OA did have a roof over its head, and cots on which patients could sleep. However, funding the program remained a tentative proposition. The Post's Officers' Wives Club donated

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[47] On acting sick in the context of power, see Sharla Fett, *Working Cures: Healing, Health, and Power on Southern Slave Plantations*, (Chapel Hill: University of North Carolina Press, 2002). Charles Rosenberg has described disease thusly: "disease should…be understood in context, as a time- and place-specific aggregate of behaviors, practices, ideas, and experiences." He continues, "we cannot discuss the *what* of disease without discussing the *when* and the *where*" and that "individual disease concepts exist as social entities…in ways complexly and not always directly related to a possible substrate in a specific biopathological mechanism." Rosenberg. "What Is Disease?". Further, what else should we call "being sick" or "suffering," if not performative? Noemi Tousignant's study of the dolorimeter is just one example of the unavoidability of subjective experiences of illness. Tousignant, "Pain and the Pursuit of Objectivity: Pain-Measuring Technologies in the United States, 1890-1875", Ph.D. dissertation, McGill University, (May 2006).

$2,000 in seed money, which administrators used to purchase a clothes washer and dryer for the in-patient clinic.[48] The minuscule seed money was hardly enough to do much else with, as OA staff reportedly had to steal chairs, cots, and, even, health pamphlets from Womack Hospital upon opening the ward. Months after opening, a high-ranking general promised in September 1970 to fund OA over the next year, agreeing to $101,390 that Tolson had requested for personnel and supplies.[49] In theory, that money would hire six new civilian employees. OA received $31,000.[50] The funding difficulties also flavored the treatments themselves: despite Crews's plan for a token economy that was supposed to use physical tokens, they couldn't afford to buy physical tokens. Staff were creative, though, and wound up devising a points-based system.

OA's dire financial situation contributed to staffing issues, but was hardly the only difficulty facing the program. In theory, OA was supposed to be a total community, a form of treatment wherein inpatients' every moves were to be organized around treatment. However, due to insufficient funding, there were too few staff persons for each shift, while the staff that was available was working double shifts. Staffing issues plagued the program throughout its

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[48] Officer Wives' Clubs were instrumental in "socializing" the Army in the 1960s and 1970s. Along with the activism of enlisted soldiers' and NCOs' wives, they helped improve access to family services. See Mittelstadt, *The Rise of the Military Welfare State*. On funding for OA, see author unknown "Army to Aid Drug Program at Ft. Bragg", *Afro-American*, (Oct. 17, 1970), 9. It's worth asking why the clinic purchased a washer and dryer. Was it related to the ipecac and withdrawals?

[49] Letter to Harold E. Hughes from Maj. Gen. William A. Becker, Sep. 23, 1970.

[50] "Army to Aid Drug Program at Ft. Bragg". As of October 1970, "the Army is putting up $101,390 — $45,000 for equipment, $2,790 for maintenance, $12,000 for supplies and $45,600 for six civilian employe [sic] salaries. The Army also will finish 51 additional military personnel". After a visit in 1971, Representative John Murphy of New York revealed the continued funding shortfall, some $70,000. See, Jack Anderson, "Dope Sold", in *The Atlanta Constitution*, (Jul. 28, 1971), 4a.

existence, and suggested deeper conflicts about the legitimacy of the new medical designation, and the appropriateness of the Army treating drug users. The day-to-day work of the clinic revolved around the therapy schedule. Perhaps most important to that work was ascertaining the sobriety status of patients. When the clinic first opened, staff depended on a rudimentary version of the Nalline Test (described in Chapter 1). However, instead of injecting the patients with Nalline, they simply measured patients' pupil sizes against a pupilometer. Chapados recalls that he never quite warmed to the pupilometer, and believed it to be too inexact of a device for the clinic's needs. Needless to say, the discretion afforded to staff for this screen contributed to no small amount of consternation from the patients, who rightly protested the comparison of their pupils to a stock card of black dots of varying sizes.[51]

Eventually, the thin-layer chromatography screen replaced OA's crude version of the Nalline Test. In order to better determine the sobriety status of patients, a chemist stationed at Fort Bragg devised the in-house TLC test. Chapados referred to the new screen as "the best management tool we had." Inventing this screen was more than just producing a new device for use in the clinic, it also entailed adding still more staff to the program, and institutionalizing the practice of testing patients as its own department.[52] As one social worker attached to Operation Awareness described it, "a toxocology [sic] program has been established to monitor 'patients backsliding,' use of drugs while in patient status."[53] Each and every day, patients assembled in

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[51] Description of the pupilometers comes from my interview with Chapados. Chapados, "Interview".

[52] Drug tests were already part of the treatment process elsewhere, including jails and the USPHS Narcotic Farm. Still, one can imagine a world in which there were no screens. Drug treatment wasn't new in 1970, so drug screens weren't inevitably part and parcel of a treatment regimen.

[53] Marsh, "Statement", 26.

the bathroom to fill a specimen cup with their urine. Chapados or other staff proceeded to watch the specimen collection, especially after they figured out that some patients were cheating the tests. The new premium on eye-to-genital surveillance ended in pithy advice shared amongst staffers: "gotta watch the angle of the dangle." Following collection of the urine, a dedicated team of lab workers ran the screens, sending back the positive and negative results.

And this brings us back to Chip Chapados. Chapados was one of a dozen or so staff members who worked in the clinic. According to Chapados, he was also one of the few, if not the only regular staff member to sincerely believe in the mission of Operation Awareness.[54] This is not to say that Chapados, Crews, or Tolson were insincere in trying to heal patients. In fact, Chapados never suggested anything of the kind. But, taking Chapados at his word, then, points toward other staff persons either being incredulous about the sickness of their patients, or just outright opposed to working with drug users. Still, as Chapados insisted, the scarcity of funding and dedicated staff couldn't have made it easy to work in the clinic. The recurrent double shifts that staff found themselves working is easily less preferable if other, easier job assignments existed.

It's worth highlighting the staffing lacuna of OA. On the one hand, the clinic was understaffed throughout its duration. On the other hand, by the summer of 1970, a toxicology lab appeared, complete with dedicated staff! While the inpatient ward suffered from a lack of full-time staff members, the lab, whose purpose was to surveil patients, buzzed with workers turning urine into test results. Extrapolating from this contradiction, was the purpose of the clinic then, installing a proper fear of chemical discovery? Was the psychological cudgel of TLC the real treatment?

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[54] Chapados, "Interview".

And, is it possible that the work of urine collection and its daily confrontations between staff and patients turned staffers off of their work?

Much of this section has amplified the negative aspects of Operation Awareness — from its funding difficulties, to stigmatization of the patient population. However, as I show later, there were silver linings. For some patients, the existence of OA was a beacon in a storm, and a second chance at redemption.

Recovery from What?: Patients Describe Their New Lives as Addicts

> I came down to Bragg, to the emergency room. I told them I was addicted to drugs. They asked me if I wanted to get off and I said, "You bet." They took me over to Operation Awareness…I talked to Dr. [Richard] Crews. He sounded real interested in the program he started so I volunteered to put myself as a patient in the program. I came into the program and stayed around 2 days and then one Saturday I climbed out the window and went downtown and bought some dope, shot some dope. I came back and they had a meeting about what happened. All my points that I had earned, and I had almost 10,000, they took them all away except 3,500. [It] just kind of blew my mind to see this happening to me, because I had really worked hard…So I looked at myself and said, "Well, for the rest of your life do you want to be a junkie?" I didn't really have a choice.
>
> J.D., Testimony to the Special Subcommittee on Alcohol and Narcotics Abuse in the Armed Forces, Nov. 17, 1970[55]

What is addiction recovery, if not a checkered period of sobriety punctuated with relapses, ending in sobriety, continued addiction, or death? J.D. was one of dozens of soldiers who

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[55] Private J.D., contained in "Statement of Captain Richard Elmore, Womack Army Hospital, Fort Bragg, N.C.; Accompanied by Two Recovered Addicts in the Rehabilitation Program", in "Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics", Nov. 17 and 18, December 2 and 3, 1970, 91st Congress, Second Session, (Washington: U.S. Government Printing Office, 1971), 28-40. The quote appears on 31. I believe that I have identified J.D.'s full name through separate sources, but have chosen to use his initials to protect his privacy.

enrolled in Operation Awareness's in-patient clinic, and traveled the path toward sobriety. However, J.D. was special: he was the first graduate of the rehab program, a select group that contained only a handful of other soldiers. This is a circuitous way of saying that the results of OA's therapies were not statistically impressive. In fact, their success rate was dreadful. Who were these patients? What were their treatment outcomes? What did they think of their therapies, and the fact that the Army was suddenly providing them? And, why did so many fail?

In order to compile a composite portrait of the patient population, I have attempted to combine multiple, conflicting reports. A couple of difficulties emerge. First, reporters often failed to distinguish between the in-patient population and the out-patient population. This means that understanding the in-patient population entails trying to wade through often undifferentiated data. Second, I haven't found any internal clinic documents, so this portrait comes almost exclusively from newspaper articles describing the program, as well as the few statistical profiles of the program that OA made publicly available.

Overwhelmingly, patients were lower- and working-class, in their late teens to early twenties, enlistees rather than draftees, and male. I have yet to uncover a breakdown of the race of OA's patients. These characteristics confirm some of the narrative descriptions of the patients — largely, that patients came from poor families. Their ages, especially, place them within the age-range in which developmental barriers might emerge.

The vast majority of patients also ranked lower in the military hierarchy. Chapados describes his favorite patient, a boy of seventeen. B., the other patient who joined the Ft. Bragg contingent in Washington, was an SP-4, essentially an Army private who had earned three promotions. J. and D. — to be sure, they are different people from the above patient, J.D. — were both twenty,

the former from Fairbanks, Alaska, and the latter, described as being from "the streets of Philadelphia."[56] Generally, any college graduate enlisted as a specialist, and after basic training moved on to Officer Training School. It's probably reasonable to assume that both privates had, at best, a high school diploma, given the commissioning of college graduates. Similarly, B., another OA patient present at the subcommittee hearings, was an SP-4, a specialist ranking below an officer.

Perhaps the most salient myth surrounding drug use, GIs, and the Vietnam War is that the close proximity of Vietnam to the Golden Triangle, and its plentiful, cheap, and pure heroin was a sneak attack on young American men. Common sense, then, might suggest that heroin use was novel to soldiers, and Vietnam was an exceptional moment where GIs got plunged into an environment riddled with cheap and strong opiates. However, as one scholar, Jeremy Kuzmarov has shown, the number of addicts never reached the hyper-inflated numbers that researchers and commentators of the period suggested.[57] This is to say that Tolson, as well as observers since have emphasized that GIs were more likely to turn onto heroin in the US, than in Vietnam.

Instead, J.D.'s story, and that of other patients, suggest something more interesting: they came into the service already hooked on heroin. In J.D.'s case, in fact, he claims to have enlisted in order to escape his past. J.D. reflected much of what even Ft. Bragg's OA teams had discovered in a handful of drug-attitude surveys: many of the patients — perhaps more than half of cannabis users and four in ten opiate users — had come directly to Ft. Bragg from elsewhere in the United

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[56] Dana Adams Schmidt, "A Few G.I. Addicts Aided in U.S.", *The New York Times*, (May 17, 1971), 1, 12.
[57] Kuzmarov, *Myth*.

States, already far along in their habits.[58] B., the SP-4, had not turned onto heroin until he began a tour of duty overseas, stating flatly that "I started dope in the Republic of Vietnam." Still, to compound matters, he claimed to have gotten clean once he arrived back in the U.S., only to begin using again after he was inducted for another tour.[59] So doesn't his story support the myth?

This is not to say that GI's couldn't become heroin addicts during their tours of Vietnam. In fact, Chip Chapados's main motivation for joining OA as a staff member was because of the path into opiate addiction that a good friend of his had taken. His friend had served multiple tours in Vietnam, only to be injured on his last tour. He received morphine for his pain, but eventually ended up turning to heroin once the formal prescription for morphine ended. His friend eventually was caught with heroin, and summarily discharged.source?

So, how did patients fare in the treatment program? Perhaps the best evidence of soldiers' outcomes emerges in the cold, statistical totals of rehab drop-outs that staff compiled. By its seventh month in operation, OA had enrolled sixty-seven patients. While ten patients were enrolled at the time of speaking with the subcommittee, fifty-three patients (seventy-nine percent) had elected to leave the program. Only four (five percent) were considered "graduated."

Later reports have been difficult to find. One estimate from 1971 suggested that between 200 and 300 patients had or were receiving treatment through OA. In the same month, one source stated that 109 patients had cycled through OA, and that forty-eight percent were reported to be doing satisfactory.[60] An estimate two months later, in July 1971, however proposed that 560

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[58] Waters, "Statement",146.
[59] "Testimony of B." Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics, Nov. 17 and 18, December 2 and 3, 1970, 91st Congress, Second Session, (Washington: 1971). 32.
[60] Schmidt, "A Few G.I. Addicts".

soldiers had gone through the program, and that commanders of 275 of those patients had been later reported as remaining "good."[61] Such inflated numbers—given the original, twelve-bed capacity of OA, and the lower initial estimates of patients listed in OA's first seven months—perhaps reflect participation in the educational portion of the program in addition to the inpatient rehab component.[62] Despite the greater ability of the program to treat more patients, an anonymous officer connected to OA, perhaps Capt. Richard Elmore, curtly explained the nagging, recurrent issue for drug rehab in the Army: "some drug addicts don't want to be cured."[63] But, if patients' decision to drop out of OA or to relapse wasn't simply addict behavior, and instead a rational choice, we might then hypothesize that patients voted with their feet.

Per Crews's design, the program was supposed to be an all-encompassing total community. The points were supposed to dictate and reinforce desired behaviors. Staff were supposed to assign, deduct, and track the points that patients earned and lost. So far so good. However, despite the planned objectivity and reliability of the token system, the recurrent staffing issues that stalked the clinic surfaced most visibly for patients in these instances. First, according to Chapados, because most of the staff disliked the patient population, and the requirements of working in an addiction clinic, they felt little incentive to actually track the points that patients earned and lost. Second, the short staffing of the clinic meant that what staff was available was regularly overworked. Their being overworked, and their disinterest in their patients by extension, is difficult to disentangle.

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[61] Author unknown, "Drug Arrests Double at Army's Ft. Bragg", *Hartford Courant*, (July 31, 1971), 11.
[62] Author unknown, "Addiction and the Military", in *Boston Globe*, (May 25, 1971), 10.
[63] Anonymous officer quoted in "Drug Arrests Double".

So, what do these staffing issues add up to with regard to patients' experiences in the clinic? When it came to earning points, there was a fairly clear consequence: while the clinic was supposed to operate on points, the staff often failed to track increases and decreases of the same. Chapados recalls that patients were vocal about the bind in which they found themselves. In other words, why attempt to abide by the rules of the clinic, if the staff didn't take an interest in abiding by the rules themselves? These points, again, were never just points. The points weren't some abstract concept or ideal. Points equaled time away. Points equaled downtime. Points were never just points, they were freedom.

J.D.'s recollection of his time in OA is a starting point for interrogating whether and to what extent patients accepted the legitimacy of their diagnosis, the extent to which they believed that the Army held a responsibility for healing them, and how they regarded the efficacy of the treatments that they received. Both J.D. and B.'s accounts of their time in OA put forward a positive view of the program. In part, their optimism and satisfaction with OA mapped onto their success as patients. Either having graduated from OA or on the way toward graduating, it stands to reason that they endorsed OA. Still, given the context in which they spoke of the program — a senate subcommittee hearing — it would also make sense that staffers presented to senators only patients who were treated and successful.

