Summer 2009

Transitions in the Mental Health Field's System of Professions from WWII until the Present: The Case of Dubville

Daniel Noam Warner

TRANSITIONS IN THE MENTAL HEALTH FIELD'S SYSTEM OF PROFESSIONS FROM WWII UNTIL THE PRESENT: THE CASE OF "DUBVILLE"

A Dissertation

Submitted to the McAnulty

Graduate School of Liberal Arts

Duquesne University

In partial fulfillment of the requirements for

the degree of Doctor of Philosophy

By

Daniel Noam Warner

August 2009

TRANSITIONS IN THE MENTAL HEALTH FIELD'S SYSTEM OF PROFESSIONS

FROM WWII UNTIL THE PRESENT:

THE CASE OF "DUBVILLE"

By

Daniel Noam Warner

Approved: April 16, 2009

Daniel Burston
Associate Professor of Psychology
Dissertation Director
Psychology Department Chair

Roger Brooke
Professor of Psychology
Committee Member

Constance Fischer
Professor of Psychology
Committee Member

Ralph L. Pearson
Provost /Academic Vice President and
The McAnulty College and Graduate
School of Liberal Arts
Professor of History

ABSTRACT

TRANSITIONS IN THE MENTAL HEALTHY FIELD'S SYSTEM OF PROFESSIONS

FROM WWII UNTIL THE PRESENT:

THE CASE OF "DUBVILLE"

By

Daniel Noam Warner

August 2009

Dissertation supervised by Professor Daniel Burston

A case study of one mental health field in a medium-sized, steel-belt city over a 50 year period is presented. Through interviews and archive data, the narrative elucidates the transitions that occurred in regards to which professions are dominant, and which services count as mental health services, in different eras and under different science and governmental regimes. The dissertation focuses on four professional categories in particular: psychoanalytic psychiatrists, neurological psychiatrists, research psychologists, and clinical psychologists. It also focuses, primarily, on three institutions: a large psychiatric research hospital, a university psychology department, and a psychoanalytic institute. Through tracing the dynamics of the professional transitions undergone by these professions and institutions, four major conclusions are reached: (1) The transitions in mental health were not just scientific advancements, but changes in the "object" over which mental health professionals are considered to have expertise. (2)

Sidestepping the “total person” has resulted in a radical transition in the very hierarchy of the mental health system of professions. (3) The problem of the “total person” continues to complicate efforts in mental health, despite having ostensibly been “sidestepped.” (4) “Totality” has become an area of technocratic expertise for those working in private practice psychotherapy. However, totality is no longer exclusively conceptualized through a psychoanalytic lens, while the ground to make a claim on “totality” is still grounded in “clinical experience.”

ACKNOWLEDGEMENTS

First, I must thank my many interviewees, who selflessly donated their time for this project. I thank them for taking time out of their busy schedules to help my edification, and hopefully to help the mental health field grow in self-understanding and maturity.

I would also like to thank my dissertation committee, Daniel Burston, Roger Brooke, and Constance Fischer, who shepherded me not only through the complex process of writing a dissertation, but through the entirety of my graduate training in psychology and the human sciences. In particular I would like to note my Committee Chair, Daniel Burston, for showing me the power of historical analysis in addressing theoretical questions.

There were also many experts who assisted me out of pure selfless dedication to scholarship. Two sociologists in particular helped me generate an analytic frame for understanding the mental health field that I only knew from “the inside.” First is Matt Schneirov Ph.D., who introduced me to the field of medical sociology, and second is John Marx who helped educated me on many dynamics in the contemporary mental health professions. Sam Knapp was indispensable in his encyclopedic knowledge on the history of professional psychology, Martin Packer pushed me to develop an appropriate method for my project, and Ken Thompson was indispensable in connecting me to sources I could never have found on my own (as well as in allowing me to check out library books on his account!)

The polish and clarity of the final product was made possible by the tireless editing of Mathew Lorenz and Joshua Gregson, who spent innumerable hours pushing me to make sense.

The Duquesne University Psychology Department office managers Norma Coleman and Marilyn Henline assisted me through the many logistical hurdles of proposing and writing the dissertation.

Last, I need to thank my wife, Erika Fricke, who not only provided unmatched copyediting help, but has been my foil and debating partner as I worked through the ideas expressed in this dissertation, and which power my commitment to mental health work.

TABLE OF CONTENTS

1 - INTRODUCTION

The following monograph is a detailed case study of the professional transitions that occurred across the North American mental health field since WWII. This story has been documented in various ways, but is well summarized in Daniel Burston's (2007) observation that in the late 1960s a patient coming to a mental health facility would have received medication as a treatment of last resort; by 1980 medications had become the first line of defense. This study documents this transition in one city's mental health community in order to gain a better understanding of the dynamics that compelled it, and to analyze what this transition might ultimately mean in the day-to-day delivery of mental health services.

Only through attention to the "facts on the ground," can we gain a sense of the mechanics of change that are overlooked in more macro scale histories—hence this "micro-history." For example, Edward Shorter's admirable *A History of Psychiatry* (Shorter, 1997) provides the contemporary, prevailing narrative of the trajectory of psychiatry and mental health healing. In Shorter's story, psychoanalysis and its emphasis on talk over somatic interventions, is represented as a "hiatus" (Shorter, 1997) from psychiatry's true historical trajectory. The biological revolution of the 70s and 80s is presented as psychiatry's return to its rightful path. The twin forces of "reason" and "scientific evidence" are presumed to compel this change, in direct contrast to what is hinted at as the "dogma" of psychoanalysis. Shorter tells his story by documenting some of the more important medical experiments and discoveries in psychopharmacology, and then references their impact to explain the changes that occurred in the actual delivery of mental health services.

Challenges to Shorter's rather Whiggish narrative already exist. One important figure is David Healy, whose three-volume history of the rise of psychopharmacology (*The Anti Depressant Era* (1997), *The Rise of Psychopharmacology* (2002), and *Let Them Eat Prozac* (2004)) gives a close study of the actual lab science behind the rise of psychopharmacology. Through this documentation, Healy implicitly challenges the notion that the demise of psychoanalysis allowed psychiatry to get "back on track." He gives copious examples of how the scientific "advances" Shorter references present as many puzzles as solutions, and shows that psychoanalysis' failure was as much linked to its anti-democratic nature, as to the fact that it could not rival medications in producing efficient results. Healy also documents the corrosive impact of corporate science over the last 50 years, and the many dangerous side effects of medications that have been underplayed in order to celebrate biological psychiatry before the field deserves unqualified praise. In sum, by closely assessing the research and researchers, Healy reveals that Shorter's story is too simple to explain the rise of psychopharmacology's predominance in mental health care, and he instead points us to the power of market forces in science and medicine to explain much of the change.

Tanya Luhrmann's work, *Of Two Minds* (2000), also adds needed complexity and nuance to Shorter's narrative. Luhrmann is an anthropologist who documents contemporary psychiatric training; she shows that despite the popular story of biological psychiatry's rise over psychodynamic psychiatry, in reality the two forces exist side by side in the day-to-day delivery of services. Most psychiatrists realize the value of both approaches to mental healing, and spend the majority of their time navigating between the two in order to provide care as best as they can. Luhrmann concludes by pointing out that the real change in mental health has not been a shift from psychoanalysis to biological psychiatry, but from a model wherein clinical excellence is embodied in a trained professional to a model in which clinical excellence is presumed to lie in the "treatment system." As a faculty member she quotes says:

> I view this now less as a rift between the biologically oriented people and the dynamically oriented ones. I see it now as more between those whose central idea of their identity is clinical work and whose central idea of their identity is being part of a treatment system. There's a growing sense in psychiatry as a whole that it's not that you're a doctor and you see a patient and the patient's best interest is what you primarily care about and what you're involved with. Now it's clear that the relationship is contaminated by the needs of the institution and particularly the needs of insurers. It was always true that the doctor's needs were involved in the relationship, but it's much more complicated now. Before you might have wanted to see a patient five times a week because you'd make more money that way. But you could wrestle with that in your own conscience. *This* is a titanic system. It goes way up past the hospital, to the insurance companies and the rest. As a doctor, you're the leading edge of this … *machine*. You're not a doctor in an individual relationship

> with a patient. And the rift seems to be between those two groups of people, people who think you're part of the engine of health care and the people who see themselves as doctors who take care of patients. The biological people tend to fit better into the machine, but not always, and the process by which this transforms the institution is so *insidious*. I used to think that I should write it down while it was happening, keep notes, but I didn't and sometimes now I sit here and think, how exactly did it happen? (Luhrmann, 2000, pp. 239 – 240)

This monograph you are reading is an attempt to provide such documentation, and to explore its implications.

This dissertation seeks to further the projects of these scholars. While Shorter's narrative provides the basic overarching narrative, my project deepens his analysis by focusing on one city's mental health community to see how these transitions actually played out on the ground. In sum, this project shall reveal that Shorter's narrative, in which psychiatric science replaces psychoanalytic religion, is incomplete. Because Healy's primary focus has been on psychopharmacology research, not service provision, this dissertation adds to his research. Unlike Luhrmann's synchronic study, following service provision and training at one point in time, my narrative is diachronic, revealing the way mental health provisions have changed and shifted from WWII to the present.

The research presented here is of the changes in the mental health field's very "object of inquiry." To explain this concept, I shall now turn to the theoretical precursors of my project.

Theory

The research that follows is Foucaultian, in so far as Michel Foucault is the scholar most associated with documenting how the history of ideas is as "imbricate" in power as it is in the accumulation of facts. The term imbricate is slightly esoteric in common parlance, but has become a technical term in post-Foucaultian scholarship. It means to overlap evenly, like roof shingles. As a technical term, it means that the collection of facts overlaps with the collection of power. In *Madness and Civilization* (1973), Foucault documents the history of the concept of "madness," a category that he shows to be historically transient—meaning different things at different times and in different places. He further shows that each iteration of "madness" has important administrative implications, in how people who are "not sane" are treated and dealt with. My project follows Foucault this far: it will show that even in the short time under discussion (WWII until the present) and in a specific location (a medium-sized Steel Belt city) the definition of mental illness and its treatment changed dramatically. This change was not simply a "progress" of understanding, but a change in what is typically called the "paradigm" (Kuhn, 1996) of mental health. The very objects that make up the field shifted in quality, which is important to attend to in understanding how biological psychiatry came to replace dynamic psychiatry as the dominant modality of mental healing.

After Foucault, documenting paradigm change in mental health is not a unique endeavor. However, Daniel Burston (1996) has added an important insight to such efforts, by pointing out a unique feature of the studies of the "mind" and behavior, especially in the contemporary milieu—namely, that no single paradigm has ever emerged to hold complete dominance in the field. Thomas Kuhn's original work on paradigms (1962/1996) looked at physics and astronomy, where one paradigm holds dominance over the entirety of the field for extended periods. The periods between paradigmatic dominance are considered "revolutionary" periods: rare and decisive. Kuhn points out that during these periods, facts alone do not determine which paradigm will emerge as dominant, and that scientists within a community in effect collectively decide on the direction of the field. Kuhn admits that there are always a few scientists who are not ready to move in the direction that emerges dominant, and it usually requires a generation for that group to, literally, die out. But when this happens, the new paradigm is mostly unquestioned, appearing to be the natural result of scientific advancement, while the awkward process of settling on the new paradigm disappears into the dustbin of history.

In his chapter entitled "The Topography of Babel," Burston (1996) follows Kuhn's work in regard to "the studies of the mind," pointing out that here too the fundamental premises of the discipline are in effect as much collectively agreed upon as decided by discoveries or facts. However, Burston adds the insight that for those disciplines that study "the mind" (psychiatry, psychology, cognitivism, psychoanalysis, etc.) there is rarely any shared paradigmatic commitment to what is real, true, countable, or factual. The multiplicity of the disciplines oriented to the study of the ostensibly "same" object is certainly telling, but even within and among these groups, scholars and practitioners have various commitments to which they adhere. Because of this, Burston prefers to use the word "models" instead of "paradigms," because he believes the latter word is misleading in the studies of the mind, where various models co-exist and continue to impact mainstream directions for the field. (As I shall discuss below, while I understand Burston's motivation, I shall continue to use the word paradigm in this project, since this term has become well accepted across the humanities and social sciences). These fields progress towards their truths like the Israelites pursuing God through their tower: eventually degenerating into a labyrinthine and fragmented structure as their ability to communicate becomes increasingly diminished (see Derrida, 1985, for an interesting analysis of the metaphor of the Tower of Babel in explaining attempts at progress.) Unlike the physical sciences, with their relatively clear revolutions and clear winners, the science of the mind is in continual chaos. We must gain some understanding of these sciences' structures and dynamics to understand the manifest transitions in the service and research areas they promote. To accomplish this, the project that follows will attend to the professions dedicated to "mental and behavioral health"—for it is professions that bind the studies of the mind to those practices of mental healing at the heart of this dissertation's study.

The System of Professions

In contemporary American society, the mental health professions supposedly apply abstract ("scientific") knowledge to the service a person receives in treatment. Sociologist of knowledge Andrew Abbott explains: "[professions are] exclusive occupational groups applying somewhat abstract knowledge to particular cases" (Abbott, 1984, p. 8).

> Any occupation can obtain licensure (e.g. beauticians) or develop an ethics code (e.g. real estate). But only a knowledge system governed by abstractions can redefine its problems and tasks, defend them from interlopers, and seize new problems—as medicine has recently seized alcoholism, mental illness, hyperactivity in children, obesity, and numerous other things. Abstraction enables survival in the competitive system of professions. (Abbott, 1988, p. 8)

Professions try to justify their claim to "jurisdiction" over a given social task (e.g. diagnosis, prescription privileges) through recourse to that profession's abstract knowledge. Conflicts over jurisdiction highlight the fact that the professions are interconnected in a "system" wherein a change in one profession's roles and responsibilities impacts those of the others. By studying this professional jurisdictional system, we are able to see the relationship between various knowledge bases (e.g. the research, the science, and the paradigmatic and epistemological commitments of each mental health profession) and the work actually done with recourse to that knowledge:

> For some, the relation between professions and their work is simple. There is a map of tasks to be done and an isomorphic map of people doing them. Function is structure. But the reality is more complex; the tasks, the

> professions, and the links between them change continually. To some extent, these changes arise beyond the professional world. Technology, politics, and other social forces divide tasks and regroup them. They inundate one profession with recruits while uprooting the institutional foundations of another. (Abbott, 1988, p. 35)

The professions link abstract knowledge to social tasks. By studying developments in the system of professions, one learns the way research developments actually impact what gets done in the field. Further, one sees the way that societal demands for certain things to get done impact what gets studied, what gets taught, and what science emerges as the newest “breakthrough.”

Fortuitously, Abbott provides a professional history of the mental health field as a demonstration case of his general theory on the professions. I shall provide a short synopsis here of this history for two reasons. First, it demonstrates the power of his theoretical edifice in describing the relationships that occur between abstract mental health knowledge and dominant mental health paradigms, and the provision of mental health services. As I have been arguing, it is important to attend to the dynamics working in this relationship, because they tell us much about which knowledge and tasks will dominate. Second, Abbott’s history provides a helpful historical background for my project, ending as it does at around the same time period that this dissertation begins its analysis.

Abbott refers to the mental health field, and especially the part that is addressed through outpatient services, as the “personal problems jurisdiction.” He points out that in “mid- to late-nineteenth-century America, there was no general public conception of problems of living” (Abbott, 1988, p. 281). Problems which today are considered mental health problems were conceptualized as “everyday life problems” and were primarily addressed by family, friends, or members of the clergy. While there were times when a person experiencing marital quarrels, career difficulties, or the like would consult with a professional such as a lawyer or doctor, these professionals were turned to because of their status in the community, and not because they were thought to have any professional expertise on the life problem.

Increased modernization in society “created new problems for individuals and required their solution” (Abbott, 1988). Individuals were torn from the old social networks that offered stability and from the traditional means for solving personal problems:

> The list of these changes is familiar: the emergence of large factories and corporations, the appearance of activist governments, the increase in physical and social mobility, the rise of cities, the immigration of a new underclass. We commonly think of these as social changes, but they were experienced, perhaps in a new way, as personal problems in particular individual biographies. (p. 282)

These changes created what Abbott refers to as an “objective” problem: there really was a new social problem to solve. However, there was not—and never is—one necessary solution to a new objective problem. Instead, various entities emerged to address the new “general unhappiness” individuals were experiencing. The churches, psychic cults, the positive thinking movement, and social clubs variously engaged and integrated increasingly isolated (and isolable) persons. "But like all such problems, it also became the target for professional claims. These claims would give subjective definition to the loose objective reality generated by the great social changes of the nineteenth century, turning them now into everyday life problems, now into nervous diseases, now into emotional problems" (Abbott, 1988, p. 285).

The first medical profession staking a claim to jurisdiction over "general unhappiness" was neurology:

> A young, colorful and elite specialty, neurology's roots lay in Civil War field surgery. As neurologists returned to peacetime work, they came to occupy a peculiar strategic role. In theory, their work was unified by the common factor of association with nerves. In practice, their clientele were increasingly made up of people who perplexed other doctors. (Abbott, 1988, p. 286)

Neurologists brought together a "ramshackle system of knowledge" (Abbott, 1988, p. 289) to address their heterogeneous client base: "As long as the theme of nerves entered somewhere—in symptoms, etiology, pathology, or physiology—a disease could be located under the new specialty" (Abbott, 1988, p. 286-287). Abbott stresses that this heterogeneity did not represent "scientific primitivism, but rather reflected the uneven knowledge characteristics of an active discipline. During the late 19^{th} and early-20^{th} Century "[a]cademic neurological knowledge accumulated very rapidly but remained in the disassembled state that makes academic knowledge relatively useless for practice"

(Abbott, 1988, p. 287). In fact, any success that neurological research did muster, such as treatments for endocrine disease, resulted in the removal of the disorder from neurology's jurisdiction. Increasingly: "it seemed that the criterion for inclusion of a disease under the jurisdiction of neurology was its untreatability" (Abbott, 1988, p. 287).

Neurology's prestige was not impacted by its status as the "residuary legatee of medicine" (Abbott, 1988, p. 287), and in fact its jurisdiction over such disorders grew. Abbott argues that neurology's approach to problems of living, rooted as it was in a rigorous scientism, regardless of its actual lack of success, made it more attractive as a profession for these problems than its competitor professions such as the clergy, "the gynecological neurologists," the "mind curists" and other such groups. Neurology's professional success lay in its approach to the problem, which appealed to contemporary social norms and expectations. In particular, neurology established a narrow (if tenuous) diagnostic criterion for nervous disorders, as well as concrete, specific therapies such as rest cures, electrotherapies, and bromide salts. Such specificity was important:

> [D]isciplines with specific therapies will generally defeat those with only vague therapies when results are equal. Thus the neurologists, who in fact accomplished little more for general nervousness than did the clergymen, won the day. Although they accomplished no more, they inspected and tested and argued, giving themselves at least the scientific legitimacy of observation and criticism. (Abbott, 1988, p. 290)

In fact, it was neurology's overwhelming success in claiming "nerve" disorders that proved its ultimate undoing. The field could simply not create enough professionals for all the available business, allowing room for poaching by other professions. The profession that poached most effectively was psychiatry.

Psychiatry was originally the branch of the medical profession which ran "mental hospitals," institutions that replaced "lunatic asylums" as personal problems were reconceptualized from moral/religious failings, into medical problems. To justify their control of the hospitals, psychiatrists produced "moral therapy:" "which promised complete cures under the properly detailed regimentation of the activities, the emotions, and the environment of the insane" (Abbott, 1988, p. 294). By 1900, moral therapy had proved to be a "clear failure," and psychiatry went looking for a new knowledge to ground its claim to jurisdictional control of mental hospital administration.

Through much discourse, psychiatry began to settle on the notion of "prevention."

> Psychiatrists reasoned that moral therapy failed because insanity reached a chronic state before psychiatrists saw it; one must stop the disease before it began … Prevention gave the psychiatrists entrée to the neurological clientele, for psychiatrists considered neurasthenia, general nervousness, and so on as 'borderline states' on the road to insanity. Between 1890 and 1920 there was a substantial interpenetration between the elites of the two professions—joint society memberships, careers traversing both specialties. (Abbott, 1988, p. 296 – 297)

Simultaneously, psychiatry was encroaching into the legal profession, arguing that criminality and delinquency were diseases under their purview. Increasingly after WWI, the theories of Freud became essential for conceptualizing criminal action in a new way, and granting psychiatry increased say on matters from which neurology had steered clear.

Abbott lists the various competing paradigms in psychiatry at this time, but argues that the need to fit into the jurisdictions of criminality and personal problems provided psychiatry the incentive to settle on an increasingly comprehensive theory of "adjustment," which became central to the rhetoric of the profession. The implicit assumptions of adjustment were

> (1) that all social factors in nervous and mental disease were important only through their effect on the individual; (2) that any violation of social rules ('the mildest psychopathies, the faintest eccentricities') signified mental problems; and (3) that the proper approach to such problems was individual, not social. These assumptions made psychiatry's general theory of adjustment an enormous popular success. They accepted the new order of society, offered an interpretation for basic social problems, and thereby anchored the borders of the new world. (Abbott, 1988, p. 298)

This rhetoric of adjustment provided psychiatry with the mandate to move out of the mental hospitals and into private practice, where they would address the problems of the “maladjusted” before chronicity set in. It was psychotherapy, particularly Freudian “psychoanalytic” psychotherapy, which granted psychiatry ascendancy over neurology in the treatment of personal problems. Freudian’s did this by arguing that people could be better “adjusted,” and then by offering the promise that further pursuit of this course should provide more means for adjusting more people. As noted previously, Shorter conceptualizes psychoanalysis’ dominance in American psychiatry circa 1920 – 1970 as a “hiatus” (Shorter, 1997) from psychiatry’s natural, neurological “roots.” On reflection, it was a very *long* hiatus, and Shorter is unable to explain Freudianism’s popularity, despite his slightly offensive theory that psychoanalysis was a Jewish “gift” to America from German émigrés (Shorter, 1997, 181-190). Abbott, by contrast, makes this long “hiatus” intelligible, by pointing out the professional possibilities that Freudianism offered through using its modest successes with some mental health disorders as grounds for promising societal transformation.

Psychoanalytic theory offered psychiatry all of the necessary tools to demarcate a distinct professional space for itself. First, it offered psychiatrists a technical procedure (the resolution of internal (unconscious) conflicts) which was said to promote adjustment in the most efficient way. Second, this technical procedure had the jurisdictional impact of clearly differentiating so-called “organic disorders” from “functional neuroses,” thus stressing the separation of psychiatry from neurology, and giving supremacy to the former in “problems of living.” Third, Abbott points out that “Perhaps the most important reason for the success of Freudianism … was its approach to professional inference” (Abbott, 1988, p. 305). By positing the existence of defense mechanisms and an ontogenic theory of “normal” development, Freud’s theory “afforded a new and more logical hermeneutic" than the neurological eclecticism that was competing with it for jurisdictional control. Freud posited actual etiological causes (e.g. regression, fixation, reaction formation, etc.) and treatments which worked directly on these causes. Through Freud, psychiatrists could imagine that they understood the basic mechanics of mind, from which they could deduce clinical prognoses, and the most efficient mode of intervention. Thus, the logic of medicine and the logic of psychoanalytic treatment were homologous, increasing the attractiveness of the theory to psychiatrists.

Keeping in mind how little was known about the nervous system at this time, there was no reason to put increasing faith in a strictly neurological conceptualization, especially because this would *de facto* give away jurisdictional control to those specializing in the nervous system. Likewise, Adolf Meyer's brand of eclecticism, which enjoyed some popularity then, was too honest about the lack of knowledge about said disorders. Meyer's psychobiological approach "combined organic, psychic, and environmental causes in a loose but formal system called psychobiology … This laudable but vague program foundered early, and new eclecticism in practice became utterly clinical." Without recourse to an abstract knowledge that grants expertise, it is difficult to claim control over the task. Everyone feels they can do trial-and-error tests on a problem, but professions need to promise more. Freudianism provided the unified approach that other approaches at the time could not. Finally, Freudianism boasted documented successes (though never as remarkable as its most vociferous defenders stressed), which certainly made it attractive to consumers and practitioners alike.

Before turning to the specifics of my project, I would like to highlight that Abbott provides a compelling history of the dominant scientific approaches to mental health by attending to how the science offers justification for professional advancement. Abbott remains agnostic as to what is "true," but instead highlights how science provides solutions to "objective" social problems. This does not mean there is no "truth" to any given scientific project such as the study of "nerves" or the unconscious. In fact, these theories have to "work" at some level, or they would simply be ignored. But what is important from Abbott's perspective is that knowledge justifies a certain kind of work, that the doing of that work justifies increased research and training in a certain direction, and that all of this impacts the whole social system. What defines solvency is not just an abstract scientific question, but the ability to solve the problem in a way that squares with reigning social and cultural values. In sum, Abbott's approach treats science as fundamentally pragmatic (cf. Rorty, 1991).

While Abbott's short case study demonstrates the usefulness of his model for describing transitions in mental health science and practice, my project is not just a continuation of Abbott's story. For, although Abbott uses mental health as a way to describe the system of professions across the whole of the United States, I am interested in his insights to get a better understanding of one mental health community in a medium-sized United States city. My goal is to understand how research, training, and practice have changed within this city as the dominant mental health paradigms and professions shifted, and what the implications of these dynamics are for the system today. Thus, my project is a sociologically informed history of science and medicine rather than a sociological study of science and medicine.

Method

This dissertation begins where Abbott's story ends. The time is the mid-1940s and early 1950s. Neurology has moved explicitly into the disorders of the nervous system, and psychiatry to the problems of "adjustment," which it addresses through psychoanalytic psychotherapy.

At the outset, I knew I wanted to understand the mental health field's development from this period until today. Beyond this general goal, however, I was unsure about how to pursue the project. In this section, I shall explain the process through which I settled on my method, which also involved refining my research question.

First, I should note that I was not yet familiar with Abbott's work when I began the dissertation. Instead, my own professional involvement in the mental health field drew me to the topic. As a graduate student in clinical psychology in the contemporary mental health sphere, I was experiencing the realities of today's mental health field, and had a curiosity as to how things came to be as they are. I began reading the conventional texts on the history of psychiatry and mental health, and became aware of the gaps in the literature as discussed above. Through discussion with my dissertation advisor, Daniel Burston, the idea of doing a case study of a mental health community that I knew intimately emerged as an effective entry point for understanding the broader history. This was especially true, because I had a sense of some interesting historical facts about the mental health community in which I was training. Currently, the community is dominated by one of the nation's most powerful and influential psychiatric research institutes. This institute is famous for forwarding what I called at the time a "biological model" of psychiatry, which I believed at the time stresses technological interventions over psychotherapeutic interventions. I also knew that at one time the institute was dominated by psychoanalytic practitioners. The transition from one leadership cadre to the other sounded like an interesting story worthy of documentation, and one that could reveal much about the transition that occurred nationally. My advisor agreed on the value of documenting this transition, and I began to study the methods of historical research to do my work.

However, the Duquesne University Institutional Review Board quickly thwarted my attempt to write such a history. I was told explicitly that such a history would cause "liability concerns" for my host institution, and thus would not be approved. The reason for the concern is interesting to note here, because it highlights another one of my false impressions when beginning the project: that the conflicts being documented hit at some raw nerve amongst the historical players. Such sore feelings never emerged in my interviews, and most of my participants were very excited to share with me their understanding of the community's history.

Without the ability to write a "straight ahead" history, I went looking for a good heuristic for organizing an abstract case-study of this same community. At first I was irritated by this IRB demand. However, with time, I began to realize the scholarly advantages it offered my project. Namely, that specific historical data can distract the reader (and writer) from the more important structural transitions which were my real interest. Histories can come off as gossipy, missing the forest of structural transition, for the trees of individual personalities and idiosyncrasies. Both the writer and the reader can become distracted catching all of the individual details necessary to pull off good history, while the real goal (of tracing large transitions through understanding of individual actors) can get lost. I think an example of this problem is found in Douglas Kirsner's *Unfree Associations* (2000), in which Kirsner documents some of the more bizarre antics in four important psychoanalytic institutes. While Kirsner's scholarship is able and thorough, I personally could not help but wonder how much of the story was really due to anything essential about psychoanalysis or its institutional structure, and how much was due to the leaders in charge at the time. After all, bizarre politics are not uncommon in any field of any kind (cf. the hit BBC television show, "The Office"), why air the dirty laundry of psychoanalysis in particular? Kirsner's "naming of names" distracts from the larger processes at work in psychoanalytic institutes, and does a disservice to his topic and scholarship.

Thus was born "Dubville," a medium-sized city in a region which will go from being called the "Steel Belt" to the "Rust Belt" during the period under discussion (WWII to the present). Becoming more comfortable with the concept of an abstract case study caused me to think through which particular historical structures I wished to understand better. It was in this process that I came across Abbott's work, and became very interested in looking at the mental health community in Dubville through attention to professional transitions. I found Abbott's approach highlighted mechanics in scientific and administrative transitions that I had intuitively felt were there, but couldn't understand or explain. Abbott's approach is rational, rooted in empirical fact, and offers an idiographic narrative that is compelling, though falsifiable if new evidence is brought forward. The professions provide an entry point for seeing the transitions in science, practice, funding streams, and social expectations: all important structural transitions that I was interested in, and which I feel further our knowledge on the topic of mental health science and practice.

The project was now solidly oriented towards becoming an abstract case study. However, it still required the use of methods found in traditional history, namely: interviews and archive work. I went looking for appropriate methods for doing these tasks, and was lucky enough to stumble on Berg and Smith's *Exploring Clinical Methods for Social Research* (1985). The book is a collection of different scholars utilizing psychotherapy tools in order to do institutional research. The authors in the collection argue that psychotherapists develop an understanding of how to access the experiences of individuals in order to understand larger structural dynamics. Typically, these "larger dynamics" are developmental sequences and their vicissitudes. However, they could also be other dynamics, such as the transitions in the system of professions. The basic point is that through coming into "psychological contact" with individuals within the system of professions, asking the right questions, and listening the right way, I could get a sense for this individual's perspective on the larger structural transitions in the system of professions. Then, through combining the various narratives, I would have a fuller sense for many of the dynamics in play through this history. Further yet, I would have a perspective that one cannot gain simply by looking at statistical data of professional transitions, because I would have data about how people went about making their own professional choices, the way they understood the value of various science regimes at various times, and so forth. In all, such an approach gives access to what I call the "professional phenomenology" of the interviewee: the lived experience of holding a certain professional role through time. It is a research method that builds off of the skills developed when one trains to be a psychotherapist, but not towards further therapy, but towards social research.

With these “clinical methods” in toe, I then had to decide on the right people to interview in order to get a full sense for the professional structure I was trying to capture. Abbott was helpful in deciding who to interview. I interviewed hospital directors, independent clinicians, paraprofessionals, students, researchers, state and local mental health administrators, members of independent institutes, and of private schools, and state schools, and members of all the major professions. These people provided a full understanding of the system of professions operating within Dubville under the time being studied.

The first question I always asked was: “Could you please tell me your professional narrative?” I would like to highlight a few features of this question. First, it is rooted in the psychotherapy tradition. It asks an interviewee to reflect on his or her history, and deliver it in linguistic form. It is interested in collecting facts, certainly, but it is also interested in collecting opinions and motivations which are buried in the narrative construction a person offers. As I shall discuss later, I substantiated the assertions of fact presented to me with reference to concrete data I mined in archives and libraries. However, more important than the facts was this professional’s self-understanding of his or her narrative, and the location of this personal narrative within the larger historical narrative of the professions. As the interview progressed, I helped the interviewee further refine his or her career self-understanding. I had many interviewees tell me they enjoyed the process of reflecting on the large scale trajectory of their career, something that had taken so much time and energy in their life, but which they had never stopped to reflect on in this deeper way.

Obviously, the narratives provided were historical reconstructions, and did not honestly capture the true crises and vicissitudes that motivated all of the individual career changes. For my purposes, however, this is fine. After all, this is the same problem that psychotherapists always encounter when working with clients to understand the client's history: the reconstruction of any given moment may not match the reconstruction of another moment. However, this research is a good first step in unraveling the transition in the system of professions as understood today (or as the Lacanians often say, psychic reality is the only important reality). Second, because of the fact that I interviewed many different people in different situations, and then brought their stories together to speak about the larger changes across "Dubville," the individual nuances and mis-rememberings should hopefully cancel out to provide a relatively stable trajectory (this is the same assumption statistical analyses do when increasing their "N," and seems to work well enough.) Last, 100 years of psychotherapy reveal that psychotherapists can be quite good in helping people identify their honest motivations in historical times, and gain what is typically called "insight." Though some people do not believe in insight, I see no reason to countenance such skepticism. It seems fair to me that people can gain an insight into their past motivations, and learn from the experience personally. There is no reason this cannot be done about one's professional history as well.

Last, I would like to note that though influenced by the psychotherapeutic approach, my interviews were *not* psychotherapy. The first reason this is true, is because I had no interest in intervening in the narrative, or helping the interviewee reinterpret their own narrative (which is typically the goal in psychotherapy.) I was merely interested in recording their narrative, deepening the interviewees' self-understanding of their narrative, and documenting it for my own purposes. This question invites participants to tell the story of how he or she moved through their careers, and why. In their answers I discovered the conscious motivations behind their professional and scientific choices. They related to me information on the opportunities that were available at various times in their careers, the knowledge bases of relevance at different times and in different spheres, and the constraints and opportunities faced by professionals working in the field. In other words, the individuals' narratives gives us insight to the structure, but also reveals how they related to that structure, and made the best choices that they could for themselves, and for their patients, in the process.

I would like to note that there were invariably some interviews and data collection which did not make the final product. For instance, I interviewed leaders in a local family research institute, two leaders in professional psychology organizations, and a philosopher who was intimately connected to the narrative at hand. However, none of these narratives are explicitly noted in the narrative below, because upon final review their stories added unnecessary complication and distraction, instead of clarity. This is a shame, and perhaps I will have the opportunity to publish those stories in future projects.

Also, due to IRB constraints I could not interview patients or clients treated during the reported history. This leaves lacunae in my final narrative, though I hope the reader will concur that this hole does not undercut my final conclusions. Perhaps future researchers shall fill-in this data through their own research.

In all, I interviewed 38 people. Most interviewees were only met with once, for an interview lasting about an hour and a half. There were some exceptions. My first interviewee was Mathew James, a key leader in the local psychoanalytic community. I was still figuring out much about the method and the project, and thus stumbled through this interview. I met with James 3 times, for about 2 and a half hours each time (he even served me lunch during one of our meetings!) It was excessive, and the fact that he was willing to give me such time is a testament to his generosity and kindness. I found through that experience that more time does not give me more helpful data, but leads to distractions that muddle the research. At first I also digitally recorded my interviews, thinking I wanted to capture everything that was said in perfect precision. I soon abandoned this for several reasons, and simply took furious notes throughout my interviews. The digital recording added work (in needing to transcribe things) and also made me a lazy listener during the interviews. Without the recorder acting as a "safety net," I was more focused on getting the data I needed, and asking the questions I needed to ask. I deleted all the sound files I had acquired up to that point in the process, and thus none of that data can ever slip out. Now, however, I have computer versions of the notes I took in my interviews. These are available to anyone interested. They are password protected on a separate hard drive from my computer. All identifying information has been removed from them, as it is in the dissertation.

Not all of my interviewees have their story explicitly documented in the dissertation. However, all of those interviewed informed my understanding of how various professions looked at different times: the work that was done, how it was funded, how it was justified, and its relation to other professions. These interviews are the bedrock of the dissertation.

Interviews, however, are not sufficient for history. Thus I also researched archives, newspapers, and other scholarly works in order to flush out the story. First, I was very fortunate to find that the “University of Dubville Medical Center,” the “Dubville Institute of Mental Health” and the “University of Dubville Psychology Department” all had internal institutional histories that provided my narrative scaffolding (in order: Paull, 1986; Schachner, 1983; Musselman 1999, 2001, 2007). I also spent extensive time sifting through two local archives, reading about developments in mental health services and policy over a 50 year period. I spent extensive time in the Dubville Institute of Mental Health’s psychiatric library (now unfortunately closed forever) reading through many old journals, institutional annual reports, and important books written by faculty, as well as listening to their extensive collection of archived audio material.

I then pieced these various pieces of information together into a narrative. There were two kinds of contradictions which emerged in this process. There were first contradictions in fact: one source would say a certain thing happened at a certain time, while another source would say otherwise. When these kinds of contradictions emerged I sought primary sources to reconcile the contradiction, or simply removed the item from my narrative in order to avoid passing on inaccurate historical information. More often than not, these items were fairly easy to clear up through recourse to primary source information, and thus there should be no glaring historical errors or omissions in the narrative.

Second, there were contradictions on matters of opinion. For instance, a psychoanalyst might interpret a certain action by the contemporary hospital administration as representing one thing, while the administrator him or herself would interpret it another way. The generation of such contradictions is the hallmark of good clinical research methods. This is the "fruit" of the approach, and its value, because it gives us insight that no other approach can capture. These contradictions are not "reconciled," but presented and explored for what they say about the differences in perspective that each party has on the situation. Such contradictions often reveal the underlying professional tensions at play in the historical narratives, and are thus worthy of exploration, instead of simple reconciliation. To use the example above, my interpretation may be that the psychoanalyst and hospital administer disagree because of some facet in the professional transition of the time. My intention in doing this is not to discount either person's opinion, but to contextualize it within the larger operating dynamics. My hope is that each actor shall feel that my interpretation validates their interpretation, while also richening it. So far, readers of this dissertation have told me they think I have done this successfully, and I hope future readers feel the same.

Notes on Bias

As I have already mentioned, I am familiar with Dubville's mental health field through my first-person participation in it as a clinical psychology student, and as provider of mental health services. I have had the opportunity to work in the Dubville Institute of Mental Health research hospital, and thus cannot make a claim for pure research objectivity in this endeavor. In fact, I would like to "fess up" to two ways that my subjective viewpoint will color this document from the outset.

First, as the narrative progresses from the 50s, through the 70s, and into the contemporary era, more and more of the observations on the dynamics of the institution will be informed by my own experience. I worked hard to document concrete instances of various phenomena, and to only point out factors which my interviewees or archive material identified. Nonetheless, I cannot deny that my take on the way the mental health community in Dubville works today is influenced as much by my participation as a front line clinician, as it is by my attempts to be a neutral observer.