Unsurprisingly, J.D. and B.'s perspectives were not universally shared. In 1971, Nora Sayre, a journalist, interviewed a drug dealer from the 82nd Airborne (the author described him as "a tall, muscular, quick-tempered product of Washington's ghetto"), named B.J. and his customer, who used the pseudonym C. (the author described him as "a qualified paratrooper who never finished high school…short, scraggly, and white" from Georgia). After recounting their tales of service in

Vietnam and their deluge into addiction, they moved on to the subject of getting clean. Both had heard about OA, but neither had positive opinions. In fact, as they relayed, patients had begun to subvert the program's objective. C. described how some patients approached OA: "I know some people who go over there…just to get the methadone that they give. You know that's addicting, too…That's right. You got dudes going over there to get high on Uncle Sam's expense."[64]

B.J. and C.'s understandings of Operation Awareness were similar to another anonymous soldier whom Sayre interviewed. One soldier talked about the Army's cynical, and possibly sinister, new plan to heal addicts. As the informant briefed her on the ballyhooed initiative:

> You get some methadone. I was really stoned on that. But some guys are bigger junkies when they leave than when they go in—smack's so easy to get from the corpsmen. One dude I knew came in with a twenty-dollar-a-day habit, and he left with a forty-dollar-a-day habit. So you just puke and earn points and don't get cured. I think the whole setup is only political propaganda. They just want the public to think they're trying.[65]

On the one hand, B.J.'s, C.'s, and this anonymous soldier's anecdotes suggest, possibly, an obvious outcome; that drug users exploited OA in order to score legal methadone, rather than resort to street supplies of illegal heroin. On the other hand, the anonymous soldier's recollection complicates explaining patients' acceptance of their diagnosis and treatment. Patients might have been keen enough to have perceived their role in a larger political drama — the "political propaganda" referred to in Sayre's interview. With the many staffing issues that

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[64] Stuart H. Loory, "High on Heroin: Drugs in Army—Soldiers Tell It Like It Is", *Los Angeles Times*, (May 27, 1971), 1, 18-19. Quote appears on 19.
[65] Anonymous soldier quoted in Nora Sayre, "Voices from the GI Movement", 1996 Revised Edition, *Sixties Going on Seventies*, (New Brunswick: Rutgers University Press, 1996), 68-82. Quote appears on 78.

suffused life in the clinic, patients wouldn't have been oblivious to the distance between the media portrayal of OA and their lived experiences on the same ward.

And still, there is a sense in Sayre's interview subject's recollection that points to pessimism about the methods themselves. Although Crews's use of methadone in OA reflected an experimental optimism that physicians might have been close to curing addiction, patients learned the flip-side of that optimism. Rather than emerge from detox into probationary sobriety, patients might leave with a bigger habit than when they entered the program. Similarly, despite Crew's grand designs for a behavioralist-styled in-patient community, the meanings of the point system and, even, the ritualized shoot-up remained mysterious or irrelevant to patients. What kind of procedure, they may have asked themselves, involved making beds, painting peace signs on the wall, and simulating a shoot-up?

It's wholly reasonable that J.D.'s experience and that of the anonymous soldier in Sayre's story coexisted. To be sure, J.D. participated in OA early on, when the funding difficulties looked like a temporary bug, not a defining feature. The persistence of staffing shortages could have revised how potential patients perceived their treatment. Still, patients were also keen to the meanings of their diagnosis, and the political stakes of Tolson's decision to initiate OA. This is to say that their sense of what they believed to be Tolson's opportunism shaped the extent to which they accepted their diagnoses and the peculiar treatments offered at the clinic.

Diagnostic Legitimacy in Institutional Context: Challenges to Medical Optimism and the End of the Clinic

A *New York Times* report in late-summer 1970 blew the cover on Tolson's secret experiment. As a result of the revelation, General Westmoreland, the top general of the Army at the time, arranged for an immediate tour of the clinic. According to Chip Chapados, Westmoreland flew to Bragg the day after the *Times* report came out. Westmoreland reportedly stormed out of his helicopter onto the tarmac, and into Ward 30, and then, just as quickly, stormed back to the chopper and returned to Washington. A similar situation played out again after a CBS report on the clinic. Following a primetime story on OA, Chapados recalls that a drunken Vice President Spiro Agnew dialed the clinic, demanding that an American flag painted on the wall with the peace symbol over the field of stars be removed forthwith. Chapados states that he ribbed Agnew, claiming that he couldn't remove the painting without a direct order from his superior, and that Agnew was not in his chain of command. The next morning, a Secret Service agent showed up at the clinic, having driven from Washington to visually confirm that the flag had indeed been removed. Within a few months, staff, patients, and supporters of Operation Awareness made their own trip to Washington to testify to a Senate subcommittee. In June 1971, OA closed its inpatient ward.

The most positive consequence of the report, however, was a series of Senate subcommittee hearings, which extended a longer investigation into drug and alcohol use in the military. How did other GIs receive the opening of OA? Did they accept OA's patients as sick people, and not criminals? How did outside commanders and politicians regard the program? Finally, what consequences for the military and the local community followed from the creation of OA?

The uncertainty of the moment — both in terms of diagnostic legitimacy, and in terms of the propriety of the Army's running an experimental addiction program itself — suffused how OA supporters described the program. "I want to emphasize at the very outset," Tolson expounded, "that everything we have done has been basically experimental. We haven't found a lot of set procedures in this endeavor." Tolson reemphasized his point, stating that "[OA] is an experimental concept…it is still in its infancy and will undoubtedly be subject to a number of alterations in the future."[66] Still, in the experiment, Tolson perceived hope: "every division in the Army could conduct a survey [of drug attitudes] similar to that conducted in the 82nd Airborne…it would provide an enlightened understanding of the drug problem and to what extent drug abuse had penetrated the particular command."[67]

The revelation that OA existed met with a number of opinions, many of which were negative, and even fewer of which understood the enlightenment that drug treatment supposedly offered. According to one anonymous master sergeant, "the Army is not meant to be a social welfare agency…if [a commander] is to be loyal to troops trying to do their job, he cannot arbitrarily reject consideration toward administratively discharging *any* drug abuser discovered." While Tolson and the DOD may have believed that the Army was actually a social welfare agency, the novelty of that position may not have penetrated the service very deeply. Another officer made his criticism differently: "the rehabilitation bunch are squandering two trained men for treatment

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[66] Tolson, "Statement", 19.

[67] John Tolson and Harold E. Hughes, "Questions Submitted to Lt. Gen. John J. Tolson III by Senator Harold E. Hughes, and Answers Subsequently Received", in Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics, Nov. 17 and 18, December 2 and 3, 1970, 91st Congress, Second Session, (Washington: 1971), 240-242. Quote on 240.

of each addict." Describing the kinds of patients whom he believed came to OA, he claimed that "drug addiction results from criminal acts which could have been avoided entirely. Why should my family be penalized while delinquents are coddled?"[68]

Even more to the point, the legitimacy of the treatment modality and the Army's potential to effect positive health outcomes rankled others. One non-commissioned officer tempered his criticism of OA with his own support for treating addicts in the general sense. "I am not against helping addicts," he claimed, "but psychiatrists say that the addict can be helped only in an environment where he is most comfortable. Shouldn't that rule out Army rehabilitation? Ideally, the Army addict needs to be released and treated in his home town." He concluded his observations, "the cheapest, wisest contribution the Army could make would be the loan of knowledgable doctors and medical personnel on a rotation basis to communities most afflicted."[69] Ironically, Pat Reese had made sure that Tolson realized a similar plan.

Even Tolson's support for the addicted soldiers was qualified, and suggested lingering suspicions about the legitimacy of the project. Almost as if responding to the most bitter of OA's opponents, Tolson once noted that, "I want to emphasize the fact that we still give the help to the man who comes and volunteers for help. The man that does not, the apprehension and punishment are the same as always. We are not *coddling* these people."[70]

Despite the effort to improve how commanders treated known drug users, commanders continued to stigmatize them, threatening the success of patients and OA more broadly. According to one social worker attached to OA, "it is sometimes difficult for the rehabilitated

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[68] Bost, "Ft. Bragg Style", 26. Emphasis in the original.
[69] Bost, "Ft. Bragg Style", 27.
[70] John Tolson. "Statements of Pat Reese and Doran Berry", 206. Author's emphasis.

drug users to regain acceptance in the military units. There is ambivalence on the part of some commanders to receive the ex-drug user once he graduates or leaves the rehabilitation program."[71] Supporters of the program were aware of how, in spite of a treatment, outsiders might subvert their plans. According to Gen. Tolson, while "the young soldier [is] the man we are really interested in," he continued, noting that nonetheless "there had to be a real understanding by the senior officers and noncommissioned officers on the drug culture and its problems."[72] To be truly successful, in Tolson's view, OA had to convert non-drug users, or at least convince them of the efficacy of the treatment and the appropriateness of it.

Beyond these criticisms, heroin addiction treatment made strange bedfellows of Fort Bragg's radicals and its commanding officer. To wit, when word came down in June 1971 that Operation Awareness was to close, authors at *Bragg Briefs*, a local G.I. Movement paper, lamented its end: "as of the first week in June [1971], the very promising in-patient part of the program has ceased to exist because of the Army's refusal to provide enough personnel to staff it." Still, next to their support of OA, movement members at *Bragg Briefs* were just as keen to interpret the program in light of its political substance: "the brass seized upon Operation Awareness and milked it for every bit of good publicity they could. The army has used O.A. as a public relations gimmick, showing it off to big shots, bragging about it to the press."[73]

At least for authors and editors of *Bragg Briefs*, there was a one-to-one relationship between heroin addiction and military life. They were convinced of the political and occupational hazards

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[71] Marsh, "Statement", 27.
[72] Tolson, "Statement", 20.
[73] Author unknown, "Operation Awareness is Dying", *Bragg Briefs* 4, no. 5, (Jun. 1971), 1, 11. First quotation appears on 1, and the second quotation appears on 11.

that fostered addiction. In their radicals' etiology, "many GI's are fed up with the system. To escape from the system they turn to dope."[74] One article in *Bragg Briefs* from 1969 had a more conspiratorial idea in mind. When pondering why there were so few drug busts on Fort Bragg despite a superabundance of drugs — in their words, "Fort Bragg is a sewer. Drugs flow through the arteries of Fort Bragg and permeate the entire base" — they claimed that "the military has a related, but different reason for tolerating dope. Dope is an escape from things as they are and the Fort Bragg reality is not the best of worlds. The military uses dope to keep Fort Bragg cool."[75]

From a more jaundiced point of view, drug control around Fort Bragg extended the heavy hand of military authority, and conservative social policies. Soldiers behind *Bragg Briefs* said as much. According to their sources, at least, two cases — one involving military police purging female soldiers on suspicion of "drug use and gayness," and another with military police dispersing anti-war protesters during a rally on the supposed crime of drug possession — suggested more potentially subjective interpretations of AR 635-212, the military SPIN code for discharging the mentally, physically, and sexually unfit.[76] Nonetheless, even the Post radicals had inadvertently come to Tolson's defense.

In November 1970, members of Operation Awareness joined other drug-abuse service committee members, jurists, and physicians from across the five service branches. While in the Senate, they discussed all manner of topics related to policing, detection, treatment, and

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[74] Author unknown, "Dope or Politics", *Bragg Briefs* (Aug 1970), 9.
[75] Author unknown, "Dope", *Bragg Briefs*, (Christmas 1969), 2.
[76] Author unknown, "Army Uses Scare Tactics Against WACs", *Bragg Briefs* (May 1971), 6. Author unknown, "GI's and Friends Busted in Rowan Park", *Bragg Briefs* 3, no. 7, (Sep 1970), 1, 7.

prosecution of drug abuse. By November, patients and staff journeyed to Washington where they filled in the assembled senators with tales of OA, complete with charts, statistics, and patients. As if to buffer the program from criticism, one officer stated that "it is still too early for us to make a definitive evaluation of the results of our efforts."[77]

These committee hearings continued earlier hearings about drug and alcohol abuse. The earliest hearings, initiated in summer 1969, as the Subcommittee on Alcoholism and Narcotics of the Labor and Public Welfare Committee, focused on existing anti-drug laws. Democratic senator, Howard Hughes, led the subcommittee. Subsequent hearings, including the November 1970 hearings, continued in this vein.[78] However, there was a shift, from talking generally about drug abuse and anti-drug laws, to military drug abuse. Similar investigations and hearings repeated in 1971, and, in 1972, new hearings focused on U.S. troops stationed in Germany. Such investigations and hearings became routine, and continue into the present.

The assembled senators seemed to praise Tolson for his efforts. Peter Dominick, a Republican senator representing Colorado, had this to say of Tolson's program: "I was impressed with the work that General Tolson and his group are doing at Fort Bragg…to find someone with the innovative spirit that has been shown here is excellent."[79]

Other commanders and medical staff from across the services presented a diversity of treatment approaches that had cropped up. OA was, to be sure, the most explicit adoption of the newest in addiction treatment methods. As other commanders explained, in contrast, their responses had

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[77] Tolson, "Statement", 24.
[78] Grischa Metlay, "Federalizing Campaigns against Alcoholism and Drug Abuse", *The Milbank Quarterly* 91, No. 1 (Mar. 2013), 123-162. On the formation of the Senate subcommittee, see 136-137.
[79] Tolson, "Statement", 23.

been conceived within the existing limitations and prohibitions on drug users. The most radical Army treatment outside of Bragg was group therapy, as the work of staff at Fort Benning made clear at the hearings. The variety of responses suggested that most responses were makeshift, at best, and regressive at worst. Fort Bragg, instead, was a beacon of medical progressivism.

No single answer to drug abuse emerged out of the hearings. Still, following staffers' appearance in D.C., the senators assembled promised to increase the funding available to OA. The skies were beginning to part. Their moment in the sun portended a brighter future.

OA continued after the hearings, but its existence was not long for the world. By June of 1971, Ward 30 closed. Fort Bragg's experiment in in-patient rehab was over. Writers at *Bragg Briefs* crowed that the decision to terminate OA's in-patient program came from new commanding officer General John Hay, whom they claimed had strangled the program of staff and support. The truth was both more promising and more pernicious. In the same month, the DOD declared that the Veterans' Administration system would provide drug detox and rehab treatments to soldiers returning from Vietnam. In some ways, the centralization of these treatments within the VA signaled a new legitimacy for addiction and addiction treatment within military medicine. With centralization, supposedly, came permanent, dedicated resources. Centralization also entailed a more institutionalized version of drug abuse therapy, rather than the uneven, makeshift development of treatment modalities across Army Posts. Yet, as the case of OA suggested, funding was always bound to be endangered.

According to Chip Chapados, the DOD paid Tolson back for his sympathy toward addicted soldiers. He claims that Tolson ended up in a dead-end appointment at CONUS upon leaving Fort Bragg, despite the possibility that he would have been appointed as a four-star general.

Regardless of whether or not his sympathy for addicted soldiers came from within, he nonetheless made a bold move to treat drug users at a time when indifference, hostility, or, worse, denial flavored most commanders' responses. He had a vision of military life that was both at odds with, and in parallel with military social policy. This is to say that OA bore little fruit for Tolson's career, and, in fact may have held him back. He retired from the Army in 1971, but in retirement, he served on North Carolina's commission for veteran affairs until 1977, continuing his work in support of soldiers and veterans.[80]

Richard Crews led, perhaps, the most colorful fate of staff attached to OA. Maybe in reaction to his time as the director of OA, he became interested in alternative healing. According to his obituary, Crews "lost faith and interest in what he called 'western medicine.'" He eventually became a certified homeopath and nutrition instructor, and, in 1978 he helped found Columbia Pacific University in California, which the same obituary writer described as using a "radical, non-traditional method of education." His journey into medical and educational orthodoxy ended in the 1990s, when the state of California investigated and then closed Columbia Pacific University for fraud.[81]

Pat Reese continued in his man-about-town, jack-of-all-trades role well after OA ended. He presided over the Myrover-Reese Fellowship House, a meeting place and counseling center for addicts, while remaining a well-regarded reporter at the *Fayetteville Observer*. As a result of his investigation into corruption at Fayetteville's mental health center in the early 1980s, he received

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[80] "Tolson, John Jarvis III". *NCPedia*. www.ncpedia.org.
[81] "Obituary: Richard Lawrence Crews", *Marin Independent Journal*, (Jun. 6, 2012).

a gunshot to the face from the suspect of Reese's report. In his final years, Reese even continued to act and write, despite the lasting injuries that resulted from his attempted murder.[82]

Chip Chapados experienced the best possible ending to Bragg's experiment. He had been transferred out of Ward 30 prior to its closing, but received new orders to attend to addicts elsewhere. As a result of his work with OA, Pat Reese requested that the Army loan Chapados to Reese's own rehab facility. Importantly, the Army paid Chapados to serve as a staff member in the civilian facility. Again, Chapados's employment reinforced the working relationship between the Army and Fayetteville that Tolson and Reese had initiated. In effect, despite the disappearance of a formal rehab clinic, and despite the failures of the community to secure federal support, the Army stepped in to provide the community with treatment resources and experienced staff.

The patients referenced in this chapter have, perhaps, the most pressing, but least accessible perspective on OA. The rightful limitations around medical privacy have made the discovery and identification of patients difficult. Their stories await telling.

Conclusion: About Face? Says Who?

As one commander in the 82nd Airborne recommended to his officers, "recognize that most individuals who abuse drugs and seek assistance are ill, at least emotionally. They should be treated as sick people and not as criminals."[83] Beyond its particulars, both OA and this

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[82]"Pat Reese Obituary", 1B.