Second, as a psychotherapist, my training biases me towards attending to the complexities of human consciousness, and being skeptical of attempts to reduce individuals to simple clinical indicators: be they chemical, behavioral, dynamic, or whatnot. I have a bias towards appreciating those traditions in mental health which take concepts like "psyche," "mind," "unconscious," "depth" and so on, seriously. I am appreciative of the complexities of these terms, but also have good empirical reasons to believe that they are all pointing at something that (a) actually exists, and (b) we do not have a powerful, coherent understanding for yet. It is typically hard to convince me that these are irrelevant concepts in mental healing. To me the only question is: how are they addressed, in what fashion, and what are the consequences?

Having laid these biases bare, let me add that as a researcher of science, however, I learned early on to try and avoid committing to any "dogs in the fight," as it were. I have tried to enter Dubville's mental health community without attempting to prove one school right, or to value one research or professional tradition over another. I honestly believe that most people in our field are doing the best that they can, based on their beliefs on how things work, and where they believe they can be most helpful. My job as a researcher is to help them articulate these beliefs and practices, and document them. I do not want anyone to feel worried that I will jump on their assertions, critique them, and advance my own agenda. I am a strong believer in a pluralistic academy, and am trying to open dialogue, not win a one-sided debate. I ultimately believe that my "insider" knowledge on Dubville's mental health community helps me be more fair and even handed, since I understand more of the underlying debates, can more efficiently draw attention to underlying tensions in any given scientific assertion, and am not invested in having any side in particular win.

Notes on Technical Issues

As noted previously, all of the names and institutions within Dubville are pseudonyms. To help clarify this, institution names which are pseudonyms are italicized throughout the document. Also, in order to assist with clarity, I have added a table in the appendix (Appendix B) which has all of the character pseudonyms listed in alphabetical order, with a general explanation of their professional role in our narrative.

I follow American Psychological Association Style protocol throughout the document. The only variation from the APA rules is that all my level 2 and 3 headings have been bolded, and spaced from the preceding paragraph. This was done at the request of my committee for clarity's sake.

Appendix A is the consent form that all of my interviewees signed before the interviews began.

Summary

I now turn to the project itself. It begins in the early 1950s, in Dubville's premier mental health research hospital, the Dubville Institute of Mental Health (*DIMH*). The first chapter recounts what the hospital looked liked through the 50s and 60s, when it was run by world-class psychoanalysts. The second half of the chapter shows the tensions that psychiatrist psychoanalysts faced in maintaining control of the institution and the field. Chapter two shifts attention toward the activities of psychologists during this period, with a specific focus on the University of Dubville's clinical psychology program. I shall show the unique tensions experienced in this discipline/profession, and the various moves made by psychologists to gain entrance to the lucrative mental healing world, which psychoanalysts were guarding jealously. Chapter 3 introduces the leadership team that brought the end of psychoanalytic psychiatry at *DIMH*, and illustrates how science, politics, and the pursuit of accountability conspired to re-make the mental health professions in a way that transcends the simple story of the rise of "pills" (or psychopharmacology) over "talk" (or psychotherapy). Chapter 4 looks again to psychologists, showing how they adjusted to this new system by picking up what was rejected of psychoanalysis, and advancing themselves as a profession with the new leaders in town. The dissertation's conclusion highlights the implications of the professional transitions in the field for the science and administration of mental health.

2 - *DIMH* 1950 – 1970: THE PSYCHOANALYTIC PSYCHIATRISTS

The Dubville Institute of Mental Health (*DIMH*) came into being as a part of the post-WWII enthusiasm for psychiatry and mental health services. Due to a confluence of factors (including the success found in treating World War II veterans with “war shock,” the immigration of the talented and driven European psychoanalysts, and the increased reliance on psychological theories and practices by elites in governance) mental health and its associated research base increasingly became a part of the national consciousness. Under the leadership of Robert Felix at the National Institute for Mental Health, post-War America significantly increased resources for mental health personnel, services, and research (Curtis, 2002; Grob, 1994; Zaretsky, 2004).

Dubville is located in a state, Dubstate, which was at the forefront of this emerging trend. Prior to World War II, Dubstate had already been pouring significant sums into mental health services through its extensive state hospital system. The costs of these hospitals were growing, and there was a general fear that this custodial system would continue to expand indefinitely. The state decided to build three mental hospitals that would focus on treatment research to forestall a situation where the state would be the lifelong custodian of a growing population. One was built in the eastern part of Dubstate, one was built in the center of the state, and one was built in the western part. *DIMH* was the western institution.

In 1942, Dubville State Hospital (*DSH*) was officially opened in Dubville, and was run as a research institution in the state hospital system. *DSH* would receive its patients from the other state hospitals, who would “pass along” these patients due to their being of particular scientific or clinical interest. However, as a public hospital, local

Dubville residents looked to it to provide mental health services for the community. *DSH*'s doctors began turning people away, resulting in public relations difficulties for the hospital. In a surprising commitment to research, the state responded by siding with the doctors, and stepped up to defend the hospital's research focus. The name "hospital" was removed, and instead the word "Institute" was put in its place creating what is known today as the Dubville Institute of Mental Health (*DIMH*)—highlighting the research aspects of the institution. Further, in 1949 the State sold the institution to the University of Dubville (*UDub*) for $1, in recognition that the university had expertise in research with which the state simply could not compete (Schachner, 1984).

The University spent two years finding the appropriate director for the new addition to the University medical center. Picking the right director is an important and difficult decision: the person has to excel not only in the everyday problems of large scale administration, but also in sensing what is most promising in the scientific field, and in knowing how to orient the institution towards excelling in that area. After all, science does not take care of itself. *DIMH*'s director was hired with particular attention to the kind of knowledge he was going to cultivate; he would be responsible for picking the major research priorities the institute would pursue, as well as for hiring the researchers who would pursue them.

The man chosen for the job was Robert Hanks, and it is his scientific vision that shepherded *DIMH* through the 1950s and 60s. Obtaining Hanks was quite a coup for this small, fledgling research hospital in a medium-sized city. Trained in electrical engineering and English literature before turning to medicine and psychiatry, Hanks was already the director of a major academic psychiatry department. He was wooed, however, by the prospect of crafting a great psychiatric “empire” (Schachner, 1984, p. 107). There was no question for Hanks on what foundation this empire would be crafted: Psychoanalysis would be its Magna Carta.

Psychoanalytic psychotherapy promised a method to treat the institutionalized mentally ill, and, even better, if done on a wide enough scale, actually to prevent severe mental illness before it took hold. Psychoanalysts had proven their wares: Freud’s cures for hysteria were well known and many shell-shocked troops did in fact improve through talk (Grob, 1994). Due to their documented success, particularly with returning soldiers, psychoanalysts were becoming a highly esteemed group, sought by government and business for the insights they offered into the workings of the mind (Curtis, 2002, Zaretsky, 2004). I have already sketched Abbott’s account of psychiatry’s paradigmatic commitment to psychoanalysis in the mid-20th century as a means of gaining leverage over “adjustment” problems. In and of itself, psychoanalysis need not be connected to any particular profession, and in fact, Freud preferred that psychoanalysis not be subsumed under medicine, believing that such affiliation would limit the field’s potential (Freud, 1926/1947). However, in the United States during the late 1940s and early 1950s, when this narrative begins, psychiatrists were vigorously protecting their monopoly, ensuring that only MDs could enter the institutions which trained analysts.

Most psychoanalysts claimed to have an objective understanding of the human mind, or ‘psyche’ -- a claim that most philosophers and psychologists had abandoned as being beyond the purview of their disciplines. While practitioners of these disciplines turned their attention to neurological structures or observable behavior, measurable features more easily deemed “objective,” psychoanalysts felt that Freud had provided ground for looking past behavior into motivation and unconscious processes. This distinction explains much of how *DIMH* was structured.

Franz Alexander articulated the fundamental philosophical assumptions of 1950's psychoanalysis as understood in *DIMH* in his text book, *Fundamentals of Psychoanalysis* (1963). Alexander was affiliated with the Chicago Psychoanalytic Institute, where he was a colleague and teacher of Robert Hanks and other *DIMH* faculty members that Hanks brought with him (one of whom is mentioned explicitly by Alexander in the acknowledgements page.) In the second chapter, Alexander makes the case for what he calls "psychological understanding" (Alexander, 1963, p. 23) by arguing that the human capacity for *introspection* is the fundamental building block for any proper psychology.

> The importance of introspection as an aid in interpreting human behavior cannot be overemphasized; it constitutes the basic difference between psychology and the natural sciences. All psychological methods which fail to recognize and exploit this unique advantage of psychology must have a limited value for the study of human personality. (Alexander, 1963, p. 24)

People are able to read the basic mood states and motivations of others on an everyday basis. We see someone smiling and hypothesize that person is happy, we see them slunk over and can tell they are sad, etc. Introspection provides "knowledge" of our own states, through which we can make sense of the psychological world around us. Alexander grants that this system is not perfect, and that there is much room for bias and misapprehension, but he retorts, "is not the task of every science to improve on natural faculties of observation?" (Alexander, 1963, p. 24). Alexander goes on to elucidate various blind spots that necessarily emerge in introspection, and the manner in which psychoanalysts attempt to fill them in. But nonetheless, the capacity to introspect—mine one's own "depths" in an effort to mine the depths of another—is central to the rest of Alexander's work, and to psychoanalysis.

Psychoanalytic training encouraged introspection in its trainees throughout lectures and course work, and all budding analysts were required to undergo a "training analysis" where they came to better understand their own "internal worlds" in order to avoid biases in their interpretation of their patients' symptoms. Further, most of the healing in psychoanalytic work was done through facilitating introspection in order that the patient may release psychic energy buried deep in his or her unconscious (more about this below.) Last, psychoanalytic research in this era sought to elucidate "psychic reality," or the dynamic interactions of endopsychic structures (e.g. id, ego, superego) to explain the nature of the psyche, and to provide guidance for clinicians addressing patients with similar profiles.

Some of the most important tenets about the nature of "psychic reality" were:

1. Underlying conscious thoughts are unconscious mental processes that can be described and interpreted.

2. Many symptoms of mental disorder are the result of repressed traumas or unresolved unconscious conflicts. Accessing and interpreting repressed emotions will result in symptom diminution, as well as increased insight and control over the psyche.

3. A healthier psyche is a more "mature" or developed psyche. Psychoanalysis invested in a development model, in which the organism develops its

rational/executive functions ("the ego") through growing and interacting with the environment in increasingly sophisticated ways. Freudian psychoanalysis during the early- to mid-twentieth century, believed in a very specific developmental model in which "libido" (or psychic energy) moved from an oral phase, to an anal one, to a phallic one, and so on, through the first 12 or so years of life. One's ego-development was understood to be dependent on the progress of libido through these ontogenic stages.

4. If the ego did not develop correctly, there would be mental disorder, and an analyst's job was to help move the process along from where it became "fixated" or "regressed."

5. Psychoanalysis is a theory of the "total person." Any disorder of function brings the whole organism out of balance. A therapist's job is to elucidate the meaning of symptoms by bringing repressed memories into consciousness, dissolve fixation and neurotic inhibitions, and liberate libidinal energy so as to return the person to proper balance.

6. A psychoanalyst is someone well trained in all the processes of the human organism: material and mental. The material aspect is learned through his or her science-based, physiologically focused medical curriculum. The mental is learned through undergoing one's own training analysis. Training analysis gives the trainee an understanding of the way the psychoanalytic therapy works in his or her own psyche, while also helping to clear up the trainees' various libidinal fixations and regressions so that he or she can safely encounter somebody else's conflicted psyche without getting caught up in it.

7. In their treatment approach, analysts work to maintain a detached and "objective" position with respect to the client. The "gold standard" of analytic treatment involves an analyst sitting out of the patient's sight, and offering only sparing commentary on the patient's "free associations"—the words they utter in compliance with analysis' "basic rule," to not repress or holding back anything.

Analysts believed that Freud's theories, though still provisional, were pointing at the basic building blocks of mind. The psyche was conceived as deep and historical in its nature—akin to what religious traditions typically call a "soul," but which was now understood 'scientifically' (Zilboorg, 1941).

Given contemporary science's perspective on psychoanalysis, it might be surprising to know that Hanks' definition of science was rather conservative. Writing the opening article for an in-house, student run journal that *DIMH* began publishing in the late 1950s, Hanks stated that science is about hypotheses, rigorous methods, and "data whose basic postulates can be stated clearly and compared with the classic explorations of Francis Bacon's *The Advancement of Learning* (1605) and Claude Bernard's *An Introduction to the Study of Experimental Medicine* (1865)" (Hanks, 1958, p. 3).

For Hanks, psychoanalysis met these requirements; he was a true believer in psychoanalysis as the scientific basis for psychiatry. A voracious reader in the philosophy of science, Hanks often stressed that what made psychoanalysis an important contribution to medical practice was its location in the history of the scientific tradition, and its power to push science forward.

He believed that psychoanalysis had identified certain facts that would require appropriate tools and modes of engagement, which call us to "have the imagination and courage to invent and use methods which are appropriate to the material which is of paramount interest to us whether it be nomothetic or ideographic" (Hanks, 1958, p. 4). Thus, *DIMH*'s research staff included psychoanalysts in all of major positions.

What are the facts that psychoanalysis discovered, and how would they alter science? For Hanks, the answer to this question is in Alexander's concept of the "total" person. Understanding the "total person" will help us make sense of much of the structure that Hanks developed to understand, train, and treat patients at *DIMH*, as well as how he structured *DIMH*'s research program. Understanding the "total person" will also help us understand the changes that future *DIMH* leaders would bring, when they abandoned theories of the "total person" as the touchstone for psychiatric research.

The Total Person

The concept of the "total person" (a.k.a. "total behavior," "whole person," or other such locutions) was ubiquitous in 1950s psychodynamic psychiatry. *DIMH*'s Director of Psychoanalytic Services, Albert Roland, gave an extended exposition on the concept in his 1958 article, "Dynamics of Total Behavior in Man—the Concept of Disease" (Roland, 1958). Roland explains that a person is an interlocking system of smaller units operating in seeming independence, but combining into higher order entities which seem to have their own *raison d'etre*. These higher ordered units can in turn impact the function of the lower ones. In order to understand any part of the organism, we must understand this complex ecology. Heuristically, Roland divided the organism into three levels. First is the biological dimension. Here we have cells interacting on their own, combining to make organs, which combine to make the whole body. Second is a person's psychology, which determines how the experiences of the living organism will shape and alter its biological processes through time. As Roland says:

> "The *patterns* of response that are developed, whether thought of in terms of conditioning or of ego development, in turn play a part in determining the opportunities and capacity for responsiveness of the organism. This is to say that psychological aspects are determined at base from physiology, but soon begin also to influence physiology, and to play a part in total dynamics" (Roland, 1958, pp. 14-15)

The last important facet of the total person is its location within a society and culture that impacts these psychological and physiological ecologies. The most common example is the infant-mother relationship, which has a fundamental impact on the person the infant grows into, yet comes from outside his biology and psychology.

Roland goes on to suggest that this biological, psychological, and sociocultural "total person" is a fluctuating ecology, embedded in larger ecologies, and wrestling to stay in homeostasis. In this model, biological and psychological symptoms come about when homeostasis breaks down. If a breakdown of homeostasis is a fluctuation in the biological process without psychological impact, it appears as a traditional disorder: cancer, heart disease, and so on. When the problems with homeostasis are on the psychological end of the spectrum, these symptoms appear more psychiatric in nature, (e.g., shame, guilt, depression). Under Hanks' leadership, symptoms that demonstrate how mind and body are on a continuum—so-called "somatized" symptoms—received the most attention.

Somatization was the main focus of study for Hanks' *DIMH* because it provided a clear, empirical demonstration of Freudian insight. Before Freud, such phenomena as glove anesthesia (where a patient loses feeling exactly in the shape of a glove on their hand, independent of the fact that neural structure would never create such a pattern) were utterly mysterious. However, Freud demonstrated that through unraveling the patient's free associations, the linguistic mechanics of this somatization could be identified and the symptom lifted (Ellenberger, 1970). Hanks made somatization the focus of study and treatment, and structured the *DIMH* research program around better understanding of somatic phenomena in psychoanalytic terms.

Placing somatization at the heart of psychiatric study was a bold gesture. Since its foundation, psychiatry had been the branch of medicine dedicated to the treatment of people with florid hallucinations and completely unregulated behaviors who, for the most part, were kept in institutions. In contrast, for much of the history of Western medicine, somatization was not even considered under the purview of psychiatry, but was left to the care of various other medical specialties including gynecological surgeons, reflexologists, and family doctors (Shorter, 1992). Hanks challenged directly psychiatry's historical location in the system of professions in an essay he contributed to an important psychiatry compendium of the day, entitled *Dynamic Psychiatry* (Alexander, 1952). In the excerpt below, when Hanks refers to "ambulatory" or "nonpsychotic patients" he is referring to people with "problems of living" (see introduction), and who he believed were manifesting psychiatric symptoms in ways that were not traditionally considered psychiatric.

> "The problems of general psychiatry, centering largely around the institutional care of psychotic patients, continue to dominate the field, to the neglect of individual psychotherapy of non-psychotic patients. To be sure, the social and financial burden caused by institutionalized patients is enormous and deserves major attention. But it is also true that the number of ambulatory patients who require psychiatric care has grown to unprecedented proportions, and they, too, deserve consideration." (Hanks, 1952, p. 513)

Hanks continues, arguing that it is only through engaging these nonpsychotic disorders that knowledge necessary for the treatment of the more psychotic disorders will emerge.

> "Before 1900 very few psychiatrists were in private practice for the primary purpose of psychotherapy. Few were able or were permitted by circumstances to carry out intensive, long-term psychotherapy with individual patients suffering from psychoses, severe neuroses, or psychosomatic disorders. Few, therefore, could gather the specialized data which are essential to provide a foundation for theories of perception, learning, memory, and

> motivation to expand the frontiers of psychiatry." (Hanks, 1952, p. 513)

Hanks established *DIMH* as an institution that would pursue this knowledge, and thereby open psychiatry towards its expanding frontiers. He belittled nondynamic psychiatry for providing "innumerable detailed descriptions of human behavior but no explanation of *how* people become mentally ill because of their emotional conflicts" (Hanks, 1952, p. 515). He characterized the traditional laboratory-based research psychiatrists as good-hearted bean counters, collecting endless data on behavior, but lacking the (Freudian) insight that makes behavior (both deviant and normative) intelligible as the consequences of psychological conflict. For Hanks, researching biological processes such as sleep rhythms is not true psychiatry, but simply "a waste of scholarship. It is good energy spent in collecting trivia" (Hanks, 1952, p. 515).

DIMH's research would not be so trivial. Hanks managed to recruit highly regarded Martin Arlen to lead the clinical research division. Arlen was described by one colleague as "as close to a scientific genius as I've ever worked with" (Paull, 1986, p. 171). By the time of his hiring, Arlen had already published over 140 research articles in physiology and biochemistry. Arlen was also a trained analyst with a side practice, and he was interested in developing better scientific understanding of the mind from the psychoanalytic perspective. He described his research project in *DIMH*'s 1951 annual report

> "The studies which have been initiated to date represent segments of a more comprehensive program which is based on the concept that all reactions of living matter are

> directed towards some specific end, i.e., all reactions are 'goal-directed', irrespective of whether such reactions be on the intercellular, cellular, organismal, or inter-organismal levels of organization. …Disturbances of 'goal-directed' reactions at any level of organization may be manifested as some total dysfunction of the organism which is then recognized clinically as a physical or mental disorder." (Arlen, 1951, Appendix II, no page number)

He accomplished a coup against prevailing trends in psychoanalytic/*DIMH* research in this regard, publishing a "classic" psychosomatic study in 1957, where Arlen and his colleagues identified army recruits with disproportionately high and low levels of the gastric chemical pepsinogen in order to see "whether there might not be a correlation between pepsinogen levels and certain psychological features found in patients with duodenal ulcers" (West, 1982, p. 49). The researchers found that on the basis of psychological tests, they could predict with relatively high certainty, which of the trainees would develop duodenal ulcers. In a 1958 article on the same data, Arlen concluded:

> [T]here are three parameters that contribute to the development of duodenal ulcer, a physiological parameter, reflected by high pepsinogen; an accompanying psychological parameter, characterized by the conflict between persistence of intense infantile wishes and adult shame and pride repudiating those wishes; and a social parameter, represented by the environmental event that

> induces psychic tension by mobilizing the psychic conflict." (Referenced in West, 1982 p. 52)

Arlen was only able to do such research because of his analytic training and mindset. To cultivate this kind of understanding in the residents, Hanks built *DIMH*'s residency training around one-on-one psychotherapeutic interaction. Residents spent one year only, their first year, walking the inpatient wards of floridly psychotic patients with the older faculty members, most of them neuro- and general psychiatrists making the daily rounds. Andrew Richter, a resident during this time, notes in retrospect: "We did all these out-of-date tests: reflex time, eye dilation, the Funkenstein—sometimes as often as every day! None of it made a difference, but it was all that they knew to do, so we did it" (Richter, 2008). In this context, the promise that analysis could actually make such disorders intelligible—and perhaps curable—was very heartening, and thus residents looked forward to their second and third years of resident training, which were primarily dedicated to psychotherapy.

Even during the late 50s and early 60s, when *DIMH*'s residency program was one of the most popular in the country, each resident had his or her own private office for individual psychotherapy. That arrangement came at much expense to the institution, since it required building many private offices, so many, in fact, that it worked into lab space. However, according to psychoanalytic thinking, individual psychotherapy sessions with patients *constituted* research, making the resources dedicated to it seem appropriate. Each resident had the patients brought to his or her office by a nurse, who also collected the patient at the end of a 50 minute session.

Psychotherapy sessions at *DIMH* in part followed the principles of formal psychoanalysis: The patient was directed to open up about anything on his or her mind, and to avoid self-censoring if possible. But it was not full analysis, and most patients were only seen once or twice a week. There were also opportunities for residents to experiment in other modalities of therapy, including groups and family therapy (Program Catalog, 1964). However, these other modalities were considered ancillary to the real focus--analysis.

Psychiatric residents also had the opportunity to consult and observe at the affiliated *Balk* clinic. The clinic, independent of *DIMH*, was funded through a grant the University of Dubville had received from a foundation funding programs for "the treatment and care of persons suffering from curable neurotic, mild mental and kindred ailments … without being brought into contact with those suffering from incurable forms of the same trouble" (Craig, 1990, p. 31). This grant, in place before *DIMH* joined the University Medical Center, oriented the University leadership towards psychoanalysis as the "frontier" of psychiatry (Craig, 1990). Trained analysts, who also served as faculty at *DIMH*, ran the clinic, where patients with interesting psychosomatic complaints from around the state received intensive psychoanalytic therapy, a practice resulting in many "miracles" (Melton interview, 2006). Samuel Melton, a resident throughout the late 50s and early 60s, recounts seeing a woman discussing a past trauma, and having her hives simply lift from her face as she opened up. "Moments like that make you a believer," he said (Melton interview, 2006). It comes as little surprise that residents were eager for their second and third year of study.

Hanks believed that the only hope for finding a legitimate cure for such profound disorders as autism and psychosis was through better understanding of their underlying unconscious dimensions. Thus, the research program at *DIMH* was set up to develop an understanding of these dimensions by following the preferred methods of psychoanalytic research: depth psychotherapy (or analysis) with individual clients, carefully documented in process and session notes (See Burston, 2007, pp. 91-92, for a thorough discussion of this form of psychoanalytic research). The process of engaging an individual person in therapy constituted research. The goal was to analyze each patient and to see him or her as a working organism, with the assumption that the various mental organs were Freud's: ego, id, super ego. Through close analysis of the patient's psyche, the clinician-researcher would then compare his patient's mental structure to that found in the scientific record (i.e., case studies and theoretical expositions others had published in psychoanalytic journals) hoping to gain both a better understanding of what is wrong with his particular patient's functioning, and perhaps also adding some insight to the record. If the clinician-researcher found something lacking in the literature, he or she could document the case and make an argument that previous research missed the way a diagnosis (or such) actually functions, and use the case to advance the field. This process of adding cases to help elaborate Freudian concepts was assume to progress psychoanalysis, the way the accumulation of experimental data furthered other sciences.

Obviously, built into this model is the idea that the id, ego, transference, and the like, were consistent and universal psychic phenomena. One should be able to go anywhere in the world and find that a person's functioning is intelligible in these terms, regardless of how normal, or deviant a person's behavior might be. Thus *DIMH* training encouraged students to believe that learning the Freudian model, developed a “solid ground” from which to do psychiatric research, much as budding physicists were developing solid ground when they learned such concepts as force, velocity, inertia, and the like. And just as in physics, finding atomic correlates for such notions as velocity was merely confirmatory of the original notion, so in psychoanalysis finding biological correlates of its central concepts was considered secondary and confirmatory research, but not primary. Primary research came out of the understanding of psychic phenomena in their own terms, in one’s own patients.

By 1959, *DIMH* was producing an average of 100 papers and books a year. Publications consisted primarily of case studies and theoretical expositions, including articles on the philosophy of science and medicine broadly. But they also included a handful of papers of experimental studies such as Martin Arlen’s, rigorous studies of hospital administration issues, and longitudinal studies of clinical prognosis (Schachner, 1984).

There was a general sense that to fulfill its public health mission, *DIMH* had to create professionals who would serve the community in practice. Because the hospital cherry-picked interesting cases from other state hospitals for research and training purposes, they generally did not accept intakes from the local community. That meant the community would only gain the benefits of the hospital through the production of doctors who would serve local residents instead of the hospital. Thus, *DIMH* staff and leadership did not encourage an academic career trajectory for its residents, but instead a private practice one. As one administrator told Samuel Melton as he was trying to decide if he should go into the academy or private practice, “Only give the university what you want." Heeding these words as wise, Melton went on to have an extensive private practice, while still leading an (unpaid) group supervision at *DIMH* for over 40 years. Further, an internal study conducted in 1959 revealed that a majority of *DIMH* graduates not only kept a private practice, but also did extensive work in the public settings. Thus the school felt assured it was not producing a service only for the elite (Chavern, 1959, p. 132).

The 1960s brought the fruition of much of Hanks' psychiatric vision. Most psychoanalysts in the 1950s and 60s were taught and trained in stand-alone institutes. However, Hanks and Albert Roland shared a dream of psychoanalysis being much like any discipline of study. They wanted every analyst to have a background in basic physiological processes (thus requisite medical training), then receive extensive humanities training in such topics as cultural anthropology, literature, and linguistics. This training would provide an education addressing the "total person," a necessity for analysts. For Hanks and Roland (and many of the psychoanalysts of this era) psychoanalysis' special purview was at this meeting point of the life sciences and the humanities. Each graduate was expected to write a publishable paper in order to graduate, as well as pass the rigorous training analysis which confirmed they were in control of their own equilibrium enough to effectively treat the disequilibria of others.

There was a strong push for the residents to pursue analytic training after finishing their residency, but when Hanks first began, Dubville did not have an analytic institute. He set up a program where local Dubville analysts (there were a few of these during the 1950s, all of whom had trained in other cities) would provide the 5 times a week training analysis for residents, while every weekend the residents would travel eight hours to the institute on the eastern part of the state (Eastern Dubstate Psychoanalytic Institute (*EDPI*)) for intensive classroom training beginning at 8 a.m. and continuing until 6 or 7 at night. . Thus, training to become an analyst required a substantial dedication of time and energy, particularly given the regular demands of residency. The training was also expensive—a typical cost of each session being around $15 dollars ($130 in contemporary dollar values).

Hanks recruited Albert Roland from *EDPI* to become *DIMH*'s Chief of Psychoanalytic Services and to build Dubville's Psychoanalytic Institute. In that same year (1956), Hanks convinced Dubstate's legislature to enact the *DIMH* Commonwealth Fellowship Program (Schachner, 1984, p. 164). It defrayed the expense of analytic training for psychiatrists, including the cost of the training analysis (the State covered $10 of the $15). It also defrayed part or all of the annual tuition to *EDPI* (Program catalog, 1964). Hank's promise to the legislature was that this was not just the defrayment of an individual's medical school bill, but an investment in ensuring competent practitioners for the community.

In May 1961, the Dubville Psychoanalytic Institute (*DPI*) was granted provisional status by the American Psychoanalytic Association ("The American"). It graduated its first two analysts the following year, and received full accreditation in 1964. Possessing six training analysts, six lecturers, and starting with forty-five candidates (the majority of the residents in training), the institute stood out in many ways. Compared to most analytic institutes, it was put together quickly. More notably, it was one of the few analytic institutes officially affiliated with a higher education institution.

In 1962, *DIMH* opened its psychoanalytic clinic to the public, offering psychoanalytic services directly to the community and finally addressing the service/research gap which continued to haunt *DIMH*. The clinic had three goals: (1) Assisting candidates in their search for suitable cases for their supervised clinical work; (2) Offering reduced fee service to the community; (3) Finding suitable cases for the special interests and research programs of the local psychoanalysts (Schachner, 1984, p. 202). With its affiliation to the *Balk clinic*, and now the psychoanalytic institute, *DIMH* was emerging as one of the country's major psychoanalytic centers.

Community Psychiatry

The establishment of *DIMH*'s community and social psychiatry curriculum was the next accomplishment of the 1960s. Hanks had always been a proponent of community and social psychiatry, since it squared well with his conception of the total person. Social stresses can adversely impact mental well being, and understanding how these stresses operate, and how best to intervene, seemed only logical to Hanks. Hanks shared this belief with National Institute of Mental Health director, Robert Fisk, and contemporary community mental health luminary, Gerald Caplan, a British scholar who ran the Community Mental Health Program at Harvard University (Caplan, 1961). Caplan argued that psychiatry was too focused on what he called "tertiary" care, or working with people once they are already psychologically damaged (e.g., analysis and psychotherapy are tertiary care.) Caplan advocated for "primary prevention … the process involved in reducing the risk that people in the community will fall ill with mental disorders" (Caplan, 1961, p. vii). This model for conceptualizing community psychiatry was popular at NIMH, and prompted the increased federal support for community mental health during the 60s.

Hanks appointed William Jax, one of “Gerald Caplan’s lieutenants” (Johnson interview, 2006), to direct State Hospital and Community Services in 1958 (Schachner, 1984, p. 181). Jax is one of those people that everyone falls over themselves to say positive things about. He is characterized as funny, sweet, and “one of the most caring physicians I have ever met” (Johnson interview, 2006). Jax was community-focused. He agreed with the basic premises of the Freudian model of mind, and with psychiatry’s role in increasing adjustment, but felt that individual analysis was elitist and esoteric. To him, community mental health provided access to psychiatry for those who could not afford the expenditure of time and effort of full analysis (Schachner, 1984, p. 263). He wanted to bring the hospital’s resources to the community.

In Caplan’s community psychiatry model, psychiatrists typically do not provide direct service (this being considered an inefficient use of their time,) but instead act as consultants to community groups and key community leaders, who were assumed to be the front-line clinicians in this assault on the “primary” causes of mental disorder. Individual psychotherapy and analysis would be reserved for the most damaged individuals, but communities would receive “emotional inoculation” (a concept Caplan borrowed from psychiatrist Irving Janis), if key community leaders (church leaders, politicians, leaders of community centers, etc.) had basic training in psychodynamics, and intervened to help distressed families, new mothers, and otherwise provide family and community support. Towards this end, starting in the mid-60s, Jax assigned third-year residents to work as consultants to various social agencies around Dubville. He also developed a psychiatric emergency on-call roster, through which third-year residents made themselves available for psychiatric emergencies in the community (Schachner, 1984, p. 198).

In 1964, Jax opened the Community Study Center. Originally housed in an old church in a medium-sized, economically declining neighborhood in Dubville called "Sisterton," it was designed to study "a community in depth over time with selected action research and various multi-disciplinary approaches to factors in the environment as they relate to mental health and illness" (Schachner, 1984, p. 223). This attention to "depth" and "time" demonstrates the underlying affinities between community mental health and psychoanalysis. Moving from the micro-cosmos ("psychic reality") to the macro-cosmos ("the community") did not mean relinquishing attention to history and dynamics, but merely extended the scope of the analysis. Thus, while analysts were producing case studies in which they discussed different ways to access the unconscious to liberate the patient, the CSC produced research on how to engage a community, on which factors in a community were blocking adaptation and progress, and on how to overcome those barriers and enhance communal well-being.

An example of this 'social-turn' appears in the above mentioned address that Albert Roland gave on the total person. In this address, Roland shares a case about a household appliances repairman with a chronic headache. Initially, medication alleviates his pain, but after a while it loses efficacy. The doctor invites the patient in and learns that the man is extremely docile and pleasant, but has a fairly difficult marriage to a demanding wife, and that he is increasingly frustrated with his occupation. Might some of these factors be behind the headache? The doctor, knowledgeable in psychoanalytic basics, helps the patient see his repressed anger. The consequence: the repairman "quit his job, left his wife, and began to drink cold beer" (Roland, 1958, p.17).

Roland uses this case as an example of the limitations of psychological interventions. Changing internal impulses does not necessarily result in a "healthy" balance—the goal should be to find the right piece of a whole ecology that is not only out of balance, but whose change will result in greater health, not just the elimination of symptoms. In light of this story, Roland offers a conceptualization of health at odds with prevailing medical notions:

> "For the sake of discussion, I would suggest that a person is to be considered ill when his response to a life situation involves, by reason either of intensity or chronicity, a threat or a block to his potentials for choosing and getting the gratifications available in his culture. The response in question may be organismic, ego-wise, or societal (internalized or external); generally all three will interact. … Usually the whole picture will be much more complex than we might wish, but also much more interesting." (Roland, 1958, p. 18)

Such a "social" or "structural" understanding of health undergirded the residency training at *DIMH* throughout the 60s. It led the department and the analytic institute to see its allies in the fields of sociology, anthropology, linguistics, social work and the like, rather than in the cardiologists and oncologists who had not yet made a structural transition in their approach to their topic.

Though run by a psychiatrist, the CSC was an interdisciplinary affair, involving a psychiatric nurse, a sociologist and an anthropologist. The team reportedly got along famously, doing research on the community's health profiles and disease prevalence (Johnson interview, 2006; Edmunds interview, 2008; Trafford interview, 2006). They conducted qualitative studies, such as reviews of patients' experiences in aftercare, the impact of cultural disadvantage, and the attitudes and behaviors of varied groups of people (Schachner, 1984, p. 258). The interdisciplinary nature of the CSC highlights psychiatry's increasing affinity with disciplines not traditionally a part of the health system. Meanwhile, as I shall discuss shortly, the center did not, in fact, grant many direct mental health services to the community, instead primarily seeing its roles as consultation and research

Tensions

Abbott argues that, "Jurisdictional boundaries are perpetually in dispute, both in local practice and in national claims. It is the history of jurisdictional disputes that is the real, the determining history of the professions" (Abbott, 1984, p. 2). Since we already know that psychoanalysis has been replaced as the leading paradigm in psychiatry, my goal is to describe how psychoanalysis lost its hold over psychiatry. As noted previously, psychoanalysis had provided the potent abstractions psychiatry needed to justify its control over the mental health field. Because psychoanalysis granted insight into the disordered psyche, and techniques for healing it, it dominated other mental health professions and extended the range of psychiatry's jurisdiction well beyond the confines of the asylum into the community as a whole. However, already in the 1960s psychoanalysis' ability to justify psychiatry's continued control over *DIMH* was being challenged. I now turn to the locations where such challenges occurred.

As a public institution, funded by public money, and mandated to serve the good, *DIMH* had to convince the public that it actually provided services that justified its continued financing. That justification could not be sustained effectively under Hanks' stewardship. In the final analysis, neither the state hospital system, nor the local public, nor educational and mental health providers, nor the broader scientific community, nor the mental health advocacy community in Dubville or in the state more broadly felt that *DIMH* was a beneficial institution. When almost all community stakeholders see an organization as incorrigibly self-serving, power transitions are inevitable.

Abbott uses the term "audience" to demarcate the groups to which a profession appeals for the rights of jurisdiction (Abbott, 1984, p. 64). From the outset, *DIMH* was in a difficult position, suspended between two audiences with conflicting demands: the state hospital system which desired services for the increasing numbers of mentally ill in the state hospital system, and the local community which desired mental health services. Pleasing both of these groups simultaneously seemed impossible to Hanks and the original *DIMH* leadership. This early *DIMH* leadership believed that since they were mandated to work with state hospital patients, treating the local population would be a misappropriation of resources. Further, the real gains in knowledge were to be made with the state system's more 'difficult' cases, not with the more mundane local cases.

However, despite Hanks' and Roland's efforts, *DIMH* never developed a productive research machine. *DIMH* had two major audiences for its research: the scientific and medical community, and the state, which wanted *DIMH* research to reduce its state hospital population. *DIMH*'s research output was too disorganized and heterogeneous to make an impact on either of these audiences. Why?

As psychoanalytic research intertwined with the social sciences and anthropology, it moved further from the scientific and medical communities. And it was these communities that buttressed and supported medical research. Even Martin Arlen, whose research was surely the most significant in regard to basic science, had not made an impact on the issues of mental healing. Though his duodenal ulcers research remains pertinent, it produced little follow-up research by either Arlen himself, or any other researchers in the field (West, 1982). While the research itself was not necessarily a dead-end, no other scientist followed the path he seemed to have blazed. Basic science that impacts neither clinicians nor other researchers has a short life in a world of limited scientific and medical resources.

At a fundamental level, *DIMH*'s leadership simply believed that such research was secondary to analytic insight. Analytic insight offered an understanding of each person's individual depth and situation, which had to be addressed in its specificity. Large scale research projects, which looked at norms across large groups of people, would "overlook" (Hanks, 1952, p. 518) what is fundamental about a person's psyche, and thus Hanks considered it sloppy research. Hanks also held a general hostility to brain focused psychiatry. This was not because he felt that the brain was not a part of the mental life. Of this, he was very much convinced. However, he felt most of the research produced on the relationship between brain and behavior required arbitrary reference to "some unknown organic process the presence of which could not be demonstrated" (Hanks, 1952, p. 516). In other words, without attending to depth one gets stuck looking for biological phenomena that may not even exist. Dynamics should be attended to on their own, and trying to by-pass them by going directly to the nervous system, would result in grave scientific errors.