[83] Letter from Major General George S. Blanchard, commander of the 82nd Airborne, Aug. 24, 1970, in "Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics", Nov. 17 and 18, December 2 and 3, 1970, 91st Congress, Second Session, (Washington: 1971), 157.

commander's endorsement surface a series of hard-to-answer questions, then as well as now: is addiction a disease, and who says?[84] If addiction is a disease, then how do you treat it? What counts as success or failure in treating drug users? Can and should the military — or, even the federal government — be responsible for treating drug-addicted soldiers? In 1970, the Army offered one potential answer, but it was a road not taken.

OA elaborates a few additional points, too, with regard to the broader issue of drug control technologies and the Army. Most importantly, it shows how a focus on drug control in the domestic US fostered an ideology about social assistance and the linkages between military responsibility and public health. Second, OA's path from secret initiative to popular knowledge suggests how dim the way forward for the Army was. Put differently, there was no clear directive from the Pentagon as late as December 1970, and Tolson offered a compelling justification for drug treatment based on his understanding of the Army as social leveler. OA also detracts from the significance of June 1971 — as local activists, not just Pentagon orders, shaped how Tolson and others pursued solutions to heroin abuse.

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[84] On the creation of a disease, see Rosenberg, "What Is Disease". On the categorization of disease at different times based on political contexts, see Keith Wailoo, *Dying in the City of the Blues: Sickle Cell Anemia and the Politics of Race and Health*, (Chapel Hill: University of North Carolina Press, 2001). David Rosner and Gerald Markowitz, *Deadly Dust: Silicosis and the Politics of Occupational Disease in Twentieth-Century America*, (Princeton: Princeton University Press 1994). Samuel K. Roberts. *Infectious Fear: Politics, Disease, and the Health Effects of Segregation*. (Chapel Hill: University of North Carolina Press, 2009). According to scholars interested in the history of addiction treatment, the question of its "disease-ness" has been central to how Americans approach narcotics control broadly. See Musto, *The American Disease,* 82-87. Caroline Jean Acker, *Creating the American Junkie: Addiction Research in the Classic Era of Narcotic Control*, (Baltimore: The Johns Hopkins University Press, 2002). Nancy Campbell, *Discovering Addiction: The Science and Politics of Substance Abuse Research*, (Ann Arbor: University of Michigan Press, 2007).

Chapter 4

The Many Lives of Drug Facts: The Burdens of Knowing in an Information Revolution; or, Fighting Dope in the Information-less Society?

Introduction: Talking Dope

> There was this young lieutenant up there and he was reading from one of those things and he could read real well, I don't want to put him down, but he was saying things like "mar-i-jew-wa-na" and he wasn't doing a damned thing. I told Tolson his program wasn't worth a damn.
>
> Pat Reese, November 1970[1]

It's no exaggeration to claim that Pat Reese was frustrated with the state of the Army's anti-drug education. This and other, less ham-fisted, more successful attempts by the Army to create and disseminate supposedly accurate, reliable, and credible information about heroin abounded. But, back at Fort Bragg, how were soldiers supposed to learn what they needed to know about heroin, if they would've rather fallen asleep, or, better yet, killed the messenger?

This chapter addresses the creation and dissemination of *drug facts* — supposedly discrete, tangible, calculable, printable, graph-able, exchangeable, as well as credible and believable information about heroin production, trafficking, and use.[2] Drug facts were a premium; knowing

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[1] Pat Reese, "Statement", 213.

[2] As historian Ronald Kline notes, "information" is a sticky term with little agreed upon definition. Even at its dawn, experts disagreed as to whether or not information was discrete facts or a measure of uncertainty. The former won out. According to Kline, "the profusion of the word *information*—one social scientist counted thirty-nine meanings of the term in 1972—was reduced in popular discourse to a transmission of commodified, equally probable bits in computer networks." See Ronald Kline, *The Cybernetics Moment: Or Why We Call Our Age the Information Age*, (Baltimore: The Johns Hopkins University Press, 2015). Italics in original. See also, also Kline, "Cybernetics, Management Science, and Technology Policy: The Emergence of 'Information Technology' as a Keyword, 1948-1985", *Technology and Culture* 47, no. 3, (Jul. 2006), 513-535.

the number of heroin and cannabis users in a single stockade, knowing the property owners in a Vietnamese village, or, in Pat Reese's case, knowing how to relate to nineteen-year-old soldiers. The creation and dissemination of drug facts, as I go on to show, tied drug control to larger institutional commitments, technological capabilities and limitations, and social conflicts unique to the era — an era which some scholars have described as a revolution in information technology.[3] It's a claim of this chapter that understanding the origins of drug facts, as well as the limitations and possibilities of the various information technologies behind them, entails contextualizing drug control in the advent of electronic communication and information technology. Put differently, the disparate projects to translate new information technology into drug control shaped how drug warriors valued their own work, and set the terms for how they imagined effective solutions to drug abuse. One of the consequences was the elevation of information-gathering to the detriment of translating that information into substantive interventions.

As part of its counteroffensive against heroin, the Army unevenly developed and deployed drug-use surveys, crime-data processing and reporting, and anti-drug education. Activists and commanders alike turned *drug facts* into small- and large-scale projects, including when researchers lobbied to conduct the service's first Drug-Use Survey in 1971, and MACV initiated

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[3] While there is hay to be made with urinalysis, vis a vis information technology, it exceeds the scope of this chapter. From the identification labels on urine-sample bottles, through the computer analysis of the contents, to the print-outs on which results might be relayed, the organization of the testing program was a behemoth of logistics and a testament to the techno-management of mass institutions at midcentury. Differently, on information ages within the context of pre-digital/electronic information technology, see Michael Hobart and Zachary Schiffman, *Information Ages: Literacy, Numeracy, and the Computer Revolution*, (Baltimore: The Johns Hopkins University Press, 2000).

Operation Tan Turtle, a data-sharing initiative between military, police, administrative, and diplomatic agencies in 1971 as well.[4] Meanwhile, in 1970 the US supreme command ordered every battalion in the world to develop drug-education teams, and Posts in the US and abroad created "rap houses," meeting places where soldiers could learn from service-certified drug experts and recovering addicts about drugs and drug abuse. This chapter examines efforts to create drug facts in Vietnam and at Fort Bragg between 1967 and 1972. How did their proponents settle on the creation and dissemination of information as a viable avenue through which to pursue drug control? And, what kinds of difficulties operating each technology did technicians discover, and how well did information technologies work for the purposes of drug control?

According to historian Richard Kline, the 1960s and 1970s were "a period of rapid change in computers and communications that formed the basis of what became widely known as

[4] As I expand on later in this piece, the Army's massive drug-use surveys preceded even the National Drug Use Survey and Household Drug Abuse Surveys that the National Institute of Drug Abuse (NIDA) took over in 1974. These surveys are now part and parcel of how Americans and their government identify and interpret trends in drug use, drugs of abuse, and the size of the nation's drug-dependent population. I believe that I've found the National Institute of Mental Health contract from 1968 responsible for creating the diagnostic tool that became the annual Drug Use Survey performed, eventually, by NIDA. See Roger Roffman and Ely Sapol, "Marijuana in Vietnam, A Survey of Use Among Army Enlisted Men in the Two Southern Corps", *The International Journal of the Addictions* 5, no. 1, (Mar. 1970), 1-42. See 1-3.

information technology."[5] This was also a period of rapid change in how the Army produced and organized *data* and *information*; a period in which the DOD and the Army led by the nose research in computer science, systems theory, and the creation of what has come to be known as the internet.[6] The burdens of good and bad data, when turned on drug control, posed a series of opportunities and problems.

How the Army produced and managed data and information in the postwar period steered how it approached drug control. Social science had, since World War I, been a primary means for assessing troop morale, and enemy fatigue.[7] Moreover, investments in social science overlapped with moves to introduce new intelligence methods into warfare, resulting in the Psychological Operations group (PSYOPS) and the Hamlet Evaluation System (HES), the latter a program particular to Vietnam that involved agents from the Civil Operations and Revolutionary

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[5] Kline, "Cybernetics", 514. I still think Steve Shapin and Simon Schaffer's work on scientific expertise and the air pumps offers the most expansive, and simultaneously most specific definition of technology. According to them, technology is not equal to a tool or a machine. Instead, they define technology as "knowledge-producing tools." In their use, they refer to literary, material, and social technologies to understand the dense connections between the air pump, and Robert Boyle's attempts to popularize the scientific method through the demonstration of his air pump. 22-79. Shapin and Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, Reissue, (Princeton: Princeton University Press, 2011).

[6] On the DOD and communication technology in the Cold War see Paul Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America*, (Cambridge: MIT Press, 1996). Arthur Norberg, Judy O'Neill, and Kerry Freedman, *Transforming Computer Technology: Information Processing for the Pentagon, 1962-1986*, (Baltimore: The Johns Hopkins University Press, 1996). Atsushi Akera, *Calculating a Natural World: Scientists, Engineers, and Computers During the Rise of U.S. Cold War Research*, (Cambridge: MIT Press, 2007). Dave Young, "Computing War Narratives". On information processing and non-defense research, see Joy Lisi Rankin, *A People's History of Computing in the United States*, (Cambridge: Harvard University Press, 2018).

[7] I have already discussed psychology in the military in Chapter 2. I will not rehash it here for the sake of word economy.

Development Support (CORDS) surveying Vietnamese citizens about the probability of their becoming Viet Cong or joining the North Vietnamese Army.[8] In still another way, the Army's investments blurred its role vis a vis other national agencies, leading Army officials to see in such surveys an opportunity to fill much needed, but sorely lacking research areas. In each way, information had become the cutting edge of midcentury warfare.

The Army's first drug-use surveys came from Roger Roffman, an Army social worker. Roffman and an assortment of psychologists and psychiatrists conducted and analyzed their own surveys through 1970 in Vietnam, largely left to their own devices. The former lobbied that year to drastically expand the scale of the drug-use surveys, and the DOD contracted its first Drug-Use Survey through the Human Resources Research Office (HumRRO), a federally contracted research center (FCRC). At stake, they claimed, was the only valid way to know soldiers' needs.[9] The two constituencies, that of the social workers and professional researchers, pointed toward the Army centralizing data collection and dissemination about drug users.

Tolson stitched together the ligaments between intelligence gathering and drug control in 1970, when he requested that PSYOPS conduct initial surveys of soldiers' cannabis use. Similarly, in 1971, Military Assistance Command Vietnam initiated Operation Tan Turtle, an ambitious plan to collect and warehouse all manner of data for the purposes of drug control — from crop

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[8] John D. Root, *The Encyclopedia of the Vietnam War: A Political, Social, and Military History*, 2nd ed., "Hamlet Evaluation System", (Santa Barbara: ABC-CLIO, 2011), 449. David M. Berman, *The Encyclopedia of the Vietnam War*, "Civil Operations and Revolutionary Development Support", 209-210. Stanley Sandler, *The Encyclopedia of the Vietnam War*, "Psychological Warfare Operations", 942-945. On HES, see also, Young, "Computing War Narratives".

[9] I detail the efforts of Roffman and others to suggest drug-use surveys as the best starting point for attending to heroin abuse.

rotation schedules to drug-use surveys. This matched efforts in the United States, where local police and military police formed an anti-narcotics intelligence cooperative, the Interagency Bureau of Narcotics (IBN). IBN, unlike Tan Turtle, was less a marvel of information technology, and instead was a testament to how information technology might make up for disinterest at the federal level in domestic enforcement.

U.S. Army Directive 190.11, formulated in 1971, mandated that all battalions develop anti-drug education. In Vietnam, this resulted in programs including Operation JANUS, a way to reach young soldiers considered at risk of heroin addiction using certified counselors, known as the Drug Abuse and Rehabilitation Team. At Fort Bragg, soldiers received a rap house and Awareness Counselors, service-certified drug experts, where and with whom they received an authentic education in drug addiction. At the top, by 1971, MACV re-strategized, centralizing education, and creating dedicated drug education teams and a single curriculum.[10]

As much as the period between 1967 and 1971 was one of gradual centralization, network building, and scaling up between surveyors, police, commanders, and others, it was also a period of coming apart. For one, the sheer scale of information collected was a bear. The collection and dissemination of drug-use surveys attested to how decentralized, and often cacophonous the various projects were. Further, extensive problems existed in terms of sharing research results, and still more problems turning surveys into actual interventions. Researchers operated largely

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[10] On centralization, see General Accounting Office, "Review of Drug Abuse Program in Vietnam", date unknown, RG 472 Records of the United States Forces in Southeast Asia Headquarters, Military Assistance Command Vietnam (MACV) Provost Marshal Office of the Provost Marshal, General Records 1970 Box 2, Drug Working and History Papers Folder 1 1971. On anti-drug education, see 10-18.

in ignorance of one another, and their results were just as likely to disappear into a paperwork abyss as they were to receive an audience.

While the creation of IBN and Tan Turtle attested to the union of information technology and policing, it also attested to the fragility of information sharing in the age of the computer. Conflicts concerning the *lack* of assistance from the federal government resulted in, as the case of Doran Berry and IBN suggests, cooperative policing, while agents deployed new information technology, ostensibly, to paper over disinterest in drug control from peer police agencies abroad.

Anti-drug education was the least effective of the information technology conceived for the counteroffensive. On the one hand, while the Army ordered mandatory drug education, the policy left anti-drug education up to each Post, and Posts abroad and in the U.S. organized classes with varying levels of efficacy and preparation. Further, other social and staffing conflicts ensured that anti-drug education might be permanently hobbled for want of capable instructors.

The following chapter is divided into three sections and a conclusion. In respective order, I deal with drug-use surveys, crime-data sharing and cooperative policing, and anti-drug education. In each of their respective sections, I describe in more detail the origins of each information technology in the practice of drug control, and identify the problems of disseminating information using those technologies.

Paper Wars: Drug-Use Surveys in an Age of Paperwork

In this section, I detail the origins of the Army's drug-use surveys, pointing to their utility in the management of the postwar Army, and their centrality to strategy in Vietnam. I also discuss

efforts by the 13th PSYOPS battalion to influence drug control at Fort Bragg by staging their own drug-use survey. I then discuss the issues that surveyors encountered when they tried to share their findings.

Roger Roffman was already stationed at Long Binh, an Army Post in southern Vietnam between Saigon and Bien Hoa, when the Provost Marshal's Office requested a study of marijuana use. At the time, Roffman was a social worker from the Army's medical division. The division, formed after World War II, essentially tied the Army to providing social services the Army's newly created Army Community Services division, itself only organized two years earlier. His job at Long Binh involved, in part, part-time work with soldiers incarcerated at the Post stockade.[11]

It was early 1967 when the MACV Provost Marshal, effectively the head of all military police in Vietnam, contacted Roffman. The office requested that Roffman study soldiers incarcerated at Long Binh Stockade, specifically to "ascertain among the Stockade inmates the percentage who had ever used marihuana and the extent to which it had been used in Vietnam."[12] The study that Roffman completed surveyed ninety-five soldiers: twenty-one specifically convicted for cannabis offenses, and another seventy-four incarcerated for other offenses. His findings were that 63% of those surveyed had used cannabis, while 37% claimed not to have smoked it. They also found that the overwhelming majority, some 85% of those surveyed, had received cannabis from a

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[11] Roffman offers a detailed account of producing the surveys and his time at Long Binh. See Roffman, "Prepared Statement of Roger A. Roffman", in "Hearings before the Subcommittee to Investigate Juvenile Delinquency", 91st Congress, Mar. 24, 25; Aug. 18-20, Oct. 30, 1970, (Washington, D.C.: U.S. Government Printing Office, 1971), 6434-6439.
[12] Roffman, "Statement", 6434.

friend. The biggest finding, which was maybe not a surprise, was that soldiers imprisoned for cannabis possession were more likely to smoke cannabis, and to use it more often.[13]

Still, when Roffman received the request, he did so in part because of the Army's recent emphasis on human resources management — which had evidenced itself in projects as distinct from one another as Army Community Services to Project Transition to the creation of HumRRO. Roffman belonged to a then twenty-year-old corps of social workers, who used professional training in sociology and social work as the basis of intervening in the lives of soldiers.[14]

Unfortunately for Roffman, he was asked to complete a second study shortly after completing the first. Impressed by or fearful of the results, the USARV Consultant in Psychiatry requested that Roffman expand his study to "the non-institutionalized Army population" in the summer of 1967. As Roffman recalls, the request came through the intermediary from the USARV Surgeon. In the summer of 1967, Roffman began the second study, collaborating with Captain Ely Sapol, a psychologist stationed in southern Vietnam. Both were attached to the 935th Medical Detachment. For the second study, Roffman and Sapol surveyed 584 soldiers from the general population in the Southern Corps area who were finishing their tours in Vietnam.

Following Roffman and Sapol's studies of cannabis use among soldiers stationed in Vietnam, others followed suit. It must be said that they were piecemeal studies. One such study was by Captain Morris D. Stanton, a psychologist. Stationed at the 22nd Replacement Battalion in Cam Ranh Bay, he presented 2,547 soldiers with questionnaires, ultimately culling a little under 200

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[13] Roffman, "Statement".
[14] Herold, "The Evolution of Dependent Medical Care".

questionnaires to arrive at 2,372 soldiers surveyed. In July 1968, Edmund Casper, an Army psychiatrist collected a study of soldiers in Chu Lai, while Wilfred Postel, another psychiatrist, surveyed patients at a surgical hospital in Pleiku. Similarly, following riots at the Long Binh Stockade in 1968, the prison's Chief of Mental Hygiene Consultation Service ordered the collection of questionnaires of prisoners until at least August 1969.[15]

Across Vietnam, psychologists and psychiatrists began to survey soldiers about their drug use. The situation was not much different, as it played out at Fort Bragg. When Lieutenant General John Tolson activated Operation Awareness in spring 1970, he began with a pilot survey of drug use. Codenamed Project SODA, the survey was a project by the 13th Psychological Operations (PSYOPS) given to all enlisted soldiers of the 82nd. Tolson described the purpose of the survey as vital to constructing interventions under the umbrella of Operation Awareness, i.e. that survey data might reveal patterns in drug use that inform revised interventions. At the invitation of the commander of the 82nd Airborne for such a survey, Tolson delegated the survey to Lieutenant Colonel Douglas Waters, the commander of the PSYOPS brigade responsible for the survey. Tolson wanted quick turn around on the survey too, in order to continue building up their understanding of heroin abuse. As Tolson explained mere months after the first survey, "follow-on phases are presently in progress to verify and extend the data derived from the initial study."[16]

Waters described SODA with the flair you might expect from a career soldier. Explaining the purpose of the survey, he relayed that his battalion "was directed to assist the commanding general, 82d Airborne Division, with the design and execution of a psychological campaign to

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[15] Roffman provides a great overview of the other surveys conducted in Vietnam. See Roffman, "Statement".
[16] Tolson, "Statement", 15.

turn the average soldier against the use or sale of marihuana and drugs, particularly needle-injected drugs."[17] As Waters implied, Tolson had found a new use for his PSYOPS potential — that of drug control. Counterinsurgency begat counternarcotics.[18]

That Tolson had recruited Waters and PSYOPS to perform the study, rather than a social worker is telling. Although psychological warfare goes back millennia to the use of battlecries and war paint, PSYOPS was a specific invention of the Cold War intended to perpetrate propaganda and misinformation campaigns specifically in counterinsurgency campaigns. Founded in 1952, its former names offer some indication of its purpose, the Loudspeaker and Leaflet division. The group created war propaganda, and used surveys to evaluate their work. This is to say that surveys were the bread and butter of their work.[19] The 13th PSYOPS had only recently returned from Vietnam.

PSYOPS plied their militarized social science elsewhere in Vietnam, while MACV continued to splurge on other modes of psychological warfare. For example, MACV instituted in 1967 the Hamlet Evaluation System (HES), a method of surveying morale amongst the South Vietnamese at the level of the hamlet, to be led by the Civil Operations and Revolutionary Development

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[17] Waters, "Statement", 145.

[18] On a recent treatise on the transition from counterinsurgency to counternarcotics, see Weimer. Seeing Drugs. On US, the Cold War, and anti-narcotics policing, see Matthew R. Pembleton, "Imagining a Global Sovereignty: U.S. Counternarcotic Operations in Istanbul during the Early Cold War and the Origins of the Foreign 'War on Drugs'", *Journal of Cold War Studies* 18, no. 2, (Spring 2016), 28-63. On Mexico and US anti-narcotic efforts abroad, see Teague. "Mexico's Dirty War on Drugs".

[19] There are woefully few histories of PSYOPS specifically. On PSYOPS, see Mervyn Edwin Roberts, "Let the Dogs Bark: The Psychological War in Vietnam, 1960-1968", Dissertation, University of North Texas, (May 2016). Charles H. Briscoe, "A Clearer View of Psywar at Fort Riley and Fort Bragg, 1951-1952", *Veritas, Journal of Army Special Operations History* 5, no. 4, (2009), 60-69.

Support (CORDS).[20] The idea behind the evaluative system was to gauge occupied populations' morale and political sensibilities. There are a few observations here. First, that social workers and PSYOPS turned to surveys to connect soldiers with benefits *and* to wage war against peasant populations. Second, that the services took such surveys seriously as the basis for evaluating military strategy. Third, Tolson believed that the collection and interpretation of survey results might best direct his strategies in a heroin counteroffensive.

Interest in such surveys continued to build. Indeed, it was through initial research that military investigators began to notice a pattern: that there just wasn't much in the way of large data sets about drug abuse. Lieutenant Colonel Jimmy Hatfield, then chief of the Army's Biomedical Stress Research Division, commented on the situation facing the department: "we have a paucity of significant civilian and military research findings to provide us with realistic solutions to these problems."[21] He specified the situation further, noting that "few of [the 2,000 such publications] meet modern standards of scientific investigation," and the studies were "ill-documented and ambiguous, emotion-laden and incredibly biased, and can, in general, be relied upon for very little valid information."[22]

The Army's response to the supposed knowledge gap had resulted in individual researchers initiating studies. Hatfield hoped to address the piecemeal work dripping out of Vietnam, utilizing a grander vision of the same surveys. "The Department of the Army should initiate a

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[20] On the Hamlet Evaluation System, data production, and war strategy, see Young. "Computing War Narratives".

[21] Jimmy Hatfield, "Prepared Statement", in "Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics", Nov. 17 and 18, and Dec. 2 and 3, 1970, 91st Congress, Second Session, (Washington: 1971), 437-448. Quote on 442.

[22] Hatfield, "Prepared Statement", 441.

worldwide survey of…the marijuana problem" because such a survey, in his words, "is vitally needed to supplement previously reported criminal investigations data and to augment the small survey studies that have developed in an unsystematic manner."[23] Further, Hatfield described the Army's willingness to conduct the survey as "most favorable," and noted that "Lieutenant Colonel Kibler, as a representative of Department of Defense Research and Engineering, strongly endorsed the…worldwide survey."[24] From Hatfield's perspective, big numbers meant better prediction and enforcement.

That Hatfield could even begin to suggest that the Army study the effects of marijuana in the military depended on the Army being in the position to set unlimited boundaries around its research. Or, put differently, broad imperatives including "military necessity" and "military relation" justified capacious interpretations of the same. The infinite reach of military relation caused some amount of confusion, as Hatfield also noted the occasion to study marijuana had led him and NIMH staffers to set up a research program so as not to duplicate efforts.

By 1971, the DOD contracted a much larger version of this survey for soldiers in Vietnam and in the U.S., a project that fulfilled Lt. Col. Hatfield's wishes. Submitted for the fiscal year of 1971, the Human Resources Research Office sent a draft copy of their "A Study of the Use of Marijuana Within the Army." Contrasting the potential HumRRO survey with those of the past, Hatfield drew a sharp distinction. The earlier studies, he claimed, did "not permit generalizations about the circumstances surrounding marijuana usage." HumRRO's planned study, he claimed,

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[23] Hatfield, "Prepared Statement", 444.
[24] Hatfield, "Questions for Lt. Col. Jimmy L. Hatfield, USA", in "Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics", Nov. 17 and 18, and Dec. 2 and 3, 1970, 91st Congress, Second Session, (Washington: 1971), 451-469. Quote on 452.

"a similar worldwide survey plan should receive serious consideration" because "such a survey is urgently required in order to essentially define the nature and magnitude of the marijuana problem," which, he concluded, "is a mandatory prerequisite to a major research commitment in marijuana research."[25] HumRRO's survey occurred in 1971, and then again in 1972. For 1973, the DOD contracted A.D. Little, another human-resources research corporation, to conduct, more or less, the same study.[26]

The difference between the pre-1971 surveys, and those conducted by FCRCs after 1971 is telling. In large part, the studies differed in terms of scope. The latter studies included a sample population much larger than the efforts of Roffman, Stanton, and others. Whereas those earlier studies had rested on less than 1,000 respondents answering upwards of forty questions, the 1971 study alone included over 49,000 respondents from the Army, Navy, Air Force, and Marine Corps answering over seventy.[27] Additionally, the work that HumRRO and later A.D. Little added was that they brought counting machines and weighting algorithms into the analysis of questionnaires.

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[25] Hatfield, "Prepared Statement", 446.

[26] HumRRO's survey can be found RG 341: Headquarters U.S. Air Force (Air Staff), Entry P3, Drug Abuse and Alcoholism Control Program, Second QTR CY 74 RCS HAF DPZ (Q) 711 THRU DOD Survey of Illicit Drug and Alcohol: A Report of Major Findings, Box 5, Folder The 1972 DOD Survey of Illicit Drug and Alcohol Use A Report of Major Findings 30 Dec 1974.

[27] "Questionnaire Development", date unknown, Appendix A, 2. RG 341: Headquarters U.S. Air Force (Air Staff), Entry P3, Drug Abuse and Alcoholism Control Program, Second QTR CY 74 RCS HAF DPZ (Q) 711 THRU DOD Survey of Illicit Drug and Alcohol: A Report of Major Findings, Box 5 A-2, Folder The 1972 DOD Survey of Illicit Drug and Alcohol Use A Report of Major Findings 30 Dec 1974.

Trouble began early for Roffman's study. Despite requests for the survey findings, he and Sapol were barred from sharing the results. When Roger Roffman made his way back to the United States in 1968, he didn't want to leave empty-handed. Having taken his time in 1967 to create two surveys of soldiers in Vietnam, he wanted to crunch the numbers and start publishing his results. Instead, as he discovered on his transfer to Walter Reed Hospital, the objective wouldn't be easy. As Roffman describes his experience trying to travel with the information that the surveys had produced, an amorphous and anonymous bureaucracy effectively shut him down. With his tour in Vietnam coming to a close, someone ordered Roffman to request formal approval to take his data with him to Walter Reed. He was denied. It was only after a staffer in the Army Surgeon General's office intervened that Roffman received his data, and then only six months later. And yet, even still, as Roffman captured the issue in miniature, "I had been ordered to leave the questionnaires in Vietnam."[28]

More than just stopping Roffman from traveling with or sharing his particular survey, surveyors were just as likely to languish in isolation with their results. And, the difficulties weren't unique to Roffman and Sapol's work. Take the example of Captain M.D. Stanton, the author of a 1969 survey of soldiers. As a senator explained to Stanton, "not only are the field grade officers not aware of [your survey] but the public relations officer on drug abuse for the Pentagon did not seem to be aware of it…if you indicate a communications problem with field grade officers, maybe you have one in-house."[29]

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[28] Roffman, "Statement", 6472.

[29] Harold Hughes, in "Statement of Capt. Morris D. Stanton, Chief Psychology Section, Mental Health Hygiene Consultation Service, Fort George G. Meade, MD., Department of the Army", 672-683. Quote on 674.

And, it was these breakdowns that threw still other wrenches in the gears. According to Roffman, the best way to make use of the data of his survey was to widely distribute it. At the time of Roffman's testimony, he was in the process of earning his own doctorate in social work. His professional ties may have influenced him to think that quick dissemination the surveys would mean more effective interventions. Roffman himself recommended a number of changes to how confront the lack of communication of the surveys. Among those suggestions, Roffman offered that the Army establish a social scientist to work on studying cannabis in Vietnam, that the DOD "conduct its own research," and that the DOD "be required to report its findings to this committee and to the public through professional publications."[30] That said, as Dr. James Imahara explained of his own study of cannabis use at Long Binh Stockade, his own publications remained in professional journals; psychiatry in his case.[31] What Roffman identified, and what Imahara seconded, was a belief in the inability of their numbers to be meaningful in the absence of dialogue and debate with other scholars. Or, put simply, Roffman saw himself as like a single hand trying to clap. He needed stimulation, but Vietnam was a boon.

Still, Roffman's experience doesn't quite jibe with how the Army made use of survey results. Put differently, it would be easy to conclude that Roffman simply had results that would potentially embarrass the Army. But, that still doesn't explain the communication breakdowns that repeatedly occurred in transmitting the results. Further, even with those potentially embarrassing statistics n hand, the rigamarole that Roffman endured bringing his data back to the

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[30] Roffman, "Statement", 6481.

[31] James Imahara, "A Study of Violent Crimes in Vietnam, A Preliminary Report", in "Hearings before the Subcommittee to Investigate Juvenile Delinquency", 91st Congress, Mar. 24, 25; Aug. 18-20, Oct. 30, 1970, (Washington, D.C.: U.S. Government Printing Office, 1971), 6426-6428. Quote on 6428.

United States suggests how little centralized control there was over the enterprise of creating and sharing the surveys.

Further, Tolson's use of the 13th PSYOPS battalion suggests something different — that these surveys were intended to be used, at the least, and that the Army did take seriously the information that it could glean from questionnaires.

The problems with these surveys, according to the survey's contemporaries, were myriad, and weren't reducible to commander's caprice, as Roffman and others' experiences demonstrate. For instance, social workers and social scientists remained wary of applying their survey statistics to actual interventions on broad populations. Suspicions about accuracy and applicability abounded. Speaking in 1970, Captain M.D. Stanton explained that Roffman and Sapol's study was already irrelevant, because it "has the contamination of being 2 years prior, and they were studying people in the southern provinces." Stanton even said of Roffman and Sapol's study that "I was aware of it but I had not known whether it was released."[32]

And, it was this early period in which surveyors had to contend with an unwilling or disinterested army bureaucracy. When complimented for initiating the study, Stanton corrected that "this survey was primarily the brain child of Dr. William Cates and myself," and that when he began "[he] was not aware of the Roffman-Sapol study." As one congressman lauded Stanton for his efforts, he descried the opportunity as one for which Stanton had to excel on his own. Or, as the congressman put it, "you had to do some hustling to get the study down on your own…I

[32] Stanton, "Statement", 682.

am trying to point out your efficiency and initiative."[33] In the pre-1971 period, there was little central control over the production of these surveys.

There was more at stake in the surveys than just communicating the results. Indeed, professional skepticism stoked the need for more surveys. As Captain Stanton explained, "it is the opinion of this author that if distortions have been introduced into the data, that they are generally in the direction of underestimation."[34] Stanton acknowledged further that "for a drug user to admit on paper that he has been or is using these substances may be asking a great deal, especially if he fears the paper will be fingerprinted and traced to him."[35]

Lt. Col. Waters, the commander of the 13th PSYOPS Battalion, also hedged the survey that he and his soldiers had conducted at Fort Bragg. While the survey had provided "some very interesting data," Waters cautioned that "the statistical results and conclusions should be viewed in the context of the experimental nature of the survey itself and its limited parameters. We do not consider the results conclusive. They have not been validated." Further, he claimed that despite the sample size his team had selected, "the actual sample was degraded by no-shows when the questionnaire was administered" and "the data should not be extrapolated to overall Army troop strength."[36]

The case of drug-use surveys suggests that while the military committed itself to surveying the problem away, concerns about the applicability of the findings, as well as the preponderance of multiple, competing findings meant that the surveys rarely translated to practice. Indeed, it was also the additional ways that researchers saw values in their surveys — that their isolation

[33] Stanton, "Statement", 682.
[34] Stanton, "Statement", 672.
[35] Stanton, "Statement", 677
[36] Waters, "Statement", 146.

rendered their results incomplete, that to have value other scientists had to read and confirm their work, and that big numbers were somehow better indicators of the Army's drug problem.

Into the Data Hopper: Crime Data in the Electronic Age

In this section, I detail two military programs — the Interagency Bureau of Narcotics and Operation Tan Turtle — intended to share data between policing agencies. I show how actors attempted to put them to use to paper over social and institutional conflicts with regards to data sharing, without solving the underlying problems. These problems continued to beset even the new technologies.

Operation Tan Turtle and the Interagency Bureau of Narcotics (IBN) were birds of a feather — drug-enforcement cooperatives intended to sew together the efforts of civilian and military police in Vietnam and in the US, respectively. Tan Turtle was a joint effort by MACV and the American mission in Vietnam, while the IBN was a joint effort by city, state, and military investigators.