Roland (1958), expressing a sentiment held by many in the analytic leadership, added that ignoring depth is scientifically wrong headed, undersells the real goals of healing, and points to a particular cowardice on the part of the clinician:

> [W]e must, as physicians, come, as I said before, to a decision. We must decide whether we are really interested in the dynamics of total behavior, or, on the other hand, in the tradition of medicine (combating symptoms), we would prefer some other way of understanding illness and of obviating or obtunding the [symptoms.] Now any physician in our times is a busy man, *so*, given (1) his desire to prolong life, and (2) his tendency to apply that which is effective and (3) his traditional tendency to remove symptoms—given these, any of us would prefer to prescribe Acid Acetylsol gr v q 2h p r n, and be satisfied. But, I must ask, what has this to do with the dynamics of total human behavior? Is it the patient's total behavior we are hoping not to have to bother with? Or our own?
>
> Frankly, I suspect its both. (p. 13)

Roland here expresses antipathy to the idea of "symptom reduction," arguing that such a focus distracts from the real work of psychic healing. However, other branches of medicine viewed symptom reduction as an important goal, as did the state. *DPI* was out of step with prevailing scientific and bureaucratic attitudes.

The research produced at *DIMH*, following the individual case-study method identified earlier, was neither organized nor impactful. In a typical example, a resident would become interested in a phenomenon exhibited by a client, would read how other analysts explained the phenomenon, and then would continue the dialogue in a paper adding his insights. When residents did look at long-term trends in a particular population, the research yielded few publications, and did not spur additional follow-up studies. One resident became interested in the way a patient's clothing choice demonstrated something about his psychic state on a given day, and wrote 3 articles on the topic. A faculty member, Harold Johnson, became interested in psychoanalytic consultation, and published several articles on the topic. There was little value in such research to any of the those institutions funding or legitimating *DIMH*. Moving as they were towards a humanities model, they were hoping for sustenance the way a humanities department receive it—through a noblesse oblige of the power structure. But medicine does not work that way.

Thus one of the ironies of Hanks' insistence on psychoanalytic research as the route to improving psychiatry was that he did not monitor the development of faculty research. The following anecdote highlights that lack of oversight.

Hanks and Arlen became embroiled in a spat that resulted in Arlen completely separating from the general life at *DIMH*. Arlen's labs were on *DIMH*'s top floor, and he obtained a key for himself and his researchers to that floor, of which Hanks did not even have a copy (Paull, 1986). Arlen and his team stopped sharing information on their work with the department (annual reports throughout the late 1950s stopped publishing research updates), and his team even stopped attending the Christmas party (Johnson interview, 2006).

DIMH had an unfortunate track record of research and treatment projects that simply went nowhere. "I really spent a lot of my time talking to psychotics and hoping they'll get better, and you know, it really never worked," says Andrew Richter (Richter interview, 2006). The inference chain through which the successes that the analytic method achieved in some neurotic disorders would pay off, in due course, simply did not bear fruit. Hanks also devoted money and time to a large-scale research project on Gregory Bateson's 'double-bind theory,' which proposes that a child becomes autistic or psychotic due to certain forms of interaction with his primary caregiver (Bateson, 1972). Significant sums were spent over a period of many years. Though the literature on this subject grew quite voluminous by the end of the Sixties, I was not able to track down a single published paper on the research from *DIMH*.

These projects were funded at the expense of others. Richter tells the story about the end of his first year of residency when Hanks invited him into his office to talk about Richter's future. Richter was developing an interest in psychopharmacology ("When I was a resident, chlorpromazine was still a numbered drug!" he laughs (Richter interview, 2006)), and was keeping a note card collection of all the psychoactive substances known at the time. His goal was to follow the impact of these medications on individual patients through time. Amidst his explanation, Hanks "came around from his desk, lifted me by my chest, and said 'come with me.' He took me to the library, showed me the collected works of Freud, and said 'This is all you need to know.' He then turned on his heels and left" (Richter interview, 2006). This interaction led Richter to turn away from his interest in the study of psychopharmacology.

DIMH's inability to adjust to the rise of psychopharmacology was one of its largest organizational (and jurisdictional) failures. As Abbott states, "Changes in technologies and organizations provide most new professional tasks. Correlatively, the two are central destroyers of professional work" (Abbott, 1988, p. 92). As chlorpromazine emerged as a miracle cure, *DIMH* remained convinced it was just a passing fad. The staff was not completely opposed to medication. An internal study done in 1958 revealed the use of "Tranquilizers" (which included both benzophenidates like Miltown and neuroleptics such as chlorpromazine) on 25% of the hospital patients, and openly admitted to the fact that medication use had significantly reduced the use of ECT and coma therapy (Homer, 1958, pp. 115-116). However, a completely chemical approach to psychopathology flew in the face of prevailing preconceptions regarding the "total person." As stated in the report: "There seems little doubt that tranquilizers alone may not always satisfy or meet the total needs of a given patient" (Homer, 1958, p. 116).

Instead, *DIMH* doctors were encouraged to think of medications as one tool in their engagement of the total person. *DIMH*'s psychopharmacology "expert" (Richter, 2006), Christopher Homer, stated *DIMH*'s ideology succinctly:

> There are many tranquilizers available to the medical profession. Few investigators or clinicians can have experience with all. However, all practitioners can become expert with a few and learn to use a few with discretion in

> selected cases to physiological effect. (Homer, 1958, p. 117)

This conceptualization of medications is at variance with the medical tradition, following from Paracelsus through Paul Ehrlich, which sees medications as treating distinct pathological processes inside the body, righting them, and thus bringing health. However, *DIMH*'s "total person" did not have such simple or specific problems. The "total person" is suspended among her unique physiology, history, and social context. Health is achieved through a right balance in this system. Certainly some physiological balance may be in order, but no "healing" occurs so simply in the life of the "total person." *DIMH* doctors were to gain familiarity with some physiological balancing concoctions, but not to become tempted by their promise of easy cures, which posed the danger of covering up exactly what must be understood and healed: "We have found the tranquilizers as a group to be valuable adjuncts to psychotherapy either in hospitalized cases or out-patients. However, anxiety must be understood and not merely suppressed" (Homer, 1958, p. 117). Thus, at *DIMH*, Thorazine was not a miracle cure for psychosis, but one form of intervention helpful for achieving balance in the total person.

This position towards psychopharmacology impacted *DIMH*'s relationship with the state and the rest of the University of Dubville Medical Center (*UDMC*). As far as the state was concerned, psychopharmacological research was exactly what *DIMH should* be producing. Across the nation chlorpromazine's alleged effectiveness was being used as a justification for reducing state hospital populations, yet here the state's own research hospital actively discouraged an attitude towards medication that was consistent with the emerging trend.

In regard to the University of Dubville Medical Center (*UDMC*), however, the tension stemmed from disagreement about the very nature of medicine and healing. The relationship between *DIMH* and the rest of *UDMC* had been strained for some time. *DIMH*'s funding continued to come directly from the state, instead of from the University, and thus Hanks could make decisions independent of the University structure. If a doctor needed a new desk, for instance, that desk came from the state and not the medical school. This made it possible for Hanks to build his "empire" as he wanted. Hiring, firing, and wage decisions were made as independently of the university as possible (Richter interview, 2006). The climax of this independence came with the establishment of the Psychoanalytic Institute, which answered to the American Psychoanalytic Association more than to the medical school.

Relationship to the State

DIMH's institutional independence was mirrored in the epistemological independence the institution cultivated. Albert Roland had publicly taken traditional medicine to task for being cowardly in the face of the total person:

> It is as if the physician has done extremely well, but that the time is arriving for him to take another courageous look and see whether he has done enough. His main job is to see that life is not shortened—and in this his strides have been magnificent on the basis of the organismic approach. His second job is to alleviate discomfort—and in this too, he has done, within limits of physical (organismic) illness,

> very well; but he has not as yet been sufficiently willing to see that he must also be willing to cope with mastering an awareness of the sources of illness as they may stem also from his patient's psychological and societal aspects since these too affect the organism, and can be matters of life and death. He must learn to conceive of illness or disease not as somehow an attack from without, but as a maladjustment of the individual patient in all three of his aspects, in which he (the patient) is involved as a totality. (Roland, 1958, p.18)

These words did not move the rest of the medical profession to start thinking about their task in a new way. Instead, the relationship between *DIMH* and the rest of the medical center halted. The psychiatrists at *DIMH* increasingly saw that they were not being invited for case consultations by others in the hospital (Richter interview, 2006). What the traditional doctors felt they were there to do, and what the psychiatrists were trying to do, were on different plateaus. Furthermore, *DIMH* lacked impressive outcomes to demonstrate the merits of its approach. In fact, *UDMC*, the state, and the local community were increasingly doubtful that *any* legitimate healing was occurring at *DIMH*. An important clinical "failure" was seen in *DIMH*'s Children's Residential Unit. In *DIMH*'s original charter, the only treatment *DIMH* was expected to provide directly was in children's psychiatric services. For this reason, the department had always put an emphasis on children's services, bringing in national leaders in child psychiatry, all of whom were analytically informed. The cost was exorbitant, amounting to $10,000 per child per year, and children were staying at *DIMH* for an average of 22 months (Schachner, 1984, p. 211). The CRU was referred to as a "research floor" with the implication that the expenditure of time, effort and money, would result in tangible results. However, no such research had, as of yet, been produced. But the psychoanalytic approach did not feel this represented a failure, as Dr. Martin Thompson, the chief of the unit, explained: "'It is doubtful that any other inpatient treatment unit has done markedly better.' There were children who started talking who had not done so before, and several boys now attending special classes in the regular public school. Though these may seem to be small marks of achievement to some, they were not to this staff" (Schachner, 1984, p. 211). Still, *UDMC* and the state were not convinced by what they considered meager results.

At the same time, regular annual reports were showing that *DIMH* was leaving many of its beds unoccupied—a public relations issue that left locals feeling that the hospital did not prioritize care. Dorian Hasselback, an administrator at the hospital, recognized the problem and articulated the position of the hospital: "We must not let expanding service demands so consume our energies that we destroy or adversely affect our teaching and research programs, but neither can we so protect ourselves from the needs of the community that we are looked upon as the greedy, rich uncle with many times his needs stored in the basement and a nonsharing attitude" (Schachner, 1984, pp. 185-186). The community did perceive *DIMH* as the "greedy uncle"; the research and teaching was not compelling enough to counterbalance the perception. *DIMH* experienced a significant drop in support from many institutions essential for its survival.

When *DIMH* opened, 90% of its funding was covered by the state. By 1959 only 70% was covered by the state, and by 1961 *DIMH* met its first shortfall: $174, 212 (Schachner, 1984, p. 188). Hanks cut the budget in many ways to address this problem, yet he refused to curtail the analytic program in anyway. In fact, the program was growing and flourishing. Clinical supervision for residents increased to at least twice a week, and a psychotherapy course and outpatient experience was added for first-year fellows (Schachner, 1984, p. 198).

An internal study of time distribution best encapsulates the values of *DIMH* at this time. Forty-five percent of psychiatrists' time "was spent in the psychotherapy of children, family counseling, psychological test and diagnostic evaluations of psychiatric problems in children; twenty percent in administrative duties, fifteen percent in the teaching and training of residents and professional groups; fifteen percent in participation in community planning and consultation to various agencies and services; and five percent devoted to research" (Schachner, 1984, pp. 216-217).

Relationship to the Local Community

Finding the right balance between service and research was most clear in *DIMH*'s community component. As noted, the community psychiatry ideology of the day supported the notion that psychiatrists are most helpful as consultants to community providers. Gerald Caplan explains this in his book, *An Approach to Community Mental Health*

> The goal here is to reduce preventable stress, or to provide services to assist people facing stress to healthier problem-

> solving, by means of governmental or other administrative actions. … The role of the mental health specialist in this type of work is to act as the consultant and adviser to administrative and governmental bodies. He seeks to introduce a point of view … that is dependent upon his own specialized knowledge of interpersonal forces and, in particular, upon his knowledge of psychological needs of individuals and groups. His goal is that the emerging plan or regulation will take account of the mental health needs of the total community, and that at least it will not add to the mental health burdens. (Caplan, 1961, p. 3)

William Jax set up the Community Study Center (*CSC*) with this model in mind, though the *CSC* also had a research component which was designed to track the impact of various forces on the community's mental health. The *CSC* was originally opened in a small, economically-declining neighborhood in Dubville called Sisterton. Sisterton's Community Action Program (*SCAP*) originally invited the *CSC* to come to Sisterton, thinking that it could only benefit the local community. However, the *SCAP* quickly found that the *CSC* "would not assume responsibility for the so-called 'difficult' cases," (Ellison et al., 1971, p. 313), creating friction between the group and the *CSC*. Tensions grew worse when one of the sociologists at the *CSC* explained to *SCAP* leadership that the *CSC* would be happy to consult and provide helpful research results to the community. However, "From a research point of view, it is not our primary concern whether the community improves or continues to decline" (Ellison et al., 1971, p. 314).

Even this most community-embedded portion of *DIMH* appeared out of touch with community needs. Jax quickly apologized for the comment, and reinforced that the *CSC* did want to help the community improve its health.

In fact, the *CSC* staff worked tirelessly to produce research which linked structural changes in Sisterton to mental health concerns. However, this research ruffled feathers in different ways. For instance, research that made comments about the "poverty level" of the community upset the community's more affluent members. Likewise, research on black-white relations disturbed some community members (Ellison et al, 1971, p. 314-315). This research, though probably accurate, talked about the community unconscious, and ignited the same kind of defensive reactions that an individual might produce to a troubling, though perhaps appropriate, intervention.

The federal government provided a boost in direct community care in the late 60s, with an influx of community mental health center funds. *DIMH* became one of the primary base service units (BSUs) in Dubville, and in September of 1967, *DIMH* opened its first Community Mental Health Center, which established *DIMH* as a large scale local service provider for the first time (Schachner, 1984, p. 216). It included outpatient facilities, daycare, after-care, emergency services, and had consultation services to serve the population of various Dubville neighborhoods. A Children's Community Team and an intake Crisis Clinic were also put together, which allowed treatment to come to the needy public, instead of centralizing all care in the hospital. This service relied little on psychoanalytic thinking or interventions, and was mostly oriented towards helping with mental health crises (e.g. psychotic breaks, suicidality, and so on). It was utilized and appreciated by the community, and in fact would remain the most viable part of *DIMH*, as much of its psychoanalytic edifice began to collapse.

Analytic Regression

The situation was clear: *DIMH* was losing funding because it was not fulfilling its social task of reducing the clinical load in the state hospitals, or satisfying local demands for services, nor was it linking well to the scientific and medical apparatus. Robert Hanks could have initiated changes to address these problems, but did not. We could speculate on why Hanks, as an individual, did not react to the writing on the wall, but for our study I think it is best to treat Hanks as a professional, making professional decisions, and avoid speculation as to what he may or may not have been thinking on a more personal level.

From the perspective of the profession, then, Hanks' actions can be made intelligible through reference to Abbott's notion of "professional regression" (Abbott, 1984, p. 118). Since professions are attached to the knowledge base which gives them jurisdiction over their social task, they protect that knowledge most. Thus, Hanks protected psychoanalysis. He honestly felt that some great analytic discovery was just around the bend, and that further commitment to analysis would solve the problems that *DIMH* was chartered to solve. Addressing the problems of reducing hospital loads directly, without accounting for the total person, would be akin to addressing symptoms without addressing their underlying emotional conflicts: it would simply cover over the problem, perhaps deferring the need to address it to later, but would not "cure" anything (Hanks, 1952). The implication of this move on the level of professional politics, is that he was betting on the power and prestige of the "analytic audience" to protect his efforts while the state and broader medical community rejected him. Instead, he found the analytic audience also rejected his efforts at *DIMH*.

For the analysts in *DIMH*, psychoanalysis was psychiatry's basic science. Though other approaches might also be helpful (learning theory, biological theories, family theories) they all had to be explainable through analytic concepts to be considered valid. Thus, Hanks continued to appoint only analytically informed people to leadership positions. Throughout the 60s, *DIMH* decreased nonmedical psychotherapy in the various capacities where it had existed (Schachner, 1988, p. 211). Increasingly, social workers were moved to consultation, and nurses were doing more of the therapeutic work. They would not do analysis proper, but would utilize milieu therapy and early forms of family therapy, two approaches rooted in analytic logic, and which the analysts could supervise.

Another important location where an analytically trained psychiatrist was put in charge was the Community Study Center (*CSC*). Noted previously, Hanks and Roland followed the model of Gerald Caplan, in which total behavior extended outside of the person, to the system in which a person was situated. "Hanks wanted analysis to be bigger than drives and impulses, he wanted to look out to the family, community, groups, society," noted Dr. Harold Johnson (Johnson interview, 2006). In the mid-1960s, Hanks recruited Johnson to help run the *CSC*, because of Johnson's residency experience with both community psychiatry and analysis. Johnson worked closely with William Jax, publishing papers on community mental health, and providing consultation to the local community on various psychiatric issues. At the same time, Johnson underwent training analysis at Dubville Psychoanalytic Institute. These two influences inspired Johnson to begin to conceptualize much of his community work from an analytic perspective. He recalls a group of mothers who came in complaining that the younger children were being picked on by older children at the local playground, and they wanted to build a fence between the play areas to protect the younger children. Johnson and Jax were both present for this consultation, and Jax responded: “What needs might the older kids have?” (Johnson interview, 2006). Johnson was struck by how this simple question altered the nature of the discussion, opening up attention which had been solely focused on the younger children as “victims.”

Consultation of this nature began to make Johnson think about the similarities between community consultation and analysis. In the mid-60s, Johnson and a colleague composed a paper on the topic for publication, and submitted it to a major psychoanalytic journal only to have the paper rejected. When Johnson followed up to learn why the paper was rejected, he was informed by an editorial board member that community work was not considered “real” analysis, and was actually seen as “resistance.”

“We were told the paper was fine and good, but that the journal did not want to put their stamp of approval on something that was not real analysis—that was actually a misuse of it” (Johnson interview, 2006).

Johnson responded by turning around and submitting the article to *The Archives of General Psychiatry* (an AMA journal), where it was accepted for publication. Johnson continued to reflect on this experience throughout his career, and eventually has come to accept the rejection of his paper from the analytic journal as appropriate. I discuss this later in this dissertation, but for now it is important to note the emergence of a difficult tension within analysis itself. After all, Johnson eventually received tenure at *DIMH*, not in small part due to the publication of this paper (Johnson interview, 2006). His leadership position in a major medical center could only serve to advance analytic interests, and the same could be said of his attempt to expand the use of analytic logic to other fields such as community psychiatry. Hanks put Johnson in the *CSC* leadership position because of his analytic insight, and because he felt this was a good use of analytic knowledge. The analytic hierarchy outside of *DIMH* disagreed, and adhering to rigid definitions of analysis, the analytic hierarchy made a decision that could have stunted Johnson's career irreversibly.

Such "undercutting" by the analytic hierarchy is one of the largest difficulties that Hanks faced as he tried to ensconce analysis into *DIMH*. Not only was he failing to please the professional audience of the state and community, but orthodox Freudians were not allowing him to expand analysis in ways that could help him please those audiences. The "protection" of analysis from "dilution" and "perversion" are central themes in its history (Kirsner, 2000). From Freud himself shunning Adler and starting the IPA, to the endless legal battles over nonmedical analysts (Wallerstein, 1998), those who are most "orthodox" disparage or dispute the purity, legitimacy, and loyalty of others. This culture of sectarian infighting complicates efforts to build a bureaucratic department of psychiatry based on psychoanalysis.

Andrew Richter also met opposition from the analytic hierarchy as he attempted to advance his career at *DIMH*. This time, however, the limits to his advances came from within *DIMH*, and more specifically from within the Dubville Psychoanalytic Institute

(*DPI*). To understand this story, one must understand the importance of the “training analyst” in analytic circles. As Douglas Kirsner explains:

> The advantages of training analyst status are politically, economically, professionally and psychologically considerable. These include power, prestige, involvement in the institute, increased professional reputation, access to analyzing candidates and more referrals. Training analyst status can be the mark of professional success in psychoanalysis, of being a ‘genuine analyst’, of no longer being excluded from the analytic parental bedroom. The status of training analysts is not simply that they are in a position to exercise political power. Training analysts are often seen to be the ‘real’ or genuine analysts. They are the analysts of the analysts-in-training and their status is redolent with fantasy and myth. (Kirsner, 2000, p. 104)

Richter applied for the position at age 38, after having reached all of the requisite clinical hours and supervision requirements. However, at that time, the average age of a training analyst at *DPI* was 48. His application was viewed as radical—it was felt he was not yet experienced enough for the position, regardless of the fact that he had met the objective requirements. Referred to as the “young man in a hurry”—a common diminutive in analytic circles (Wallerstein, 1998, p. 90)—his ambition was read as the result of some internal conflict which he had not yet overcome, and which could lead to blind spots in his doing of analysis. In fact, just applying to become a training analyst was seen as a dangerous precocity. "You're supposed to act like you don't want it," said Richter (Richter interview, 2006). Wanting it is a sure sign you're not ready for it.

Richter ignored these cultural mores, and continued to pursue the position based on his adherence to the official requirements. He presented his final case for acceptance, and the presentation went well. The training analysts conferred on his presentation, and came out with a qualified acceptance: Richter could become a training analyst, but from now on new training analysts would have their training analysis work supervised. The new program director, Matthew James, was appointed Richter's supervisor.

Richter was incensed, but accepted the decision: "I had no choice" (Richter interview, 2006). However, with time it seemed that new conditions were added. The training analysts began to hold meetings without inviting Richter, further highlighting the fact that they had no intention of granting him all the rights and privileges associated with his new position. Richter continued to buck this provisional status, but secret meetings without his attendance continued.

This tale highlights another difficulty of analytic leadership: the changing definitions of what constituted competence in analysis. Richter had the knowledge base expected of a training analyst, but lacked something else, something not measured objectively, which made the training analysts nervous. They thus slowed his professional progress arbitrarily. I show later how Richter's drive, or need to "be in a hurry," could have been a saving power for analysis under the leadership which replaced Hanks in the mid-1970s. In many ways Richter's personality was well-suited to the changing paradigms; in a position of leadership, he could have helped achieved a *rapprochement* with what came next. Nonetheless, the orthodox analysts did not appreciate this possibility, and, quite to the contrary, saw Richter's energy as dangerous.

Finally, this story illustrates the manner in which criteria of competence and credentialing were changed in an *ad hoc* fashion by the analytic hierarchy. Such disregard for its own collective criteria frustrated participants outside of scientific leadership, to whom the decision-making body felt more like a cult, protecting its inner circle.

Analytic Maturity

Abbott notes that at the turn of the 20th Century, the medical professional argued for his or her "professional legitimacy" through a "reliance on social origins and character values" (Abbott, 1988, p. 195). The doctor was to be trusted because of his well-toned character. He interacted with the client through knowledge, experience, and intuition. The doctor's class position was an important part of why one trusted his diagnosis and advice, and why one went to him for help. However, the 20th Century saw a widespread shift regarding how a medical professional grounded his (and increasingly her) legitimacy. With the passage of time the medical professional's status came not just from the right background and rearing, but from his or her access to (and control over) medical knowledge and technology, both of which promised rationality and efficiency in the provision of medical service (Abbott, 1988, p. 195). A key moment in this transition was the Flexner Report (Flexner, 1910), which admonished American medicine for not adhering to the strict protocols of mainstream science. The Flexner report resulted in a mass transition in medical education and training. Increasingly, doctors were expected to be trained in the most up-to-date science, and to implement their work as scientists. A doctor was not just someone with experience in medicine and venerable social credentials, but a person who had more scientific knowledge about the processes of the body than the lay person, and thus was fit to make interventions into these processes for health.

As fate would have it, 1910 is also the date that Freud published his famous paper on “wild analysis” (Freud, 1910/2002) in which Freud shares a short scenario in which a doctor with an untrained understanding of psychoanalysis makes an ill-fitting psychological interpretation to a patient. Luckily, this client responded to the “wild analyst’s” intervention by going to see Freud, and thus Freud felt he was able to stop this process from getting out of control. However, Freud opines in the piece about the dangers of rogue physicians using ill-informed interventions based on their incomplete understanding of psychoanalysis. He points out that

> It is not enough, therefore, for a physician to know a few of the findings of psycho-analysis; he must also have familiarized himself with its technique if he wishes his medical procedure to be guided by a psycho-analytic point of view. This technique cannot yet be learnt from books, and it certainly cannot be discovered independently without great sacrifices of time, labour and success. Like other medical techniques, it is to be learnt from those who are already proficient in it” (Freud, 1910/2002, p. 226).

Freud expounded on this position in his book, *On Lay Analysis*, in which he argues further that “the only guarantee of the harmless application of the analytic procedure must depend on the personality of the analyst” (Freud, 1913, quoted by Wallerstein, 1998, p. 8).

While Freud tries to argue that psychoanalytic training resembles training in “other medical techniques,” his lingering emphasis on the cultivation of a certain personality was specific to psychoanalysis. Certainly surgery wants sober minds, and for this reason a typical surgery residency is around 10 years, in the hopes of ensuring that a mature clinician is released upon the world. However, the apprenticeship of a training analysis is not just a cultivation in skills, but is also a cultivation of self. The psychoanalytic leadership believed that this work required an appropriate personality, and that individuals with this personality should be running things in psychiatry. Those who did not have the personality type, independent of their knowledge of Freud, were nonetheless not fit to practice. Because an objective measure of this personality was hard to reach without literally judging other peoples’ “self,” this facet of psychoanalysis resulted in a suspicion-filled—and *political*—environment (See Kirsner ,2000, for a discussion of the dramas that riddled several of America’s most prestigious analytic communities.)

It was a situation that posed problems at *DIMH*. In 1959, *DIMH* held a conference at which some of the most important names in psychiatric research, science, and policy development spoke. One of the speakers was Lawrence Kubie, a luminary in the field of psychiatry and psychoanalysis. His remarks, entitled “Research in

Psychiatry: Problems in Training, Experience, and Strategy", begins with a clear statement:

> It will be my essential thesis that training for research should mean more than an education even in many fields of scientific data, theory and technique; and that it should include at least the opportunity to resolve the latent emotional problems of the student. It is this part of the training of the future investigator which has been most neglected; yet it is of critical importance for all scientific research, and especially in psychiatry. (Kubie, 1961, p. 213)

Kubie continues to lay out his vision of the emotional training a psychiatric researcher needs to undergo to do the work,

> [L]et me state right here my wholly practical program. There should be a minimal age of consent before which it becomes statutory rape to force anyone to teach psychiatry, or to do research in psychiatry. Perhaps about 45 or 50 years would be a proper age to set for this. Before the young psychiatrist should be generously subsidized: first for a minimum of five years of half-time analytic training, followed second by ten years of half-time analytic practice. The other half-time of these 15 years would be spent in laboratories of experimental psychology, neurophysiology,

> neurobiochemistry, mathematics and modern electronic neurophysiology. This will give us a new generation of mature clinicians and mature experimentalists. (Kubie, 1961, p. 222)

Kubie goes on to point out that the scientist needs such "maturity," because "the investigative process calls upon qualities of human nature which run the gamut of all psychopathological mechanisms" (Kubie, 1961, p. 217). At times the scientist is obsessively counting data, at times he is disassociating from his prejudices to observe phenomena anew, and at times he is psychotically seeing endless instances of theory coming together. "No other creative activity demands such a complex series of successive roles," he says, and emotional maturity is necessary to switch among them with ease (ibid).

This understanding of research training was thoroughly built into *DIMH*'s structure. As Hanks tried to increase the research production in his institution, he also increased the clinical training and supervision requirements of the interns—as if one would not be possible without the other. Further, from the perspective of the analysts, the character profile which was being cultivated could (a) be achieved through the training analysis, and (b) was one scientifically well crafted for the task. Psychoanalysis had made personality its purview, and psychoanalysts honestly believed they could identify the fit from the unfit, as well as foster fitness using their techniques. Analysis would hasten and deepen a personality more than if nature were allowed to take its course. Undergoing their own psychoanalysis, considered the scientific technique for cultivating emotional maturity, was deemed essential for analytic scientists.

However, as the Richter story demonstrates, it was not always so simple to figure out how to cultivate a young analyst's character into the right mold. Zelda Lucile, a psychologist who went through *DPI*'s training track, remembers that people were interviewed for acceptance into *DPI* on whether they were analyzable or not—"something I still think is hard to figure out really" (Lucile interview, 2006), and typically, as I discuss later, at this time "analyzability" entailed that the person not be homosexual.

DIMH leadership was in a bind. On the one hand, they felt that their science of personality demonstrated that only some people were truly fit for certain clinical tasks. On the other hand, they did not have a truly objective way of demonstrating this fitness, much less guaranteeing that their criteria were ever completely met. This is one of the paradoxes of a science built on "insight" (as Franz Alexander articulated earlier in this chapter). A researcher's introspective awareness may result in the strengths Alexander identified in regard to understanding oneself and empathizing with a client. But it also spawns many debates beyond about things that cannot be proven. The result at *DIMH* was widespread rancor and internal disputes about the status of the various clinicians and researchers. These arguments around hierarchy and personality development did not address any of *DIMH*'s public relations problems, nor did it lead to the kind of research that mattered to *DIMH*'s professional audience.

Analytic Science and Accountability

In *DIMH*'s analytic literature during the 1950s and 60s it is the analyst—alone among medical professionals—who confronts human suffering subjectivity. The psychoanalyst was presumed at the top of both the knowledge hierarchy and the wisdom hierarchy, and this was thanks to "science." His legitimacy in regards to science was based on his education, rooted strongly in the post-Flexner science based medical curriculum, and his claim to wisdom was rooted in his having gone through a training analysis, which ensured his personality was "mature" or "well-adjusted." Analytic practitioners fervently clung to this doctor-hero identity as a guiding mythology. However, though both sides of this identity were premised on the prestige of "science," orthodox psychoanalysis and ego-psychology *refused* to subject psychoanalytic *practice* to rigorous outcomes, efficiency, or process studies. To put this in a different way, psychoanalysts staked their claim to expertise in being scientists, but refused to subject their "medical" practice to scrutiny using any conventional scientific criteria.

An essential feature of a scientific theory is that it is exposed to public discussion and debate. For instance, Newton was not taken at his word. Other people too used his equations to describe matter in motion. The shared success that people had with his ideas created the emerging consensus that gave Newton's theories credence. Today, in a post-Einsteinian world, physicists know that Newton was "wrong." However, he is still taught in introductory physics classes, because he offered an approach to the physical world that is replicable and testable. Students are not being taught faulty information, because through Newton they learn the basic tools utilized in the physical sciences to assess the validity of truth claims. When students continue on past introductory physics classes, they utilize the tools Newton would surely have endorsed in order to critique Newton and catch up with the discipline. The essential lesson is in how to assess claims about the material world in order to improve understanding across the discipline.

Similarly, there are features of psychoanalysis that are open to public debate and verification. Psychoanalysis' original popularity derived from the fact that Freud's language allowed people to discuss patients in ways that afforded more precise and fluid communication. To this day, concepts such as "defenses," "transference," "the ego", "Id" and so on, are regularly referenced in clinical discussions across the mental health field and the public. In this respect, psychoanalysis is an exemplary "open" science. Psychoanalytic journals are full of articles that take on analytic concepts such as anxiety or the defenses and compare their empirical observations with those shared by previous researchers. These papers did not just endorse Freud's original formulations, but challenged them and pushed them in new directions.

However, if the "technology" of analysis is the analyst himself, then exposing *him* to public debate is problematic. For instance, criticizing a piece of medical technology is common in the history of science, but were I to do that in this narrative, I would need to actually criticize the clinicians themselves—thus resorting to gossip and what would feel like *ad hominem* attacks. Utilizing gossip in a piece of historical scholarship is always risky, and I have no intention to dwell on the personal lives of the characters in question. However, the issues I am bringing forward now were called to my attention by several of my interviewees, and speak to an unflattering appraisal that the folks at *UDMC* were developing about *DIMH*'s leadership. Nothing belies the claims to maturity more than the pervasive sense amongst the medical center leadership, the residents, and doctors outside of *DIMH*, that *DIMH* and *DPI*'s leadership were eccentric, to say the least. Various members of the *DPI* leadership were rumored to have food and alcohol addictions and/or improper relations with their patients. There was a general sense of “laziness” in *DIMH*. And, of course, changing the rules for access to the club based on *post hoc* criteria of who is and is not mature does not give the impression of advanced maturity or emotional control. However, the logic of psychoanalysis inevitably draws attention to such eccentricities, because in the absence of demonstrable research prowess, or tangible advances in treatment, so much analytic authority is rooted in training analysts’ claims to “personality maturity.”

Last, more traditionally “objective” research projects on analytic practice and theory simply went nowhere. Zelda Lucile was originally recruited to join *DPI* because she promised to bring credible research strength to the program. Albert Roland recruited Lucile to join *DPI* the day after accreditation. Lucile felt that her research background in physiological psychology should have served her well in probing scientific questions on analysis, and solving them in the way she had previously researched the brain and medical education. However, she found barriers she did not expect. “Analysts say they want research, but they really don’t” she told me during our interview, and shared with me a short piece she wrote on research in psychoanalysis entitled “Our New Mythology” (Lucile, 1979).

In this piece Lucile identifies six myths psychoanalysts hold in regards to the scientific status of their work. Here I would like to point out four of them: Myth #1 is that psychoanalysis is a science. Myth #2 is that psychoanalysts are in favor of research. Myth #3 is that methodological problems in psychoanalytic research are so overwhelming that we are not yet technologically in a place to actually research it appropriately. Myth #4 is that allowing research psychologists, such as herself, into analysis is supposedly proof of psychoanalysis' sincere desire to submit itself to scientific testing. Lucile's conclusion:

> Freud bequeathed us a beautiful, slim craft with which to sail the seas of our clinical work and explore the thought and products of Western civilization. Over the years its hull has been barnacled so that we are left with a barely seaworthy scow. To think that research in psychoanalysis will be supported is like asking the barnacles to endorse drydock. (Lucile, 1979, p. 5)

In our interview, Lucile acknowledged that there are a few psychoanlytic researchers who perform good research, and she thinks that there is much to substantiate the practice and theory. She herself never produced such work, but moved deeper and deeper into clinical work, which increasingly gripped her attention. Besides publishing occasional review articles on the status of psychoanalytic research, Lucile never fulfilled the mission she set for herself by joining *DPI*. I never got a straight answer about why she did not pursue psychoanalytic research further, so I can only reference the answer she poses from her essay, which is no less than an analysis of the analytic field at large:

> Perhaps they [the psychoanalysts] fear that psychoanalytic research will provide crisp and clear results; a body of evidence which will be clean, but barren. This might put an end to the exegetic pleasure, the endless rococo decorations, which have served so many so well as a defensible intellectual indulgence and a vehicle for

> professional ambition. Then there is always the worst phantom to be faced. What if we don't like The Answer? Considering all this, ambivalence is no mystery, and the most reasonable solution is obviously to support vocally the idea of psychoanalytic research and at the same time to make certain it will never happen. (Lucile, 1979, p. 5)

DIMH leadership had based their professional legitimacy on science. However, they neither produced helpful research, nor convinced anyone that their scientific personality was real, much less desirable. Thus, to many, both in Dubville and across North America, the analysts seemed to be presenting arguments aimed at protecting their own jobs, rather than advancing any kind of healing practice, or science of the mind.

Summary: Unheeded Research Findings

By 1970, the specialized knowledge that psychiatrists at *DIMH* were clinging to in order to stake a jurisdictional claim for psychoanalytic psychiatry was failing. However, does that failing mean that analytic knowledge itself was failing?

On one hand the answer to this question is clearly yes. Those concepts which Freud identified as the basic organs of mind could not be consistently manipulated in a way that instilled confidence that the mind really is structured in such a way. However, during my interviews with professionals from this era of *DIMH*, professionals with varying feelings about the value of psychoanalysis, many expressed that this model, for all its failings, encouraged clinicians to spend time getting to know their individual clients. While there is a question as to how accurate claims about "oedipal struggles" or “oral fixations” actually were, there is a feeling that the effort to mine for “internal structures” itself guaranteed a better understanding of the client, and resulted in better clinical care. As a social worker, Fannie Victoria, who has worked at *DIMH* consistently since the 1960s, told me, “Clinical excellence was of the highest concern in those days” (Victoria Interview, 2005). However, she also adds that diagnosing clients according to the dominant psychoanalytic method was “pretty hard – it’s hard to tell what anyone’s ego strength really is.” Empathic reflection was not sufficient for rigorous diagnoses, and thus the prevailing system for the classification of disorders could not lead to good reliabilities, much less validity.

The imprecision of the analytic language was something that many “post-Freudian” thinkers were coming to terms with at this time across the globe. Thinkers such as Erich Fromm, Eric Erickson, and followers of Carl Jung, were all taking Freud’s original concepts and looking at them as powerful heuristics by which to organize psychotherapy practice, but not as hard scientific entities. However, *DPI* leadership was mostly uninterested in these developments. Zelda Lucile reported a story about how she once asked about how a certain concept her *DPI* instructor was mentioning was taken up by Carl Jung. He responded that he did not read that literature, and could not comment on it. Lucile was floored, “here was a major thinker, and at *DPI* he didn’t even exist.”

Although this clinical instructor was not available for interview, I feel it is safe to say that these thinkers were not read, because they violated the strict professional rule that Freud must be understood as a scientist. Psychoanalytic psychiatrists were staking their claim to dominance in mental health on the grounds that they were compliant with the Flexner Report: they were scientists, executing the most scientifically viable theory of the day. Opening oneself to these schools that questioned the scientific solidity of psychoanalytic concepts would have been professionally dangerous.

However, by the end of this dissertation it shall be shown that psychoanalysis turned out moving in this “post-Freudian” direction, where the concepts are understood more philosophically than scientifically. And, interestingly, it did so for reasons that scientific research on psychoanalysis during the 1950s made obvious, but was not heeded.