Formed in 1968, IBN came from calls from a local prosecutor in Fayetteville, Doran Berry, who demanded that the drug problem at Fort Bragg required a multi-agency solution. He intended for it to be a thirty-day pilot. According to Berry, "we don't want to appear to like a buzzard to our community, flying around and just seeing a dead carcass…But from a drug-oriented standpoint, our community is in trouble."[37] Further, Berry claimed in regards to the source of heroin available in Fayetteville and at Fort Bragg, "all of the information that we have received is that

[37] Doran Berry, "Statements of Pat Reese…and Doran Berry", 197.

there is a very little bit coming in directly."[38] Instead, Berry imagined that the city's heroin problem resulted from the international trade, and especially because the proximity of Fort Bragg and nearby Seymour-Johnson Air Force Base to Duke University, the University of North Carolina tied the city's youth to an outside problem. As Berry put it, "there is no fence between Fayetteville and Fort Bragg."[39] From this perspective, IBN was a solution to a problem that ignored legal jurisdictions.

The effects of heroin in the community were legible as statistical data points — crime stats, man hours, and police training. Indeed, as the Cumberland County Drug Abuse Committee, an activist group, put it in an informational pamphlet: "the ever-increasing illegal drug traffic has brought extreme burdens upon our law enforcement agencies and their resources." But, what were those burdens? "Not only have we had to allocate manpower, facilities, equipment and money to the enforcement," but, as the members concluded, "we have had to undertake new training programs, institute new methods and techniques for…apprehension."[40] According to the members of the group, the drug problem imported new problems into police work. IBN, then, was a chance to lead policing changes by the nose.

Due to what locals and Fort Bragg officials believed was the consequence of both the international trade in heroin and a lack of credible, ongoing information exchange with other policing agencies, Berry averred that his city was fighting with one arm tied behind its back. Berry described the situation thusly: "from a law enforcement standpoint . . . we have been unable to get back to the ultimate source within our own country." Still, the situation, depending

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[38] Berry, "Statements of Pat Reese…and Doran Berry", 196-197.
[39] Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", 215.
[40] Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", 221.

upon your perspective, devolved into what Berry described as "a slapstick comedy-type thing."[41] Describing the situation further, Berry explained how determinative the lack of information-sharing had become:

> During one of my sessions? at court as I was preparing the cases, I found there were people out buying drugs for the sheriff's department arrested by members of the city police, people out buying for the State bureau of investigation being arrested by the sheriff's department-type thing.[42]

But, Berry's frustration with the policing mishaps was not just a matter of comedic failures. Instead, the problem with information sharing, Berry decided, came from disinterest on the part of Federal Bureau of Narcotics (FBN) agents to assist in enforcement.

Indeed, Berry had planned for IBN to include city, county, state investigators, and military police as well as the FBN. Berry recounted that while an FBN agent had attended the very first meeting of IBN in 1968, he had basically left all the other parties to figure out a solution on their own. FBN's rejection of the offer to join IBN resulted in yet another unfortunate situation.

> The [FBN], they came to our first meeting and were going to do this, were going to contribute that, and do all that they could. We haven't seen them since. Every time we call them in to help with information back from out of the State it is almost like a slapstick comedy-type thing. We spent $700. We got out and gathered $1,000 because of a major source in New York City that was shipping it directly into our community. We got a rental care?. We had the informant. We had our people right with them. They agreed they were going to handle everything on the other end. We spent every bit of our $700 getting the information and the people up there, and they wouldn't even give their own agents the money to make the buy. They didn't do a thing except make us waste all our money.[43]

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[41] Berry, "Statements of Pat Reese…and Doran Berry", 198.
[42] Berry, "Statements of Pat Reese…and Doran Berry", 198-199.
[43] Berry, "Statements of Pat Reese…and Doran Berry", 198.

In another instance, Berry described the mysterious calculus of FBN agents. As Berry told the story, "I was in court one day and word was left that a new man from [FBN] was out there to see me. When court recessed he was gone and to this day I still haven't seen him, and I was in office 2 years after that, almost."[44] From Berry's perspective, and likely the perspective of other locals, FBN actually detracted from their ability to effectively police, rather than provide an obvious improvement.

The situation in Fayetteville played out again in Greensboro, a small city in eastern North Carolina. FBN agents, Berry claimed, "agreed to contribute some of the money to make purchases there," but, as in Fayetteville, "we haven't gotten 1 cent from them anywhere within the State, to my knowledge."[45] To make matters even worse, as if rock bottom were an infinite well, FBN only placed two agents in the entire state. At most, Berry hypothesized, FBN had accepted a few local investigators for police training at FBN.

Why FBN was so disinterested is a bit more interesting. As Kathleen Frydl has argued, FBN and later BNDD agents turned toward international drug control rather than increase their domestic reach in the Vietnam era.[46] This lines up with Berry and Fayetteville authorities' fraught relationship with narc agents operating in North Carolina. It also points to how locals were pushing the federal entities to expand their anti-drug activities, despite federal indifference.

Despite FBN's apparent disinterest in the program, the state and military investigators were more enthusiastic. Set up as "a basic experiment," the program launched with great success. According to Berry, there was evidence of the program's apparent success within a year — in

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[44] Berry, "Statements of Pat Reese…and Doran Berry", 198.
[45] Berry, "Statements of Pat Reese…and Doran Berry", 199.
[46] Frydl, The Drug Wars, 387.

1969 Cumberland County, the county that partly surrounds Fayetteville, had more drug arrests than the rest of the state. And, it was the collaborative policing system that made up, in part, for FBN's disinterest. "'Cooperation with the federal people,'" Berry began, "'has been terrible. I'm not blaming these boys out (at Fort Bragg), they can't help it.'"[47] Instead, the teams composed of military and civilian investigators "worked as a single unit, compiling information, conducting intelligence and surveilance [sic] and began to penetrate the drug-oriented culture."[48] And, here, the situation was as plain as day: with the federal agency tasked with drug control playing hard to get, locals lobbied the military to expand surveillance capabilities.

IBN's success in its first year of operation led to IBN transitioning from an experiment to a permanent mode for policing the area. Both city and county investigators had appreciated the experiment so much that they requested from the city council additional officers in 1970. By 1970, the former experiment had metastasized into two separate squads, with the military supplying clerical and administrative soldiers, as well as numerous informants.

To be sure, IBN wasn't just a means of assigning more police to narcotics work, or to coordinate stings and supply funds. Instead, as Berry and Reese both claimed, "our law enforcement agents in the area of drugs and narcotics have no effective structure or organization within which to process our intelligence about out-of-state suppliers."[49] Instead, when investigators attempted to get tough on narcotics and cannabis, they simply didn't have the institutional connections to spread investigations beyond the city and county, let alone the manpower available to focus just on drugs.

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[47] Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", 212.
[48] Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", 231.
[49] Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", 223.

Berry's experiences here attest to the depth of disarray within which IBN was formed. It also attests to a growing sense of collaboration and centralized drug control operated, not from FBN headquarters in Washington, D.C., but locally, from concerned activists. One senator, speaking to Berry, explained that "you are giving us a grassroots thing."[50] It also attests to how novel and acute the sudden focus on narcotics had become. To wit, in 1970, North Carolina's State Bureau of Investigation was only beginning to put together a narc team, which Berry described as "the skeleton phase" and that the state's investigators had only recently begun "to build a statewide network for some correlation."[51]

IBN's origins contrast here with those of Tan Turtle. Tan Turtle was a project initiated in 1971 by MACV to create and share crime-data, as well as all military, diplomatic, and scientific data gathered about narcotics production and distribution in southeast Asia (Illustration 6).[52] Envisioned as data hopper, Tan Turtle collated all the disparate forms of information that it and related agencies produced that had direct or indirect relevance to drug control.

Tan Turtle reflected a different version of police centralization in the period. Whereas IBN premised itself on manpower and face-to-face intelligence sharing, Tan Turtle emphasized the

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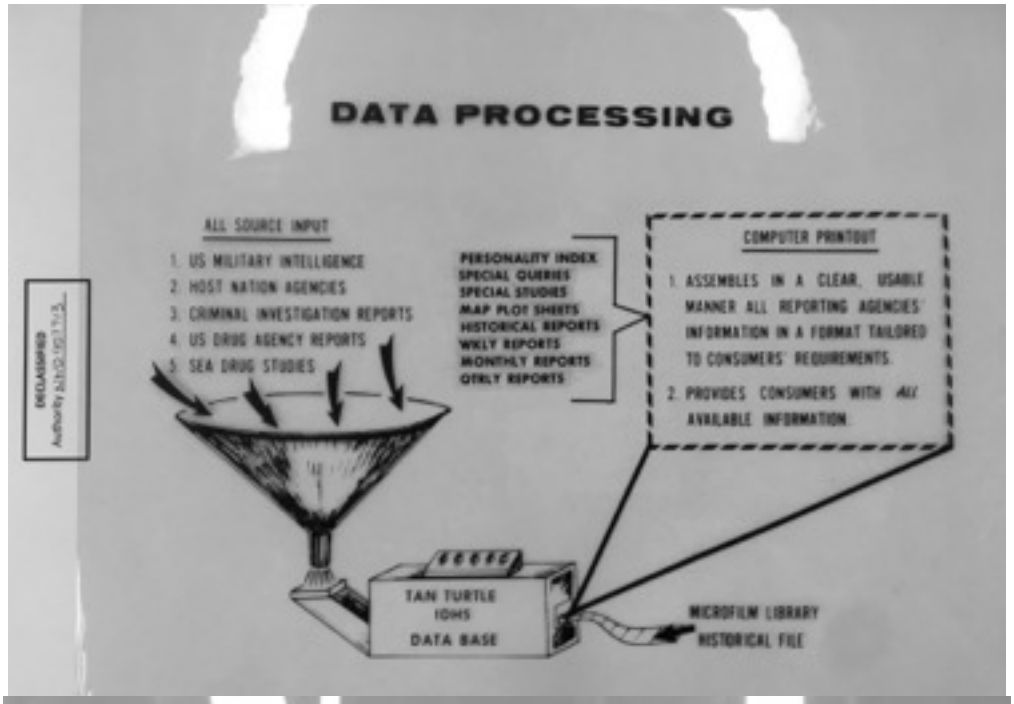
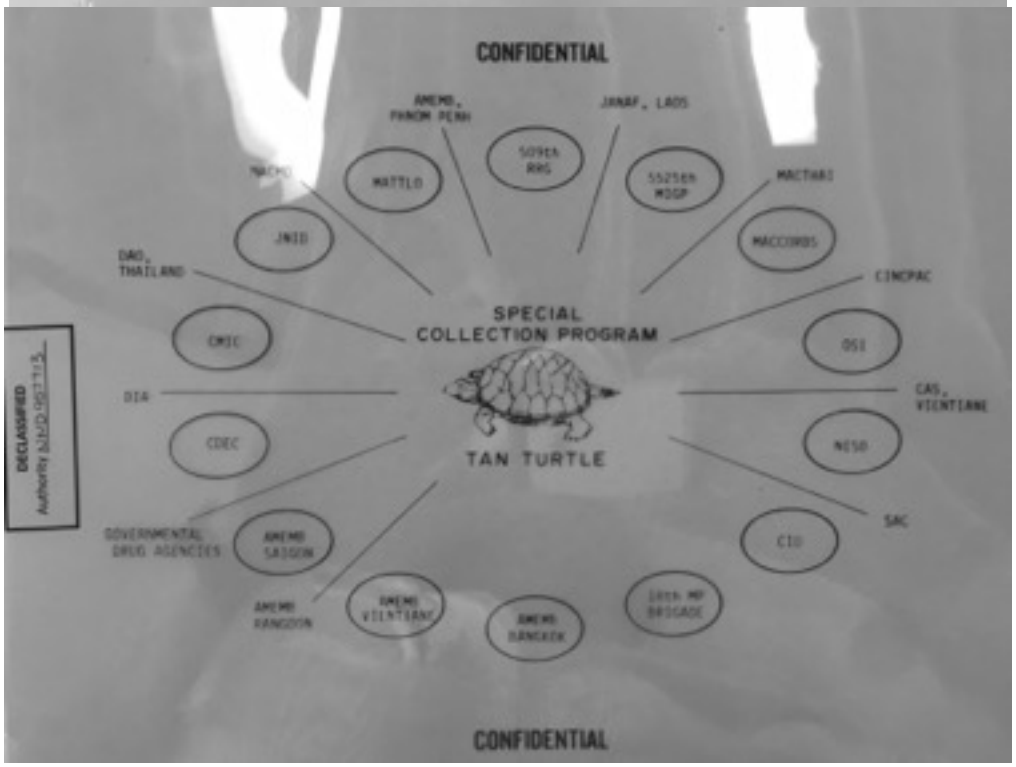
[50] Ralph Yarborough, in "Statements of Pat Reese…and Doran Berry", 200.
[51] Berry, "Statements of Pat Reese…and Doran Berry", 199.
[52] On flowcharts and systems thinking, see Hunter Crowther-Heyck, "The Organizational Revolution and the Human Sciences", *Isis* 105, no. 1, (Mar. 2014), 1-31. On machine as metaphor in the Cold War, see Edwards. *The Closed World*.

Illustration 6

Operation Tan Turtle — the data hopper. Source: RG 472 Records of the United States Forces in Southeast Asia Headquarters, Military Assistance Command Vietnam (MACV) Provost Marshal Office of the Provost Marshal, Briefing Files, 1971 Box 1, Briefing File — Tan Turtle.





union of technology and manpower, of intelligence and electronics. According to one directive, Tan Turtle "[established] a centralized collection and machine processing system [sic] to satisfy MACV EEI concerning the production, transportation, and distribution of drugs in the MACV area of interest."[53] In their own graphic, the program "assembles in a clear, usable manner all reporting agencies' information in a format tailored to consumers' requirements," thus "[providing] consumers with *all* available information."[54] And planners did mean all, as reports produced were to make information available related to topics as diverse as property ownership as well as purity of quantity of heroin. Using an unnamed data processor, the device supposedly offered instant information, culled from all available sources. Knowing drugs never seemed so easy.

But, the electronic angle was just one of the supposed benefits of the initiative. Tan Turtle, MACV may have stated, was a way to link its counterinsurgency activities with drug interdiction. According to military officials, the close associations between the supposedly invisible Viet Cong and the Vietnamese responsible for trafficking heroin meant that drug interdiction necessarily had to co-exist with counterinsurgency.[55] Tan Turtle is an interesting

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[53] "Transmittal of Special Collection Program, Narcotics and Dangerous Drugs, SEA), Aug. 14, 1971, 1. RG 472 Records of the United States Forces in Southeast Asia Headquarters, Military Assistance Command Vietnam (MACV) Provost Marshal Office of the Provost Marshal, General Records 1970 Box 3, Tan Turtle (Spec. Collection Program) Code Book '72.

[54] "Data Processing" transparency slide, date unknown. RG 472 Records of the United States Forces in Southeast Asia Headquarters, Military Assistance Command Vietnam (MACV) Provost Marshal Office of the Provost Marshal, General Records 1970 Box 3, Tan Turtle (Spec. Collection Program) Code Book '72. Emphasis in original.

[55] Scholars have questioned the evidence of North Vietnamese and Viet Cong troops growing and selling opiates as part of its campaign. Instead, as hindsight has shown us, it was the Central Intelligence Agency behind the narcotics. On the evidence, see Kuzmerov, *Myth*. Schneider, *Smack*. Frydl, *The Drug Wars*. On the CIA's role in growing and trafficking opium in Southeast Asia, see McCoy, *The Politics of Heroin*.

case in which federal agents and MACV administrators found common ground, especially compared to their peers at Fort Bragg.

Tan Turtle did not materialize from the ether; there was precedent for centralizing police records in the United States. In 1929, the International Association of Chiefs of Police generated the first Uniform Crime Report (UCR). Significantly, as one historian has noted, it was social scientists at the Bureau of Social Health, an important Rockefeller Foundation laboratory in New York City, who helped steer interest in such a report.[56] The first report came out in 1930, the same year that the Bureau of Investigation — soon to be the Federal Bureau of Investigation — took responsibility for assembling it. UCR received a long-overdue update in 1962, when the FBI created the first Supplementary Homicide Reports, which added characteristics of the crimes rather than just head-counts.[57] This is to say that in the 1960s, policing agencies renewed interest in the value of data sharing.

Even with the influences of UCR and counterinsurgency, the programs still shared an important similarity. Both were recipients of the Law Enforcement Assistance Act of 1965 and Omnibus Crime Control Bill of 1968 (LEAA).[58] The bill, signed by President Lyndon Johnson in 1968, set up incentives for police departments to expand their available materiel, information tech, and

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[56] Lawrence Rosen, "The Creation of the Uniform Crime Report: The Role of Social Science", *Social Science History* 19, no. 2, (Summer 1995), 215-238.

[57] On UCR, Donald Faggiani, *Encyclopedia of Law Enforcement*, (Thousand Oaks: Sage Publications, Inc., 2004), accessed online, www.sagepub.com, (Jun. 2019).

[58] While the literature on LEAA has emphasized how the bill fostered tech transfers between the US military and civilian policing agencies, namely the arming of domestic police departments with riot gear and assault weapons, its status in the "information revolution" has been sorely under-recognized. Elizabeth Hinton has drawn up a shopping list of areas in which historians might interrogate the information revolution alongside attempts to modernize policing. See Elizabeth Hinton, *From the War on Poverty to the War on Crime: The Making of Mass Incarceration in America*, (Cambridge: Harvard University Press, 2016), 17.

available manpower. One of those incentives was the use of LEAA funds to develop the use of electronic records by police departments.[59] Using the LEAA's research center, the National Center for State Courts, for example, LEAA advised state courts on how to involve computers in cataloguing information. No less than fifteen states accepted LEAA funds to computerize their records by 1973.[60] In short, LEAA encouraged police to go electronic. In 1964, St. Louis became the first city to use a "real time police computer system." The tech spread like a disease. 1968, as one MIT professor at the time noted, "the number of computer applications reported by police departments throughout the country have more than doubled." Or, put differently, "in 1971 one out of every five computer applications was devoted to the rapid retrieval of information pertaining to outstanding warrants, stolen property, or vehicle registration."[61]

But, there's another factor to consider. As other scholars have noted, despite American interest in expanding drug control in the period, they discovered that Vietnamese officials (and officials in neighboring countries) were not fans of new anti-narcotics imperatives. In fact, it was often those same officials and their own police agencies that fostered and protected the opiate trades in

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[59] On the LEAA and tech transfers between the federal government and local police departments see Micol Siegel, "Objects of Police History", *Journal of American History* 102, no. 1, (Jun. 2015), 152-161. She speaks directly to tech transfers on p. 159. On uses of drug control and tech transfer as a means of U.S.-funded intervention and development abroad, see Weimer. *Seeing Drugs*, 113-131, 172-214. Although there is not sufficient space to unpack this problem here, I am noting a persistent theme, that of "systems thought." Supporters of new crime tech, Tan Turtle included, envisioned new information technology as creating a more efficacious "system." Or put differently, that information cemented different actors into a total system. On systems thought and the sciences, see Heyck, "The Organizational Revolution".

[60] "LEAA 1973: LEAA Activities July 1, 1972 to June 30, 1973", U.S. Department of Justice, (Washington, D.C.: Government Printing Office, 1973).

[61] Kent W. Colton, "Computers and Police: Patterns of Success and Failure", *Sloan Management Review* 14, no. 2, (Jan. 1972), 75-98. In order, the quotes appear on 75, 79.

their respective countries.[62] This is to say that, taken from the perspective of diplomatic motivations, it's possible that Tan Turtle was as much an attempt to bypass disinterested peers in Vietnamese, Thai, and other national police. If IBN was a solution, in part, to federal indifference, is it possible that Tan Turtle was a similar strategy, to bypass disinterested foreign peers?

The significance of the new tech on crime control is tangible elsewhere in still other ways. One of those was the quick digitization of policing. In a 1970 book on the creation of automated police-information systems, a criminology professor and a data specialist claimed that "information is the life blood of any law enforcement agency."[63] From their perspective, information tech and policing were a babushka doll of nested systems: conceptual and analytical (systems theory), technical (processors), institutional (organization of the police), and the informational (the actual automated police-information system).[64] The influence of computers fostered an optimism about the union of data-processing and policing interventions.

It's worth acknowledging the similarities here between the drug-use surveys and the crime-data reports, too. All three shared a fixation on collecting and processing as much data as possible, with no clear objective other than amassing and curating unlimited quantities of records. In this sense, it mirrored other military records that emphasized statistical data points to counter guerrilla soldiers, namely the Hamlet Evaluation System.

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[62] Frydl, *The Drug Wars*, 410-411. Schneider, *Smack*, 161.
[63] Paul Whisenand and Tug Tamaru, *Automated Police Information Systems*, (New York: John Wiley and Sons, 1970), 3.
[64] Whisenand and Tamaru, *Automated*, 5.

In a sense, while Tan Turtle and IBN both shared a vision of improving police work via information sharing, they came to the technologies for disparate reasons.

The problems facing crime-data reporting largely resulted not from an interest in the quick transmittal of documents and data — or the appeal of technical efficiency in and of itself, but from non-participation of some policing agencies in drug control.

The kinds of problems that stalked IBN, and the exchange of crime data were imposed by the federal government. Doran Berry described a situation that sums up the kinds of issues facing investigators in and around Fort Bragg. When asked to provide an estimate of the financial cost of thefts that investigators believed were related to drug use, Berry stated that "the FBI report that the sheriff's department was requested to fill out had the term 'No secretarial help available to assist with your request."[65] In a sense, then, despite new federal infusions of cash for crime-data sharing from the LEAA, the manpower just wasn't available to realize the opportunities of near instant information.

Still there was more to this, as Berry alluded. When asked about the degree of success that IBN had in stemming heroin traffic, Berry replied with his own question: "what is success? We have had a tremendous increase."[66] Indeed, while IBN contributed to an increase in arrests, as Berry noted, there was some uncertainty about what the effect of IBN really was. Was it intended as a program to eliminate drug abuse, or to facilitate cooperation and information sharing? To be sure, these are distinct goals. From Berry's perspective, IBN succeeded at collating information,

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[65] Berry, "Statements of Pat Reese…and Doran Berry", 200.
[66] Berry, "Statements of Pat Reese…and Doran Berry", 198.

but failed to address the ostensible goal of the cooperative. And was there any discussion of treatment?

Further, while Berry and Tolson gushed about the success of the program in tying together different policing agencies, they nonetheless discussed the needs of IBN going into the 1970s. "We need more training for all law enforcement officers to orient them to this problem" and that "we need assistance from the FBI Lab for laboratory and chemical work."[67] One must only reflect on Doran Berry's personal experiences with the FBN to come to the conclusion that federal disinterest continued to hamper policing actions. Even with the successful demonstration of IBN, FBN may have even compromised the work of IBN itself. The flakey behavior of FBN agents no doubt negated the carefully laid plans of participating IBN members.

Berry and activist Pat Reese were also aware of how tentative the entire IBN was, as both a demonstration model and an ongoing relationship between civilian police and the military at Fort Bragg. Given the federal government's repeated stonewalling of requests for assistance from the NIMH, FBN, and other agencies, Berry and Reese feared that the military's assistance rested on the whims of the Post commander. They described their fear thusly: "we need constant liaison between the civilian community and Fort Bragg. We are eternally grateful to General Tolson, but we can't help but be concerned when we think of his being moved someday and replaced by another not 'turned on' to the problems."[68] As it turned out, the Department of the Army reassigned Tolson in 1971. Unfortunately, I have yet to find evidence of the program after Tolson's tenure at the Post.

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[67] Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", 238.
[68] Berry, "Supplemental Statement of Mr. Pat Reese and Mr. Doran Berry", 238.

In this sense, Berry and Reese understood the benefits of cooperating with the Army. And, they understood how, given their mercurial encounters with FBN agents, how much of their collaboration depended on a personal relationship with Tolson, rather than any definitive or permanent institutional connections.

I have yet to uncover direct evidence of the performance of Tan Turtle. However given that it was intended as an umbrella program to lace together American and non-American agents, we can look at how other related actors described the relationship between Americans and their non-American peers in narcotics policing after 1971. When Senator Mike Mansfield visited southeast Asia in 1976, he described the situation facing American agents tasked with narcotics control. "Although the growing of opium in Thailand has been illegal since 1959," Mansfield began, "the law is not enforced." Speaking of Hong Kong, Mansfield noted that its government was "critical of the Thai Government's laxity in dealing with drug traffickers." [69] Sprinkled in with a little old corruption, Mansfield was convinced that US investments in drug control in the area would all be for naught. Even in Burma, Mansfield explained, the Burmese resisted the Drug Enforcement Administration.

Still, could there have been more fundamental problems in terms of using data processing to improve drug control? According to one professor of police science in 1972,

> During the last decade, hundreds of automated information systems and studies… have been conceptualized, started, and implemented. For at least this long, many contemporary law enforcement practitioners have been perplexed, if not completely confused about the growing preoccupation with computer-based information systems.[70]

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[69] Mansfield, "Postwar Southeast Asia", 6.
[70] Ronald A. Pincomb, "Book Review of *Automated Police Information Systems*", *The Journal of Criminal Law, Criminology, and Police Science* 63, (1), (Mar. 1972), 146-147. Quote on 147.

The police departments of 1972, per this perspective, look far less successful than the stories that others used to describe the turn toward electronic records. In fact, one might credibly ask whether, despite having the computer equipment available, the police of the time remained hesitant to utilize the new tech?

As both Tan Turtle and IBN demonstrate, the new information technologies of crime-data sharing and cooperative policing didn't resolve the underlying conflicts that hobbled the exchange of ostensibly pertinent information.

Anti-Drug Education in an Age of Youth Revolt

This section deals with the origins and efficacy of anti-drug education in two separate programs, Operation Awareness and Operation JANUS. Unlike the previous sections, this section is especially concerned with the kinds of instructors unique to Vietnam-era anti-drug education, the Awareness Counselor (AC) and the Drug Abuse and Rehabilitation Team (DART). Rather than cutting-edge technology, AC and DART were responses to the backlash against the information revolution of the 1960s and 1970s. I show how commanders and planners continued to lean on a version of anti-drug education built on authenticity, to the detriment of improving working conditions for instructors. I also show how the decentralized structure created for teaching anti-drug education contributed to program problems.

The anti-war movement, the GI movement, and other justice-oriented projects of the era concerned the Army. Such movements directly attacked the military, as well as the technological projects that they pursued alongside research corporations and universities. As Thomas Hughes,

a historian, has described, these movements nonetheless imported necessarily pessimistic views of technology into American life.[71] Since World War II, Hughes shows, a small group of critics had expanded and decentralized into a broader skepticism about the potential benefits of a newly technologized nation. Similarly, American youngsters challenged their elders for the society that technology had created. In the Army, that meant that commanders confronted a credibility gap, and that a generational conflict swirled around the issue of drug control.

Thus, when Pat Reese complained about the origins of drug use, he pointed to the technological society that had produced the drug users. One Army chaplain explained the relationship between information technology and the counterculture, explaining that "a television set is all but unknown" in the homes of young soldiers and addicts, the groups in his mind overlapping, and describing the situation as "their silent protest against television."[72] For these soldiers, then, the information revolution was actually all the more evidence of the aberrant conditions of modern life.

The lecture format must have seemed like drudgery to the young lieutenant, but to Pat Reese the anti-drug lecture, what he referred to as the "love rap," was fundamental to saving young soldiers from heroin addiction. According to Reese, soldiers and all drug users needed the kind of intervention that laid down what he and others referred to as a "true rap." The true rap, undertaken inside so-called "rap houses" or in soldiers' illegal apartment rentals, connected soldiers to the establishment interests responsible for their safety. Soldiers needed to hear that

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[71] Hughes, *American Genesis*, 453-459.

[72] John McCullagh, "Statement of Lt. Col. John P. McCullagh, Division Chaplain, 82d Airborne Division, Fort Bragg, N.C.", "Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics", Nov. 17 and 18, and Dec. 2 and 3, 1970, 91st Congress, Second Session, (Washington: 1971), 150-154. Quote on 152.

someone cared about them, and that there were medical professionals and social workers there to help them.

In an issue of *Air Force*, an author described the landscape at Fort Bragg, with its needle-fix treatment. As the author went on to explain, "the Air Force has no such program. While it is watching what the Army does at Bragg, the Air Force will probably concentrate instead on improved techniques of drug education and prevention."[73] Further, according to the author, "these will include much smaller drug-education classes…[which] create a more intimate and realistic framework for exchange of information and ideas." The author was more specific, though, about the supposed benefits of small classes: "[small classes] should help overcome the obvious shortcomings of the mass-lecture approach in which 'instant experts,' instructors who are themselves inexperienced with drugs and who have received only a few hours of instruction and a syllabus to work from, do a quick run through on the evils of drugs."[74]

But, Fort Bragg hadn't always been so forward-looking here. According to Lt. Gen. John Tolson, "drug education at the post consisted of a semi-annual orientation of all personnel on the penalties that could accrue from apprehension for drug abuse, generally presented in the form of a lecture to large groups." He concluded, not so unsurprisingly, that "it was not very effective."[75] More so, though, Tolson acknowledged that anti-drug education actually was relevant to more

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[73] William Leavitt, "Meeting the Drug Challenge", *Air Force*, Volume and number unknown, Undated, but probably written sometime between late 1970 and early 1971. Appears in "Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics", Nov. 17 and 18, and Dec. 2 and 3, 1970, 91st Congress, Second Session, (Washington: 1971), 1216-1223. Using the hearings pagination, the quote appears on p. 1223.

[74] Leavitt,"Meeting the Drug Challenge", 1223.

[75] Tolson, "Statement", 15.

parties than he or others may have expected, that "there was a lack of awareness by commanders at all levels of the nature and magnitude of the drug culture and its problems. We had a communications gap to bridge." In the classroom, soldiers received an "education…based on facts."[76] A chaplain attached to OA described the paucity of reliable information available to commanders — "one aspect of the current youth drug problem is that adults have had to go to young people for answers to their questions."[77]

In order to establish a course, Tolson initiated Bragg's Drug Abuse Committee, whose work had an "emphasis in the field of education." The committee, too, met weekly to round out their ideas.

Further, the program itself made the relationship between Project SODA, the drug-use survey of paratroopers at Bragg, and the educational program of OA. According to Tolson, "[the survey] has given us some valuable information on which to base elements of our educational efforts in the division." Those survey results led, in part, to the creation of "a comprehensive educational effort" amongst the paratroopers. More importantly, Tolson's education plan also included fifteen "awareness counselors," more or less on-call drug educators whom Tolson had sent to a "two week [sic] pilot training course," which grew specifically out of OA's Education Sub-committee.[78] Tolson described the preferred counselors: "natural magnets to whom other soldiers gravitate in search of information and advice, and to orient these individuals on the facts pertaining to drugs and drug abuse." In a sense, Tolson was looking for personalities, rather than skills or other demonstrable criteria.

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[76] Tolson, 'Statement", 15.
[77] McCullagh, "Statement", 184.
[78] Tolson, "Statement", 15-16.

The education drive at Fort Bragg began in June 1970, when Tolson launched a series of meetings with commanders from around Fort Bragg. OA's attached chaplain described the content of the original meetings, relaying an anecdote. Contrasting his method with the commander of the 82nd Airborne, the chaplain chastised the commander's "'tell it like it is'" attitude. According to the chaplain, "we must first of all establish credibility. In order to do this we must not approach the soldier in a threatening manner but rather appeal to his own concern for his personal welfare."[79] And it was this credibility on which supporters of OA fixated. Anti-drug education was not supposed to be just about relaying discrete information, but trailering it to a relatable instructor. Their solution was the Awareness Counselor.

The decision by OA's Education Subcommittee to bring in what they called "Awareness Counselors" paralleled efforts at Operation JANUS, where efforts culminated in the Drug Abuse and Rehabilitation Team (DART). Operation JANUS was a goulash of narcotics abstinence interventions, a combination of amnesty program, urinalyses, arrests, and anti-drug education. If soldiers failed to reveal their transgressions voluntarily, they would eventually be discovered through the other two methods. According to one pamphlet, "YOU CAN FOOL SOME OF THE JUICERS ALL OF THE TIME AND YOU CAN FOOL ALL OF THE JUICERS SOME OF THE TIME BUT YOU CAN'T FOOL THE P TEST!"[80] (Illustration 7) It's important to emphasize here that Operation JANUS offered drug education while attaching it to the education wing. Here, the emphasis appears to have been on identification, not education.

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[79] McCullagh, "Statement", 15.

[80] Operation JANUS pamphlet, (1971), RG 472 Records of the United States Forces in Southeast Asia Headquarters, Military Assistance Command Vietnam (MACV) Provost Marshal Office of the Provost Marshal, General Records 1970 Box 2, Drug Working and History Papers Folder 1 1971.

The language that its supporters used was central here. Adopting the lingo of the counterculture, its agents framed narcotic sobriety as the new, hip thing. The pamphlet announced the purposes of the initiative. Listed first, the pamphlet stated that "we want to put out honest and straight information about drugs, especially scag" and described their work as "going to lay down a true rap about drugs in Nam [sic]". In order to carry out the mission of educating soldiers with "true raps about drugs in Nam," JANUS coordinated for battalions to appoint two men who "know the drug scene in Nam and they really want to help their friends keep from getting strung out."[81] Their responsibilities included "put[ting] out honest information about the effects of drugs" and "be[ing] ready to rap about drugs at any time of the

[81] Operation JANUS pamphlet.