I turn to an article produced by *DIMH* faculty member, Victor Rotelle. Rotelle mined clinical case files at *DIMH* in order to “define in some measure psychotherapy as practiced by the residents” at *DIMH* (Rotelle, 1958, p. 158). Clinical files were very thorough in the analytic days, documenting every aspect of the therapy, including the development of transference, the various crises the clients were experiencing, dreams, and so on. Through careful analysis of this work, Rotelle discovered that the clinicians were not always consistent in their understanding of what various features might mean, or in how they formulated a case (one comical formulation simply stated “the patient wants a penis” (Rotelle, 1958, p. 157)). Further, Rotelle found that the number of sessions alone was not a clinical indicator for success (in fact, even what constituted “improvement” varied between clinicians) (Rotelle, 1958, pp. 156-157). Instead what Rotelle found to be most predictive of “improvement” was what he called the therapists “style,”—something he gauged by cultivating a sense for how the therapist did his or her work. Using a model offered by another researcher, he breaks the therapists into two groups: the “interactive” therapists, and the “cathartic” therapists, the first being more successful on average than the latter. His conclusions were as follows:

[T]he interactive charts show a psychotherapy that

(1) has an attitude of offering explanations,

(2) has an aim of insight and catharsis,

(3) has an indeterminate ‘depth’

(4) lasts about five months or twenty hours

(5) has Freudian hypotheses for behavior, but does not use the couch,

(6) appears to follow leads from the Sullivan school of psychiatry,

(7) treats individuals

(8) is directive and

(9) uses a minimum of adjunctive techniques

The tendency in the cathartic group appears to lean more toward

(1) an attitude of exhortation,

(2) an aim of catharsis,

(3) a superficial depth,

(4) a duration of several months,

(5) an imitation of psychoanalysis without an understanding of it,

(6) no explicit relation to ex-Freudian schools,

(7) treatment of individuals,

(8) non-directive measures,

(9) the use of medication

(Rotelle, 1958, pp 158-159)

This article had little impact at *DIMH*, and a focus on the "cathartic" approach continued to predominate—at least within the training ranks at *DIMH*. However, Rotelle here seems to be pointing to a very different kind of psychotherapy as most successful. It does not require medications, has a link to Freud but is not wedded to his methods and, in fact, looks to contemporary theorists who were re-working his protean concepts, is directive, has a typical time trajectory, and so forth. Rotelle here was identifying what would become the future of psychotherapy. I now turn to the group that took that future on as its own.

3 – CLINICAL PSYCHOLOGY: 1950 – 1970

By the late 1960s, psychoanalytic psychiatry was losing its hegemony in the mental health field, unable to satisfy its various audiences (the state, the local community, the rest of the medical establishment, the analytic hierarchy) and unable to make the internal changes necessary to satisfy the demands of its diverse patient base. Regardless, the need for mental health service practitioners increased: The federal government increased funding to community mental health services and people were increasingly willing to foot the bill for their own mental health care.

New approaches to psychotherapy developed across the country as this high demand created the opportunity for other practitioners to "poach," redefining the bounds of professional jurisdiction once held almost exclusively by psychoanalysts. One of the most important groups that benefited from this jurisdictional battle was clinical psychology, a field undergoing its own struggles during the 1950s and 60s, attempting to unify itself enough to become a viable profession. Tracking transitions in the identities of mental health providers in this period helps explain the contemporary prominence of clinical psychology and the dramatically diminished role that psychoanalysis now has in mental health care.

Psychology – A Disputed Discipline

Before turning to Dubville, let us survey the status of academic psychology leading up to the 1950s. As historian of psychology Bernard Baars points out, psychology is one of the few academic disciplines with a birth date: 1879, when Willhelm Wundt (1832-1920) opened the first laboratory specifically dedicated to studying psychology separate from physiology or philosophy (Fancher, 1996, p. 147). Of course, people have been thinking about the mind in various ways for millennia. What differentiated Wundt and his followers was that Wundt studied specifically mental processes, and did so in a lab. Baars points out that this was the moment that the role of the *professional* psyche-studier was institutionalized (Baars, 1986). In contrast to physics, which has no official birth date, but underwent significant changes in its relationship to philosophy, religion, and the academy over the centuries, psychology has a "birth date" but few epoch-making discoveries to its credit. Psychology is a discipline united by a dream: to study the mind (consciousness, or behavior, or cognitions, etc., depending on one's preferred approach to the discipline) with objective, scientific accuracy. It is a job that psychologists have approached in such disparate ways that the only unifying thread is the overarching project of achieving scientific knowledge of mind and behavior.

Understanding psychology as a profession helps us understand some of its inherent dynamics. In order to begin as an independent discipline, psychology needed resources to afford lab and office space in universities. Psychologists had to find ways to open departments, obtain positions for their graduate students, receive grants for research, and be generally considered full members of the academic community. Those familiar with academic psychology know that still, today, psychologists are touchy about their status as "scientists." Many of the debates in the field are attempts to accomplish enough common ground to have a unified field, for otherwise it consists of various, at times quite disunited, projects (such as social psychology, developmental psychology, organizational and school psychology … the list goes on.)

Rapprochement within the discipline seemed a remote possibility in the early and middle-parts of the 20th Century. In 1913, John Watson (1878 – 1958) published "Psychology as the Behaviorist Views It," a paper that would shortly be known as "The Behaviorist Manifesto." In this article Watson attacked "introspectionism," and by implication, Wundt, who trained research subjects to reflect upon, and report, internal processes in controlled laboratory conditions. Watson argued that the reports of trained researchers were not objective data, because "feelings are never clear" (Watson, 1913, p. 159). He argued that introspectionism produced data that are hard to replicate, and that this stituation would not improve by simply improving the training of research subjects. Instead, psychology had to pick better—more objective—data markers. He lays out his vision succinctly in the essay's opening paragraph:

> Psychology as the behaviorist views it is a purely objective experimental branch of natural science. Its theoretical goal is the prediction and control of behavior. Introspection forms no essential part of its methods, nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation in terms of consciousness. The behaviorist, in his efforts to get a unitary scheme of animal response, recognizes no dividing line between man and brute. (Watson, 1913, p. 158)

Watson's paper was received coolly at first, and was never adopted wholeheartedly by the research psychology establishment. Certainly, many in psychology heeded the call to focus on observables, and "predict and control" became central goals in the field.

However, as Bernard Baars points out, very few research psychologists fully committed to behaviorist tenets. For instance, Watson denied the very existence of internal psychic states, and rejected the idea that there are any innate psychological tendencies. Further, he argues that there is a "unitary scheme" between animals and humans that should be the focus of research. These more "radical" commitments of behaviorism were not necessarily shared by all who started working within the behavioral paradigm. However, Watson's call to move attention away from subjective states, to observable facts and behavior, gained paradigmatic adherence by many research psychologists until the mid-1970s (Baars, 1986).

During the same time that behaviorism trounced Wundt in academic psychology, clinical psychology was on the rise. It is important to note what the term "clinical psychology" meant during the first third of the 20th Century. The term was coined by Lightner Witmer (1867 – 1956) in his 1907 article of the same name. In this article, Witmer explained that, "While the term 'clinical' has been borrowed from medicine, clinical psychology is not a medical psychology" (Witmer, 1907). For Witmer, the clinical psychologist is a "psychological expert, who should find his career in connection with the school system, through the examination and treatment of mentally and morally retarded children, or in connection with the practice of medicine" (Witmer, 1907). The clinical psychologist brought his or her scientific expertise to bear on the topic, not in the form of therapy, but in the form of knowledge on how people behave and develop. Although the clinical psychologist may occasionally work directly with a client, it would not be for an emotional imbalance or adjustment problem. Such "healing" tasks were left to doctors. Clinical psychologists instead worked in clinics with students having learning difficulties, children developing at a particularly slow or too rapid pace, or any other such nonmedical condition. Witmer's vision avoided a turf battle with medicine, and in effect found a unique niche for the psychologist in the education and human services arena.

In fact, the earliest resistance to clinical psychology came not from psychiatry, but from research psychology. Researchers feared the clinicians' "applied" focus would interrupt the discipline's efforts to construct a science of mind and behavior. Further, by the late 1940s, clinical psychology was growing beyond Witmer's original vision into "counseling" and psychotherapy. Psychologists such as Carl Rogers and his students began to publish articles about psychotherapy practice, which described it in a scientific manner (i.e. trying to use objective, visible criteria, including psychological measures) (Rogers, 1939, 1942). However, the treatment itself involved introspection, and none of these measures met the strict standards of the laboratory researchers, where behavior was strictly predicted and controlled. Instead, this research identified principles of psychotherapy interaction, and talked about "the self," the "person," "empathy," and "congruence," concepts that seemed immeasurable to the hard-nosed researchers.

These tensions were all in play in Dubville's psychology community—particularly in the University of Dubville's psychology department. The University of Dubville (*UDub*) psychology program was one of the first in the country, and designed on the German model (i.e., heavily invested in research and experimentation). The program had several labs for researching animal, behavioral, developmental, and physiological psychology (Musselman, 1999). The department also had a small psychology clinic in which research was done, which at this time meant intelligence and aptitude testing, but which had no direct bearing on remediation strategies of an either educational or counseling kind. Across campus, in the School of Educatio, there was a "counseling" clinic. Here, masters students receiving a degree in school counseling, worked with children having learning difficulties, and provided general counseling on career choices and life decisions (Musselman, 1999; Florid, 1946).

Originally, these two clinics were completely separate. However, in 1920, a new dean, Billing Jaspers, interceded in department affairs and merged the two clinics. Jaspers had an antipathy to experimental psychology generally, writing to a colleague that he disapproved of the idea that "'the pattern of life to which we all belong' could be subjected to laboratory investigation,' and concluding that 'the soul of man cannot be stretched out on a laboratory table'" (Musselman, 2007, p. 3). Instead, Jaspers preferred the clinical vision of psychology in which psychologists are rigorous helpers, but not Dr. Frankensteins. Though not embracing a psychoanalytic perspective, Jaspers affirmed the primacy of the "total person." However, his conception of the "total person" was a religious one. *UDub* was dominated by religious scholars and researchers at this time, and Jasper's goal was in step with those of the local community (Adalwin interview, 2008). His strategy was to strengthen the psychology program's emphasis on "counseling" (Musselman, 1999).

In the late 1920s, Jaspers appointed Teresa Florid, a faculty member from the School of Education, to head the newly merged clinic. To make matters worse for Watson's followers, Florid had only a master's degree. Unsurprisingly, the psychology faculty was apprehensive about her position (Musselman interview, 2008). However, Florid was strongly committed to Witmer's vision, and believed that the viability of clinical psychology depended on its scientific knowledge base. She obtained a PhD in child development from a prestigious North East institution, and worked to make the clinic the exemplar of a new clinical psychology: one that transcended psychologist's traditional focus on testing and measurement, while basing the newer counseling component in science and research. Clinic services were utilized by *UDub* students and faculty, as well as members of the broader Dubville community. Under Florid's leadership, the *UDub* clinic handled between 500 and 700 cases annually, with each case requiring individual interviews, analyses of test results, diagnoses, and report writing (Musselman, 2007, p.3). This work was time consuming for the students as well as the clinical teaching staff. However, neither publications nor research were of high priority. Instead, the program measured its success with its production of experts for the local area. During this period, many of *UDub*'s psychology faculty were hired from *UDub*'s own Ph.D. pool, since they were committed to the area and were affordable, while intellectual commingling with the academic community was not held to be of highest importance (Musselman, 1999).

Dubstate State Hospital, which would become *DIMH*, was one of the institutions employing *UDub*'s clinical psychologists during the 30s and 40s. *DSH*'s psychology department provided testing services throughout the hospital, and it served as a clinical opportunity for students training at *UDub*. Interning at *DIMH* was a popular option for *UDub* students, because they had the opportunity to utilize projective tests (tools Dean Jaspers forbade) (Pearson, 1943). However, many students avoided this internship opportunity because of the para-professional status *DIMH* accorded psychologists. Zelda Lucile, a graduate student at *UDub* through the late 1940s and early 50s, recounts that "the psychologists were seen as subordinate to the psychiatrists—they were just given stuff to test and report" (Lucile Interview, 2006). From the perspective of psychiatrist psychoanalysts, psychologists were not much different from nurses or other "support personnel." Psychologists at *UDub*—all of whom were pursuing PhDs—had a more expansive sense of their own worth, and their rightful place in the mental health hierarchy. They chafed against this subordinate role. Lucile, for her part, spent most of her graduate education studying physiological psychology, and simply avoided the department's clinical track.

Nineteen Forty-five was a significant year for *UDub*'s psychology program, and for clinical psychology broadly. First, it was the year that the American Association of Applied Psychology merged with the American Psychological Association to unify clinical and research psychology. This merger brought psychologists interested in clinical practice into the same fold as the professionals advancing academic knowledge. It was, and continues to be, a tense relationship. Though the clinicians and researchers asserted their common "psychologist" identity in this move, as we shall see, it did not actually guarantee that the practice and the research was really of the same cloth, nor did it ensure paradigmatic commitments. Although this merger helped psychology take a step towards the goal of unification, it was not fully accomplished (See Pickren & Schneider, 2005). In Dubville, 1945 brought Billing Jasper's retirement, and the death of the department's long-term chair, who had occupied the position for some 20 years. The department could now bring in a new chair and return the department to its research mission.

In 1946, the department hired Clark University graduate, Daniel Welter. Welter was already pursuing his own successful child psychology research project, and upon entering *UDub*, instantly opened a line in contemporary development theory, and physiological research (two research lines which were *verboten* during Jasper's reign.) Welter worked to unify *UDub*'s clinical and research programs. Some of the faculty, who wanted to see an end to the clinical program, hoped he would fail; however, clinical psychology was simply becoming too important a field from which to turn away. Just as psychoanalysis was benefiting from the post-World War II enthusiasm for mental health services, the opportunities made available to psychologists were also vast. Both the NIMH and the NSF began issuing training and research grants in clinical psychology (Dewsbury, 2005). For one of the first times in the field's history, opportunities to do psychological research were plentiful, as long as one structured one's research in terms of mental health. The researchers were becoming dependent on the clinicians, because it gave relevance to their—at times faltering—research project. Severing this connection would have been disciplinary folly.

What emerged as a complicated "compromise solution" (Baker & Benjamin, 2005, p. 194) within the department, not to mention the discipline generally. Each group had much to gain through attaching themselves to the other, but just how to do this was not obvious. A now famous NIMH hosted conference in Boulder, Colorado, in 1949 on the topic of graduate education in clinical psychology, made this issue a central topic. Daniel Welter attended this conference with one of *UDub*'s clinical faculty members, and worked with some 70 other academic psychologists to put together the training model which continues to dominate clinical psychology training: the scientist-practitioner model. Ingrid Farreras explains:

The consequences of the scientist-practitioner model that was established at the Boulder conference were both positive and negative for the field of psychology. On the one hand, it bridged the academic-versus-practitioner rift by terming both *psychologists*, independent of their place of work … Earlier, psychologists had been described on the basis of the content of what they studied: traditionally, experimental topics such as perception, learning, memory, and so on. Clinicians had been expected to ground their theory in basic research on such topics, but that research was not readily applicable to social problems, and thus clinicians ended up using tests and psychotherapeutic techniques that were not derived from traditional, experimental psychology. The scientist-practitioner model now provided a way for academics and practitioners to find common ground through a shared methodology … Since APA's 1945 reorganization, with its consequent expansion in mission from the advancement of psychology as a science to include its advancement as a way to promote human welfare, practitioners were now expected, in return, to be psychologists first and clinicians second, thus allowing for all PhD students to be trained in research. (Farreras, 2005, p. 172)

Without its clinical component, psychology is a fairly small and specialized field. Labs and researchers are expensive to maintain. Internal conflict within the field about fundamental questions—such as what counts as evidence—made it hard to chart progress, let alone argue for more resources for continued investigation. Clinical psychology offered research psychology a *raison d'etre* of sorts, but it required that the clinicians accept the preferred methods of the researchers as their basis for treatment. Unlike the psychoanalysts, who had adopted research methods specifically designed to help clinicians (individual case studies describing processes most relevant in the clinical encounter), research psychologists were invested in the experimental methods of the natural sciences, which they felt were the direct route to Scientific Truth. The Scientist-Practitioner was a practitioner attached to these methods of the lab, with the aim of working in a clinical environment. It was a great professional accomplishment, but as Baker and Benjamin note, "As a compromise, it allowed both sides to save face, but it held the seeds of conflict between traditional psychological scientists and professional psychologists that within a decade began blossoming" (Baker & Benjamin, 2005, p. 194).

Zachary Edmund, a graduate student at *UDub* during the mid and late-60s recounts an illustrative tale of how these contradictions bloomed. Once, while attending a clinical research class, the faculty member shared his research on learning in monkeys. A clinical student raised his hand and commented that this analysis of the monkey did not seem transferable to humans, and that even if the research could be applied to humans, it did not seem an accurate description of how humans actually solve problems. The professor's response was, "I don't care."

"Right there you could feel it," Edmunds recounts today, "Half of the class thought this guy was completely irrelevant, and the other half was in complete agreement" (Edmunds interview, 2008). The first half were the clinical students, desiring research relevant to the work they wanted to do, and the other half were the researchers who wanted to understand behavior in a "scientific" way, regardless of its relationship to actual, clinical questions. This kind of disagreement about relevant knowledge put a strain on the "Scientist-practitioner" model—because "scientist" was defined in a way that lacked clinical relevance. And the researchers often did not care.

Perhaps not surprisingly in such a disjointed department, clinical students and faculty were made to feel, as Susan Elisa says, "not up to par." Elisa originally came to study experimental psychology, but then moved into clinical psychology after her comprehensive exams. She saw first hand how the two faculty groups were treated differently: "The experimentalists were held in more esteem, because they were the people publishing and getting grants" she explains (Elisa interview, 2007). Unlike the 20s and 30s, now it was the clinical training faculty who would never receive tenure (Musselman, 2001). The fact that much of the experimental research was, in Elisa's words, "a lot of foolishness," and that many of the clinical trainers seemed to have been very dedicated teachers, who did in fact publish case studies and text books, did not seem to matter, because from the researcher's perspective, only experimental research counted as legitimate academic production (Musselman Interview, 2008).

One example of clinical marginalization was the department symposium series, which brought in major researchers to share their work with the department on a bi-weekly basis. One year Zachary Edmund checked the schedule and noted that not a single clinician would be speaking. When he inquired about this with the chair, he was simply shrugged off. Edmund's response was to start his own speaker series, to which he invited local clinicians from across the area to share their work. The series was held on Fridays, and was titled "The Friday Undertaking in Clinical Knowledge." "I made nice flyers and posted them around the department," Edmund says with a mischievous grin (Edmund Interview, 2008).

Edmund's hostility was not solely a product of Sixties radicalism, but also stemmed from the "increasingly aggressive experimental stance" (Musselman, 1999) of the department, in which clinical psychology was marginalized, despite the fact that many of the researchers were technically producing "clinical research." Beyond the department's increasing reliance on clinical research funds, during the 1960s more than half of the *UDub* graduate students went into clinical work (Musselman, 1999, p. 32). "Here we were, on our own time, reading Milton Erickson, Jay Haley, Gestalt people—this was what we wanted to learn. … This was clinical research, and to the department it didn't even exist," Edmund told me (Edmunds interview, 2008).

Beyond problems within the department, clinical students also found it difficult to find practicum opportunities in the community, because clinical psychology was still coming into its own. Clinical psychology students working with the counseling faculty wanted to go out and provide psychotherapy, but during the 1950s and 60s the community was mostly interested in psychologists for their testing skills, and not for their psychotherapy skills. Psychotherapy was still assumed to be the province of psychiatry. Susan Elisa managed to find a few venues where she could practice psychotherapy with one or two clients (typically under direct supervision of a psychiatrist.) She remembers this training as invaluable, and it jelled well with the training she was receiving from the counseling oriented faculty at *UDub*.

Thus was the status of psychology in Dubville in the 50s and 60s: a tenuous profession held together by sheer determination and despite inner contradictions. The clinicians paid lip service to a research base that they found of only intermittent value, while researchers grudgingly conceded some small validity to a clinical approach of which they were profoundly suspicious. It was through increased cooperation across this divide, that clinical psychology emerged as strong enough to "poach" (Abbott, 1988, p. 44) in the psychiatric domain, and wrest psychotherapy out of the hands of the psychiatrists.

Workplace Assimilation

UDub's clinically motivated faculty, most of whom came originally from the School of Education, continued to work in the clinic and train the students in counseling and psychotherapy. Although no one I interviewed gave overwhelming praise to the classes, many described the supervision groups and clinic discussions as invaluable clinical learning opportunities (Edmunds interview, 2008; Elisa interview, 2006; Regent interview, 2006).

Those students taking advantage of the clinical opportunities made available to *UDub* students, found many resources. I shall back track here slightly, in order learn some more details about an important student and faculty member at *UDub* who will help us learn how the clinical/research divide impacted *UDub*, and was traversed throughout the post-World War II period.

Lance Davidson originally entered *UDub* in the 1930s to become a researcher, and pursued research projects on animal behavior and learning theory. However, he became increasingly interested in the clinical modalities made available through the Psychology Clinic, and particularly in the burgeoning movement of counseling psychology. Davidson read extensively in psychotherapy literature such as Freud & Harry Stack Sullivan (he owned the collected works of both men), as well as leaders in counseling psychology like Carl Rogers (Musselman interview, 2008, Davidson interview, 2008). By the end of his graduate education he was actively running various parts of the *UDub* Clinic (along with Teresa Florid and future community psychology luminary, George Albee), and was hired as faculty in the mid-1940s.

"Davidson was always carrying a new clinical book around with him," recounts his colleague, and eventual wife, Georgia Davidson (Davidson interview, 2008). Davidson was drawn to a wide swath of theories and ideas—mixing his interest in research psychology with ideas from the counseling tradition. As *DIMH* became more psychoanalytic, he was also a regular participant in the Institute's many activities, participating in open trainings with the likes of Benjamin Spock and Erik Erikson, and in supervising clinical students.

Davidson's clinical experience resulted in what Abbott calls "workplace assimilation" (Abbott, 1988, p. 67), a form of knowledge transfer that happens in institutions where members of different professional affiliations work together on similar tasks, occasioning blurring of professional jurisdictions. Such assimilation occurs in all work places: It is common to hear tales of subway janitors who learn how to run the trains, or paralegals who become experts in various parts of the law, and so on. However, *DIMH*'s structure was such that even if psychotherapy knowledge flowed past professional boundaries, the professional structure did not adjust in response. "Everyone had their place," says Mary Packer, a social worker who taught family therapy and theory at *DIMH* (Packer interview, 2006). Regardless of her training and expertise (impressive enough for her to acquire a teaching position), analytic knowledge was still the gold standard, and only medical doctors were allowed to wield it.

However, outside of *DIMH*, the professional structure was more porous, allowing for more flexibility. One such location was Dubville's Child Guidance Center (*DCGC*), a stand alone child guidance clinic with a board that included *DIMH* faculty, but which was otherwise independent of *DIMH*'s academic structure. The Child Guidance Center movement was born of the early 20th Century belief in the importance of helping children and families to develop healthily from the start. *DCGC* was integrated into the community psychiatry movement, and provided psychological services to the poor. The center provided a multitude of services to help Dubville families and children, ranging from parenting classes, assistance for learning disabilities, and counseling (Victoria interview, 2005). *DCGC*'s staff was mostly composed of nurses, teachers, and social workers, all of whom worked under a psychiatrist. The medical doctor did little direct work at the *DCGC*, leaving much of the clinical supervision and oversight to the psychologists on staff who impressed him with their clinical acumen. One of these psychologists was Davidson, who worked at the *DCGC* as a part of his job with the *UDub* department. (As noted, under Jasper's reign service was more highly regarded than research.)

The *DCGC* was a marginal space in the Dubville mental health field: clients were children from lower and working class families, as opposed to adults and/or lucrative private practice patients. Because *DCGC* existed outside the field's center, it occupied the kind of space in which jurisdictions become more lax, and professional hybridization is likely to occur.

As stated earlier, the power to draw on one's own diagnostic system is an essential component of a jurisdictional claim. "A classification system is a profession's own mapping of its jurisdiction, an internal dictionary embodying the professional dimensions of classification" (Abbott, 1988, p. 41). Within *DIMH*, the diagnostic system was orthodox Freudianism, as encapsulated in the Diagnostic and Statistical Manual versions I and II. Upon intake, every patient was diagnosed within this Freudian schema, typically by a social worker. It was commonly accepted across *DIMH* that this system was suboptimal. First, and as mentioned earlier, Freudian entities like the ego, super ego, id, and so on, are not readily visible, making it difficult to gauge accurately a patient's "ego strength" at any given time (Victoria Interview, 2005). Second, it was also known that many different labels could fit the same client, depending on one's perspective of the situation. That is to say, it can be true that a client is regressing, and also that his libido is over-running his ego--which diagnosis is given depends on the focus of the particular analytic approach. Further, as many analysts contend, diagnosis can take the whole span of therapy, for analysis is the work of discovering the historical trauma that is behind a person's symptoms. The initial categorization from the DSM was often regarded as tentative, provisional, and subject to change as treatment proceeded and the underlying disturbance or conflict was finally laid bare. Nonetheless, this process of classification continued for many reasons, not the least of which is that it defined patients' problems in psychoanalytic terms, thus legitimating the profession which controlled jurisdiction of the treatment.

However, if that same patient were to arrive at a place not dominated by psychoanalytic thinking, he or she would receive a different label, with correspondingly different treatment. "Childhood schizophrenia," a disorder which today is called either "autism" or "pervasive developmental disorder," provides a clear example of the implications of this difference. Analytic psychiatrists considered autistic children under its purview, and readily accepted autistic children for treatment. Although the analysts did not presume complete understanding of the disorder, they believed that problematic interactions within the mother-infant dyad were the fundamental cause. Their treatment approach to autistic children was interpretive play therapy to help the patient escape the internal world in which he or she was locked (much of the interpretation was focused on mining the oedipal subtext of the play) (Johnson interview, 2006). The success rate of this approach was not spectacular, an unsurprising fact, given that Freudian psychoanalysis achieved prominence because of its successes treating neurosis and hysteria, not schizophrenia and developmental disorders. As noted, it was only through an "inference chain" that these diagnoses were brought under analytic psychiatry's jurisdiction in the first place. Abbott points out that failure does not necessarily lead to the end of jurisdiction, as long as a competing approach is not more successful. For an intractable problem, professions can always claim that their approach eventually works—failure is one step closer to success.

In contrast to *DIMH*, *DCGC* was not tightly structured. Those individuals actually completing the work were not in need of any abstract jurisdictional control over the task, and thus were freer to try to find something that worked. Patients entering its doors were not experienced or treated from a purely analytic perspective.

Lance Davidson earned his leadership position at the *DCGC* through his extensive knowledge of psychoanalysis, which impressed the "higher ups," but his knowledge interests were broader. Susan Elisa, who worked as Davidson's assistant for two years in the early 1950s, recounts extended supervisions conceptualizing clients in Rogerian, Sullivanian, Freudian, and many other ways (Elisa interview, 2006). "It was through Davidson that I learned that theory doesn't matter much once you're in the room," Elisa told me. She recounts one patient, a young child who was socially withdrawn. Davidson told her to "just go in there and try to connect. Do what feels right" (Elisa interview, 2006). Nervously, Elisa entered the room to attempt connection, while Davidson watched through a one-way mirror. After several sessions, Elisa entered into a play relationship with the child, but did not think much of her work, until Davidson told her that the client was autistic, with a history of head banging. "He told me this was the first time he had ever seen a case of head banging cured by psychotherapy" (Elisa interview, 2006).

Davidson and Elisa's work did not result in a revolutionary approach to the treatment of autism. In fact, considering the success, it is highly likely this child was not "truly" autistic to begin with (at least not by today's neurologically based standards). However, such successes on the periphery of a professional jurisdiction can result in professional poaching. These successes embolden members of a profession to stake a claim for work technically under another profession's jurisdiction. Blocking entrance to individuals with documented successes to showcase can be difficult. As Abbott says,

> Vulnerability begins not with the most commonly treated professional problems, but with the peripheral ones. … Such implicit claims often include vast areas of residual problems not conceptualized in clear types like the standard professional problems, but loosely labeled as 'nervous exhaustion,' 'emotional difficulties,' 'marital troubles,' 'financial difficulties,' 'tax problems,' or whatever. These residually conceptualized areas are a standard site of inter-professional poaching. (Abbott, 1988, p. 44)

Buoyed by his clinical successes, Davidson attempted to open a private practice of his own in the mid-1950s. By this time he had well over 20 years of clinical experience, was a director of an APA accredited clinical psychology program, provided clinical supervision at the local Veterans Administration hospital and Child Guidance Clinic, and was a regular member of various psychotherapy events around Dubville. He intended to provide "counseling" services. Davidson was friendly with many in the Dubville analytic community, and had become comfortable thinking of counseling as a service distinct from analysis. Consequently, he did not anticipate a turf war with the analysts.

After all, there were precedents: Teresa Florid had opened her own private practice in the mid-40s and avoided provoking the wrath of the psychiatric community. But her private practice did not address adjustment difficulties, and only served children needing “classical” clinical psychology services. Davidson’s practice was the first to offer something akin to psychotherapy for adults—and he met a wall of opposition.

Davidson wanted to partner with a psychiatrist for consultation and referrals for medical problems. He approached his supervising psychiatrist at the *DCGC* with this suggestion. The doctor accepted, but returned a few days later to report that he had posed the question to Robert Hanks at *DIMH*. Hanks had responded that a psychologist would do psychotherapy in Dubville over “my dead body” (Davidson interview, 2008). Hanks threatened to deny bed privileges to the psychiatrist were he to work with Davidson, and even threatened a malpractice lawsuit. The psychiatrist recused himself, and Davidson decided not to fight it.

Hanks’ response was typical of psychoanalytic psychiatrists of the time. Psychotherapy was psychiatry’s response to “problems of adjustment,” and no other approach would be tolerated. Psychoanalysis was the basic science of the mind, and attempting to heal the mind without proper training in it, would be akin to a massage therapist taking on muscle surgery. (The fact that Davidson was well versed in analysis was insufficient: He had not undergone any official training, and his lack of knowledge of physiological processes meant he could not truly engage the “total” person.) How much of this prohibition was turf protection and how much the expression of concern for public welfare is an open question. Regardless, the impact was the same—only physicians could practice psychotherapy.

Davidson’s story is typical for clinical psychologists at the time: Professional conflict with analysts in the community, and professional tension with researchers in his own department. Instead of calling the analysts’ bluff in the community, or fighting the researchers more in his department, Davidson decided to help develop the relationship between the research and clinical bases in psychology, in order to create better grounding for psychologists interested in becoming clinicians in the future. Davidson seems to have believed that it was simply a matter of time before clinical psychology became a dominant provider in psychotherapy services; but for this to happen the scientific base of the approach had to be developed and expanded. He turned his attentions to that effort.

The Emergence of Behavior Therapy

In 1958, Loren Peters joined the *UDub* psychology faculty. "Loren and Lance hit it off right away, and enjoyed sitting and talking about ideas in the field," remembers Georgia Davidson (Davidson interview, 2008). Peters was interested in research, and particularly a clinical research project that would vindicate the scientist-practitioner model. He pushed Davidson, with his more thorough clinical background, to develop a hypothesis for a combined research question. Davidson found it in a book written by Joseph Wolpe (1915-1997), a South African psychiatrist trained in analytic psychiatry, who was developing his own brand of psychotherapy based on behaviorist theory.

Wolpe's book *Psychotherapy by Reciprocal Inhibition* (1958) became a foundational text of behavior therapy. In it, Wolpe takes on the fundamental tenets of psychoanalytic psychotherapy, questioning whether every symptom indeed has an underlying meaning, and whether unearthing said meaning is essential to effecting change. Instead, Wolpe argued that symptoms, especially isolable phobias, can be individually eliminated using what he called "reciprocal inhibition"—a form of desensitization akin to how a parent teaches a child to face any new obstacle: slowly exposing the child to the new stimulus and helping the child become comfortable with each new level of exposure to the negative stimulus until the child has relaxed enough to tolerate the stimulus without the onset of symptoms. This continues until the child (or patient) is completely comfortable with the stimulus that had previously scared him or her.

The term "reciprocal inhibition" speaks to some of the underlying mechanics in the theory. For Wolpe, the phobic object is a stimulus for anxiety. He proposed conditioning the patient to react to the stimulus with relaxation impulses and thus inhibit the previous maladaptive phobic response. Patients first learned deep relaxation. Once mastering relaxation, the patient slowly received exposure to more and more intense representations of the phobic object: first just thinking about the object, then looking at a picture of it, then existing in physicial proximity to it, and so on. With each new level of exposure, the client is prompted to practice relaxation techniques to counteract the phobic response. By the time the client attempts *in vivo* interaction with the phobic object, he or she is conditioned to feel relaxed by it instead of anxious. A cure has been effected.

Wolpe's work was considered revolutionary at the time, because it was one of the first attempts to make behaviorism clinically relevant. Though behaviorism had existed for some 40 years by the mid-1950s, few behavioral researchers had given serious attention to clinical matters. Further, those psychologists interested in clinical questions tended to focus on counseling and psychodynamic models, rejecting behaviorism's approach. Wolpe conceptualized clinical work in a way that researchers could understand; better yet, he showed results.

Wolpe's arguments intrigued Davidson, who brought Wolpe's book to Peters. Together, they crafted a research project whose results produced a dramatic impact on the future of the field. Davidson and Peters recreated Wolpe's experiments to determine if his success could be replicated.

The article resulting from their research is a direct attack on psychoanalytic orthodoxy, and against the knowledge base through which psychoanalysts claimed sole jurisdiction over psychotherapy. The assault on psychoanalytic dominance is explicit, and embedded in the research itself, which addressed snake phobia. As stated in the article, the authors selected snake phobia:

> "because it is frequent in a college population, approximately 3 in 100 students are to some degree snake phobic, and also because of the symbolic, sexual significance attributed to this fear by psychoanalytic theory. … The fact that snake phobias are held to reflect conflict in more fundamental systems of the personality, suggests that this is good ground for a stringent test of behavior therapy. (Fenichel, 1949, p. 49)." (Peters & Davidson, 1963, p. 519)

The reference to Fenichel, an important Freudian thinker, demonstrates Davidson's knowledge of psychoanalytic theory. It also makes clear his tactic of taking the offensive in challenging psychoanalytic hegemony by directing his argument at a Freudian of Fenichel's stature. The article might as well be a direct answer to the psychoanalytic psychiatrists who bullied him away from private practice.

Also significant was the display of mathematical knowledge about the prevalence of snake phobias in college-aged populations. This use of social science data to make a decision in experimentation is emblematic of the scientific approach that Davidson and Peters were advancing. Thus, the research was not just an attack on psychoanalysis, but also an affirmative step, demonstrating a better way clinical research could be done.

Davidson and Peters devised an "Avoidance test" in which a test subject was told that inside a room there was a nonpoisonous snake in a glass cage. The person was asked to enter the room, get closer to the snake, and then eventually to pick it up. The distance between the person and touching the snake was measured in feet—and subjects were given a score depending on how far they could go in this avoidance test. A score of 15 meant the person could go in and pick up the snake. A score of zero was given to a person who could not even enter the room. This test was given to members of both the experimental groups (of which there were two) and the control groups (also two). The first experimental group was given the avoidance test, then given the desensitization training, then the test again, then after 6 months the test was repeated to see if there was any regression. The second group was put through the same protocol, except that there was no initial avoidance test: this allowed Davidson and Peters to isolate the impact the training alone had on the reduction of avoidance score (between the second and third testing), without worrying about confounding interference from a person simply having done the avoidance task several times. The control groups, which were also divided according to how often they took the avoidance task, received no training in relaxation, thereby acting as further controls to test if the relaxation techniques were actually responsible for the reduced avoidance, or if repeating the avoidance task alone would improve the score.

The results clearly showed that the relaxation therapy had a significant impact on reducing the subjects' snake phobia, which was operationalized by the score between zero and 15. In one of the concluding sections of the paper entitled "Therapy Terminated and Unterminated" (an obvious reference to Freud's "Analysis terminable and interminable") Peters and Davidson drive home the point that one of the advantages of this rigorous approach, is that a subject's improvement is measurable, rendering it possible to tell exactly how much better a person's phobia is after any number of sessions, and to establish a point at which therapy could end (something which analysts were not always willing or able to do). The authors close their article with a three point conclusion:

> 1. It is not necessary to explore with a subject the factors contributing to the learning of a phobia or its "unconscious meaning" in order to eliminate the fear behavior.
> 2. The form of treatment employed here does not lead to symptom substitution or create new disturbances of behavior.
> 3. In reducing phobic behavior it is not necessary to change basic attitudes, values, or attempt to modify the "personality as a whole." The unlearning of phobic behavior appears to be analogous to the elimination of

> other responses from a subject's behavior repertoire. (Peters & Davidson, 1963, p. 525)

This conclusion shows how Peters & Davidson drew on the research data not just to advance a hypothesis, but to argue for a paradigm change. Thomas Kuhn argued that it is impossible to “falsify” a paradigm (Kuhn, 1996). Paradigms consist of the untestable assumptions which define the limits of a science. Those who fully commit to the paradigm can always marshal an explanation for the occasional anomalies that develop. Using anomalies to move a scientific community into a whole new approach requires that scientists interpret the data as requiring a new, more elegant explanation only available through the new paradigm (Kuhn, 1962/1996). In their conclusion, Peters and Davidson argued that a more elegant theoretical approach to *therapy* than psychoanalysis can provide results. They are testing psychoanalysis through the lens of quantitative science and finding psychoanalysis lacking. Thus they are issuing a rallying cry to look at psychotherapy interventions through quantitatively, which would in turn strengthen the support for thinking behaviorally.

While I could find no direct psychoanalytic responses to Peters and Davidson’s research, psychoanalyst Irving Bieber published a general critique of Wolpe and the “behavioral approach” in *The Journal of the American Academy for Psychoanalysis* (1973). In this article, “On Behavior Therapy: A Critique” Bieber accepts the impact of the behavioral method, but then argues that (a) its theoretical edifice is unsound, and (b) psychoanalytic thought can explain behaviorist results. In short, he accepts the addition of the method, but refuses to allow a paradigm change. Let me summarize his argument below:

Bieber noted several deficiencies in the behavioral model. First, he pointed out that there seem to be as many behavioral theories as there are behaviorists, and without a unitary approach it cannot claim to be a unitary theory (Bieber, 1973, p.40). He also pointed out that behaviorism is inconsistent. Consider the use of guided imagery in the above mentioned desensitization plan: does not such a practice assume that a person has a mind where he or she can rehearse future behavior? If behaviorism assumes there is no mind, why the imagery? Additionally, Bieber argued the stimulus-response sequence begs the question of what occurs *between* the two. He said:

> The basic point agreed upon, and which I think defines behavior therapy, is that neurotic and psychiatric behavioral symptoms are maladaptive bad habits acquired through somehow having learned them. The bad teacher (or situation) is the noxious stimulus—the bad habit, the response. The somehow, which is that vast area in between, has no place in their schema. (Bieber, 1973, p. 40)

It is this "area in between" that Bieber feels Freudianism adequately addresses. Because behaviorism does not, he considers it an insufficient theory. He then goes on to give examples of the clinical significance of not theorizing the "in between." For example, he evaluates a case Wolpe shares in one of his books on "assertiveness training."

> Illustrative is his report of a 32-year-old woman who developed a phobia of knives when she was 26 years old while still in the hospital following the birth of a son. She had the persistent fear that she might use a knife to injure

> her child. All knives had to be removed from her home. Wolpe concluded that her fear seemed beside the point, but she first had to learn to assert it before being desensitized about her fear of knives. (Bieber, 1973, pp. 42-43)

Bieber then goes on to give a very careful analysis of all that Wolpe has overlooked in this analysis, and that I would like to quote in full because the quotation shows the depth of insight provided by the psychoanalytic perspective:

> As a psychoanalyst, my assumption would be that the patient had had a postpartum, depressive reaction with an accompanying fear of injuring her child, a common symptom in postpartum reactions. A frequently noted fear during the pregnancies of such women is that something will be wrong with the child. Many normal women have such a dread but it evaporates with the birth of a normal baby. The later postpartum psychiatric syndrome may include the fear of injuring one's child. In my clinical experience with patients having a reaction like the one Wolpe described, the psychodynamics almost invariably involve a masochistic need to injure a highly valued object. The masochistically derived destructiveness is a defense against unconscious expectations of hostility and predation from the mother, a fear usually generalized to include other women. Wolpe's failure to associate the symptom with a

> postpartum reaction especially since the symptom appeared so soon after the delivery, and his exclusion of the psychodynamic processes inherent in postpartum reactions, led him to an erroneous interpretation and, I think, inappropriate therapy. (Bieber, 1973, p. 43)

Bieber closed his argument by asserting that behaviorism's popularity is due to its promise to provide a "quick cure," but that this rapidity is purchased at the price of good clinical work. Behaviorism as a theory jumps the gap between stimulus and response, and there by jumps over the essentials of good, lasting, psychotherapy. Bieber argued,

> We psychoanalysts welcome innovations that promise to facilitate treatment, and behavior therapy has developed ways of treating discrete sequences of pathological behaviors, i.e., symptoms. Stimulus-response learning theory may be of value to some of us as a way of ordering our observations or planning therapeutic strategies. Some of the behaviorist practices are easy to incorporate into existing psychoanalytic theory and practice. This sort of influence, however, has only to do with technique and is conceptually quite peripheral to dynamic psychiatry. Psychoanalysis is not likely to be significantly altered by the techniques of behavior therapy. (Bieber, 1973, p. 51)

Bieber's critique is a common example of the kind of evaluation made during paradigm shifts: Adherents to an old model offer reasons to maintain the paradigm, and instead encourage treating the new anomaly as merely an opportunity to augment the old paradigm. His insistence on the need to attend to the "in between" (which he adumbrated generally as "feelings, thoughts and emotions" (Bieber, 1973, p. 46)) is quite compelling, and I shall show that it will be addressed by future innovations in the field. However, the story we have been following is not just the scientific debate, but the corresponding professional debate, and it is here that Bieber's arguments fell short.

Bieber's response may satisfy those not wishing to abandon the psychoanalytic worldview. From the Freudian perspective, the behavioral model of the mind is surely impoverished. However, Peters and Davidson's conclusion does not argue for the behavioral paradigm to explain the mind and behavior, but to do therapy. Peters and Davidson argue, in the language of experimental psychology, that therapy can be done "better" (as understood through outcome measures—which are those most appreciated by experimentalists) than the psychoanalytic method. It is irrelevant if behavioral models can be reconciled with analytic ones. That is not the point of their argument. They are at odds with psychoanalytic case formulation, and do not see the value of it.

All of Biebers arguments above are rooted in his "clinical experience," which is not a valid ground which experimental psychology values for criticism. In this community, experimental data grounds claims. This is true not only because psychology is rooted in experimentalism, but clinical psychologists were not allowed to practice psychotherapy, thus *clinical experience was an impossible ground for psychologists to reason from at this time*. Peters and Davidson are arguing from a different ground (results found in an experiment) and providing outcomes which demonstrate that behavioral interventions are more effective. Because Bieber cannot refute them in the language of experimentalism, his arguments fall on deaf ears in such quarters. A new approach to clinical work was being piloted, and Bieber was not able to critique it *on its own terms*, rendering his critique an ineffectual preaching to the converted—i.e. those in his same profession.

Bieber didn't demonstrate that behaviorism sidesteps the question of what's "in between" stimulus and response. However, here too he came up against problems. First, those people to whom Peters and Davidson were appealing were not going to be concerned with this question. Second, Bieber's assertions of what *does* occur "in between" feels quaint and simply wrong-headed by readers today, encouraging any reader to side with the more skeptical and limited claims of the behaviorists. For instance, Bieber's assertion about "a masochistic need to injure a highly valued object" leaves many questions begging and was controversial even within analytic circles. Even more questionable were Bieber's other examples from his "treatment" of homosexuals. Bieber argued that behavioral methods for overcoming homosexuality, involving electroshock, were doomed to fail because they gave no reason for a homosexual to begin practicing heterosexual sex. The most they can achieve, he asserted, was "asexual" adjustment. Homosexuals have an inherent "fear of heterosexuality" (Bieber, 1973, p. 45) Bieber asserted, and this must be overcome through psychoanalytic therapy for behavior change to take place. His claim seems specious to this reader, and my sense is that both the behaviorist and the analyst who seek to heal homosexuality are confused about the relationship between sex and desire. So once again, Bieber offered an example of what is "in between" a behavior and its motivation, and it is lame.

In short, Bieber's argument is compelling for those who already see the world his way. But for those who find research, data, and outcomes more persuasive than "clinical experience" he offers little. Peters and Davidson offered much more. Most importantly, the underlying professional reality was that psychologists had little reason to listen to a critique such as Bieber's, because still *only psychiatrists were allowed to be psychoanalysts*. What benefit would there be to a psychologist to countenance Bieber's "clinical experience" if the psychologist is never allowed to have such an experience to assess it for him or herself? Thus, the scientific question of what should ground clinical research (the lab or the clinic) fell into a no man's land between the professions.

Peters and Davidson were among the first clinical researchers to begin to argue as they did, and they found a growing and receptive audience. One such professional was Randal Aaronson, who had replaced Lance Davidson as the clinical director at the Dubville Child Guidance Clinic. Aaronson was also feeling constrained by the hegemony of psychiatry. He was growing "disenchanted" with the analytic model he was compelled to use with his patients, as well as with needing to justify his competence to do therapy. In an interview he granted Baars in the mid-1980s, he cited the impact Peters and Davidson had on his own professional identity:

> There was always this thing that ultimately the only real doctor is an MD. In some cases the psychiatrists had to supervise the psychologists in doing therapy. No matter how senior the psychologists were, some young punk psychiatrist had to supervise him. I guess there was this undercurrent of 'looking for a thing on our own.' With

> behavior therapy, we had something to teach them—if we cared to, and we didn't decide that they weren't entitled because they didn't have the right kind of training. (quoted in Baars, 1986, p. 127)

Aaron's claims here are particularly telling, because by this point in his career he had already published a book on psychoanalytic developmental models. Thus, we know that at some level, he must have been satisfied with the theory enough to publish on it and expand it. However, the professional frustrations met in trying to act within the psychoanalytic frame pushed him towards experimentalism, and the authority it granted him as a professional.

Peters and Davidson's research demonstrated that psychologists can produce a new and evolving approach to symptom removal, and that psychologists no longer needed approval from psychiatrists to work effectively. This was a scientific and a professional coup. This research proved neither that the behavioral view of the mind was accurate, nor that Freud was "wrong." We've seen Bieber, among others, remained unconvinced. Further, Peters and Davidson admitted that not all of their research subjects improved through this approach, and that this approach would probably not work with patients experiencing a more "generalized" anxiety (Wolpe, 1958). However, if analysts wanted to convince psychologists that the domain of therapy must remain exclusively in the hands of psychiatrists, they would need to do so in a way that met psychologists on their terms: in terms of data independent of occupational jurisdictions (or let more psychologists benefit from the claims of "clinical experience," something we have already seen they were not willing to do). As I shall show, psychoanalysts continued to refuse to rise to this challenge, and this would be their fundamental undoing.

Tensions

The pay-off of moving toward an outcomes-based approach to treatment was significant. If psychoanalysts no longer held “cognitive control” (Abbott, 1988) in the mental health field, then psychology could establish its own terms for its work. And instead of merely mimicking psychiatry, psychology put forth its own approach. Peters and Davidson’s work has become one of the most-cited works in “behavioral therapy” (Davidson interview, 2008). Behavioral clinicians apply the scientific method in their clinical work just as researchers do in experimental work. Their focus is symptom mitigation, with scant attention, if any, to “total personality.” Behaviorism provided an elegant reconciliation of the scientist-practitioner divide, and oriented clinical psychology toward its own niche in the mental health sphere.

Some psychologists took to this new approach whole heartedly. For example, Randall Aaronson, noted above, eventually left the *DCGC* and obtained an academic position in a department whose clinical program is based entirely on behavioral therapy. Today he simply rejects clients who come with complaints of “identity problems”: “go to the applied philosophy department” he says (Baars, 1986, p. 133). Aaronson was completely committed to the vision of clinical psychology as a lab-based field. He spends much of his time absorbing research findings, and working to “translate” (Baars, 1986, p. 133-134) these findings into clinical applicability. This approach has become dominant in about half of contemporary clinical psychology, and for this group of practitioners, the scientist-practitioner model has provided a viable professional identity.

However, the lingering problem of the "in between" hovers around the edges of the field. Max Musselman, a faculty member at *UDub*'s program for the majority of the time under discussion, explains, "If you have a fear of snakes, behavior therapy can be helpful. If you have a fear of relationships … I'm sorry, I'm not sure desensitization is going to do much for you" (Musselman interview, 2008). While Aaronson abandoned clients who did not present problems that were in his behavioral purview, others in the field were not ready to abandon such cases. Humanistic approaches demonstrate considerable improvement for identity problems, and Peters and Davidson's seminal research was important for clinicians who work in the Humanistic psychology vein as well, although in a different way than it is for clinicians who ascribe to the "behavioral therapist" model.

One such clinician is Ned Regent, a graduate research assistant for Peters and Davidson's snake phobia research. Regent had already received an education in psychoanalysis while training for his master's degree, and was working in the field before returning to *UDub* for his Ph.D. "It was obvious I needed a Ph.D. if I wasn't going to play second fiddle to psychiatrists for the rest of my career," he explains (Regent interview, 2006). He was drawn to *UDub*'s program because of its rigorous scientific background, and because he was interested in moving beyond psychoanalysis: "My relationship with psychoanalysis is like Voltaire's with God: we are aware of each other, but we're not on speaking terms." Much like the other clinical psychologists interviewed, Regent found analytic concepts helpful, but limited for the direction he wanted to take his clinical work. Further, he shared the exact same phrase many interviewees did: “I wasn’t impressed with the outcomes” (Regent interview, 2006).

In graduate school, Regent learned Peters and Davidson’s brand of behaviorism. However, upon graduating, Regent went to work at a stand-alone clinic in Dubville, where he began to receive supervision in client-centered therapy. “It was like a revelation to me,” Regent says.

On the national scene, Carl Rogers had already explicated the fundamental premises of his “client-centered” or “person-centered” approach by 1951. By this time he had won recognition for effectively treating WWII combat veterans, and demonstrating treatment alternatives to psychoanalysis. However, the Rogerian approach had difficulty establishing a beachhead in academic programs for reasons that should be obvious by now: psychoanalysts held dominion over psychotherapy, and the psychologists preferred their rigorous behavioral approaches. In a field where analysis dominated psychiatry, and behaviorism dominated psychology, client-centered therapy was a “3rd Force” and thus access to learning about it was, and still is, an issue of location, and somewhat of luck (McLeod, 2003, 88 – 112). For Regent, an opportunity to learn about Humanistic psychology did not present itself until after he received his Ph.D.

The term “3rd force” was devised by Rogers, Abraham Maslow, and other psychologists trying to describe a psychology that was more appropriately “human” than either psychoanalysis or behaviorism. Like the psychoanalysts, humanistic psychologists grant that a person has an “inner life” that can be addressed in psychotherapy. However, unlike the analysts, humanistic therapists do not believe that the contours of human experience fit the procrustean bed of Freudian theory. Humanistic psychologists believe it is worth researching this “inner life,” and in fact Rogers and his associates provided many new measures of psychic states and processes (Rogers & Russell, 2002). Further, Rogers granted that analytic concepts can at times effectively describe how a person became the person “they are today,” that is, the person entering therapy (Rogers, 1961). However, he considered this irrelevant to the process of treatment, and treatment was Roger’s goal.

Rogers was one of the first people to tape-record his sessions, and then publish the transcripts with commentary and analysis. In these transcripts one sees a very different relationship to the “total person” than the analysts had. Instead of trying to post a “blank screen” and offering interpretations of past events and fantasies, humanistic psychologists focus on building a strong, empathic relationship with the client, and motivating them toward change in the present and toward the future. Like psychoanalysis, humanistic therapy’s training regimen typically encouraged its trainees to undergo their own therapy. Unlike the behaviorists, humanistic therapists were not expected to apply knowledge in doing their work. Instead, they brought their “congruent” selves to help establish a healthy relationship with their client, and it is this relationship which was assumed to be the *sine que non* of accomplishing mental health (Rogers, 1961).

Thus, much like psychoanalysis, humanistic therapy was interested in the “in between” that behaviorism ignored. However, it did not see that “in between” in the same way. Instead of sexual and aggressive drives, the humanistic therapies were much more philosophical—and proudly so—about their understanding of what falls within the cracks of an organism’s psyche, as well as being fundamentally more positive in their appraisal of human nature. It was a philosophy very much in line with prevailing American principles of individualism and can-do-ism. In his "19 Propositions" of client-centered therapy, Rogers asserted, among other points, that "The organism has one basic tendency and striving - to actualize, maintain and enhance the experiencing organism," and that "The best vantage point for understanding behaviour is from the internal frame of reference of the individual" (Rogers, 1951). Rogers encouraged therapists to empathically enter the patient's (or "client's") "internal frame of reference" in order to access that space of "striving." Like Bieber, Rogers agreed that it is not enough to condition a person to change, and that a person must be engaged at a deeper level. But he was more willing to enter into a warm and respectful relationship with the client to achieve this state of "actualization" than a typical analyst would have been—even one who professed concern for the "total person."

Ned Regent was moved by the humanistic approach to psychology, as well as the practice that followed from this approach—in which careful listening to the client was encouraged, as well as empathic connection, and facilitation of helping clients change their own behavior, on their own terms. However, he adds, "I knew what behaviorism could do." Thus he set to reconcile humanistic psychology with behaviorism, in order to maintain behaviorism's outcomes, while still doing work that more fully captured the "total person." In 1970 he published an article entitled "Client-Centered And Behavior Therapies: Their Peaceful Coexistence" (Regent, 1970), in which he succinctly described the division that haunted the department he had graduated from since the days that Billing Jaspers tried to snuff out the research program, and which continued with the researchers who belittled the clinical project as unscientific.

Regent began by arguing that the “therapist's view of his role as it relates to man cannot be ignored and, unless squarely faced and clarified, may prove to be the major stumbling block in a rapprochement between the two points of view” (Regent, 1970, p. 156). That is to say that simply sidestepping the “total person” does not resolve conflicts in the field, because the problem lingers.

> Client-centered therapists are deeply committed to the belief that man should be free to choose the direction in which he wants to move and that, furthermore, he is capable of making that choice. Behavior therapists, on the other hand, often sound callous in their implicit assumption that they know "what is best" for their patients. There is the danger that human engineers may take their role literally and assume the responsibility for deciding how appropriate behavior should be defined. Yet, to disregard what we know about behavior because of the dangerous ways in which such knowledge may be used is tantamount to denying ourselves the use of atomic energy because it enters into the fabrication of atomic bombs. One can only hope that as psychology grows in knowledge, psychologists will continue to grow in maturity and concern for the dignity and individuality of man. (Regent, 1970, p. 156)

Before moving on to the thrust of Regent’s argument, I would like to highlight the last sentence in the quotation above. Here Regent is distinguishing a science’s growth in knowledge, from its "maturity." This notion of "maturity," is similar to the psychoanalysts, who believed that their practitioners needed more than just research knowledge on mental disorder, but some other kind of development that is also moral and philosophical, and inherently a part of this kind of work.

Regent then went on to give a detailed description of his work with a "client" (client-centered language for a "patient") with severe agoraphobia and claustrophobia. The first 4 months of treatment were spent primarily teaching Wolpian desensitization, which he noted significantly improved the client's functioning. However, as these symptoms reduced, generalized anxiety increased for the client—the exact form of anxiety that Wolpe admitted desensitization does not effectively address. Regent, however, took this opportunity to subtly transition into humanistic/client-centered psychotherapy. He noted that he was careful not to "overdose" the client with anxiety through a "relentless probing of 'unconscious' material," (Regent, 1970, p. 154) but instead followed the client's lead. Interestingly, he uses behaviorist language to criticize such pursuit of the unconscious, arguing that such probing "conditions" clients to become scared of their unconscious, because every time it is elicited they are overwhelmed by anxiety. This argument is fascinating, because it endorses an open-ended humanistic approach to psychotherapy, by using behaviorist language to criticize psychoanalysis, *yet fundamentally endorsing the existence of unconscious process that must be worked with in therapy*. And throughout the whole therapy Regent measured the validity of his claims through reference to the client's (measurable) increased functioning. This was an integrated approach, for the total person. As Regent says in his concluding remarks:

> Thus, we finally have in our possession the tools given to us by the creativity and imagination of clinicians such as Rogers and Wolpe and by the dedication and perseverance of researchers. The everyday practitioners must use these tools wisely, without dogmatism and with an open mind to the welfare of their patients as an ever-present goal. (Regent, 1970, p. 160)

It is interesting to note that Regent's vision is still controversial in clinical psychology circles. During one of our meetings he told me, "they're still figuring this stuff out at APA!" (Regent interview, 2008). It also did not lead to a reconciliation in *UDub*. In fact, in the mid-60s, the clinic's director, Frank Gogle, decided to return the counseling clinic to the School of Education. Max Musselman explains that Gogle felt that "his group would be stifled by the increasingly aggressive experimental stance of the department" (Musselman, 1999, p. 37). Within a few years, the counseling program received APA accreditation to train clinicians, and thus *UDub* had *two* separate departments producing clinicians. The psychology department's clinic primarily trained its students to become "clinical psychologists" who applied laboratory knowledge on their patients, while the School of Education's program trained its students to become "counselors" whose expertise was gained through the more extensive clinical experiences the program offered. Here we see the same divide previously separating the psychoanalysts and psychologists (which is a better ground for scientific claims on psychotherapy, the lab or the clinic), but it is emerging within psychology. However, it is emerging between counseling and clinical psychologists, because counseling psychology was the only clinical ground available to psychologists at this time. I highlight this to show, once again, the science is not really resolved in these debates, because professional realities dominate what claims people are open to acknowledge.

Summary

Through the 1950s and 1960s clinical psychology underwent many transitions. One was its move into psychotherapy, which brought it into conflict with psychoanalysis. Another source of tension was the divide between researchers and clinicians within the ranks of psychology. Peters and Davidson were just one small part of this movement that tried to find a "scientific" means to bind the field against psychoanalysis. I shall soon show the success this brought clinical psychology. However, the "in between" remained a problem for scientific psychologists' attempts to become competent psychotherapists. This was a problem Lance Davidson was well aware of, which he struggled with throughout his career, and that he tried to reconcile when he became department chair through a new approach known as "cognitivism."

However, outside of academe, private practice psychologists like Ned Regent were emerging. These psychologists found that the psychiatric threats were mostly bluffs, and no malpractice suits were filed. One of the first psychologists to call this bluff was, Mendel Reardon, who was publicly lambasted by Robert Hanks for daring to practice psychotherapy without a medical license, found that his private practice became so popular that psychological testing actually became ancillary to his primary service: providing psychotherapy (Reardon interview, 2006 and 2008). Pragmatic psychologists who did not identify with psychoanalysis, behaviorism, or humanistic psychology exclusively, began to develop their own unique approaches to the problems of the psyche, which borrowed liberally from all of the paradigms vying for supremacy in the academy. All of these therapists expressed the conviction that they bring to their patients and clients what is called for in the situation, and that theoretical debates and turf wars are too limiting for actual clinical care. They appreciate the fact that experimental psychology challenged psychoanalysis, but few were ready to submit to behaviorism, either. What they wanted was the freedom to bring the best individualized care to each patient as was called for—to have the right to control treatment as *they* saw fit.

And, interestingly for our project, what links the various styles of these private practice psychologists (beyond their eclecticism and, of course, their status as psychology Ph.D.s) were those same factors that Victor Rotelle had noted as most important in the analytic psychotherapy at *DIMH*. And so progress in the clinical field seemed to advance *despite* ideological turf wars, through eclecticism and hybridity based on a judicious respect for what clients need and what "works" *in the clinical setting*—regardless of whether that satisfied the guardians of orthodoxy, analytic, behaviorist, or otherwise.

4 - *DIMH* IN THE 1970S: FLATTENING THE "TOTAL PERSON"

When Robert Hanks retired in 1968, he offered to help with the selection of a new leader in order to ensure a smooth transition. The University board declined his offer. The institute was running deep in the red, with legislators and administrators scuffling over accusations of elitism and lack of productivity. The board had decided that the Dubville Institute of Mental Health (*DIMH*) required radical change if it was to survive. Either *DIMH* would chart a new direction, or the institute would be shut down (Schachner, 1984, p. 296).

The University put together a search committee consisting of successful hospital directors from across the country. It took several years to find a replacement—not because it was difficult to find a qualified person, but because few qualified people wanted the job. Possible leaders expressed that the institution had potential to be a successful hospital, but it was clear that the analytic culture that dominated *DIMH* needed to change. It was also clear that this culture was so embedded that changing it required forcing a radical transformation few were willing to take on. Four years later, they found someone willing to accept the challenge.

Meanwhile, the relationship between *DPI* (Dubville Psychoanalytic Institute) and the University had become increasingly strained. In 1969, Albert Roland, the chief of psychoanalytic services, unexpectedly passed away, leaving the analytic program in disarray. His replacement was Matthew James, a soft spoken and insightful psychoanalyst who said of himself: "I'm not a leader." Although many of his students disagree with his self-appraisal, his was definitely an unfortunate personality in relation to Derik Teo, the man the University eventually hired to run *DIMH*, who made all of his decisions with a sense of the responsibility that a director of a medical institution must have to the institution and society in general.

Teo visited *DIMH* for the first time in 1972. The hospital was in deep financial difficulty, and Dubstate announced that deeper cuts were on the way. He described his first impressions as follows:

> I saw one funded research project, the temperature of the building was between 74 and 75 degrees, there were long coffee and lunch breaks, and many of the people were going into private practice. Most were analysts, with a few community psychiatrists. There were around 75 beds, and the hospital was unrated nationally. It received money from the state, but didn't really do anything for the state. (Teo interview, 2006)

But he also saw potential. A faculty member at Yale's medical school, Teo had greeted that school's recent shake up, during which analytic dominance gave way to a more empirically oriented department, very favorably. In addition, as a Jewish émigré from Hungary who fled from Nazi persecution, Teo harbored a general hostility against rule by authority. "I like facts. Schools of thought like Freudianism develop where there are no facts. As medical professionals, where there are no facts, we shouldn't be talking," Teo said (Teo interview, 2006).

He enjoys relaying a story from his early days teaching at Yale, when a few of his students complained to the department chair that he kept on responding to their questions by telling them he did not know the answer, because they were asking about things that were “still research questions.” The students felt that the analytic faculty gave reasonable answers to their questions, and that Teo’s skepticism was really just a cover for his ignorance. The chair responded that the students should return to Teo, take his hand, and kiss it, “Because he knows there are still things he doesn’t know” (Teo interview, 2006). This department chair recommended Teo to the *DIMH* search committee.

Teo accepted the job at *DIMH* only to discover on his first day that the state had gone through with its promised budget cut and *DIMH* was slated to lose half its funding. Teo immediately drove to the state capital to try and roll back the cuts. When he walked into the office of the legislator leading the attack on *DIMH*’s funding, the legislator invited him to session, where he was about to give a speech he wanted Teo to hear.

The legislator proceeded to give a presentation excoriating *DIMH* as wasteful. He attacked its analytic bias, describing analysis as an elitist middle-class preoccupation irrelevant to the community needs, and he castigated *DIMH* for focusing on research at the expense of service. Upon finishing his speech he met with Teo in his office and asked him what he thought of the presentation. Teo responded, “You’re wrong on one thing: they’re not doing any research there either” (Teo interview, 2006).

“I said: give me 6 to 8 months and we’ll get it up to snuff.”

It is a tribute to Teo’s charisma that the legislator decided to trust him. Immediately Teo gave 28 of his former Yale colleagues jobs at *DIMH*. (“They helped reduce my loneliness,” he says in his characteristic understatement (Teo interview, 2006)). With his trusted associates by his side, Derik Teo began transitioning this sleepy and divided analytic hospital into a state of the art research mill.

Teo envisioned *DIMH* as a top-ranked research institution. By research he meant scientific lab research based on objective indicators. (As one of *DIMH*'s most successful researchers notes, *DIMH* "worships at the altar of the random clinical trial" (Ebert interview, 2006)). This research methodology, implemented across campus at the *UDub* psychology department by Peters & Davidson, was already helping to reconcile that department and had helped bring in grant money. This vision of research was predicated on different assumptions about the mind and how best to understand it.

Teo implemented three significant structural changes to the department. First, to ensure research production, he immediately put together a Research Planning Committee. Second, Teo started an Office of Community Services, designed to improve the hospital's relationship to the State and the local community. Last, as will be shown, Teo radically changed the training and service structure at *DIMH*, dismantling psychotherapy's (and analysis') dominance. The cumulative impact of these moves was to eschew the "total person" while promoting better professional cooperation within *DIMH*, and greater rewards for *DIMH*'s professional audiences.

Research

Derik Teo began our interview explaining that the story of psychiatry since the 1960s is not about the rise of biological psychiatry over psychoanalysis, but "the rise of treatments the efficacy of which have been proven in carefully controlled clinical trials, versus the alleged efficacy of treatments that were never proven" (Teo interview, 2006) When reviewing his career narrative at *DIMH*, this is the story Teo is telling: How he rationalized services by linking them to good research.

What the Flexner Report was to psychoanalysts arguing for a strictly medical psychoanalysis, the Kefauver Act of 1962 is for Derik Teo. Remember: the Flexner Report, published in 1910, argued that medical education in the United States should be rooted in science, not "character." The psychiatrist analysts used this report to justify locking out nondoctors from analytic training. The Kefauver Act states that medicines crossing state lines must have proven "efficacy." This term was interpreted by lawmakers to mean that it had been tested in a randomized controlled trial (Healy, 2002). "This is what has changed in medicine – We used to believe in personal wisdom, now we have evidence," Teo says (Teo interview, 2006). Clinical data generated in therapy does not constitute evidence according to the dominant interpretation of the Kefauver Act, since it is not objective, controlled or quantified.

Just one month after his official start date at *DIMH*, Teo installed the Research Planning Committee (RPC). The committee evaluated current research in light of rigorous criteria such as: "intrinsic importance and creativity of the research hypothesis, the effectiveness of the research design and procedures employed to test those hypotheses, and an estimate of the past, present and future productiveness of the proposed research.' Priorities for funding were set, financial support for research increased, and test of quality assured" (Schachner, 1984, pp. 317-318).

No longer would the institute support research that the committee's director, Kevin Dershowitz, referred to as the "lone ranger type" (Schachner, 1984, p. 324). Instead, new research production would be multi-disciplinary. "An internal peer review mechanism was established for quality control. When encouragement was given for a particular research project, seed money and pilot funds were provided. Long-range funding was sought from multiple sources in order to establish larger programs" (Schachner, 1984, p. 324). Just as lab research had promoted reconciliation between clinicians and researchers in psychology, empirical research promoted better cooperation *across professional lines* at *DIMH*—and not just between clinicians and researchers. Dershowitz's vision extended across the medical field, including doctors, social workers, nurses: any profession with expertise which could be added to a controlled experiment. "Isolation is a problem—almost nothing can be done by a single individual. Even team teaching is necessary for most courses" Dershowitz explained to me during our interview (Dershowitz interview, 2006). "We became gender blind and degree blind very quickly. 40% of this department is nonpsychiatrists, it is one of the largest clinical psychology departments in the country. And one of the most academically driven departments. A lot of women. And we don't wear white coats—we have a very integrated way of thinking of things" (Dershowitz interview, 2006).

First and foremost, *DIMH* would produce "programmatic" and "evaluative" research: research that tested which treatments work best, and under what conditions. The goal was to find out which "treatment modalities that were in use were medically and economically effective" (Schachner, 1984, p. 354). Increased paperwork was kept on all hospital patients monitoring progress on any objective indicators available: mood charts, blood work, cortisol levels, etc. "There was a pre-admission checklist on patients, the inclusion of a complete treatment plan within forty-eight hours of admission, the integration of progress notes and medication updates, the determination of the appropriate length of time for hospital stay for a particular psychiatric syndrome (and review when the limit is exceeded), and the use of a computerized tool for systematic interviewing and taking of a patient’s psychiatric history. The development of an information system made data available on the parameters of quality care and patient flow” (Schachner, 1984, p. 337). A culture of assessment was being developed, in which service was increasingly linked to research on the quality and efficiency of the service (in theory).

Research production became the great leveler at *DIMH*—a means for creating a professional meritocracy in place of the old analytic hierarchy. “The hierarchy in *DIMH* is not so strong,” Dershowitz explains. “We’re supposed to pay MDs more than PhDs—it is negligible. Women are not paid less than men. Reimbursement has to do with grants. Rainmakers can be in any branch” (Dershowitz interview, 2006).

Dershowitz had been a student of Teo’s at Yale, and had worked as a bench researcher at both NIMH and Yale before Teo recruited him to join *DIMH*. Not only was Dershowitz an excellent researcher, but his experience with NIMH provided him with the know-how of applying for NIMH research dollars. This marked the beginning of a new trend *DIMH* would continue: pulling in researchers with federal grant distribution experience, and trying to get their researchers onto these funding boards.

“We were just a bunch of people coming from Yale who thought we knew everything,” Dershowitz reminisced fondly (Dershowitz interview, 2006). He quickly qualified this statement: they never thought they knew everything about mental disorders, but about how best to study them. “When I started seeing people, every person was a single case experiment. So I started looking to other areas of medicine, and began to see randomized controlled trials run in hypertension, asthma, cardiology—I began wondering: Why can’t we do this in psychiatry?” Kevin Dershowitz won *DIMH*’s first federally funded research project. In an ironic twist of fate, Dershowitz’s sleep research project quantified participants' sleep cycles in order to help understand mood disorders—exactly the kind of research that Robert Hanks had ridiculed earlier as "good energy spent in collecting trivia." Dershowitz disagreed, believing his research would identify those aspects of mood disorder impacted by sleep disregulation, so that clinicians could design appropriate treatments that restore the patient's equilibrium. For his part, Dershowitz characterized dream analysis as "a nice walk around someone's head" (Dershowitz interview, 2006). "We are trying to work psychiatry into a science similar to medicine—quantitative," Dershowitz explained (Dershowitz interview, 2006).

Modern medical empiricism as practiced at *DIMH* turns mental disorders into quantified and schematized problems. One of the primary means by which experimental results are analyzed and communicated are in visual terms. Charts on client's sleep schedules, changes on symptom scales, and the like take precedence over extended narratives about the history of various symptoms. For analysts, charting a symptom's improvement—such as a patient's anxiety getting worse or better—was insufficient; they wanted to know what the symptoms and changes "meant" and how they reflected transformations in the client's unconscious. By contrast, contemporary *DIMH* researchers are satisfied to note the diminution of symptoms, period. Data interpretation is limited narrowly to a patient's symptoms and associated variables, and is chiefly visual/mathematical rather than narrative in its structure. Progress is plotted on line or bar chart over time, or aggregated into some other visual representation with patient information. Symptom improvement provides a much simpler goal for collaboration than "discovering meaning." The clinicians need to agree on a method of moving a number up or down, instead of agreeing whether or not an interpretation of a symptom is accurate or not.

Modern medical empiricism blends with a biological model of mental disorders, and thus images of the brain also became plentiful at *DIMH*. For the past 30 years, *DIMH* has been one of the top producers of what Nancy Andreasen calls “broken brains”—visual presentations of what is wrong with the brains of people with mental health problems (Andreasen, 1984). In fact, increasingly such images have come to dominate the *DIMH* annual report, which during Hanks’ days was completely a document of text and data, and today is a full-color catalog, chock full of such images.

These images are powerful tools. Fontina Ebert, one of *DIMH*’s most famous psychotherapy researchers, was a television journalist on assignment when she first met Kevin Dershowitz; she did not even have a mental health background. After an hour-long meeting with him, Ebert felt she had had a revelation. “He told me about studies they were doing with cortisol and EEG – I didn’t realize that we could know stuff like this about the mind. But the idea that there were biological bases for psychological disorders was just true based on the data they were showing me.” Both Ebert and Dershowitz anticipate that one day brain scans shall be used to diagnose mental disorders (Ebert Interview, 2006, Dershowitz, 206). Dershowitz even performed a little scenario for me: “Imagine a patient coming to the therapist with their brain scan, holding it up, and saying, ‘look, doc, it’s depression.’” Treatment would follow from the objective, visual proof of the nature of the disorder.

Treatment

Clinical services adjusted to this new research logic. One example was the development of *DIMH*'s "module system" (Schachner, 1984, p. 392). Previously, inpatient, outpatient, and partial hospitalization had all been separate clinical services for any given patient. Now, multidisciplinary teams would work collaboratively with a given patient—sharing information throughout the various clinical encounters (1980-81 *DIMH* Annual Report). This change meant more than just treatment coordination; it was a complete reconceptualization of what the mental health professions would focus on in treatment. For the earlier psychoanalytically oriented psychiatrists, symptoms per se did not dictate the course of treatment. Unconscious impulses undergirded anxiety and depression. The effort to elucidate these impulses organized *DIMH*'s treatment structure; requiring individual offices for each faculty member and resident in which they conducted individual psychotherapy with each client. For the earlier psychoanalysts, internal dynamics matter. In contrast, under the new regime, patients were grouped into modules according to their symptoms: anxiety disorder, depressive disorder, schizophrenia, and so on.

To illustrate, in the late 1950s two *DIMH* faculty members published a paper on a group of alcoholic patients who all shared a "masochistic identification with mother" and a "domineering identification with father" (Boogle & Alter, 1959, pp. 62-67). The analysts were not particularly interested in the alcoholism itself: They did not quantify how much (or with what frequency) each client drank. The researchers also ignored the different affective states the patients passed through during treatment (e.g., happy, sad, etc.) More interesting to the researchers than the states themselves was the way unconscious family dynamics played out in patients' lives, resulting in different affective states at different times, as well as the same "surface" symptom.

In contrast, Teo's *DIMH* is completely oriented towards symptom removal, eschewing the analytic focus on underlying "etiology" (something which Teo says psychiatrists can typically only "speculate" upon, and has "no proven relevance to patient care" (Teo & Jarnowski, 1971, p. v)).

Symptoms were now conceptualized as "syndromes" with a logic independent of the individual patient's life narrative and dynamics. Expertise began to develop around these syndromes themselves. As the 1981 *DIMH* annual report explained, "The module system was structured to enhance health professionals' understanding of the biological, sociological, and psychological aspects of the *individual's illness* as well as to facilitate the development of longitudinal protocols in which the natural course of illnesses can be followed and studied" (*DIMH* 1980-1981, Annual Report, p. 3, my emphasis). For Teo and company, the illness, independent of the person, is the focus, unlike Hanks et al., who saw the illness within the framework of the "total person." Some modules addressed mood problems, some focused on cognitive dysfunctions, but each module brought an interdisciplinary approach to a clinical entity or process that was assumed to be stable over time: the syndrome of psychiatric symptoms. As mentioned, this module approach fit in well with the research methodology, which compared different approaches to symptom reduction, as well as seeking to understand the course and structure of symptom clusters.

Fundamentally, what binds a symptom to the concept of a "syndrome" is the presumption that the disorder is brain based. Thus pharmacological agents became the leading intervention at *DIMH*. As Teo said in *Modern Treatments in Psychiatry*, a psychiatry handbook he published in 1971,

> Because pharmacotherapy constitutes most of what is modern and effective in contemporary psychiatry, and empathy and common sense—psychotherapy's most essential ingredients—cannot in our opinion be conveyed by the printed page, we discuss drug treatment in greater detail than we do psychotherapy. (Teo & Jarnowski, 1971, p. v)

This is, of course, the opposite of Robert Hanks' opinion, that a symptom-focused psychiatry was out-dated in the Freudian world. Under Teo's leadership, *DIMH* psychiatrists could no longer get along with knowing one pharmacological agent well. In fact, such an approach did not fit within the medical psychiatric world view promoted in Teo's new *DIMH*, which asserts that all psychological problems are reducible to neurological problems. The appropriate purview for mental health intervention is the nervous system, whether through medications, genetic engineering, shock treatment, or other somatic interventions. Unlike the analysts who saw the brain as a part of a complex interlocking "total situation," for the reductive materialists the brain itself is like a central processing unit—all other aspects of psychiatry are secondary or peripheral at best.

Thus the analytic model, which considers medications as one tool in balancing a total person, was no longer viable at *DIMH*. Just as no family doctor would simply prescribe penicillin to solve all the ailments she may see (acid reflux, a headache, diarrhea, etc.), Teo did not see medications as an issue of doctor preference. Each disorder—and each client—had certain biological parameters, which called for certain biological interventions. Practitioners should heed this science of medications. *Modern Treatment in Psychiatry* provides many lists and tables for making diagnostic decisions. The lists include clinical markers such as blood pressure, pupillary reactivity, energy level, well-being, eating, sexual behavior, autonomic functioning, anhedonia, patterns of change, circadian rhythms and the assessment of illness, patterns of decompensation and remission, and sleep patterns (Teo & Jarnowski, 1971, p. 32). "Sexual fantasy" or "identifications" are nowhere to be found. Such information was no longer considered relevant.