Illustration 7

Operation JANUS pamphlet. 1971. 18th Military Police Battalion. RG 472 Records of the United States Forces in Southeast Asia Headquarters, Military Assistance Command Vietnam (MACV) Provost Marshal Office of the Provost Marshal, General Records 1970 Box 2, Drug Working and History Papers Folder 1 1971.

OPERATION JANUS

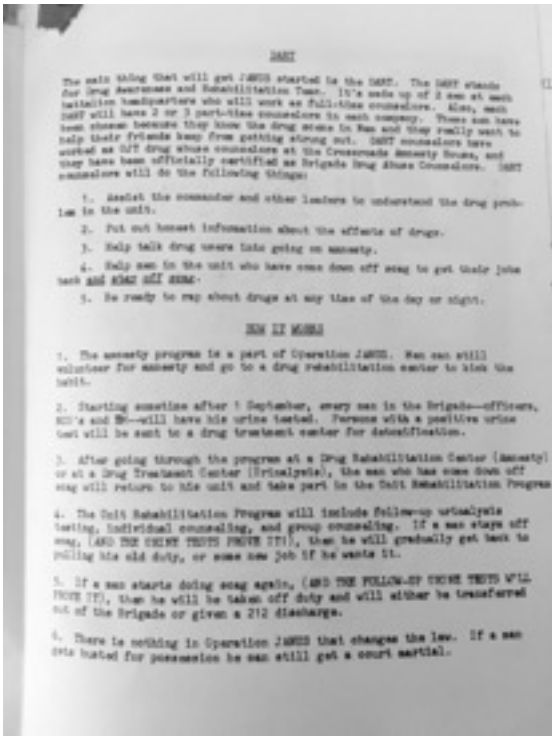
Good things have good beginnings. That's why the Brigade Drug-Abuse Counter-Offensive is named after JANUS. JANUS was the ancient Roman God of Good Beginnings.

The purpose of Operation JANUS is really very simple. We want to do something about the use of drugs by men in the Brigade. More exactly, we want to do 4 things. First, we want to put out honest and straight information about drugs, especially scag. For example, we want everyone to realize that you just can't compare scag to grass. Smoking a little grass now and then is illegal, and you can get busted for it. But, smoking or snorting scag is a whole different thing--a trip that always ends up a bummer. EVERY

The second thing we want to do in Operation JANUS is find out who is doing scag, or other hard drugs like uppers and downers. To do this, every single person in the Brigade is going to have his urine tested. Within a few weeks we will know who the scag users are--and then we'll be able to do the third thing we want to do in Operation JANUS--which is help scag freaks off the drug. We figure that most guys on scag would really like to come down, and stay down. But they don't because cold turkey with the junkeese is pretty bad. Plus, getting down off scag means putting up with a lot of other hassles too. But Operation JANUS is going to help men get down and then get their heads straight so that they can stay down.

Finally, there's a fourth thing we want to do in Operation JANUS. There may be some men in the Brigade that just won't stay off scag, even after we help them down the first time. We'll know if a guy starts using the stuff again, and we just can't afford to have a dude like that in the Brigade. Also, if a man gets off scag once, but then starts using it again, we figure there's not much we can do for him in the Brigade. So we'll have to get him out of the Brigade--and maybe even out of the Army with a 212 discharge.

To sum it up, Operation JANUS is 1) going to lay down a true rap about drugs in Nam, 2) find out who is using the hard stuff, 3) help men who are strung out to get down and stay down, and 4) get rid of the ones that won't come down and stay down.



DAST

The main thing that will get JANUS started is the DAST. The DAST stands for Drug Awareness and Rehabilitation Team. It's made up of 2 men at each battalion headquarters who will work as full-time counselors. Also, each DAST will have 2 or 3 part-time counselors in each company. These men have been chosen because they know the drug scene in Nam and they really want to help their friends keep from getting strung out. DAST counselors have worked as GI drug abuse counselors at the Crossroads Amnesty House, and they have been officially certified as Brigade Drug Abuse Counselors. DAST counselors will do the following things:

1. Assist the commander and other leaders to understand the drug problem in the unit.
2. Put out honest information about the effects of drugs.
3. Help talk drug users into going on amnesty.
4. Help men in the unit who have come down off scag to get their jobs back and STAY off scag.
5. Be ready to rap about drugs at any time of the day or night.

HOW IT WORKS

1. The amnesty program is a part of Operation JANUS. Men can still volunteer for amnesty and go to a drug rehabilitation center to kick the habit.

2. Starting sometime after 1 September, every man in the Brigade--officers, NCO's and EM--will have his urine tested. Persons with a positive urine test will be sent to a drug treatment center for detoxification.

3. After going through the program at a Drug Rehabilitation Center (Amnesty) or at a Drug Treatment Center (Detoxification), the man who has come down off scag will return to his unit and take part in the Unit Rehabilitation Program.

4. The Unit Rehabilitation Program will include follow-up urinalysis testing, individual counseling, and group counseling. If a man stays off scag, (AND THE URINE TESTS PROVE IT!), then he will gradually get back to pulling his old duty, or some new job if he wants it.

5. If a man starts doing scag again, (AND THE FOLLOW-UP URINE TESTS WILL PROVE IT), then he will be taken off duty and will either be transferred out of the Brigade or given a 212 discharge.

6. There is nothing in Operation JANUS that changes the law. If a man gets busted for possession he can still get a court martial.

day or night." DART was to anti-drug education what the pusher was to selling drugs, the hip middleman.

In addition to DART, the 18th MP Brigade invited soldiers to the Crossroads Amnesty House, an on-Post rap house. In the rap house soldiers could talk with DART members, discussing their questions and issues with "officially certified…Brigade Drug Abuse Counselors."[82] Planning documents for JANUS suggest the other kinds of qualities that commanders envisioned as an effective educational team: "mature volunteers who are highly motivated to help their fellow soldiers in combatting drug abuse", adding that "it is desirable that they have some education in the behavioral sciences and/or experience in counseling." Even more, the brigade must have seen additional utility in the rap houses, or at least separate needs, requesting that "each DART should include black members and representatives of other minority groups as appropriate."[83]

The responsibilities of DART members exceeded their teaching and mentorship activities. In addition to "[devising and conducting] innovative programs for education and drug abuse prevention" and "be[ing] available for counseling and 'rap' sessions on a twenty-four hour basis," DART members were required to "maintain informal progress records on personnel participating in the Unit Rehab Program." And, it was data the program was supposed to collect, from daily drug abuse reports on soldiers in rehabilitation, weekly administrative separations

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[82] Operation JANUS pamphlet.

[83] "Annex B: Drug Awareness and Rehabilitation Teams", in Circular 600-32, "Operation JANUS", Aug. 15, 1971. RG 472 Records of the United States Forces in Southeast Asia Headquarters, Military Assistance Command Vietnam (MACV) Provost Marshal Office of the Provost Marshal, General Records 1970 Box 2, Drug Working and History Papers Folder 1 1971.

involving drugs, and monthly amnesty reports and "monthly eliminations as a morale indicator report." All members of the initiative were turned toward observing and reporting.

In both OA and JANUS, soldiers encountered instructors who had either "been there" or had received and ostensibly passed a certification course. The premium of employing DART counselors to educate soldiers was that it reinforced how commanders and activists had framed the relevance of new drug knowledge, that it had to be embodied within the counselor, a "natural magnet," as Tolson had put it. In doing so, both programs blurred the objectives of drug treatment. Was it to re-establish a relationship between older and younger soldiers, to prevent heroin addiction, or some combination in between?

It's worth acknowledging here, the differences between the programs. JANUS came from military police, the program's emphasis on data collection seeming to fulfill the interests of commanders to know addiction through its statistics. Further, the 18th MPB devised the program to work in tandem with mandatory urinalysis, given that it launched in a time much different than just a year earlier at Fort Bragg. Prior to the implementation of a massive drug-testing program for all soldiers, and not just prisoners and patients, OA anti-drug education operated as an extension of Pat Reese, and of OA's attending psychiatrist, Major Richard Crews.

In a formal review of the various MACV commands' drug-abuse suppression activities, investigators claimed that "educational programs…are sufficiently extensive to make military personnel aware of the availability of drugs in Vietnam and of the dangers of abusing these

drugs."[84] Similarly, they commented that the most effective teaching method available was the education teams. As it turned out, the education teams may have also been a Trojan horse.

In large part, many of the problems that confronted both OA and Operation JANUS's educational efforts were dependent upon their staffing arrangements. Although Lt. Gen. John Tolson had described the Awareness Counselors at Bragg as possessing a natural magnetism, there was more there. One military police officer once described the responsibilities of Awareness Counselors, explaining that "this individual must be willing to volunteer large segments of off-duty time and truly desire to participate in a program designed to help young people on an individual and group basis."[85] A similar situation played out in Vietnam in JANUS. They specifically chose younger, enlisted soldiers to volunteer to be counselors. Part of their responsibilities as volunteers, like the Awareness Counselors, though, involved being available to other soldiers at all hours.

There were also problems embedded into the role of being an Awareness or DART counselor. As part of their role as a counselor, the 18th Military Police Brigade expected that every counselor would keep records of soldiers who visited them. As the frontmost face of JANUS, the same instructors responsible for engaging their peers and bridging relational gaps were also charged with tracking their students and. Given the anxieties and fears that others had noted with the drug-use surveys, it's not a leap to expect that the records may have scared off other soldiers.

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[84] General Accounting Office, "Review of Drug Abuse Program in Vietnam", date unknown, RG 472 Records of the United States Forces in Southeast Asia Headquarters, Military Assistance Command Vietnam (MACV) Provost Marshal Office of the Provost Marshal, General Records 1970 Box 2, Drug Working and History Papers Folder 1 1971, 10.

[85] Carl Chase, Jr., "Operation Awareness", *Military Police Journal* (Oct. 1970), 16-17. Quote on 17.

Still, while staffing needs rested on volunteer work, commanders continued to emphasize the need to choose relatable instructors. One commander emphasized the relationship between "juicers-heads," alcoholism, and accurate drug information. Rather than duck the question of alcoholism, the commander explained that "if you meet that head on and say we know that alcohol is a dangerous drug, we have a good body of medical knowledge to prove that."[86] In this regard, the common sense was that instructors needed to look credible before they even began to teach. And again, considering the data that instructors were to assemble of participants, it's hard to imagine counselors fully embodying a relatable peer for soldiers. In fact, the DOD eventually revised the data policy, perhaps inadvertently, when a new directive eliminated the keeping of class rolls for mandatory meetings.

Finally, there was the issue of curriculum and instruction. Despite the directive to establish anti-drug education at all battalions, the DOD left the organization and the content to individual commanders. Analysts described the effect of the decentralized structure, concluding that "the quality and effectiveness of the presentations varied significantly from location to location."[87] Attempting to tie an example to their claim, they described one particularly poor class in which "there was no spontaneity in the presentation — the presentation of about 55-minutes was read verbatum [sic] from the lession [sic] plan with practically no attempt to involve or establish

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[86] Kenneth Nelson, "Statements of Brig. Gen. Richard R. Taylor, M.D., Commanding General, U.S. Army Medical Research and Development Command; Maj. Kenneth Eric Nelson, M.D., Letterman General Hospital, San Francisco, Calif; and Capt. Morris Duncan Stanton, Chief Psychlogy Section, Mental Hygiene Consultation Service, Kimbrough ArmyHospital, Fort George Meade, MD", "Hearings on Drug and Alcohol Abuse in the Military before the Special Subcommittee on Alcoholism and Narcotics", Nov. 17 and 18, and Dec. 2 and 3, 1970, 91st Congress, Second Session, (Washington: 1971), 739-760. Quote on 744.

[87] General Accounting Office, "Review of Drug Abuse Program in Vietnam", 14.

rapport with the audience." The solution? Poor instructors would be replaced by "personnel specializing in drug presentations" and "former drug users assigned to MACV by the National Center for Prevention of Drug Abuse."[88] Again, this also lines up with Pat Reese's memory of going to a mandatory anti-drug class at Fort Bragg.

More to the point, what should soldiers learn? Commanders at Fort Bragg were convinced that only accurate information about drugs would work, not the fear-mongering of the past. For the general in command of the Army's medical research and development division, the relationship between medical facts and effective anti-drug education was determinative: "as medical research progresses and defines what the facts really are, policy and educational programs can be changed to reflect those facts."[89] This commander, however, may not have realized the then-harmful combination inherent to medical authority and social authority alike. Put differently, no amount of scientific facts

But, just how much did supposedly accurate information about narcotics presented by a relatable peer influence the operation of the program? For the analysts looking at MACV's program, "responses to our questionnaires indicated that military personnel generally did not have a great deal of knowledge about drugs." And, while they realized that knowing drug slang or the composition of opium may not have ever prevented a soldier from getting high, the situation, they concluded, "is indicative of the limited effectiveness of the educational program."[90]

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[88] General Accounting Office, "Review of Drug Abuse Program in Vietnam", 14-15.
[89] Richard Taylor, "Statements of Brig. Gen. Richard R. Taylor, et al.", 744.
[90] General Accounting Office, "Review of Drug Abuse Program in Vietnam", 14.

What's more, commanders weren't sure of whether or not to hold the education programs responsible for possible successes. For one, there were no other complementary surveys. But, two, and as one officer noted, trying to examine the programs was "like evaluating various teaching techniques — no satisfactory indicators have been developed to determine how effective a particular technique is."[91] Another added that "he did not believe an objective method of weighting each of the program factors [surveys, urinalysis, education, raids, etc.] could be made."[92]

The cases of the two anti-drug education endeavors confronted problems around authority, social relations between commanders and enlisted soldiers, between counselors and students, and between counselors and their superiors. While the Army belatedly centralized its education efforts, they still remained unclear as to how even to evaluate the contributions of the education programs.

Conclusion: The Information-less Society?

Did the military's many responses to heroin abuse actually produce an information-less society? The disparate reasons for initiating new information technologies in the service of stemming heroin abuse contributed to a chaotic archipelago of isolated research islands — so decentralized that there were few shared objectives. There was no one, coordinated voice demanding these information technologies, and these groups pursued these technologies for multiple, different, and sometimes competing reasons. Further, as the case of the early drug-use surveyors

[91] General Accounting Office, "Review of Drug Abuse Program in Vietnam", 18.
[92] General Accounting Office, "Review of Drug Abuse Program in Vietnam", 18.

illustrates, their isolation from one another led to a situation in which their research was a case of "if a tree falls in the forest."

Further, because the kinds of information and information technologies that the military prized had opposing and unclear objectives, the individual cases suggest that only crime-data sharing succeeded, though it very much did not resolve the tensions underlying its invention. The case of information technologies, based on this chapter, demonstrates that various actors looked to information technologies to resolve less tractable tensions or to establish new relationships that existed between surveyors and the surveyed, between contractor and purchaser, between individual researchers and opaque bureaucracies, between the federal government and local police, between commanders and commanded, between teachers and students.

This is all to say that the creation and implementation of new information technologies for drug control, as well as the many standing conflicts and tensions, shaped the limits of what was possible. (nice) The information-less society that these technologies created is a chance to reflect not on the progress of information technology, but to marvel at its failures and mishaps, as well as to acknowledge the social relations that underlay the creation, implementation, and flops of those same technologies.

At its broadest possible meaning, the stories of drug-use surveys, crime-data reports, and anti-drug education attest to how hobbled, asymmetrical, and unfinished so much of the Army's response, and often the federal government's response, to heroin was. It also shows that any semblance of centralization was always more an illusion than a reality, as even orders from the US command and MACV — responsible for the creation of new i.t. — were vague instructions, at best.

The Army's many responses to heroin catapulted new technology to the front lines of drug control. But, as often turned out, they were attacking their own castle the whole time.

Conclusion

Confronting Drugs in the Age of Disruption

When Lt. Gen. John Tolson testified before the Senate Armed Forces subcommittee in Fall 1970, it wouldn't have been implausible for him to have asserted something to the effect of "generals make their own history, but they do not make it as they please." He had initiated a bold new program in the hopes of rescuing soldiers who found themselves addicted to heroin with no way out, but he also inherited the political conflicts that pitted soldiers and commanders throughout the Army against one another, as well as a difficult-to-shake commitment to eliminate drug abuse through police action. And, in a moment of upheaval in addiction treatment, he and his clinical staff addressed heroin addiction with the experimental tools at hand.