"Psychopharmocology has improved psychiatry tremendously" Teo told me, pointing out that people with major mental disorders such as schizophrenia were once hospitalized upwards for decades, but now with medications "80% can be discharged in a few week's time. … There is nothing psychotherapy can do except teach and help you to cope" (Teo interview, 2006).

This is not to say that psychotherapy was completely eliminated at *DIMH*—far from it. *DIMH* continued to provide psychotherapeutic services, but their nature changed. Most noticeably, psychotherapy was "demoted" and handed over to master's level clinicians. The social workers who were once reserved for family psychoeducation (or, more typically, processing paperwork) were now performing frontline psychotherapy services. Fanny Victoria, one of Dubstate's first clinical social workers, remembers this as a very exciting time at *DIMH*. Previously, the social workers attended ample clinical training programs with the analysts, but were never allowed to directly utilize the knowledge. Further, many of the social workers were studying the emerging therapies of the time such as family therapy and strategic therapy on their own, and wanted the chance to try them out. *DIMH*'s new structure not only allowed that, but encouraged it. Teo's model no longer expected obedience to any particular clinical modality, but instead tracked clinical outcomes. And now the social workers (and other master's level clinicians) felt they could contribute. Social workers now provided individual, group, or family therapy in private offices. Clinicians consulted with each other on problem cases during team meetings, and experimentation was encouraged—as long as it brought results. "There were all sorts of approaches," Victoria explained, "some people were doing expressive therapy with kids, some were doing strategic stuff … I think there was one person who did Bowenian family therapy" (Victoria interview, 2005).

The composition of clinical teams changed in this environment. Previously, practitioners for each individual profession held weekly meetings: the psychiatrists would meet alone, as would psychologists, social workers, nurses, etc.. When collaboration did occur, social workers were informed that only the MDs could have a total understanding of the situation, and that because of "clinical confidentiality" this information would not be passed on to them. But *DIMH*'s new regime expected multi-disciplinary treatment teams to meet and discuss client progress openly. Each client would have a treatment team leader (typically an MD, but at times a psychologist or social worker) whose job was to organize and oversee the various professional efforts to improve service.

Training

DIMH's training model also changed. Initially, Derik Teo discussed maintaining the analytic institute at *DIMH*. However, only four months into his tenure, he introduced the new *DIMH* policies to the clinical faculty: no more analytic dominance of the research and training; a focus on empirically validated treatments and short-term therapies; and medications and outcomes research as central to the hospital research, training, and service provisions. And, most starkly: as department chair Teo would decide who provided clinical training, not the American Psychoanalytic Association. In fact, he insisted that he would pick *DPI*'s director, and *DPI* would answer directly to him, not the American Psychoanalytic Association.

The analysts were stunned. "No one can tell an analytic institute how to run who isn't an analyst," DPI's director, Matthew James, explained to me. "He wanted to come in and just take over—but we couldn't do that" (James interview, 2005). Shortly thereafter, *DPI* voted to remove itself from *DIMH* and become a traditional, stand alone institute. Many analytic faculty took this opportunity to quit—moving to other cities to join other analytic and mental health communities.

Teo had staged a professional coup, and much like the transition happening simultaneously in *UDub*'s psychology department, its justification came from the ascendancy of experimentalism over clinically based knowledge.

Andrew Richter's story sheds additional light on this coup. Recall that Richter had tried to become a training analyst when, by prevailing standards, he was still 10 years too young. He had continued to work at *DIMH* through this time, and was initially optimistic about Teo's entry. "Teo really moved us out of the dark ages," he says (Richter interview, 2005). Richter felt that *DIMH*'s treatment, training, and services were woefully out of date, and that its institutional ossification required a shake up. Further, he had always had an interest in psychopharmacology, and felt that a modern institution like *DIMH* should have a much better grasp of it. He hoped Teo would offer an opportunity for *rapprochement*: "analysis is good for understanding how people think. There is a place for this in psychiatry, especially in clinical work. I felt that if we—the analysts—really met Teo where he was, and showed him what we had to offer, it could have been great" (Richter interview, 2005).

Derik Teo invited Andrew Richter to run *DIMH*'s training committee (Levin, 2005a). He devised a program that attempted to mix knowledge in the syndrome oriented psychiatry that Teo favored, with regular analytic case supervision. Significantly, the greatest resistance came from the analysts. "They felt I was Teo's guy. Every analyst I called to help with clinical supervision told me the exact same thing: that they would not supervise this year because they did not believe in the philosophy of the program. So I realized that this had been discussed at the Institute, and they had agreed not to participate in the residency training. I still think that was a big mistake" (Richter, 2005).

Without any opposition from participating analysts, Teo took the opportunity to move the clinical training completely in the direction he wanted: psychotherapy training was almost completely removed from the clinical training. A few months later Richter was fired, because "I wasn't pushing the training committee in the right direction" (Richter interview, 2005).

“Teo changed what it meant to do psychiatry and psychiatric research. There was no room for what analysts did anymore,” says Richter. Nevertheless, he still holds that if the analysts had put forth the effort, they might have had an impact on Teo. Instead their retreat facilitated the consolidation of a psychiatric vision in which a client’s “totality” is simply not relevant anymore. Instead, clinical work at *DIMH* became outcomes-focused, with a bias towards reductive materialism. And without the psychoanalysts to argue for the relevance of psychotherapy, Teo moved the department in a *medicinal* direction. Medications reduce symptoms, and therefore became treatment of choice for *DIMH* psychiatrists.

In addition, Derik Teo brought a major transition in *DIMH*’s training culture. No longer were eager psychiatrists like Richter dissuaded from being “young and in a hurry.” Now such energy was cultivated. “When I interview incoming residents,” says Kevin Dershowitz, “I make it clear that a 40 hour week doesn’t work here. I’m not interested in 100 hours, but 60 makes sense. Everyone makes time” (Dershowitz interview, 2006). The department began winning federal Career Development Grants in droves, which encouraged young professionals to push themselves to rise in the professional ranks as quickly as their individual competence would allow (Dershowitz interview, 2006). However, they were expected to use their time differently than during the analytic days. Now, *DIMH*’s leaders would be researchers with large, well funded, research projects. For Teo, research training is the heart of good clinical training. In an article on the topic, he explained

> Regardless of whether the medical students’ career choices will involve patient care, clinical investigation, or

> laboratory science, the emphasis in their training should be on the critical examination of problem solving and validation methods to both prepare young physicians to handle the explosion of new information and to protect them from the embarrassing mistakes our generation has made. (Teo in Dershowitz, 1992, p. xiii)

Lawrence Kubie’s belief that only middle-aged doctors who were thoroughly analyzed can produce good research had no place in this environment. Instead, therapy was almost completely eliminated from the curriculum. Moreover, the program greatly diminished opportunities for psychiatry students to learn psychotherapy, and there was no encouragement for residents to receive their own therapy. All that remained of the old model were a few adjunct analysts who made their time available for the few psychiatry residents and fellows who wished to learn more about the approach. While these meetings could occur in spaces left vacant in *DIMH*’s building (a conference room in the library was a popular location) this was all done for free, and on the resident’s “own time.”

The Community

“He [Teo] always showed a real concern about what we felt we needed at the Department for Public Welfare,” said Nathan Scully, a Dubstate medical officer who worked with *DIMH* through the 70s and 80s (Scully interview, 2007). “If I needed him to come and testify about something, he would. If we needed a consultant, we’d always get one." Scully then adds, "I mean … he knew that this was getting him what he wanted too …"

"This is the side you don't hear about Derik" says Hal Trafford, a psychologist recruited by William Jax to help run the Community Study Center right as Teo was entering (Trafford interview, 2005). Jax recommended Trafford meet Teo so that the two could start with the same understanding. Trafford flew to New Haven to have the meeting, "He had a really clear vision of what he wanted *DIMH* to look like from the beginning," Trafford explained to me (Trafford interview, 2005). The first piece, as noted, was increasing its federal research production. The second piece was "to redo *DIMH*'s reputation in the catchments area and the broader mental health community from this ivory tower, analytic look-down-your-nose-at-every-patient stuff, to a high powered academic clinical operation that would help people. And for better or worse, he did that" (Trafford interview, 2005).

Trafford was never a "rainmaker" by Kevin Dershowitz's criteria. He never presided over any large scale research, nor sat on a funding board. However, Trafford had extensive experience in community mental health systems, and Teo turned to him to build this part of *DIMH*'s clinical services.

> Teo wanted to start something where people having acute mental health problems could come in—like an emergency room. We have this today, it's called the Information Resource Center, but at the time we really didn't know what we were talking about. There was a little emergency service, and that was about it. So Teo [and his assistant]

> called me in one day and said, “we want to create an *IRC*,” and I said “What’s an *IRC*?” They said, “We don’t know either: why don’t you go travel around and look at other models and see what you come up with.” So I did. So I structured it, and I was the first director of it. It became the intake room, the consultation liaison, admission services: it was a little bit of everything. (Trafford interview, 2004)

Below I shall discuss the *IRC’s* structure and relevance with more depth, but for now it is simply important to note that it was organized to create a community good, and to make the local community feel that *DIMH* was responsive to their needs.

The Office of Community Services (*OCS*) was the second institution established to reinforce *DIMH*’s relevance to the community. *OCS* was designed to “systematically plan, coordinate and implement educational, consultation and research programs throughout the Region” (Schachner, 1984, p. 340). *DIMH* faculty members were expected to donate an average of 3 hours a week of consulting or education in schools, community mental health centers, and state hospitals. *OCS* was particularly important for the large rural segments of Dubstate that have very few mental health professionals or services available. For the first time they could expect semi-regular consultation with Ph.Ds and MDs who would travel out to their location.

Just as *DIMH* put its researchers on funding boards to get an “inside scoop” on how to win grants, Teo encouraged *DIMH* faculty to get involved in state projects. For example, when the State Mental Health Commissioner came looking for help on a project, Teo paid Hal Trafford to work for the state 2 days a week (including the flight to the state capital.) After six months, the state asked for more of Trafford's service, and he began to work for them full time—all on *DIMH*'s ledger. "Teo just let me alone. Eventually I went off the *UDMC* payroll, and went completely on state payroll, and Teo put me on 'instant leave,' which meant I could come back to *DIMH* at any moment I wanted." Trafford is not naïve enough to believe this was a personal whim on Teo's part, but a calculation that a positive relationship with the state was good for *DIMH* (Trafford interview, 2004).

And it was. Within two years of his fighting off the enormous budget cut, the state was *upping* its contribution to *DIMH* by $500,000. By 1979, for the first time in state history, the governor was initiating an increase in *DIMH*'s budget, not just the legislature (Schachner, 1984, p. 368).

Extracting Psychiatry from Politics

Derik Teo also brought changes to *DIMH*'s community mental health component, one of the few of *DIMH*'s services that were popular with the community. Though the clinical staff felt ill-prepared to handle the severity of some of the cases, most agreed that the services were all around quite good (Trafford interview, 2004, Edmund interview, 2008). In fact, when Robert Hanks retired, only the community mental health program was economically solvent, due to the influx of state and federal community mental health funds. At the time, some felt that Robert Hanks had misspent these funds on his pet analytic projects. For instance, he used money meant for building a community mental health center to construct an 8 floor garage, in the basement level of which he put the center. ("I think the analysts got the parking spots," says Zachary Edmund, who worked in the center through his last years of graduate school.) Teo's goal was to use CMHC services to improve *DIMH*'s contributions to the community, and simultaneously to improve its relationship with the community.

In doing so, Teo had to confront the community psychiatry curriculum's dynamic orientation, and shift the philosophical orientation of the program. As previously noted, *DIMH*'s community mental health program was organized along the lines advocated by Gerald Caplan, in which "primary prevention" was stressed as much as "tertiary care." As one of *DIMH*'s community psychiatrists told a public audience in the late 1950s, "We have to take a look at the total situation in the total community" (Friday, June 12, 1959, P.G). "We must use the particular interest of every one of us to fit into the total interest of the community." Following this philosophy, *DIMH* set up satellite centers around Dubville which provided outpatient, emergency, evaluation, and day services, and its community mental health program worked to become more thoroughly integrated into the local community (Hanks, 1966).

We have already discussed the political problems that the Community Study Center's approach in the Sisterton neighborhood of Dubville fomented. This community health work also led to other political imbroglios. One came from the push to use "lay" or community people to perform various aspects of the work. These "paraprofessionals" held no official professional designation—they were typically intelligent, personable community people hired because of their knowledge of the local community. "Knowledge of the community was more valued than abstract scientific knowledge," explains Beverly Sharon, a counseling Ph.D. who worked in various community mental health organizations during this time (Sharon interview, 2007). "It was a little unfair, because we got paid more than the para-professionals, even though we did the same work. But they had the skills we wanted, and could work for this lesser wage" (Sharon interview). A paraprofessional was usually the first person someone engaging the health apparatus in the Sisterton community would meet. Paraprofessionals would go to community groups to give presentations on mental health issues. They also did the intakes at community centers, and some of the more established paraprofessionals would even provide psychotherapy services in their private offices.

Justine Patterson worked as a paraprofessional in the Dubville community mental health community for 25 years. She was originally hired as a community liaison between a mental health center and various community groups: "no one wanted to go to those meetings, but I was willing to. And I knew what was what, so I could help" (Patterson interview, 2007). With time she moved into providing education on pill administration to seniors, providing psychoeducation to community groups about anger management and conflict resolution, and even providing psychotherapy under the supervision of a psychiatrist. When asked what she thinks the program could have done differently, Patterson responds that she always felt "undertrained" and "unprepared" for her work. "I didn't let that stop me, though" she adds.

From Teo's perspective, the whole model of community mental health smacked of a dangerous deprofessionalization. "If doctors want to march for welfare moms on off-hours, that's fine. But not when working, then we should do what we're trained to do" he explained to me. "Community mental health was really a nasty legislation," Teo says. "It promised to solve all of these problems: poverty, racism, so forth, which are really out of the bounds of psychiatry. I am a technocrat. I believe in the most modern available knowledge and technology, narrowly focused on training as to what our priorities are" (Teo interview, 2006).

One of Teo's first acts was to fire all of the "streetologists" (his term for para-professionals.) At the time this caused much public consternation. Originally, the decision to employ nonprofessionals in mental health work was both political and ideological. Community people were valued because of their indigenous knowledge. Supervision cultivated this knowledge into a medically proficient approach, but the autochthonous insight of the community members continued to be valued and reimbursed. Paraprofessionals were groomed to be the community leaders who increased the emotional stability of the whole area.

Teo did not see it like that. "It's clinically inappropriate for community members to have their first interaction with the medical community be with an untrained lay person," he explains (Teo interview, 2007). He considered their street knowledge irrelevant to a patient seeking mental health treatment, and considered their lack of knowledge about medical issues, differential diagnosis, and the like ethically specious.

As money dried up for community mental health throughout the 70s, Teo further reigned in the community component. Increasingly, community mental health at *DIMH* (and thus, across Dubville) was about the provision of health services to alleviate symptoms, and not about "prevention." In 1975, Teo "consolidated" the program, removing many of the extended community teams. Many had hoped he would fight with the government to keep the money coming, but Teo did not feel this was in his purview. "I always worked to keep resources available, but I don't make such decisions," he noted to me during our interview. In Schachner's history of *DIMH*, she quotes Teo as saying

"*DIMH* subsidized some of the CSC's services, but this creates a severe strain on its budget" (Schachner, 1984, p. 344). In the mid-1980s, the CSC in Sisterton was closed, leaving only a few satellite offices, and otherwise leaving the community mental health program at *DIMH* centralized in the hospital itself.

The "New" Community Mental Health

Derik Teo and Kevin Dershowitz laid out their vision of a community-relevant psychiatry system in an article they co-wrote in 1975. This system is similar to what they ended up developing in Dubville, and thus the article is helpful for elucidating the underlying logic of the system.

Teo and Dershowitz began their article by noting that a good mental health system should over-emphasize neither psychosocial nor biological interventions. They argue that "until about 1960 treatment programs essentially followed two models: patients would receive either psychoanalytically oriented treatment[,] or a therapeutic regimen consisting primarily of the so-called biological therapies (such as drugs, electric shock and, in some instances, insulin coma) with little else in the way of psychological care" (Teo & Dershowitz, 1975, p. 608). They explain that this "polarization" in the field was unproductive, and that there is a "growing recognition that the patient's needs were not met by either a strictly insight-oriented psychotherapeutic approach or a purely organic approach" (Teo & Dershowitz, 1975, p. 609).

We are already familiar with many of their critiques of the purely psychosocial approach. They argued that psychotherapy is too economically inefficient to exist in a hospital setting, and that it is also not the appropriate intervention for many people suffering from a psychological crisis: “The majority of those who come to an emergency room for psychiatric treatment are in the midst of an immediate crisis and are seeking human contact, not active treatment in the conventional sense” (Teo & Dershowitz, 1975, p. 612). However, the “purely organic approach also proved disappointing, for it was soon discovered that unless the clinician is prepared to deal with the patient’s social and family-support structure to insure compliance even biological treatments of proven value are doomed to failure” (Teo & Dershowitz, 1975, p. 609).

In contrast, Teo and Dershowitz laid out their own vision for a psychiatry system. It was a vision of a large and extended mental health system, with satellite units and many interacting communication networks, all centralized in a community or university hospital, and its “information-reception center,” or *IRC*, which coordinates the system’s movement of patients and information. This, of course, was the system that Trafford was in the process of organizing for *DIMH* during this same time. The system was designed to take advantage of what a hospital has in Teo’s model: good equipment, experts, information technologies, acute care facilities, and so forth. The *IRC* is the central hub, and “single-portal,” of this system. Teo and Dershowitz explained:

> The purpose of the single-portal entry would be: (1) to register the patient upon entering the treatment system, collect the necessary demographic, fiscal, and clinical data, and pursue appropriate sources for additional information when necessary; (2) to have personnel available capable of assessing the patient’s needs and referring the patient to the appropriate treatment facility within or outside of the

> system; (3) to assure that all data relevant to diagnostic and treatment decisions reaches the facility to which the patient has been referred; (4) to monitor the patient's movements throughout the various treatment systems and maintain a continuously updated central record system; (5) to coordinate all auxiliary assistance from the community, public welfare, family, physicians, visiting nurse, and other personnel, thus insuring optimal care and preventing wasteful, multiple utilization of community resources; and (6) to conduct research to identify backup facilities and initiate action where the appropriate facilities are lacking. (Teo & Dershowitz, 1975, p. 613)

This was a system for moving people to the appropriate place for their specific syndromes and level of need. It does not seek to solve mental health problems by improving society, or to put people into acute treatment that heals them *before* returning them to the community. Instead, it is designed to catch people when their symptoms are worst, stabilize them, and then send them to the next station where they can improve. Teo and Dershowitz emphasize that much of the work done in such a system is psychosocial, providing necessary "human contact," instead of "active treatment in the conventional sense. The health-care delivery system's ordinate emphasis on medical expertise and its lack of social support and human services has prevented it from providing considerate and thoughtful attention to patients in need" (Teo & Dershowitz, 1975, p. 612). Teo and Dershowitz even grant that "approximately 10 percent may need rather extensive long-term individual psychotherapy" (Teo & Dershowitz, 1975, p. 615).

However, once again, this was not simply a more rational system of care than the previous one. It represented a fundamentally *different* conception of a mental health system than that which preceded it. In particular, the locus of treatment moved to the system itself, away from the clinicians who worked in the system. This is a system of care in the literal sense: the system itself is to do the "caring." As they say,

> [A]n aftercare program must stress *allegiance to an institution, rather than to a particular individual* such as the patient's physician or social-service counselor. (Teo & Dershowitz, 1975, p. 616. My emphasis)

Or, to put this same argument more succinctly, and as Teo reportedly explained it to an analytic adjunct faculty member who inquired about continuity of care in this new system, "the only continuity is to the bricks in the building" (Melton interview, 2006). Teo's model of community mental health places the expertise in the structure itself. Teo and Dershowitz explained their reasoning for this. First, they argued such an approach "is necessary because the teams taking care of patients rapidly change their composition, especially the ones located in community general hospitals or university-based teaching hospitals" (Teo & Dershowitz, 1975, p. 616). The rapidity of the change in the system requires that the system learn to hold more of the healing power in it, separate from the clinicians working the system. Thus the *IRC* replaced the individual clinician's expertise with a giant information system conveying the clinical insight necessary for treatment (especially through recourse to algorithms of care deduced from research.)

Teo and Dershowitz critiqued previous schools of psychiatry for over-emphasizing the role of the clinician, rather than psychiatric knowledge and the professional healing system. The Freudians were taken to task for thinking they can cure more than they actually could, but the "Meyerian psychobiological approach" also received unfavorable reviews. As they state,

> What both camps had in common was the conviction that the physician is the only important therapeutic agent, a conviction that was clearly reflected by the staffing patterns in nearly all hospital psychiatric units. (Teo & Dershowitz, 1975, p. 608 - 609)

They went on to argue that lack of extensive aftercare services was a "logical outcome" of this physician focused model (Teo & Dershowitz, 1975, p. 609), since clinicians were all pursuing their own ideas of how best to "cure" this mostly incurable person, instead of thinking of how to continue providing this person with access to supports to keep their symptoms under control.

Just as Teo saw the most important knowledge as generated in the lab, and the most important healing agents in "empirically validated" medications and interventions, he conceptualized a mental health structure in which the structure itself—rather than the practitioners—was the primary healing agent. This de-emphasis on the individual clinician as the source of "care" limited the clinician's recourse to experience and working heuristics, and increased his or her reliance on medical algorithms that anyone could administer if they were properly trained. While Teo and Dershowitz recognized that we were far from this situation at the point of their paper's writing, they believed it was merely a matter of time before their new approach prevailed. According to Teo and Dershowitz, we simply need to better study disorders, in order to understand their psychological and neurological components, and then perfect our interventions. Caring people in the structure help this process along, but nothing about the relationship between the patient and clinician is required except that they follow the diagnosis and treatment algorithm.

Within five years of Teo's hire, *DIMH* was emerging as one of the pre-eminent psychiatric research and treatment institutions in the country. One of the surest signs of *DIMH*'s ascendence was its designation as a "Center for the Study and Treatment of Affective Disorders" by NIMH in 1977 (Teo, 1977). As such a Center, it was entrusted to "coalesce and augment our [NIMH's] twenty-five ongoing research projects focusing on the treatment and diagnosis of depression, and to initiate new programs in the areas of human genetics, neuropsychology and neuroendocrinology" (Schachner, 1984, p. 360). This was not only a great honor, but signaled the size of its increasing research departments. At the height of Robert Hanks' leadership, *DIMH* received $400,000 in grant aid. By 1983, *DIMH*'s grants were well over $8 million. Grants came not only from the state and independent foundations, but increasingly from federal sources and private corporations. Ciba-Geigy Company, Eli Lilly Company, the Upjohn Company, Hoffman-LaRoche, and the Alcoholic Beverage Medical Research Foundation were just a few of the larger research funders in the late 70s and early 80s (Schachner, 1984, pp. 393-394). This money was used for research and treatment, and for biological and psychosocial interventions. For instance, a grant from the Gulf Foundation was used to build a lab in psychiatric genetics, as well as to develop the genetic counseling program—both of which were emerging subfields of psychiatry at *DIMH* (Schachner, 1984, p. 360).

Teo's success at *DIMH* brought him a promotion to become the Senior Vice Chancellor for the Health Science at the University of Dubville in 1982. Dershowitz replaced him as psychiatry chair and continued to follow his model within *DIMH*, while Teo spread his approach of a research-led clinical program across the Dubville University Medical Center. The model proved successful in these other arenas as well. Most important, perhaps, is how successful *DIMH* continued to be in grant-winning and growth after Teo left, demonstrating that his model outlasted his own presence. It was the surest sign of the success of the Teo model.

Tensions

Teo brought a paradigmatic shift to *DIMH*. He abandoned psychoanalytic knowledge, and the idea that psychiatry must attend to the "total" person. Psychiatry's purview was now a person's symptoms. The psychiatrist was an expert on these symptoms, and how to decrease them—primarily through the use of medications, but also with reference to a therapist (typically of a master's level training) if this seemed appropriate (though "appropriateness" has not been defined). This shift brought researchers into leadership in psychiatry, with clinicians as the deliverers of research knowledge. Both of these roles fit into the larger mental health system, which served the needs of the community by providing services efficiently, as resources became available.

However, despite its numerous successes, Teo's model also produced its own tensions. After all, for this system to work flawlessly, the following must be true. 1)

That symptoms can be treated independently of the whole person. 2) That laboratory studies generate sufficient knowledge to dictate treatment. 3) That psychodynamic knowledge is irrelevant for treating serious mental disorders. I hope to show that if these axiomatic assumptions are not in fact true, problems inevitably emerge.

Tensions with Research

Teo's ideal, in which clinical services are dictated by research findings, has not materialized as he hoped. This can be seen both in *DIMH*'s neurological and psychological programs. Under Teo, *DIMH*'s approach came to be rooted in biologically reductive neurology. But problem-free neurological intervention continues to be elusive. Though many of my interviewees stated that *DIMH* is not necessarily wed to a neurological model, and that their epistemological commitment is actually to "empirical data," experimental facts do not often lend themselves readily to clinical application, and at *DIMH* all experimental results are supposed to be explained biologically. *DIMH*'s burgeoning experimentalism coincided with increased reliance on neural imaging techniques, and images of the brain—as well as graphs charting the progress of various physiological factors such as cortisone levels, sleep schedules, and so forth. These are the "facts" which are respected. Nonetheless, as of yet, there have been no images, graphs, or equations that demonstrate the connection between neurological states and discrete psychiatric diagnoses. There remains a huge degree of latitude in how one interprets brain images, and no credible scientist today claims that we know what any major disorder looks like in the brain. One needs look no further than *DIMH*'s own researchers to confirm this.

In 1995, Kevin Dershowitz published a book entitled, *Modern Psychiatric Reflections*. The book is a collection of essays in honor of Derik Teo. The contributors are Teo's friends, students, and colleagues, and are also some of the most important names in psychiatric research in the last fifty years. Teo wrote the book's introduction, and ends his essay with the emphatic declaration that

> As we learn from ever more persuasive evidence that the mind and the brain are one and the same, we must—as practitioners of a specialty intent on evolving in a way that assures survival—abandon the false boundaries we have established in our game of king-of-the-mountain with other medical and health care disciplines. Our new strategy should be a decisive one, carefully conceived, meticulously executed, and dedicated to providing clinical and research training to create a new breed of psychiatrists who are truly neuroscientists. This, I believe, is psychiatry's future. (Teo, 1992, p. xiv)

The book features essays that are supposed to ground this dream of psychiatry's future, essays that present research on the status of our understanding of schizophrenia, mood disorders, psychopharmacology, and genetics—almost exclusively from a neurological perspective. All of the writers share Teo's dream. However, as good scientists they are honest about of the current status of this dream. Most clear is Bob Fairbanks, a schizophrenia researcher, who begins his essay stating:

> Two general precautions should be recognized before exulting in this soon-to-be-entered era of specific molecular mechanisms of psychosis:
>
> 1. The exact nature by which neurons are linked chemically into systems that provide the means for complex human cognitive and emotional operations are unknown. Thus, we are unlikely at any time soon to understand how such operations deteriorate in psychotic patients, even with more molecules at hand to study.
> 2. We can already be sure from the recent partial progress with the biochemical analysis of the Lesch-Nyhan defect … that even the precise identification of a genetic defect specifically linked to a behavioral/cognitive disease that is expressed through the dysfunction of an identified protein will not by itself 'explain' how the enzyme and the neurons that express it relate to the behavioral problem. In part, these problems are immense but, more simply, we may merely acknowledge that our understanding of the brain and the molecules and processes by which it functions remain at an improved but still primitive state. (Fairbanks, 1992)

These are two large caveats regarding faith in the power of neurological solutions to mental health problems. The role of the brain is not challenged by this statement—no one in the scientific psychiatric tradition has ever doubted that the brain plays a role in psychiatry's effects. Robert Hanks was emphatic that the nervous system was affected through therapy, but he argued that there is no one-to-one relationship between mental life and the brain. To Hanks, assuming such a relationship was speculative, and not empirical; it required positing "some unknown organic process the presence of which could not be demonstrated." It was thus more scientifically valid to work with what we do see: the mental life in front of us, suspended in the complex ecology that psychoanalysis disentangles. In fact, as I shall soon illustrate, the efforts to side step the mind, and work directly on the brain, has in fact produced as many complexities as Hanks predicted. Fairbanks' quote is honest: there are still many fundamental questions about the connection between soma and psyche. Perhaps the future of the field involves intervening in these processes—but we are far from understanding these processes sufficiently. Despite improvements, our knowledge is still quite "primitive."

The existence of so many questions about the relationship of soma and psyche explains why outcomes are essential to *DIMH*'s new model. In the absence of clear-cut etiology, outcomes are used to organize the neurological research by providing potential answers to the question: what produces change in symptoms? *Symptom change has become the grounds for evidence and theoretical justification* that the brain itself cannot (yet) provide. Outcomes provide guarantees that knowledge is moving in the direction of progress. There is little acknowledgement amongst *DIMH* researchers and faculty that the actual process that produces these favorable outcomes is unknown, and that the assumptions used to justify it are in constant flux—that having a limited degree of control over something does not mean you actually understand it.

I discussed cognitive research at *DIMH* with a psychology graduate student assisting a faculty member conducting brain images of schizophrenics. She described herself as one of many graduate students assisting with this faculty member's images, and stated that students typically used "their own paradigm" in the research (Anitas interview, 2005). That is to say, all of the graduate students work with the same data, but assume different processes are underlying the images. Such diversity of understanding about the brain suggests that this collaborative effort may be as much a "Babel" as the psychological theories they replaced. In Anitas' case, this was particularly acute, because she was distrustful of the conclusions the senior researcher she was working under was making, but continued to make images for him, since his grant paid her bills.

This same tension also emerged in psychotherapy research conducted at *DIMH*. For instance, one of the largest research studies at *DIMH* is centered on in Interpersonal Rhythms Therapy (IRT), a manualized and empirically validated treatment for bipolar disorder (Ebert, 2005). The model is rooted in the idea that people with bipolar disorder get knocked off of their social rhythms, which exacerbates their fluctuating moods. The therapy is a process in which a therapist helps a person realize the importance of a steady schedule, and then helps that person change his or her lifestyle towards this steadier schedule.

The researcher running the study, Fontina Ebert, is emblematic of the research leadership that Teo cultivated. She is a self-proclaimed "data manager"—regularly sifting through clinical information on the clients and the study, tracing various clinical metrics (Ebert interview, 2006). However, Ebert acts as more than just a research administrator, but also a clinical leader. As she explains:

> I like to ground my research in clinical information. Every week, I review what's going on with every client in the study with the whole research team: clinicians, d*OCS*, researchers and evaluators. Also, every other week we meet to have supervision groups. We watch videos. I try never to lose touch with the individual patient, and to have that real fine grained clinical sense of the patient, so that I can understand the data in terms of the individuals. In fact, I can often find mistakes in the data, because I know the patient. (Ebert interview, 2006)

This intense clinical atmosphere is akin to the analytic days, when clinicians met to talk about the individual clients and share conceptualizations. In fact, Ebert adds that "The best IRT therapists formulate dynamic hypotheses of what's going on" (Ebert interview, 2006). Ebert has her manualized therapy tested using some of the most reputable therapists in Dubville, many of them possessing PhDs in clinical psychology, and having upwards of 20 years experience. They come from diverse backgrounds including psychoanalysis, existentialism, gestalt therapy and Buddhism. The clients' "unconscious" is included in discussion and is presumed to be relevant for clinical utility. The client's existential anxieties and transference issues are discussed as clinical issues in the supervision sessions. It is these clinicians, engaging in this intensive collaborative project under Ebert's clinical leadership, who "validate" the manualized therapy. This manual is then distributed without any mention of the intense psychotherapeutic conceptualization behind the treatment's testing. The manual ignores the issue of the sophisticated supervision system that is superimposed over the research system. But one wonders if the treatment's success is really so independent of this treatment/research environment. The manual alone leaves out much of the concrete, difficult formulations necessary to help people change their life schedule—and it is here where clinical work is most difficult. Ebert recognizes the importance of well trained therapists, strong supervision, and thorough case conceptualizations utilizing traditions which are dismissed as "ideology" by the ruling dogma in *DIMH*.

Tensions with Treatment

The ambiguities inherent in the biological model of psychiatry have implications for the treatment provided at *DIMH*. This is not to say that medications do not reduce symptoms, nor that manualized treatments do not provide guidance for therapists working to help clients behave more healthily. The outcomes are certain. However, the research-led model does not provide as much ground for clinical work as promised, and thus the power for clinical decision making *still* rests with the clinicians themselves. As Madeline Verity, a psychiatrist working at *DIMH*, explained to me

> You know … you use that research, but really it's a process of trial and error. I mean, it's informed trial and error—and the medical education helps. You need to know how these medications will impact the organ system, and knowing that a medication worked with a certain population helps—but it's never a simple process. Every medication comes

> with side effects that are hard to predict, it takes time and trial and error. (Verity interview, 2007)

Verity disregards Freudian models, and is fully invested in biological reductionism, but nonetheless finds herself in a situation where the protocols of clinical engagement (i.e., "algorithms") provided are insufficient to guide treatment decisions. In her clinical work, which I had the chance to observe, she carefully attends to her patients' symptoms. She then chooses a medical intervention to address them. Thus Verity engages in the careful, slow connection with her clients that the analysts recommend. However, her connections are not made following analytic lines, but rather through using the language of symptom reduction.

Interestingly, the selection of these interventions is shaped by the same kind of "personal experience" reasoning that Teo disparages in the psychoanalysts, rather than the impersonal and anonymous algorithms derived from lab research.

In particular, Teo describes frustration with analysts who would always provide an explanation for why their clinical interventions failed, rather than reexamine their own theories. As he told me

> They always had these wonderful explanations: mania as denial of depression, or explaining the learning of every schizophrenic symptom. Then we give them a pill and it goes away: Now what? … What supplementary hypothesis will they give now? (Teo interview, 2006)

However, *DIMH* clinicians now use biological supplementary hypotheses with just as much regularity as the analysts. For instance, when I asked Madeline Verity about choosing a medication for one of her clients, she responded that although she had recently read a report touting Paxil's efficacy for a particular disorder a client faced, she shied away from using it because, "I don't really use Paxil that often … it's a very, uhm, dirty drug."

The term "dirty" ostensibly refers to Paxil's downstream effects, that is to say, its effects beyond the neurotransmitter system that one is trying to correct through use of the medication. However, no actual research shows that Paxil produces any more downstream effects than any other antidepressant. As psychiatrist David Healy points out, all antidepressants create multiple downstream effects that we are not yet able to disaggregate (Healy, 1997, 2002). In an unfavorable light, Verity's response may be characterized as what Healy calls "biobabble," where biology talk is used to justify clinical hunches. However, more generously we might point out that her 20 plus years as a clinician using these medications and following the literature make me believe that her hunch about which medication to use is probably correct. She is speaking from experience—though the talk of "dirtiness" is simply not rooted in "empirical" data.

And thus we see a "return of the repressed" at *DIMH*, where clinical expertise has reasserted itself willy-nilly, because research lags behind clinical realities. Ironically, Verity is prescribing drugs in a manner similar to the way that was advocated during Hanks' leadership—where doctors become comfortable with a group of medications, and use them as they see fit. Of course, now the doctors have more complicated rationalization systems, and use more than just 2 or 3 medications. However, medications are used based on their ability to reduce symptoms, and not because the clinicians really understand all of the underlying biological systems. And, sometimes, clinical choices are as much due to the doctor's comfort with the medication, as any lab-generated data.

Fundamental to Teo and Dershowitz's model of a psychiatric hospital system was that continuity of care should not hinge upon any one physician or person. Knowledge derived from lab studies was meant to replace individual clinician hunches, because it was presumed to be empirically validated. What we are finding is that this is not true: that the knowledge produced through lab research is distant from actual clinical work, and that individual clinicians still make the most important calls in treatment. In doing so, they do not necessarily refer to the data (although their public justification might) but rather to their *empirical experience* of the client and his or her self-reporting.

In order to make good treatment recommendations, clinicians feel they must understand how each patient responds to the medication "in depth and through time." Madeline Verity explains,

> Psychiatry has become more complicated. You've got to use medications and therapy—even the way you administer the medications is very important in this field, because there's a whole psychological dimension. … That's why I don't do 15 minute med checks, most of our cases are too complicated. (Verity interview, 2007)

Unlike the vast majority of her psychiatric colleagues, Verity regularly spends upwards of an hour with each of her clients, carefully checking how their mood, cognitions, and behaviors are reacting to the medications, as well as to their lived environment. She keeps copious notes on patient progress from one session to the other, and delicately works to parse out what effects might be caused by the medication, by the environment, or by some other confounding factor. Her individual knowledge of the client, their current life status and their experience with the medication through time, all inform her decision. This kind of clinical insight would be almost impossible to transfer to another clinician, because the connection between her and her clients is more than one of knowledge, but also of pure human connection, which she feels is essential to the healing. She even implies that the medication action cannot be separated from her relationship with the client. While Verity may admit the necessity of a strong personal connection to a patient, the system does not. This leads to the third important tension: the "system" itself.

Tensions with the System

Teo and Dershowitz's psychiatric service model conceptualized mental health problems as discrete biological problems best treated in specialized units, but also recognized that simple fixes are rare. These problems were chronic, and thus a system had to be ready for relapse and maintenance as much as treatment. The model was designed to fix the inadequacies of the previous community mental health model: it was more narrowly "medical" and extricated the mentally ill from the "uncritical application of social psychiatric principles" which politicized treatment.

In reality, however, extricating psychiatry from politics proved more difficult than Teo and Dershowitz had initially presumed. In fact, the rise of the biomedical vision of psychiatry came in step with a nationwide withdrawal of support for the maintenance of a competent human services network. And it was Teo and Dershowitz who argued such a network was essential for competent care:

> [T]he reasons for [the] inadequacies [in the previous system] were rooted in poor planning. … the majority of patients who are ill enough to be hospitalized tend to have a long history of marked social dysfunctioning and cannot easily reinsert themselves into the community without a *well-organized and well-funded network of human services*. (Teo & Dershowitz, 1975, p. 611, my emphasis)

This system never materialized. In 1980, Ronald Reagan became United States President and quickly passed the Omnibus Budget Reconciliation Act, which slashed community mental health funds, and eliminated funding for training and staffing grants (Grob, 1994). Reagan also substantially changed the National Institute of Mental Health, defunding programs oriented towards community issues (Pickren & Schneider, 2005). NIMH's psychosocially oriented leadership was replaced with scientists more in line with Teo and Dershowitz's biomedical vision (Pickren, 2005). In many ways, this change was a boon for *DIMH*. Federal grants for *DIMH* biological research projects exploded throughout the 80s (Schachner, 1984). However the "well-organized and well-funded network of human services" was compromised in this transition. Quite simply, federal funds that were once spent on such a network, were now directed toward biomedical research. The biological approach justified government divestment from essential community supports, because they could increasingly argue that these problems are biological and not relevant to social services.

Carl Witner was the Dubstate's Secretary of Public Welfare throughout the Mid-1980s. He had no background in mental health, and says of himself, "I never came in as an ideologue, I'm more of a manager than a policy guy" (Witner interview, 2006). With the federal government withdrawing support for community systems and for hospitals, Witner spent the majority of his time closing public facilities, and finding ways to "do more with less" (Witner interview, 2006). In particular, Witner was meant to move people out of institutions and into community care.

The Reagan administration strictly specified how medical assistance money could be spent, making accessing these funds very difficult for newly deinstitutionalized patients. Witner and the state administration simply could not find money to set up community or social support programs—the money was primarily allocated for medication payments, but no more. When asked if he felt this was sufficient, Witner replied that he was no expert on mental health, but had consulted with psychiatric experts and was reassured that medications in the community were the best possible treatment option for the mentally ill. In regards to the importance of the network, he noted that experts he consulted placed less emphasis on the importance of the social network, and therefore under tight budget constraints, fewer funds were directed that way.

Mental health service providers began to dry up across Dubstate, and in Dubville. In their place, *DIMH* began to open research driven, research funded specialty clinics. As Schachner writes in her history of *DIMH*, "research was becoming so well embedded throughout the psychiatric programs at *DIMH* that it was quite difficult to separate clinical service, research, and education" (Schachner, 198, p. 390). Schachner views this as positive. However, she does not address one serious flaw to research institutions:

consistent programmatic changes based on inconsistent research funding. If the mental health network was poorly planned previously, now it was not planned at all. “Programs come in and out of existence all the time, who can keep up?” a social worker who has been working at *DIMH* for 20 years told me (Levy interview, 2005).

This approach, which Teo brought with him as early as the 1970s, is today informally referred to as a “funding led model” by many of my *DIMH* interviewees (Levy interview, 2005, Banders interview, 2005). If a researcher has a grant to study bi-polar disorder, then the institute provides bi-polar services, and if a researcher has funds for a suicidal prevention program, then funds for that program exist. Once the funding runs out, so does the service. It is not uncommon for patients to be referred to services that no longer exist, or for patients to receive diagnoses based on the services available, rather than on what the clinician might feel best explains their symptom.

“Look, if I have a kid who is flying off the handle, and is probably doing it because early life trauma, but we don’t have a clinic for that, I can still call it bi-polar and get them treatment,” one social worker who wished to remain anonymous told me.

When asked if the program vets the patients prior to intake, the social worker asked: “You think they bother to tell the difference between RAD and bi-polar? The drugs work for both.” When I asked if this resulted in the best clinical treatment, the clinician responded: “the best services in this hospital are in the research programs.”

This is true. As noted above, take out the research component, and Fontina Ebert’s IRT research is structured much like an excellent psychotherapy service program. It uses the best clinicians, provides good supervision, and allows recourse to many different treatment modalities (medications, family and individual psychotherapy) to bring about positive change. However, ironically, once the research has “proved” the value of an approach, that approach is no longer available, as a new treatment is prioritized for research, and therefore funding.

A social worker who has been providing psychotherapy services at *DIMH* for over 25 years showed me a manualized psychotherapy approach for schizophrenia produced by *DIMH* researchers that she thinks is quite good. When I asked her if this is what she follows, she responded, “Well, I try a little bit, but we don’t have the resources here to do it right” (Mondrian interview, 2006). The research was produced at *DIMH*, but the service is not provided.

Just as analytic leadership led to problems with *DIMH* failing to provide services to the public at large, the research and science model fails to incorporate the best treatment modalities into a long term program.

Meanwhile, two forces have colluded to prohibit other providers from successfully incorporating these treatment services into their own institutions or practices. The first is the lack of funds for community mental health services. Quite simply, the public has stopped advocating for tax dollars to be spent on such services. Money is spent on research, but not on providing the treatment that the research validates. Facilities that could have provided mental health services to the public using empirically validated treatments have disappeared.

This highlights the fact that research is so lucrative that it is actually growing into spaces where services could (and should) be provided. Derik Teo’s leadership started a large movement within *DIMH* to expand and merge with other mental health providers. A few years into Teo’s tenure, for instance, *DIMH* merged with the previously independent Dubville Child Guidance Clinic. Over the years, it merged with other institutions, as a part of the significantly expanding University of Dubville Medical Center, which at the time of this writing is the largest nongovernment employer in Dubville (Levin, 2005a). Teo explained the reasoning behind the mergers:

> Why has *UDMC* grown as it has? If you want to run a good teaching hospital, you need to know about all sorts of things. You need to do research on novel approaches. In order to accomplish that, even rare diseases need to be over represented. You do this by joining with other organizations. It is intellectually and fiscally sound to have secondary and tertiary hospital affiliates with a medical center. The results are good, because it is economically sounder and it helps subsidize the school. 1 to 2% of funding for the school comes from state, most is research.

This quotation highlights the economic boost that research provides for *DIMH* and its research mission, but, it does not address the fact that this system can drive out direct service that is not connected to research. *UDMC*'s merger with a local Catholic hospital, St. Augustus, in 2002 provides a striking example (Glover, 2002). At the time of the merger, St. Augustus as a whole suffered severe financial straights. However, its mental health clinic was solidly in the black (Motley interview, 2007). St. Augustus provided outpatient and community mental health, and had a highly esteemed drug and alcohol rehabilitation service. Its director, Frank Motley, explained to me, “we were frugal, and put the resources where they needed to be: in the people who provided the services" (Motley interview, 2007). "You know," he added, "It isn't that expensive to provide therapy." However, once owned by *UDMC*, all of St. Augustus mental health services were shutdown and those services are simply no longer available in Dubville. Space spent on service represents lost dollars when the space could be used for research, because research funding is simply more lucrative than the small funding amounts available from the state or medical assistance money to provide services.

This strangling of services in order to grow research is a pervasive feature of *DIMH*. During the course of my research at *DIMH*, I saw many of their most respected child psychotherapists quit *DIMH* and move to other positions (mostly into private practice.) "It's just not clinically driven here anymore," one clinician told me (Banders interview, 2005). "Do you think anyone checks our notes here?" one asked me rhetorically, "I could put whatever I want on these sheets, no one cares what you do" (Aarons interview, 2007). These seasoned clinicians are replaced by young, inexperienced clinicians with very limited psychotherapy training. One of the administrators told me that the new crew "is better at moving paper," than working with patients (Banders interview, 2005). Entry-level clinicians are affordable, and the situation makes economic sense given the low level of reimbursement provided by insurance companies, particularly given that a large part of their job consists of referring people to research studies.

A high level *DIMH* administrator explained to me the situation, "every time we provide outpatient services at *DIMH* we lose money. It's just an unsustainable model." (Fantolli interview, 2007). Research is the only sustainable and adequate funding stream, thus most services are provided through research projects. Even if the services are good, they are neither stable nor enduring. The system has become erratic, chasing the next new miracle cure for mental illness, like Don Quixote chasing a scientific Dulcinea.

The situation frustrates some *DIMH* faculty. Ebert said vehemently to me:

> It used to be we could charge and be reimbursed for inpatient stays that were actually adequate for the treatment. People could stay 20 or 30 days—and people would leave well. But it became an impossible financial model once insurance stopped paying for it. (Ebert interview, 2006)

Ebert's complaints are telling, because it is her research on social rhythms that demonstrates the importance of life-style stability for those suffering from severe mental disorders. In fact, she herself has had some long-term patients for upwards of 20 years. "The whole idea of termination is absurd" she told me, "you don't terminate with diabetes, and these problems are just as chronic" (Ebert interview, 2006). However, Ebert is resigned to work with the system as it is, and try to do the best she can. Such pragmatism fuels much of her work. Before pursuing IRT, she had considered developing a family therapy manual for bi-polar disorder. She explained that the research is clear that having family involvement is best for severe mental illnesses. However "I was warned that I wouldn't be able to get enough participants" (Ebert interview, 2006). She quickly adds that if a family is open to participating in treatment, she always encourages such participation, "We always say that IRT is family therapy with one person in the room," but she did not want to be utopian in her ideals, she wanted to be practical.

Though IRT may be a pragmatic compromise, this is not the way it is advertised. In the 2003 *DIMH* annual report, Dershowitz asserts that *DIMH* is producing "paradigm shifts in the delivery of patient care and … advances in biomedical research that will provide the opportunities to make rapid progress in understanding neuropsychiatric diseases and improving therapeutics. *We anticipate that over the next 20 years our entire therapeutic armamentarium will be altered*" (Dershowitz, 2003, p. 3, my emphasis). But, if a stable social network is important, and if we still do not know how the brain works, and if clinicians still have to make complicated clinical decisions based on extended relationships with patients as much as from data (if not more), and if clinicians still need to understand "old fashioned" concepts such as dynamics in implementation of these various treatments—then what is the true impact of the solutions being presented?

Summary

It would be helpful to stop and take account of the transition that *DIMH* underwent in the 1970s. By the decade's end, a patient coming to *DIMH* was processed through a central node (the *IRC*), and then referred to a module for treatment. The module was designed to address that patient's exact disorder. The goal of treatment was to diminish symptom severity and improve the patient's functioning. Increasingly the treatment began with medication, and included a psychotherapy component as a means to encourage compliance with treatment regimen, but it was no longer assumed that healing occurred through unlocking an unconscious emotional conflict. The research that flowed out of *DIMH* was now outcomes-focused, with a minimal emphasis on explaining the actual etiology of the disorder. Increasingly the research became reductive materialist, and brain focused—a search for the right medication to fix a person's psychiatric disorder. Training became more medication and neurology based, with psychotherapy training steadily decreasing until it became virtually nonexistent (save for those few residents who chose to pursue such training of their own volition and at their own expense.)

This transition in service, research, and training came lockstep with a transition in hospital revenue streams. The hospital became increasingly reliant on research money, and the best services in the hospital were increasingly those funded as research. As state budgets tightened in the 1980s, *DIMH*'s services and growth were untouched due to its reliance on Federal and corporate research dollars, which actually grew in the 80s. Community mental health effectively shut down without the federal grants that kept it going. In its place, the federal government provided medical assistance funds at a much smaller rate, typically just enough to fund medications, but rarely enough to fund psychotherapy services (let alone a multi-service community mental health center).

The professional transitions are also important to note. First, psychiatry remained the dominant profession at *DIMH*. However, Derik Teo's psychiatry differed substantially from Robert Hanks'. Hanks' psychoanalytic model no longer fit the new biological/scientific model of the hospital, while Teo's was much more closely aligned with that of the medical community at large. Further, Teo's psychiatry was much less parochial and closed than Hanks'. Psychologists, social workers, and other members of the allied health fields began taking leadership positions at *DIMH*. Interdisciplinary teams became the norm in service, research, and teaching. A psychiatry degree retained its value, but under this new system, prestige accrued from one's ability to demonstrate expertise in an important area of psychiatric research or service, and one's ability to bring in research or government funding. Thus money and quantitative/outcomes research created a new research-based hierarchy, displacing the hierarchy based on indicators like "maturity" that were ill-defined and not easily visible outside of the psychoanalytic world view. The new psychiatric meritocracy ran on rules that were clear and defined: Research dollars linked to symptom change produces value. This stands in clear contrast to the shifting vagaries of psychoanalytic politics. This was a new system of professions, and with these adjustments *DIMH* excelled. Its revenue and size grew, as did its prestige and influence across the national (and international) health field.

What made this transition possible was the change in psychiatry's self definition, and Teo's understanding of his mission as a psychiatrist. Much of this chapter described psychiatry's emerging skill in sidestepping the problems that the analysts had created: the problem of how to adjudicate clinical competence, the problem of knowing what constitutes a cure (or "progress,") the way community psychiatry interweaves with political concerns. In short, all the problems posed by making the "total person" the focus of psychiatric work. After Teo, *DIMH* was not interested in understanding a person as a complex totality. Totality often drew psychiatry outside of its technocratic realm, into issues of social justice, life decisions, or any area outside where medicine excels: understanding how the body works, and intervening in its functioning to increase a person's social functioning. In contrast to Albert Roland's call for psychiatrists to master "an awareness of the sources of illness as they may stem also from his patient's psychological and societal aspects," Teo *narrowed* psychiatry. As he says in the psychiatry text book he co-wrote:

> [We] consider an individual to have a psychiatric illness only when we see evidence of social dysfunctioning coupled with symptoms of psychological and biologic discomfort, and believe that *the dysfunctioning and discomfort are caused by disturbance in the mechanisms that regulate mood and cognition.* (Teo & Jarnowski, 1971, 22-23, emphasis mine)

Thus the domain of psychiatric expertise changed with the entry of Teo. Instead of a "deep" psyche that must be mined for its underlying emotional conflicts, or whose socio-emotional environment needs to be adjusted for better stability, psychiatric symptoms were discrete, biological entities that could be described in clear, objective terms. It no longer mattered what was "behind" symptoms. The existence of symptoms, or rather their elimination, was now the focus of psychiatry. A patient's personal history no longer mattered much, especially if it was more than a mere recitation of predisposing factors (of a specifically biological variety.)

However, this form of psychiatry did not solve all the problems of mental disorder. The facts generated in laboratories, knowledge on brain processes, and the new medications did not truly result in infallible, indeed, even reliable protocols for psychiatric treatment. The new approach was good at producing the appearance of pragmatism that renders clinical judgment, and the rapport between doctor and patient expendable. But in the end it did not really result in a reliable abstract guide for mental health interventions. "Clinical facts" are by definition not generated in labs. Lab studies produce data about people in the aggregate, not real, flesh and blood individuals in all their concrete particularities. The mysteries and complexities of the “total person” continued to haunt the new experimental, quantitative, and outcomes-oriented psychiatry. Data about “clinical syndromes” trump clinical insight, and a kind of individualized hermeneutic approach returned to psychiatry (albeit, covertly.)

5 – PSYCHOLOGY IN THE 70S, 80S AND TODAY

We have just seen the changes *DIMH* underwent throughout the 70s. These changes involved jettisoning the "total person," extricating psychiatry from many philosophical and political debates, and focused on building a program based on objective, lab-based, science. I now demonstrate how the same structural dynamics played *outside* of this major institution. These stories shall deepen our understanding of the changes in the mental health professions over the time period under discussion.

University of Dubville's Psychology Department

The University of Dubville (*UDub*) continued to struggle with its clinical/research split throughout the 70s. A large percentage of graduate students continued to have an interest in clinical work and were attracted to research that struck the research faculty (committed to a lab-based epistemology) as lacking in scientific rigor. Meanwhile the clinical students felt much of the behavioral and lab-based research was out of sync with reality, and with what they wanted to do. In the middle of this controversy was Lance Davidson, who continued to hold to the view that science was the route to good clinical work. He continued to train his students in many clinical methods (and the scientific method), believing that science is one of the many inputs clinicians need to consider when intervening with clients, but that clinical work was about more than the application of facts, but also involved one's own insight and empathy—clinical faculties developed through training, and undergoing one's own therapy.

However, while Davidson held this conviction, he never produced any more research. Loran Peters, his partner for the groundbreaking desensitization research, moved on to another university where he produced some of the most celebrated clinical research of his generation. Davidson, by contrast, was content to train students in small groups, encouraging them to read widely in clinical traditions, and encouraging their cultivation of clinical acumen and insight. For a man who already has tenure this is a fine position to hold, but to the students this approach sent a mixed message. His clinical training directives flew in the face of the research faculty who expected more allegiance to research projects than to the clinical work. The clinical faculty did not value Freud, Sullivan, or other such readings that Davidson encouraged (to the research faculty such writings were "unscientific.") Thus Davidson, who had found a personal way to balance the clinical/research tension, was not really able to offer a persuasive or systemic solution for the department as a whole.

Davidson tried to alleviate this problem when he became department chair in 1974. His friend, Max Musselman, explains that Davidson hoped to forward two projects as department chair: mitigating the clinical/research split, and expanding the department's affirmative action program (Musselman interview, 2008). While this latter concern may seem extraneous for our purposes, affirmative action was actually exacerbating the clinical/research divide. Davidson was inspired by the Civil Rights era, and disturbed by the fact that the department had so few African American students (Musselman interview, 2008). He organized a comprehensive program that brought in many African American students. Most of these students were interested in pursuing the clinical track. Their entrée added a political dimension to the (already existing) clinical/research tension. Just as traditional *UDub* students felt that prevailing research methods were not providing information they needed for psychotherapy, many black students came in conceptualizing clinical psychology as a means to effect social transformation. Once again the term “clinical” was evolving: more than Witmer’s assistant to the schools, or Rogers’s counselor, or Wolpe’s behavioral consultant, the word “clinical” now meant modes of engaging and changing social structures. And, once again, the underlying assumptions and methods popular for clinical students of this kind were quite different from what psychology researchers were interested in.

One example was a project of an African American student who wondered whether teaching an Afrocentric curriculum to black high school students might improve their learning scores (Edmund interview, 2008). Unlike other clinical students at *UDub*, who joined their professors’ research projects in order to find a dissertation topic, this student developed the project on his own. Even more impressively, the project worked: students’ grades did improve after receiving an education more connected to their “own” history. However, the faculty were disappointed that the project was not set up in a way that could answer more “fundamental” questions about learning (learning, of course, is one of the central concepts of behaviorism). Further, the student was more interested in his successful education model than in continuing in academic pursuits. In short, much like the aforementioned Community Study Center in Sisterton, this research was felt to be too “activistic” by the researchers, and the researchers were felt by to be too out of touch to be relevant (Edmund interview, 2008).

In the domain of the clinical program, Davidson attempted to alleviate some of the departmental tension by having more of the clinical courses taught by local professionals who would teach adjunct courses. The motivation for this idea was two-fold. The students could study a variety of clinical approaches with people who are specialists in the work, while simultaneously freeing the clinical faculty to have more time for research (Reardon interview, 2008, Musselman, 2001). However, this had the unexpected effect of exacerbating the tensions within the department, because the two groups now had even less of an opportunity to work together. The tensions were so palpable that an American Psychological Association site visit in 1976 noted that the department's clinical program was "too diverse," a euphemism meant to address the fact that students were pursuing clinical projects that did not seem to comply with the department's research mission—and the tensions were showing (Sufax interview, 2006; Musselman, 2001).

A solution was finally found, similar to the one Derik Teo was bringing across campus at *DIMH*. The 1970s saw the emergence of a new movement in psychology research referred to as "the cognitive revolution" (Baars, 1986). Most historians of the era typically note that the "revolution" was really much more of a slow evolution, and occurred in several different areas of the field, as researchers grew tired of the constraints of pure behaviorism and wanted to start talking openly about internal states again. However, these scholars still shunned the psychoanalytic conviction that introspection could ground scientific research. Instead, they began to experiment with different objective measures.

The story of how psychologists began to settle on objective measures for internal states is a long and fascinating history separate from our story here (See Baars, 1986; Bruner, 1983). However, a general understanding of the principles at work is important, and it helps explain how cognitivism emerged as a conciliatory force within *UDub*'s psychology program. A good place to look is George Miller's groundbreaking work:

“The Magical Number Seven Plus or Minus Two: Some Limits on Our Capacity for Processing Information” (1956). In this piece, Miller points to different experimental results that continue to show that, on average, people can hold an average of seven (plus or minus) two arbitrary “chunks” of information. This finding identified a capacity for “short-term memory.” Behaviorists had shunned the study of memory, believing it required reference to undemonstratable speculations on the mind, and preferred instead to focus on “learning:” measured through objective behavior change. However, Miller demonstrated that there is a general human capacity for holding chunks of data, and that this fact is empirically sound. An experimenter can show a subject a series of disconnected data, and count how many of these objects could be held in the subject’s “consciousness.” This can be repeated anywhere in the world and the finding is the same. Here was an objective claim about something as stubbornly subjective as memory—it was the dawning of a new era in psychological research (Baars, 1986).

Interestingly, the man credited with grouping these disparate research projects under the name of “cognitivitism,” Ulric Neisser, was very influenced by humanistic psychology (Baars, 1986, p. 275). Neisser had worked with Abraham Maslow, one of the luminaries of humanistic psychology, and was deeply moved by Maslow’s critique of the “inhumanity” of behaviorism. Neisser’s 1967 book, *Cognitive Psychology*, sought to challenge the scientific psychology field to move more in a humanistic direction, without losing its grounding in objectivity (Baars, 1986, p. 273 - 284). The book became an instant hit in psychology circles, as researchers bought into the notion that objective measures on internal states were a credible form of research, and the disparate projects that were already doing this kind of work found that they finally had a name: cognitivism.

Fundamental to the cognitive approach, is the idea that thoughts and feelings are objective and measurable. In Millers's research it does not matter what the thought is (telephone numbers, people's names, abstract utterances, etc.): the size of the "chunk" does not really matter. Regardless, a person, on average, can only hold onto 7 (plus or minus two) of these chunks. Cognitive psychology offered the promise of an abstract language of mind that was quantitative, and free of the problems of meaning.

Therapists were quick to draw on this movement and began crafting "Cognitive Therapies." Most important in this movement was Aaron Beck, who published his groundbreaking *Cognitive Therapy of Depression* in 1979. Beck, an MD and a trained psychoanalyst, drew on the cognitive psychology movement to argue that psychopathology was due to "negative automatic thoughts" which can be altered utilizing behavioral principles (Beck, 1979). He put together a rigorous therapy which addressed thoughts and feelings. Like behavioral therapy, his therapy was not interested in understanding what was "behind" the symptom, but in using learning principles to change them. Beck's therapy "challenged" problematic cognitions as if they were isolated behavior. And like Wolpe and Rogers before him, Beck had data to show that it worked.

The emergence of these therapies was wholly embraced by *UDub*'s psychology program. "Cognitivism simply offered clinicians more things to do than behaviorism could. It gave more therapy options" Max Musselman explained to me in our interview (Musselman interview, 2008). The department faculty liked behaviorism's rigor, but recognized its clinical limitations. Cognitivism promised to address the full gamut of psychotherapy work. It seemed like the reconciliation that psychology had been waiting for.

In response to the APA site visit report, Davidson and the department's new clinical director, Carrie Sufax, restructured the program along cognitive therapy lines. The first step in this direction was to stop having students do the majority of their clinical training with community clinicians, and return their training to the department research faculty. Now, however, the faculty would be teaching cognitive therapy approaches, more relevant to clinical work, and less alienating to the students. Sufax's clinical training program downplayed the "open-ended" (Sufax interview, 2006) therapies, and argued instead that psychologists should be utilizing more "didactic" (Sufax interview, 2006) therapies (such as behavior plans, and skills based work). Only knowledge generated in a controlled environment was going to be taught and trained at *UDub*.

Mendel Reardon had been an adjunct clinical faculty member teaching group therapy to *UDub* students throughout the 1970s. Lance Davidson had personally asked him to teach the program, because he respected Reardon's work, and felt there was much to gain from working with him. Reardon utilized an experiential training model, in which the students learned group therapy by actually participating in a group as part of the class didactics. This was typical in the humanistic movement, similar in this vein to psychoanalytic ideas: clinical training needs to involve training in introspection and empathic connection. However, Reardon's group was removed as a clinical training option for the students. Instead, students were going to learn manualized approaches to working in groups—developing an internal sense for a group no longer seemed relevant, for clinical decisions were to be based on laboratory studies of what happens in groups, with clear steps to follow on how to interact in them therapeutically (Sufax interview, 2006, Reardon interview, 2006).

The clinical program made one more essential change during this time that had a profound impact on improving department morale: they no longer accepted students interested in becoming clinicians. “If a student came to me really interested in being a therapist, I’d typically refer them to the Counseling Program, or the program at DubCatholic,” explained Musselman, who was a part of the faculty making this decision during the late 70s and early 80s (Musselman interview, 2008). The department simply realized that they were accepting students with the wrong career goals for their department, and that clinicians should study elsewhere. For similar reasons, the department also decided to terminate its aggressive affirmative action program. As Musselman explains in the third volume of his history of the *UDub* psychology department:

> Almost 30 years after the beginning of our affirmative action program, it is still difficult for me to assess the overall effect and effectiveness of our program. It is certainly gratifying to look back at the number and the quality of the Black PhDs we produced during that period as well as the range of activities in which they’re involved. They include a college president, an APA director, and some academic and research successes, but by and large they are involved in programs or practices that are geared to helping inner city or minority populations. Certainly

> their service and expertise is needed there, but there is a real question as to the appropriateness of the training they received in our department for the kinds of services they provide. It strikes me that the most useful thing we gave them was a PhD from a prestigious and (in clinical) an APA approved program. (Musselman, 2001, p. 26)

Thus, like *DIMH* a decade earlier, *UDub* separated itself from those in its ranks with interests in depth, process, and social activism. This had positive effects for the department's clinical and research tracks. The clinicians and researchers both began to depend equally on the laboratory and the knowledge generated there. Further, the tensions in the department dropped substantially. Commenting on the contemporary feel of the UDub department, Carrie Sufax told me:

> I think this is the healthiest our program has ever been. We have a sense of a common purpose, and little infighting. *UDub* is a research institute. In the past we had students who would go into the community to be doctors, or just disappear off somewhere. Nowadays our students become professors—we are broader than the local community, we are a part of the larger community of learning. (Sufax interview, 2006)

Meanwhile, the department oriented itself to obtaining funds from the most powerful institutions of the time (the federal government and corporations) in order to advance "scientifically." Like *DIMH, UDub*'s research budgets grew dramatically, and lab space became a high capital commodity. The clinical students were now being trained to be clinical scientists who would train in empirically valid, and uniform, therapy approaches that could be used to train clinicians in a standard, and quantitatively sure, therapy approaches.

Tensions

Cognitivism offered a means to reconcile research and clinical psychology—giving better scientific ground for clinical work. However, cognitivism fell short of its promises—it did not provide a “DNA of mind” (Johnson & Erneling, 1997). The reasons for this are many. First, while cognitivism did identify facts about mind, it was not able to pose appropriate constraints on “uninformed, ad hoc hypotheses about the nature of mind” (Johnson, 1997, p. 9). The empirically demonstrated facts, like Miller’s above, did not turn out constraining the empirical projects enough to give a clear direction to the psychology field. The literature became flooded with multiple, all equally plausible, theories of how the mind works. Once again, the field began to splinter off into small research circles where researchers pursued their personal research interests, but without ground to assert that their particular cognitive model of mind would prevail as the true one.

Sufax’s own research demonstrates the mutability of empirical facts in the cognitive psychology system. Although an avid believer in manualized and behavioral approaches to therapy, Sufax never embraced the syndromal understanding of mental illness that was popular at *DIMH*. In fact, a book she co-wrote, entitled *Family Process and Developmental Psychopathology* (Cummings, Davies, and Sufax, 2000), directly confronts the idea that symptoms cluster into “syndromes [that] are categorical entities and, as such, they are relatively distinct from one another, with each syndrome having its own set of causes and correlates, a predictable developmental course and outcomes, and response to particularly, syndrome-specific treatments” (Cummings, Davies and Sufax, 2000, pp 347-348). Sufax has written powerful critiques of clinical entities like attention-deficit/hyperactivity disorder (Sufax, 2000) and oppositional-defiant disorder (Sufax, 1990) on the same grounds: arguing that the syndromal model is acontextual, and does not account for the developmental nature of human species.

Sufax’s work is important, because it was referenced frequently in defending federal early intervention programs such as Head Start. She explained to me that her work is regularly challenged in the scientific community by those who see symptoms as syndromal. Despite the fact that she uses rigorous methods to control for genetic variables and which still, to her mind, demonstrate the importance of context and development in psychopathology, some audiences are just not interested. She has found that in funding her research she has to find foundations and government bureaus who are already invested in context and development, and she provides them data which they can use as ammunition in their fights to keep social services available (Sufax interview, 2006). The research is a part of a political fight, and has not ended politics through recourse to unimpeachable “facts.”

Likewise, in therapy, cognitivism did not result in a uniform, scientific therapy approach. Kathy Dilthy, a graduate student at *UDub*, explained:

> The training now is pretty much up to you. We have weekly supervision which is as a group, and everyone can

> do their own kind of cognitive psychology. Some people just generally learn the theory and can work instinctually, some people prefer to really work with the manuals, and some people do a mix. The clinical faculty let you find your own way – I don't know, I felt lost for a lot of it. Especially since I mostly didn't like it. (Dilthy interview, 2008)

Once again, just as in *DIMH*, every individual clinician takes lab-based science and applies it in their own way, because the "facts" alone are not sufficient for actual clinical work. Dilthy, for her part, feels that the clinical training at *UDub* is, "kinda bad," and sought out extra training opportunities within the community. She admits, however, that she is one of the rare people in the program who would like to do clinical work upon graduation (albeit, as a small side practice while pursuing her research projects). She found an opportunity to assist running a process group with Ned Regent.

And this brings us to the central irony of professional psychology: of the psychologists I interviewed, none felt that the identity "psychologist" said much about them. Musselman's lament that affirmative action seemed to do little more than give black students some official status as psychologists, is actually shared by many of my interviewees—black and white. Every psychologist has carved his or her own distinct career, generally rooted in working with people, behaviors and mind, but none of them clinging tightly to their graduate school training identity. This in contrast to psychiatrists, most of whom still identify as MDs first—feeling that this says something about their professional state. In contrast, psychologists generally feel similar to what Hal Trafford expressed:

> I would tell you that very little of the specific content that I learned, either than research methodology questions, was ever really of any use in my career. Now, I had a small private practice for a small number of years, but other than that, no. It was mostly the notion that we had this Ph.D. which meant that you were a fairly smart person, and since it was in psychology people assumed it meant you had good social skills … but after that your career was your own making. A lot of what I would call the "no course" experiences, like running groups, working in the ER [on my internship] working with people in the [community] where there was lots of conflict, those kinds of experiences which were really outside of graduate school, were what gave me the skill set to do this stuff. (Trafford interview, 2006)

Trafford today is a professor of mental health policy and administration. Thus it is the Ph.D., and *its* professional status, that is most powerful in advancing careers and practice, more than any particular "disciplinary" approach. Certainly the research methods have been helpful (and more than one interviewee noted this to me), but as Sufax's conflicts with researchers shows, the methods do not imply any necessary reconciliation—many truths can be found using the same methods. The Ph.D. gives any individual clinician the authority to choose what he or she feel is right for a given case. How do they make this decision?

The response to this question is as disparate as the clinicians themselves, who pursue independent career paths, and generate their own understandings of mental health and mental health problems, and try to bring the best that they can for each of their clients. These decisions are rooted in science, certainly, but each clinician draws on the research that impacts the clinical issues he or she is working on, and which he or she finds relevant. A good example is Mendel Reardon, who has been very influenced by brain research on trauma and development (Reardon interview, 2008). "I think of what I do as integrating affective and cognitive processes," he explained.

> Therapy affects patterns of neural activity in the brain. Whether EMDR or process work, we are helping people change patterns of thinking and feeling—this is a change of how the brain works. We have had over the years many theories for this process, but it comes down to bringing about this change in the brain" (Reardon interview, 2006).

Interestingly, this articulation of the work as neurological does not impact the fact that Reardon's work, from the outside, looks like basic humanistic psychotherapy in the tradition or Rogers or Sullivan (who although not formally apart of the humanistic psychology movement, is considered a proto-figure in the field). The neurological talk informs his understanding, but the work itself is as it was when it was described without recourse to neurology. Meanwhile, Ned Regent admits that he 'probably should know more about" the biological bases of mental life and behavior, but "I never bothered enough" (Regent interview, 2006).

These men are just two examples, but they represent the way that once a person has a Ph.D. he or she can pursue their interests in improving their clinical skills and acumen. And, in many ways, these men are similar in their clinical approach despite their different interests in research and training. What grounds their work is a commitment to the psychotherapy practice that has been passed down from Freud, through Rogers, Wolpe and Beck—in which two people in a room converse, with the purpose of one helping the other. There are various ways of structuring and understanding this interaction, but no one approach has become dominant due to scientific criteria alone. Increasingly, central scientific institutions such as *DIMH* and *UDub* have undermined the epistemological validity of this interpersonal encounter. This action has improved their scientific credibility in some circles, but has also had a negative effect on their clinical training and psychotherapy services. And I believe this historical trajectory in says something about psychotherapy, and its relation to large institutions such as *UDub*'s program and *DIMH*. Perhaps there is something too multifarious about the mind and its healing, something that compels a dispersion akin, to what the multifarious nature of God did to the builders of the Tower of Babel. *UDub*'s program and *DIMH* are compromises around this work, finding enough common ground to work together, without providing the actual solid route to Truth. These are secular spaces where talk of ultimate truth are bracketed for the sake of creating relevant data, helping people, and so on. But working in these institutions alone is not sufficient if one really wants to do the work—there is still a need to leave these institutions and pursue the more idiosyncratic knowledge bases in mental healing. Spaces where professionals do a kind of "soul's" (Zaretsky, 2004; Zilboorg, 1941) work, working with more speculative knowledge, which helps them fill-in the gaps left by the constraints of the prevailing scientific worldview.

Today, outside of these institutions, Dubville possess many mental health training programs: gestalt programs, Jungian training institutes, Adlerian reading circles, mindfulness workshops, and the like. These outsider training programs are where many clinicians receive their Continuing Education credits and the clinical supervision which is necessary for licensure. Some of these outside programs are rooted in scientific data, and some are not—but they are all available for clinicians training and perfecting their craft. To close our narrative, I now turn to one of the most significant one of these outsider institutions: Dubville Psychoanalytic Institute.

Psychoanalysis

When one asks a Dubville Psychoanalytic Institute (*DPI*) member today about the institute's departure from *DIMH*, the following is a typical sentiment:

> It's all about funding and research over there. There's scant attention to clinical care, or about how it actually works, just funding. (Johnson interview, 2006)

These words were spoken by Harold Johnson, an octogenarian who received his analytic training while *DPI* was still affiliated with *DIMH*, and he has remained affiliated with *DPI* his whole life. From the perspective of many analysts, when Derik Teo brought a business mindset to *DIMH*, out went clinical care and competence. *DPI* wanted nothing to do with this, and left. (In other words, by their reckoning they were not fired, they quit.)

As has already been noted, a few months into his new post Teo explained the changes he hoped to bring to *DIMH*. *DPI*, led by Matthew James, responded by voting to leave *DIMH* and become a stand-alone institute. This stress on the institute could not have come at a worse time, since *DPI* was already losing analysts, trainers, and students, who were already frustrated with the Institute for reasons independent of the transitions occurring at *DIMH*.

"The atmosphere was oppressive," remembers Zelda Lucile. Lucile had originally trained as a physiological research psychologist, and had thought little of analysis. However, a life experience sent her into a two year analysis that changed her opinion drastically. Through her husband, an analyst, she met Albert Roland and expressed interest in joining the institute through the research track available to psychologists. The day after *DPI* received its accreditation, Albert invited Lucile to apply for the coveted psychologist research waiver. One might think that being invited directly by the institute director should have facilitated her entry to the Institute, considering her background. However, in psychoanalysis, nothing is simple:

> Getting admitted to the program was difficult! I had to assemble together everything I had ever written, 12 copies of everything—I'd written a book by this time! Above that I had to get recommendations. And then I had to be interviewed by something like 5 or 6 people—Some from the American [Psychoanalytic Association], some from

> *DPI*, some in Dubville, some in New York … and in all of that I had to be voted on by CORST [the Committee On Research and Special Training], and they didn't even know me! (Lucile interview, 2006)

The Committee On Research and Special Training was an "Athenian forum" (Lucile interview, 2006) that presided over the entrance of non-MD's to analytic institutes. Lucile later noted to me that one of the criteria they were searching for during the interviews was "analyzability," something which she thinks is "very hard to tell" and which meant for most of the faculty members that one was heterosexual. Lucile found these restrictions "daft, paranoid and limited," but underwent them because she appreciated the theory and wanted to learn more about it. "This is what you had to do if you wanted to learn psychoanalysis with the people who knew it best," she says. And while she seems to have little regrets about the experience (Lucile remained a part of professional psychoanalysis for the remainder of her career), the constraints in Dubville forced her and her husband to pick up and move to a new medical community where there were more career prospects for budding analysts such as themselves. These career opportunities included the ability to supervise professionals of all fields interested in analysis, and to "dare" and provide analysis for supposedly "unanalyzable" populations such as homosexuals, without the intention of converting them to heterosexuality (Lucile interview, 2006).

Thus, Matthew James found himself betwixt two crises: a psychiatric department in flux, and a psychoanalytic institute which was losing people, office space, and credibility—it was a task no one would wish for, let alone someone who does not enjoy being a leader. Interviewing Matthew James, I expected to hear him bemused by this period of his life, as if he'd been kicked out of a job. After all, Teo's maneuvering was nothing short of aggressive. There are credible accounts of senior analysts finding their office had been moved to a deserted wing at *DIMH*, nasty comments made about productivity and efficiency, and a diminished patience for analytic jargon (Johnson interview, 2006; Melton interview, 2006; Levin, 2005a).

When reminded of these stories, James shrugged and said that they are just examples of Teo's "brusk" style. "He's a rascal!" James says of Teo, "and I think what he's doing is dangerous—all of these chemicals passing for healing agents. But that's where things went with hospitals, it just wasn't for us anymore" (James interview, 2005). In a way, James was sympathetic to the changes Teo brought, and understood that they were necessary to save *DIMH*. But if *DIMH* was saved at the expense of analytic work, then it was truly no longer appropriate for *DPI* to be there.

There was talk of moving into another hospital and beginning an analytic service there, but few of the analysts were willing to put in the work to make it happen (Hoover interview 2006). Many analysts I interviewed explained to me that analysis draws personalities that are not necessarily builders of large empires. Analysis is small, sensitive work—delicate and humane, building large structures is not how its practitioners want to spend their time. (When asked, "What about Robert Hanks?" I was told by Harold Johnson that there was an apocryphal tale that he was granted his analytic accreditation with the provision that he never actually practice. "Could you imagine him practicing? He'd scare the patient!" Johnson chuckled. In all, Hanks' drive for leadership bristled many in analytic circles.)