Tolson's and the rest of the Fort Bragg contingent's testimony before the Senate subcommittee on the Armed Forces in Fall 1970 confirmed the kaleidoscopic forces behind the DOD's new emphasis on narcotics control, as much as it illustrated the bizarre and more familiar personages, clinical hypotheses, and organizational priorities behind Operation Awareness. There was Tolson, proud and seemingly transparent about his presumed responsibilities toward American warriors. There was Pat Reese, local gadfly, invoking the special relationship between Fort Bragg authorities and Fayetteville's civilians, and simultaneously optimistic and fatalistic about the city's drug scene. And, there was Private J.D., explaining to the senators present what it was like to be addicted to heroin without access to care, caught between an evolving understanding of addiction, the shortfalls of an experimental treatment, and his perhaps sincere desire to put heroin behind him.

Accurately or not, Operation Awareness has stood out to me as the prism through which I have come to understand the actions of scientists, commanders, physicians, and enlisted soldiers. It's the prism through which I've come to understand the patches that various units put together to commemorate the Army's first drug war.

No one person from these groups can explain why or how the DOD got behind a quest to end drug abuse, but, collectively, they help to sketch out all of the messy relationships between actors that characterized that same endeavor. All of these actors brought their own interests and motivations to the table, but none fully realized. The teetering edifice that came to be known as the Drug and Alcohol Abuse Control Program (DAACP) and June 1971 are testaments to the failure of technological fixes to social problems, to new technology that simultaneously cramped and exploded the aspirations of drug warriors, and to the disparate outcomes of how the Army attempted to govern science and govern through science at a moment of institutional and ideological rupture. Neither were monuments to unbridled empathy, antipathy, or apathy. Operation Awareness manifested and generated all of the productive, unproductive, and counterproductive strategies and ideals that the Army went on to adopt in final years of the Vietnam War and afterward. Civilian society wasn't far behind.

The DOD successfully transformed the practices of narcotics enforcement for the electronic age, setting a technological foundation for more intensive and regular forms of surveillance, detection, and treatment. This drives home others' points, especially Kuzmarov and Frydl's contention that MACV and the DOD's contributions to drug control tended toward the militarization of those practices — as with the transfer of counterinsurgency research into

narcotics detector dogs, and even mandatory drug testing.[1] However, that evolution was far from straight forward, and even its ethical consequences feel more ambiguous. There's a colloquial phrase from my youth that I think describes the Army's response: running like a chicken with its head cut off.

Between 1945 and 1980, the DOD's many institutions, agencies, and service branches reimagined the scope of narcotics enforcement, and re-engineered the connections available to it and the civilians attached to those projects. For some agencies, that connection was a long standing experiment in both experimental science and how the military might foster industry by leading research, as at AFIP. For others, there were more recent spins on related, but distinct relationships. When John Romba tested the LWL's first heroin-detector dogs, it was the unavoidable culmination of how the military, academics, and industry cross-pollinated experimental behavioral research and defense research. And, when HumRRO took over the annual Drug-Use Survey in 1971, it suggested how seriously the goal of data-collection as a task unto itself fixed hopes for drug control on durable, if not always successful technological solutions.

There was, too, the novel introduction of promising heroin therapies — methadone and in-patient rehab for soldiers — on the basis of military responsibility. In effect, as John Tolson may have described it, the Army *was* social justice. The application of promising, medically, and scientifically astute care suggested just another arena in which the Army could demilitarize care. It was also a hopeful moment — Operation Awareness promised an expansive vision of military

[1] Kuzmarov, *Myth*. Frydl, *The Drug Wars*.

responsibility coinciding with the promises of scientific progress. In this sense, it mirrored the accomplishments of the urine tests, drug-sniffing dogs, and drug surveys.

Simultaneously, a program like Operation Awareness was the exact reason why the Army's response to heroin addiction failed — it suggested the brittleness of the exchange between military needs and civilian interests. OA projected a vision of the Army as a social benefactor, but its constant funding and staffing shortfalls were just a preview of the DOD's decision to appoint the VA to lead drug treatment. As Eric Schneider's work has shown, that care was similarly found lacking — providing in 1971 only 100 beds across its hospitals.[2] Its productive relationship with Cumberland County investigators also suggested that policing remained a top priority.

Similarly, even when military equipment ended up in civilian hands, or the military lobbied to centralize and expand a drug survey, or when the military successfully jumpstarted a laboratory industry, under whose rubric should we adjudge success? On the basis of drug users caught, and lives disrupted? By the pockets lined with federal money? By the textbooks written using research that came from the US's midcentury push to improve the world through scientific innovation and greater destructive power? Who benefitted in those situations, and to whose detriment? These are ultimately the same ethical questions with which others have dealt when examining postwar social science and its scientists.[3] The answers to these questions are still forthcoming.

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[2] Schneider, *Smack*, 164.
[3] Rohde, *Armed with Expertise*. Solovey, *Shaky Foundations*.

Finally, the push to technologize drug control showed the limitations of optimism. Optimism in technological promise, in the fundamental purposes of government vis a vis health, science, medicine, and industry. It was also about the many failures of technology to work as planned — about the hard boundaries on what could or couldn't be known by empirical observation.

The science of drug control was both an attempt to engineer away a social problem through laboratory and clinical research, and a mode of building up science via its ties to the Department of Defense at a time when the DOD was the biggest game in town; its resources a near bottomless well for researchers attacking the pressing social and epistemological issues of the day. The end of that era profoundly influenced how and why the Army pursued heroin abuse before and after 1971.

In our own moment, the gravitational pulls of these different projects continue — capturing old issues of drug abuse, and netting new ones, including anti-terrorism. As always, these projects help to direct the way that the United States and the Army in particular invests in and doesn't invest in drug abuse; more often than not geared toward expanded surveillance and security capabilities, rather than improved health and wellness for individual soldiers.

Continued innovation in the production of detector dogs, has blurred the roles of narcotics detection with more novel imperatives — medical research and anti-terrorism — and persists in sorting and aligning the priorities of research on animal behavior with federal and DOD funding streams.

Take, for instance, the Working Dog Center (WDC) at the University of Pennsylvania in Philadelphia, PA. The program, conceived in 2012, ostensibly institutionalizes veterinary,

psychological, and applied research on working dogs. In a way, the WDC extends a relationship that preceded itself. In the late 1960s and 1970s, the DOD relied on WDC's cross-University City neighbor, the Monell Chemical Senses Center, to produce research on canine olfaction. While the Monell Chemical Senses Center remains, the WDC is an institution solely dedicated to working dogs. According to their own published resources, the link between the center and national security remains paramount: "with the United States national security under constant threat from attacks, detection dogs are still the best tool that we have to detect and mitigate potential threats."[4] And, the Center describes its purpose as being "committed to partnering with your [police] department to help you build an elite K9 unit."[5]

Some of WDC's more recent work, including research on cancer-sniffing and insulin-detection dogs, co-exists with the program's continued training programs for explosives-and narcotics-detection. Narcotics-detector dogs, then, remain as part and parcel of drug enforcement, a bridge between regressive uses of canines and innovative medical discoveries, and an artery between the federal government and research sites. Indeed, that canine conduit has expanded upon the imperatives of breeding and genetics, as they claim that "our puppies participate in a comprehensive program that uses scientific principles to maximize each dog's genetic potential."[6] Good genes supposedly mean more safety.

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[4] "Penn Vet Working Dog Center", www.vet.upenn.edu/research/centers-initiatives/penn-vet-working-dog-center, accessed on May 10, 2019.
[5] "Specialized Programs: K9 Law Enforcement", www.vet.upenn.edu, accessed May 10, 2019.
[6] "Working Dog Research", www.vet.upenn.edu, accessed May 10, 2019.

And, from where does the WDC derive its funding? While the Center takes grants from private corporations and donors, including Nestlé and the American Kennel Club, DOD grants continue to supplement their detector-dog research.[7]

What's more, WDC constitutes a slow-moving factory, churning out detector dogs that local police forces in Philadelphia and its suburbs then purchase. Without overemphasizing the point, while WDC produces dual-trained K9s — they can detect explosives in addition to narcotics — their purpose is to continue the process of transforming the modes of drug control on the basis of scientific research. The WDC is a node in a complicated network projecting a technological form of police work from one American university to the individual police departments that exist around the Philadelphia metropolitan region. And, regardless of any one researcher's opinions toward drugs and drug use, the imperatives of finding and securing funding, justifying research, and maintaining a working-dog center hinges on the perpetual guarantees of drug control.

Similarly, Auburn University hosts a research program that has helped to develop a patented detection training method known as Vapor Wake. The institute, Canine Performance Sciences, claims that it "play[s] a vital role in increasing the capabilities of the canine technology for a variety of uses, such as conservation and medical detection." Unlike its peers at WDC, its emphasis is not so much narcotics detection, but its relationships with diabetes and cancer detection research and national security.[8] Research at CPS led to the invention of Vapor Wake, patented in 2010, which has spawned a training business. Vapor Wake the corporation, also

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[7] See for example, Cynthia Otto, et. al, "Evaluation of Three Hydration Strategies in Detection Dogs Working in a Hot Environment", *Frontiers in Veterinary Science* 4, No. 174, (Oct. 2017), 1-10.

[8] "Canine Performance Sciences", www.vetmed.auburn.edu/research/cps, accessed May 10, 2019.

identified as VWK9, contends that it is the only training method whose trainees can track explosives concealed on the body, ie, suicide bombers.[9] Its clients include the Pentagon and the Department of Defense, as well as musician Taylor Swift and the professional football team, the Dallas Cowboys.

In a few words, then, these dogs and the research on them occupy an ambiguous ethical ground. WDC and CPS's missions involve both the application of dogs to therapeutic and criminological contexts. Were it not for the imperatives of narcotics detection, might related cancer- and diabetes- sniffing dogs exist? To what extent has research on narcotics-detection, including related work on olfaction and breeding, influenced the development of clinical detector dogs? These are questions for another study, however, for now, the centralization of research on both in the same institutions suggest that the two overlap. Clinical detector dogs just happen to be the less bitter fruit of a poisoned tree.

When it comes to narcotics detection, there is some sense of success here, that the market in specially trained dogs continues with and without DOD support.

At the end of the day, because explosives- and narcotics-detection programs grew up from the same milieu, there's a disconcerting conclusion: that the science of drug detection drives the evolution of American wars, as much as they respond to disparate and novel contexts. This may be the most literal case of the tail wagging the dog.

As of 2011, GIs' still could not access methadone for the purposes of maintenance. While they can receive methadone and related medicines for short-term detox, their insurance, Tri-Care,

[9] "Vapor Wake", www.homeofvaporwake.com/vapor-wake/, accessed May 10, 2019.

does not reimburse them for long-term prescriptions of the substance.[10] Vis a vis this policy, detox remain a legitimate therapy, but rehab is considered unsustainable or not preferable. The military's interest in dictating the supposedly proper administration and purposes of methadone remains paramount.

Further, in the Army, at least, the service branch has only solidified the stigma attached to drug-using soldiers through its policies and program positions. The relationship between clinical care, social engineering, and the evolution of drug control has been exemplary in this regard. As one anthropologist, Ken MacLeish, has begun to show in his exploration of so-called Veteran's Courts, JAG lawyers have sharpened diagnoses of post-traumatic stress disorder into rationales for continued supervision; convenient and powerful legal mechanisms to excuse and explain away all manner of offenses from drug possession to domestic abuse.[11]

Veterans' Courts traverse an unstable terrain between addressing the clinical needs of veterans, and harnessing them indefinitely to military oversight, even after they have discharged.

In a different vein, the Army's failures to construct a viable treatment infrastructure, or, at the least, guarantee civilian treatment has resulted in ongoing neglect toward drug-abuse treatment in the present. In 2012, the DOD requested from the Institute of Medicine, the medical branch of the National Academy of Sciences, a study of their existing treatment options for substance-dependent soldiers and their families. According to that report, researchers claimed that "the

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[10] Keith Humphreys, "The Military's Bad Medicine", *The American Prospect*, (Apr. 14, 2011), online, www.prospect.org.

[11] Ken MacLeish, "Present Chaos: Rehabilitating the Future in a Veteran's Treatment Court", conference paper, American Anthropological Association, (Nov. 2015). Paper in author's possession.

current levels of substance use and misuse among military personnel and their dependents constitute a public health crisis."[12]

When reporters followed up on the findings of the IOM report, they noticed even more serious flaws regarding the care network available to soldiers, veterans, and their dependents. According to a 2015 investigation by *USA Today*, the Army's weak support for addiction treatment eventually cascaded into a tragic, but predictable end: an unlicensed counselor employed at Fort Sill, Oklahoma, rated a patient as "good." The patient hanged himself two hours later.[13]

However, the most tragic consequences of the Army's neglect toward treatment options reveals itself in significantly more mundane and insidious ways. According to the same *USA Today* investigation, in 2014 providers denied treatment to 7,000 soldiers, half of whom senior clinicians evaluated as needing treatment. Or, consider that, even by the Army's own assessment, fifty-four of its treatment facilities "fall below professional standards for treating drug and alcohol abuse, and only a handful are in full compliance." In a particularly reminiscent moment, *USA Today* revealed that "a 2013 work memorandum at Fort Bragg, North Carolina reminded counselors to clean their mops and buckets after using them."[14] Unlike 1970, the patients aren't doing the cleaning. However, it's hard not to wonder how it is that counselors are responsible for both putting soldiers back together, and scrubbing the floors.

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[12] Committee on Prevention, Diagnosis, Treatment, and Management of Substance Use Disorders in the U.S. Armed Forces — Institute of Medicine (IOM), *Substance Use Disorders in the U.S. Armed Forces*, (Washington: The National Academies Press, 2012), S-5.
[13] Gregg Zoroya. "Investigation — Army substance abuse program in disarray". *USA Today*. Mar. 11, 2015, online, www.usatoday.com, accessed May 10, 2019.
[14] Zoroya, "Investigation".

For soldiers stuck in the professional limbo that is being an addict and a soldier, the military's indifference toward them may likely end in the kind of "public health crisis" that IOM identified back in 2012.

And, in our own moment as various actors clamor to address the ravages of the opioid crisis, we have what may be another new moment in the technohistory of drug control. Take, for example, a recent Food and Drug Administration (FDA) project. As of May 2018, the FDA announced what it calls the Opioid Innovation Challenge (OIC). The FDA defines the challenge as such:

> The FDA remains committed to addressing this national crisis on all fronts, with a focus on *encouraging medical product innovation* to prevent new cases of opioid abuse and addiction and to treat those addicted. As part of important efforts to address the epidemic of opioid misuse and abuse, the FDA launched an innovation challenge on May 31, 2018, *to spur the development of medical devices, including diagnostic tests and digital health technologies*.[15]

The response to their call for applications is impressive, as they claim to have received over 250 submissions. The agency describes the eight finalists' submissions thusly: "the medical devices from the selected participants include those intended to predict the risk of opioid use disorder (OUD), detect opioid overdose, dispense medication and provide pain treatment alternatives to opioids."[16] Now, as in the past, these technological solutions accomplish a handful of objectives: providing, at best, negligibly useful and potentially unreliable devices, the privatization of health resources in the hands of engineers and corporations, and an emphasis on technology as the most important of solutions to heroin and opioid use.

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[15] Food and Drug Administration, "FDA Innovation Challenge: Devices to Prevent and Treat Opioid Use Disorder". www.fda.gov, (Last updated Nov. 30, 2018), accessed May 10, 2019. Emphasis mine.
[16] "FDA Innovation Challenge".

A fantastical trust in magic bullets, a belief in the inevitable march of scientific innovation, and the structural overlay of state-funded science for addiction further shapes how Americans writ large imagine governing addiction through science, and govern science through addiction. Projects attempting to create an addiction vaccine have been and are in the works. President Donald Trump's former head of the Department of Health and Human Services, Tom Price, referred to such enterprises as an exciting prospect, while individual universities, including Scripps Research Institute California, have running projects, for example, to make an anti-heroin vaccine that would prevent heroin from reaching the brain's pleasure centers.[17] The vaccines, much like the anti-drug technology of the past, and, even the FDA's own Opioid Innovation Challenge (OIC) broadcast how technological solutions to drug abuse bring together so many resources, and become priorities in and of themselves. We can only hope that one day those benefits will accrue to the people who need them.

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[17] National Institute on Drug Abuse, "Anti-Drug Vaccine Animation", www.drugabuse.gov, accessed on May 10, 2019. See also, Michael Torrice, "Vaccines against addictive drugs push forward despite past failures", *Chemical and Engineering News* 96, no. 8, (Feb. 19, 2018), 18-21. Berma Kinsey, "Vaccines against drugs of abuse: where are we now?", *Therapeutic Advances in Vaccines* 2, no. 4, (Jul. 2014), 106-117.

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