Instead of seeking hospital affiliation, *DPI* opened the Psychoanalytic Center, which provided analytically informed psychotherapy to community members. The Center was staffed by social workers and psychologists who received free supervision from the Institute's faculty. Samuel Melton was on the Center's board and was also one of its main supervisors. He considered his work there as a service to the community, since none of the analysts were paid. The Center was popular in the community, and provided training to many psychotherapists who would grow to become prominent figures in Dubville mental health circles. However, the Center closed in the mid-80s, as the economic restrictions of managed care made even this model completely unworkable (Hoover interview, 2006; Melton interview, 2006).

Melton did not only supervise at the Psychoanalytic Center, but also at *DIMH*. As was noted earlier, Melton was told to only give the department what he wanted to give. Thus Melton has been running an elective case supervision group with *DIMH* residents and fellows for over 30 years. Melton has not been paid for this time, but feels he has seen his impact on improving the quality of the clinicians and researchers leaving *DIMH* (Melton interview, 2006). This group is still in existence today, and fellows and residents make their own time to attend. Matthew James also ran such groups, as did other *DPI* faculty and students. *DIMH* has taken advantage of this small continued connection to *DPI*, and proudly notes in its residency brochures that analytic psychotherapy is a part of the training. Of course, the university contributes no financial assistance to such programs. Today *DPI* is affiliated with the "Clinic Without Walls," a program through which *DPI* analysts supervise *DIMH* residents who are interested to learn more about analysis, and who volunteer to participate in the program.

As the analytic institutional existence became increasingly fraught, much *DPI* time was spent battling out small, internal, theoretical debates of little interest outside the world of analysis. Issues such as who could and could not be a training analyst; what exactly is the role of the analyst's own counter-transference in therapy; can psychologists be full fledged analysts; and so forth, riddled the institute with divisive controversy. These debates raged on as the numbers of those interested in analysis shrank. Matthew James himself got deeply embroiled in many of these controversies. Though James was the ostensible leader of *DPI* throughout the 70s, the "real power" over the institute was held by a cadre of analysts who James described as "shockingly orthodox" (James interview, 2005; Wilkins interview, 2007). The controversy was explained to me by Chris Wilkins, a student of James' who only made his acquaintance late in James' life. "All were ego psychologists, so certain in what they know—very skilled in the delivery of what they knew. Matthew kept asking, 'How do we know what we know?' but they didn't like these questions" (Wilkins interview, 2007).

It was during this most complicated time for the institute, that Matthew James began to write and publish. In his papers he discussed with meticulous and poetic prose, many of the ambiguities in analytic work: the analyst's own transferences, the sensations within therapeutic bodies and their impact on analysis, the thinking that one does away from the therapy settings (James, 2005). James was fundamentally questioning the same ground that Derik Teo was simultaneously challenging at *DIMH*: is introspection and the capacity for empathy an appropriate ground for clinical care and research? For Teo the answer was no, and he thus moved psychiatry to "surer" ground—the brain. James, likewise questioning, came to a different conclusion. For James, the insight provided by analytic practice makes it all the worth while. James readily accepted that much can not be proven about analysis, that it is slow, and that exact ‘outcomes’ about what it delivers are hard to measure on charts and graphs.   But these complexities attracted him to it. For Matthew James psychoanalysis is a way to learn about oneself, and the complexities of consciousness in a direct and rigorous manner that benefits from the insights of previous people who embarked on the same journey, but which is not prescribed in advance by their descriptions of the realms within.

James was interested in a variety of analytic approaches, and saw a danger in clinging to one approach, and the ossification of theory into a rote technique. Practice that follows an “algorithm” had little place in his clinical vision. For him analytic training should increase one’s capacity to ask hard questions, and “technique,” assimilated in a compliant fashion, is one of the surest ways to block the development of insight. He saw analysis as a broad theoretical approach to the mind, one that reached well beyond medicine. When asked if he thought psychologists could be psychoanalysts, he responded “I don’t think of myself as a doctor anymore, I think of myself more as a psychologist. Isn’t that what I do? Study the psyche?” (James interview, 2005). His work, pushing the boundaries as it was, was not appreciated by many at *DPI*, and he felt quieted by them.

Chris Wilkins met Matthew James in 2000, when Wilkins was invited to come speak at *DPI*. Wilkins was an infamous figure in the Dubville psychotherapy community. He was well published, taught courses internationally, and had a well respected private practice—all with private pay clients (a marker of success in the analytic community). Scandalously, he only had a master’s degree, and no professional license of any kind. (A true 60s radical, Wilkins refused to undergo many of the rites and passages that are required to become licensed at any level, and instead dedicated himself to his craft as independently of official institutions as he could muster.) Through word of mouth, his publications, and his teaching, he acquired a formidable reputation and continued to work.

Wilkins had known about *DPI* for years, and though very interested in psychoanalysis, had avoided participating in it. "I hated *DPI*," he says today. "It was very medically oriented, politically conservative, and held itself apart, and *above*, the broader mental health community" (Wilkins interview, 2007). He shared some choice tales that demonstrate the closed-off nature of the Institute

> Once I went to a *DPI* seminar on transference. The presenter always wore the exact same outfit to his sessions, so apparently everything the analysand thought was a projection! This was ridiculous, but everyone just sucked it up. Also, I brought [famed psychoanalyst] Christopher Bollas to come speak in Dubville and I invited them to the lecture, but they requested a private audience from him. He refused. He said that an interdisciplinary group was an audience of peers—they disagreed. They grant themselves a sense of superiority they don't merit. (Wilkins interview, 2007)

Thus, once again, someone who was attentive to the analytic model of the mind felt excluded from the institution that was ostensibly assigned to advance it. And the reasons for the exclusion seemed ad hoc and territorial.

However, Wilkin's publications were earning him attention, and thus he was invited to speak. After his presentation, James approached Wilkins, saying "We have a lot to learn from each other" and asked for some of his papers. The two of them started meeting regularly to discuss Wilkins's manuscripts. Through these interactions Wilkins became increasingly impressed by James, and moved by his approach to psychotherapy. It was a powerful mentorship that deeply affected Wilkins (Wilkins interview, 2007).

A few years later, Wilkins attended a conference on relational psychoanalysis (one of the "outsider schools" according to the orthodox Freudians in Dubville), and found that "every single presenter quoted James, but he wasn't there. When I asked why he wasn't, I was told that since James was associated with 'The American [Psychoanalytic Association]', they just assumed he wouldn't want to come" (Wilkins interview, 2007). In short, it was analytic infighting again.

Wilkins returned to Dubville and wrote James a letter, inviting him to compile his papers into a book. Wilkins could tell from the conference that such a collection would be well appreciated, because James' work was widely read across the psychoanalytic spectrum. Nonetheless, James declined.

"He had really suffered in the American," Wilkins explains, "He had no idea people were reading his stuff. He thought it was radical and marginal—hardly worthy of publication" (Wilkins interview, 2007). James had to constantly fight to publish his papers, having been regularly rejected by American Psychoanalytic Association journals for "publishing his counter-transference" (Wilkins interview, 2007). He thought that publishing a collection would require changing them radically in order to fit into an approach that the publisher would accept, and he did not want to do the work. "He was once told that they would publish his work if he made it fit more with the object-relations theories which informed his thought—he hadn't even read the object relations school!"

James sent Wilkins home with a flat rejection on the book offer. Wilkins' response was to write a book proposal and send it to the Analytic Press, to be published in their series on interpersonal psychoanalytic thought. The project was instantly accepted. "I called Matthew and said, 'You're either going to love this, or hate this'" (Wilkins interview, 2007).

In planning the book, the series editor came down to Dubville to meet James. When the editor explained to him the significance of his writing for the relational school, James was floored. "He really had no idea people were reading him," Wilkins explained (Wilkins interview, 2007).

A collection of Wilkins' writings was published shortly before his death in the summer of 2006. The book was presented at a *DPI* function in which some of the most important analysts from across the Dubville community attended. In the room were Jungians, object relations theorists, Lacanians, and orthodox Freudians—all came to celebrate an important career.

The *DPI* of today is very different from the one that left *DIMH* in 1973. For instance: access to training analyst status has become more transparent and more objective (causing a few of the old-line trainers to withdraw from the training committee in opposition) (Johnson interview, 2005). Also, psychologists and social workers are now accepted for analytic training without much fuss. In fact, psychologists have in many ways taken over the institute as medical psychiatrists have mostly chosen to pursue other professional routes. The new psychologists brought with them their humanistic sensibility, and today psychoanalysts at *DPI* consider their work with the “total person” in a more humanistic way than the one Albert Roland articulated in the 1950s. One example is Christy Collingsworth, who was *DPI*’s first psychologist president. Collingsworth says, “I really think of my self first as an existential phenomenologist, and then as a psychoanalyst. Freud helped us see so much about existence, but that is what we are really working with” (Collingsworth interview, 2005). Like other psychologists, she feels she learned little about clinical work while obtaining her PhD, and had to learn clinical knowledge at the institute. She credits James with teaching her how to do psychotherapy.

This shift to what we might call a “psychological psychoanalysis” is clear in other ways. No longer is there a strong opposition to noting the analyst’s counter-transference in treatment, nor that the analyst gets involved deeply in the treatment. In fact, the education has shifted tremendously. First, instead of only teaching classical-Freudianism and ego-psychology, the institute now teaches four schools of psychoanalysis: Classical Freudianism, Self psychology, Object relations, and Attachment Theory. There is also regular interaction with Jungian and other “outsider” schools. When asked how such communication is possible, most every analyst I talked to now feels these old boundaries between paradigms were of little value. “We are not as concerned anymore with coming to The One answer. What really matters is how one takes up clinical material,” Harold

Johnson explained to me as if this was obviously true, and not a hot-button controversy throughout the history of psychoanalysis.

DPI's current president, Ford Hoover, one of the few physicians remaining at *DPI,* discussed with me his vision for the future of psychoanalysis, and *DPI,* which is a 180 degree turn from the orthodoxy of the previous generation: "Analytic knowledge has a value in many domains. It is perhaps limited in its therapeutic role, but the knowledge is very helpful and needs support." Hoover went on to explain a program that *DPI* is now starting in order to provide instruction in basic analytic theory to teachers, counselors and social workers. And he proudly noted the 'Matthew James Psychodynamic Psychiatry Program,' a one-year program on analytic basics for professionals who do not wish to become analysts. Psychiatrists, psychologists, social workers, and laypeople participate in this course, hoping to gain the benefits that a psychodynamic world view can have in their work.

However, this is not to say that all analytic eccentricities have left *DPI*. For instance, debates about what is "essential" in analysis continue to vex *DPI* members. Further, a premium on age and "maturity" continues to exist in analytic culture. Even Zelda Lucile, who bristles at much of the "daft" analytic rules, intimated to me that she prefers attending specialized study groups with "grown ups" (Lucile interview, 2006). Likewise, Howard Johnson, whose paper on community psychiatry understood in an analytic way was turned down from an analytic journal because of its "impure" use of analysis, has come to accept the rejection in his advanced years. "You cannot take what is going on in the intimate psychoanalytic relationship and apply it 'out there'" he explained to me in our interview (Johnson interview, 2006). Johnson feels that the analytic hierarchy at the time was trying to stop him from making a mistake, which he could have benefited form heeding.

The general sense is that with age, an analyst has a better sense of their own dynamics and those of others. The wisdom of time is simply something that analytic culture is committed to, and they do not care if it smacks of gerontocracy—because it strikes them as true. And in a stand alone institute, such commitments are fine, because they do not upset the "younger is better" attitude which dominates in today's corporate health field. As a standalone institute psychoanalysis can develop as its proponents see fit, according to whatever constraints they want to pose upon themselves. And it seems to be working, because their workshops are still attended, they still graduate new analysts, and they continue to do their work.

6 - CONCLUSION

In reflection, Robert Hanks' psychoanalytic "empire" resembled a religious one akin to the Holy Roman Empire: nominally connected by a large central philosophy, but in reality split up amongst fiefdoms and principalities with little cooperation. In contrast, Derik Teo did in fact establish an empire in Dubville and—if one includes the various satellite offices and research centers—across the world. This empire is much more akin to Thomas Jefferson's description of the emerging American empire, where a procedural unity allows diverse philosophies and approaches to link together and grow.

Hanks use of the word "empire" is fitting for the approach I have taken in this dissertation, in which I have told the story of the development of one mental health community as a sort of political history of the professions. To run with this metaphor just a bit further: this history at times almost feels like the "Great Man" vision of history, in which the stories of individual "Great Characters" have been used extrapolate from in order to discuss larger structural shifts which occured simultaneously. The small actions of my "main characters" (Robert Hanks, Derik Teo, Mathew James, etc.) have been treated as causal forces in Dubville's history, with impact on the larger community as well. However, as should have been clear, this has been merely a technique for turning the individual specificities in Dubville, into examples of larger themes. Following cultural historian Kathy Newman, mine is not a "Great Man" version of history, but a "Nebish" version of history, which is to say that it focuses on "everyman" leaders. It is these professionals who worked at the leading edge of social forces, molding them, and excelling in them. Derik Teo is not Charlemagne, and I hope to have never given the impression that he single handedly shifted American psychiatry. However, he held a key role during a key time, and made key decisions that impacted mental health research and service delivery. He worked in a space that mattered, and thus what he did mattered. And through documenting what he did I feel I have learned much about the science and health politics of the Post-War World II period. This is likewise true of the rest of the "characters" in this dissertation, whose narratives intertwined to give us a clearer view of the historical landscape than was previously available.

In concluding, I would like to list some of the more important insights discovered in this dissertation. I do not feel the list below is definitive, and I certainly hope that a reader will find their own interesting implications from the narrative. I feel that in good history, the narrative captures verifiable events and reasonable dynamics. However, in any given historical epoch, there are too many dynamics to capture. I had to make choices on which story I would tell. This is true in two ways. First, even when simply considering the facts I did document, there are many ways of interpreting their significance, and below I will only highlight the ones that captured my attention as the most salient. Second, there are also many factors that were important over the historical period documented that I did not capture, but which are important. For instance, the rise of managed health organizations, important development in family therapies, and the internal tensions between school and clinical psychologists (just to mention three examples) are all important forces that I did not document. There are many similarly important stories that were not documented in my dissertation, and my only excuse is that this project needed a boundary. I hope that future researchers consider documenting such narratives either in Dubville, or in other milieu that can reveal much about the impact of these dynamics on mental health science and service.

However, in the research that I did pursue, I think some important phenomena emerge that are worth noting explicitly.

1. The transitions in mental health were not just scientific advancements, but changes in the "object" over which mental health professionals are considered to have expertise.

The psychoanalysts considered the "total person" their object of expertise. They felt that through Freud they had an understanding of the total dynamics of the mind and "soul." This led to many difficulties for them. First, knowledge of the "total person" did not lead to direct interventions that solved mental health problems the way various professional audiences hoped they might. Second, totality led to a focus on forces not necessarily amenable to traditional medicine's area of expertise (somatic interventions into the body's biological processes), and thus drove psychiatry's knowledge base away from its primary disciplinary habitus. Third, "totality" drew psychoanalytic psychiatry into various philosophical and political debates that did not successfully win adherents to its worldview. And last, Freud's theories proved more problematic and contradictory as time moved on, so that the field itself split amongst many different fundamental theories, thus complicating attempts to bind the professionals in a unified front.

For the research psychologists, the "total person" was not as interesting as applying the experimental method in order to generate scientific knowledge. This led to tension within psychology with those who wanted to apply its knowledge, and who recognized the importance of "totality" in doing clinical work. And these clinicians, in turn, had a tension with the psychoanalysts in how to conceptualize "totality." (The psychoanalysts, of course, believed that Freud had the last word, while most of the clinical psychologists felt that Freud may have been a first word, but there has certainly been more to say about the subject.) Through various turns discussed in the dissertation, the clinical psychologists claimed expertise over "behavior" first, and then "cognitions" later. These objects were asserted to be more objectively verifiable than those claimed under psychoanalytic expertise. Also, these objects were deemed to be more directly mutable through standardized procedures.

The neurological psychiatrists were also interested in having expertise on a more "objective" object, and dovetailed with the experimental bias of the psychologists. However, the object of expertise claimed here was the nervous system, or simply "the brain." The idea was to intervene on this object using medications or other technologies (neuro-surgery, electro-convulsion, etc.) to reduce "symptoms," which became the clinical marker of choice for the experimentally oriented professions (psychology and neurological psychiatry). Symptom reduction allowed psychiatry to fit better with the larger medical structure, as well as to accomplish goals that relevant audiences had for psychiatry.

It is important to note, that the concept of the "total person" was never disproved, but sidestepped through these professions. This leads to the second important conclusion:

2. "Sidestepping" the "total person" resulted in a radical transition in the very hierarchy of the mental health system of professions

In the 1950s, the psychoanalysts were thought to have the knowledge of the total person, and thus they held clear hierarchy in the mental health system. The rest of the professions were to serve their efforts. This resulted in many people not receiving services (since there are very few fully trained psychiatric psychoanalysts), and those people who didn't fit the psychoanalysts' interests were pushed aside to "lower" professions such as social work and psychology. Hypothetically, those who were getting psychoanalytic care were getting the best care, while those not having such access were being handled by less qualified practitioners. However, the reality of the situation was that even those being treated by psychoanalysts did not obtain simple, rapid cures. Further, the cases not being adequately treated by the psychoanalysts were of such large numbers, that other approaches had a significant client base with which to show the success of their own approaches.

In contrast, *DIMH* today is not structured with psychiatrists at the top and everyone else below them, but follows a hierarchy with *researchers* at the top, and other practitioners below *them*. This has been a result of eliminating "clinical expertise" as the ultimate reason for leadership at *DIMH*, and instead privileging laboratory generated facts on symptom removal. In *DIMH*'s new system, hierarchy is "depersonalized." In theory, the best knowledge is leading the treatment, and the arbitrary whims of an elite caste are not in charge. In Teo's vision, the clinician is armed with the best knowledge science can generate on mental disorders, and he or she brings it to help suffering people.

The clinician does not expect him or herself to have some insight into the nature of the soul, but merely in how to alleviate its pain causing symptoms.

However, a nagging question remains: in this system, is "depersonalized" knowledge about mental illness alienated from the realities of mental suffering? Are symptoms really so isolable, or is this merely a way to conceptualize the task for the professional reasons identified above? Do we really know the exact technocratic pieces necessary to treat mental health problems? Or is a "systems" approach fundamentally misaligned with the realities of mental suffering?

This leads to the next conclusion:

3. The "total person" continues to cause difficulties for mental health services despite having been ostensibly "sidestepped."

Pills and imaging have not yet resulted in perfect psychiatric solutions: drug regimens only rarely solve a problem completely, side-effects can themselves cause serious concern (under-discussed in my dissertation, but already well documented in other psychiatric histories and analyses (cf. Breggin, 1994; Healy, 2009, Olfman, 2006; Olfman, 2007)) and a gap exists between psychopharmacology theory and the way the pills are actually used in day-to-day service delivery. In fact, the notion of the total person highlights many contradictions in the projection neurological psychiatry posits of itself.

Throughout my research, I interviewed no one who thought that the idea of the "whole" person was wrong. In fact, every professional I met believed that their particular approach captured the parts of the person that mattered. For instance, while Albert

Roland argued that the general and neurological psychiatrists were hiding from the full humanity of their patients by turning to chemical interventions, Derik Teo had a diametrically opposed understanding of which psychiatric approach more properly attuned itself to a person's "totality." Teo shared a story with me about the horrors his analytic instructors expressed when he dared to touch his patients during his evaluations. He touched them, he explained, in order to do a good physical examination, as well as to show them the compassion of human touch. His instructors warned that this could inspire sexual transference reactions in his clients. With his hands waiving in the air, Teo expressed to me how ideologically callous and arrogant this position was. I felt thoroughly compelled that Teo's dedication to his patients as people was sincere.

However, without getting into a debate about which approach is more properly "humane," my dissertation has shown that transitions in the mental health system itself have impacted the ability to treat the total person *in ways that outstrip the efforts of any individual professional*.

This is nicely demonstrated through a story shared with me by Andrew Richter, about an incident that occurred in 1973, shortly after Teo took control of *DIMH*. Richter had been participating in a psychotherapy discussion group consisting of a wide diversity of clinicians from disciplines ranging from psychology, psychiatry, social work, and the humanities. One of the psychologists had participated in Lance Davidson and Loran Peters's research on desensitization, and was explaining the basic technique to the group members. Under the dominant psychoanalytic worldview at the time, only total personality reconstruction could effectively eliminate phobic symptoms, so the group was understandably curious to see this novel approach in action. They invited the psychologist to find a phobic patient whose treatment the team could monitor.

The psychologist was referred an African-American woman in her mid-30s who had a fear of the dark. He did not want to bias his evidence by talking to the woman and effecting a cure independent of the behavior plan, so she simply came in and told him a bit about her troubles, to which he responded by crafting a desensitization plan and sending her on her way.

Meanwhile, without the group members' knowledge, the woman was simultaneously referred for evaluation and treatment in the newly established biological unit of *DIMH*. She was referred there, because she had indicated in her paperwork that she had fainted once when she was 14, suggesting the possibility of physiological problems with her brain. Thus, independently of the behavioral intervention, she was also enrolled in a biological study.

On her second visit to the behavioral psychologist, the patient pleaded with him to converse with her outside the behavioral treatment frame. The psychologist responded that a conversation could sully their treatment, but she insisted. He eventually relented, and she told him her story. When she was 16 she had gotten pregnant, and her family responded by rejecting her. As a single, disenfranchised teenage mother, she took on housework in order to afford to raise her baby. She completely dedicated her life to her child, losing all interest in men. Instead, she gave all of her attention to her daughter. However, when the daughter herself turned 16, she started developing an interest in boys. Despite her mother's apprehensions, she started dating. In response, the woman started dating a man, who eventually moved in with them. One night, the mother got up in the middle of the night to go to the bathroom, and realized that the boyfriend was not in her bed. Walking down the hall she could hear that he was in her daughter's room, and in her bed, at which point she fainted. Since that moment she had been afraid of the dark.

The biological unit continued to run tests on the woman, and in fact presented her case at a psychobiology case conference. They concluded that after all their tests they still were not sure if there was something wrong with her brain, but they continued treating her as if she did, giving her a medication for seizures. During the conference, a part of the woman's history was revealed (ironically this unit learned more about the client's history in their initial evaluation than the psychologist, whose focus on behavior eschewed historical information), and a social worker in the audience said "what about the fact that the lady's boyfriend is sleeping with her daughter? That sounds oedipal to me." To this Derik Teo, who was sitting in the audience, responded, "We cannot prove any of that, therefore we won't discuss it." And that ended the meeting.

The woman continued to come to her weekly appointments with the psychologist, never having completed any of her desensitization exercises, but continuing to talk about her problems with her daughter and boyfriend. The clinical psychologist, for his part, had just finished reading a book by a cultural anthropologist, which argued that generations of aggression against black males by slave masters and their protective mothers had made them plastic and pliable. The psychologist decided to explain this hypothesis to her. He said that she was acting like a slave to her boyfriend, and she was letting him do anything he wanted to do. The woman responded to this by returning home and telling her boyfriend off. She returned to the next session very happy and satisfied. However, the following week she returned with a black eye because her boyfriend had retaliated. That was the last time she ever returned for services (Richter interview, 2007).

This story provides a glimpse into the lived experience of a patient subject to the professional and scientific battles that this dissertation documented. It is the experience of facing multiple conceptualizations and treatments, yet still being "missed" in a way that matters for effective care. This woman began as a phobic patient needing a good desensitization plan. Simultaneously, she became a prospect for a neurological treatment, and continued to be conceptualized that way despite the fact that there was no real evidence of brain pathology. This same group inadvertently accessed the historical data that the behaviorist tried to avoid accessing, but rejected the relevance of this data because it did not fit their model of mental disorder. Instead, the behaviorist ended up acting on this information through recourse to anthropological conjecture (resulting in an intervention Freud would surely call "wild analysis.")

In the midst of these competing interpretations, the patient's lived experience and felt needs clamored (unsuccessfully) for attention. From at least one off-hand diagnostic account, this woman needed someone to talk to and sort out her crumbling life. The interpretation she eventually received was well intentioned -- that her suffering was a product of the large structural problem of racism in America. The interpretation seems to have offered her some solace until—by acting on it—she suffered a violent attack from which none of the mental health professions were able to protect her. Her termination was a kind of retreat from the professional and epistemological battles which sought to "cure" her—though of what, exactly, the record can not show.

These competing conceptualizations came from different professions, and addressed different aspects of her symptoms. However, none did so in a way that encouraged her continued participation in treatment. Finally, note that her self-protecting exit from services would certainly be read by any outcomes assessment as a "lack of compliance" or "treatment resistance," instead of a failure on the part of the clinicians to establish a sustainable therapeutic rapport. The point of this story is not that this woman needed better psychotherapy, or psychoanalysis. I'm not arguing that medications would never be helpful for someone in this position, nor that they should have been used more intensively. The clinicians themselves, who are all caring and dedicated people, were not necessarily inadequate practitioners. Rather, the story highlights the way in which the hospital's meshwork of technocratic expertise, can still allow people to fall through the cracks and thus violate the *sine que non* of mental healing: that a person stay in treatment.

Modern medical empiricism and outcomes allow better cooperation for many reasons, not the least of which is that when pursuing research and treatment from this perspective, one can accomplish concrete results without needing to speculate or worry about the invisible hidden dynamics that locked up the psychoanalytic debates of the previous generation. Modern medical empiricism is rooted in Humean skepticism, and the philosophical position that it is impossible to know definitively what "causes" any other thing. Between any "cause" and "effect" could lay any number of variables that future research may unpack, but it is not necessary to know those now. A researcher need not obsess about those factors, as long as a regularity is identified—results allow science to move onward. In contrast, Hanks' psychoanalytic leadership was not only primarily focused on these internal dynamics, but under-attended to outcomes and pragmatic ends generally. Without clear outcomes, progress was hard to measure: both clinically and for the institution generally.

However, it is not clear that the move to modern medical empiricism and outcomes, to grant-chasing and research production, has increased the amount of truth produced or utilized at *DIMH*. It seems more accurate to say that debates about truth have been sidestepped for efficiency. And efficiency has its costs. *DIMH* has become so dependent on research dollars that it cannot provide the therapies that its research programs produce. Further, its psychotherapy research empirically validates the most general structure of effective therapy, leaving the more difficult questions off the books, and leaving its residents and staff untrained in these areas. Most importantly, time constraints get in the way of a clinician's actually learning the specifics of any given case; those gaps get filled with clinical hunches, or treatment based on statistical generalities that may not necessarily represent any given client.

The major need for clients receiving treatment from Dubville's contemporary mental health service sector is not more information about mental disorders. We already know much about disorders, such as the need for stability, the need for "human connection," and the need for support. Unfortunately, in today's system these non-technological, though essential, components of healing are hard to receive in treatment. It is this part of mental healing that advances in science never seem to provide, and in fact, that the mindset of constant "advancement" undercuts. Connection and support seem inefficiencies instead of clinical realities.

In response to this situation, the system of professions has adjusted once more:

4. “Totality” has become an area of technocratic expertise for those working in private practice psychotherapy. However, “totality” is no longer exclusively conceptualized through a psychoanalytic lens, while the ground to make a claim on “totality” *is* still grounded in “clinical experience.”

One of the ironies of the contemporary mental health system in Dubville, is that connection and commitment to “totality” have emerged as a sort of technocratic specialization for private practice psychotherapists. That is to say, that it is in private practice that people now get the treatment that used to dictate the structure at *DIMH*. However, unlike the psychoanalytic days at *DIMH*, there is no one ruling paradigm for how to conceptualize the “total person.”

As has already been discussed, there is a large diversity of paradigms and approaches that make up the private practice psychotherapy realm. And the moniker one gives oneself (be it cognitive-behavioral, psychodynamic, humanistic, etc.) does not always tell much about what exact service will be provided. However, across this diversity, private practice professionals are united by the premium they place on individualizing their approach to the needs of the client, of developing a solid “therapeutic relationship,” and in utilizing “talk” interventions of various kinds. Attention to a client’s history, developmental trajectory, and unconscious are considered relevant in this community, even though the various ways these are addressed is diverse.

Abbott, writing in the early 1980s, noted that “In psychotherapy the division is extremely clear, with psychiatrists treating the high end of the socioeconomic scale, psychologists the middle, and social workers such of the rest as get treated” (Abbott, 1984, p. 77). At Dubville, this is no longer true. Instead, the new structure has the lower and middle-classes treated by bachelors and masters level clinicians in institutions such as *DIMH*, while the upper classes get treated by private practice clinicians, quite often Ph.D. psychologists. The clinicians within the hospital are compelled to follow manualized approaches that are focused on symptom reduction, while the clinicians themselves have received little training in psychotherapy. In contrast, the private-practice clinicians are typically well trained in many psychotherapy approaches, as well as in “empirically validated” approaches. Those working within the hospital are compelled to use the evidence based approaches expected in their given module. In contrast, the private practice clinician has much broader leeway in drawing on any approach and model they feel is most appropriate for the care of the client before them. The clinician within the hospital is the active edge of the hospital “system,” while the private practice clinician works from authority invested in his or her training and “clinical expertise.”

As has been discussed throughout this dissertation, clinical expertise is a complicated ground for making professional claims. Abbott pointed out, in fact, that Freudianism replaced “Meyerism” exactly because Meyerists were too honest about the eclectic nature of mental health healing, while the Freudians posited a strict diagnostic and treatment system that promised more rigor (even though better results were never actually provided.) Teo, for his part, criticized “clinical expertise” as not being properly scientific and fact based, but instead rooted in what could by the dogmatic commitments of the clinicians.

These are problems which remain salient amongst private practice psychotherapists in Dubville. First, private practice psychotherapists that rely on this “eclectic” approach are not as unified a profession as others. For instance, private practice psychotherapists can come from one of many disciplines, including psychology, social work, psychiatry, counseling, and so forth. In fact, there is nothing currently to stop anyone from simply putting up a shingle declaring oneself a psychotherapist without any official disciplinary training of any kind. They are thus not able to effectively collaborate in dealing with governmental regulations or insurance company transitions that impact their field. Second, every psychotherapist I interviewed drew from many different research and theoretical traditions, some of which were contradictory. From the perspective of a hospital administrator, supervising such a system would be a logistical, and liability, nightmare.

However, despite the above noted diversity and complexity, it is interesting to note the similarities within the private practice psychotherapists. For instance, all the private practice psychotherapists I interviewed, regardless of their disciplinary training, saw themselves in a tradition established by Freud, but which has now grown and matured significantly past psychoanalysis. Also, these psychotherapists strived to expertise on the “total person,” and to excel at being able to connect with the emotional, behavioral, and cognitive needs of their individual patients. They each offered interesting and nuanced explanations for the ground they used to make claims about their individual clients’ mental states. For instance, we already discussed the way Mendel Reardon has begun to develop a neurological explanation for the value of psychotherapy which from the outside looks like humanistic client centered therapy.

Another example is Gary Inhoff, a psychologists in private practice, who explained his approach through recourse to his favorite hobby: martial arts. “In martial arts you never have the same situation appear in such a way that you can prepare a calibration for it," he explained. "You need to learn how to move through different 'niches' in order to bring the best you can to your client" (Inhoff interview, 2008). Inhoff was trained in a strict experimental approach, and in his career has found time to participate in research programs at *DIMH* as well as the state hospital system. However, he is also proud of his extensive knowledge in psychoanalysis and neuro-linguistic programming. Like most of the private practice psychotherapists I interviewed, he has a sophisticated understanding of philosophy of science, and has developed sophisticated ways of explaining how he conceptualizes a mental health problem and proper intervention. And also like the other psychotherapists I interviewed, he uses different forms of justification for different audiences. He explained to me that for some clients he tries to explain the underlying neurology of their distress, while sometimes he tries to explain the psychodynamics. Sometimes he simply reflects humanely as the humanistic movement encourages.

This anecdote points to the fundamental skill one must develop in private practice: explaining what one is doing differently depending on the audience. Private practice psychotherapists are masters of "talk." Not because they know the one right language of the mind, but because they know several languages of mental health, and can draw on them depending on which particular venue they are working in, and with whom they are working. The psychotherapists stand out in this way from the other professionals. The researchers, for instance, only need to "make sense" to the funding boards and the research community—after that their expertise is rarely questioned directly. During their halcyon days, the psychoanalysts, likewise, felt little need to make their understanding of clinical situations clear to professional audiences of worth (which ultimately cost them their authority.) Today, the psychoanalysts are effectively a group within private practice psychotherapy, and translating amidst different knowledge regimes, depending on the expectations and demands of the given audience, is commonplace.

Thus, the hospital system is assumed to have its expertise within the system itself, and each individual clinician is performing the expertise which the system holds. There is assumed to be one truth, and the clinician implements it as dictated. In contrast, the private practice psychotherapist draws from many research bases and uses what he or she feels is needed, as it is called for with each client. The need to constantly prove that one makes sense is the cost of working in private practice, but it affords a freedom to attend to those parts of care, which the hospital system itself feels is beyond its purview.

Summary

What this dissertation has hopefully demonstrated is that all of these institutions—private practice, private therapy institutes, community mental health clinics, university research departments, the hospital, and so forth—are interconnected. Whether the goal is symptom reduction or addressing the "total person," they are working to cover this same protean space of mental healing. Every psychoanalyst I interviewed admitted that they will refer acutely suicidal patient/clients to *DIMH*'s *IRC*, and that they will often work with patient/clients who use medications in order for the patient/client to "get the most out of therapy" (Wilkins interview, 2007). Even those psychotherapists who are most hostile to the changes at *DIMH* trust *DIMH's* expertise on certain aspects of care.

And even Teo, who told me “there is nothing psychotherapy can do about [serious mental illness] except teach and help you to cope” (Teo interview, 2007), has published and argued that at least 10% of persons with serious and persistent mental illness will need “rather extensive long-term individual psychotherapy” (Teo & Dershowitz, p. 615). One must assume that there is some expertise people providing that extensive psychotherapy should have. Further, it seems reasonable to assume that more than mere support is being offered in that service—especially when one studies the careful documentation of the kind of change that such psychotherapy brings to people’s lives.

However, this is not a happy functionalist vision, in which all the professions are cooperating to provide care: there is much struggle within this “total system” (i.e. the hospital system and the independent private practice community). The struggle is, of course, for resources and patients, but it is also a struggle on the very way in which we conceptualize a patient, a client, healing and wellness. The great professional achievement documented in this dissertation *hospital* psychiatry’s ability to avoid the problems of the “total person,” *psyche* and all, in advancing its professional goals. However, we still do not know what a psyche is, and ignoring the question does not solve the problem of the mystery.

APPENDIX A

DUQUESNE UNIVERSITY

600 FORBES AVENUE ♦ PITTSBURGH, PA 15282

CONSENT TO PARTICIPATE IN A RESEARCH STUDY

TITLE: An Analysis of the Transitions in the Mental Health Field's System of Professions: 1960-1990.

INVESTIGATOR: Daniel Noam Warner M.A.
242 46th St.
Pittsburgh, PA 15201
412.600.3646

ADVISOR: Daniel Burston Ph.D.
Psychology Department
412.396.6520

SOURCE OF SUPPORT:

This study is being performed as partial fulfillment of the requirements for the doctoral degree in psychology at Duquesne University.

PURPOSE:

You are being asked to participate in a research project that is investigating how the mental health landscape changed from 1960 to 1990. The project is a case study of the mental health field in the town in which you worked during this time period, dubbed "Dubville" for the purpose of this study.

You will be asked to allow me to interview you. The interviews will not be taped, but notes will be taken.

These are the only requests that will be made of you.

RISKS AND BENEFITS:

There are no risks greater than those encountered in everyday life.

COMPENSATION:

There is to be no compensation for participation. Participation in the project will require no monetary cost to you.

CONFIDENTIALITY:

Your name will never appear on any survey or research instruments. No identity will be made in the data analysis. All written materials and consent forms will be stored in a locked file in the researcher's home. Your responses will only appear in occasional quotations which are linked not to your name, but to the position you held within the field. For instance, if you were the director of clinical training at a mental hospital ("University Hospital") during the 1970s, and you tell the researcher that a certain decision was made due to funding issues, the statement will be cited back to "the director of clinical training at University Hospital," and no identifying information on your life will be shared.

The interviews will be conducted in a location of your choice. We can meet in your office, in a private location at the Duquesne University library, or the researcher's home—whichever space you find most comfortable.

RIGHT TO WITHDRAW:

You are under no obligation to participate in this study. You are free to withdraw your consent to participate at any time.

SUMMARY OF RESULTS:

A summary of the results of this research will be supplied to you, at no cost, upon request.

VOLUNTARY CONSENT:

I have read the above statements and understand what is being requested of me. I also understand that my participation is voluntary and that I am free to withdraw my consent at any time, for any reason. On these terms, I certify that I am willing to participate in this research project.

I understand that should I have any further questions about my participation in this study, I may call Daniel Noam Warner M.A. (412.600.3646), Daniel Burston (412.396.6520), or Dr. Paul Richer, Chair of the Duquesne University Institutional Review Board 412-396-6326).

_______________________________________ __________________

Participant's Signature Date

_______________________________________ __________________

Researcher's Signature Date

APPENDIX B

Table of Acronyms and Pseudonyms

Acronym	
DIMH	Dubville Institute of Mental Health
EDPI	Eastern Dubstate Psychoanalytic Institute
UDub	University of Dubville
DPI	Dubville Psychoanalytic Institute [Note: during it's history, DPI changed it's name to the Dubville Psychoanalytic Society and Institute. This was not relevant to our story, and thus the acronym DPI was utilized throughout to reduce confusion.)
DCGC	Dubville Child Guidance Clinic
EDPI	Eastern Dubstate Psychoanalytic Institute
UDMC	University of Dubville Medical Center
CSC	Community Study Center
SCAP	Sisterton Community Action Program
IRC	Information Resource Center
OCS	Office of Community Services

New Name	Position
Robert Hanks M.D.	DIMH's director throughout the 1950s and 1960s.
Albert Roland M.D.	DPI's director throughout the 1960s.
Martin Arlen M.D./Ph.D.	DIMH's chief biomedical researcher during the 1950s and 1960s.

Andrew Richter M.D.	Resident and fellow at DIMH throughout 1950s and 1960s. Psychoanalytic candidate at DPI during the 1960s. Chief of residency training early in DIMH's transition during the mid-1970s.
Samuel Melton M.D.	Resident at DIMH during the 1960s. DPI analytic candidate during the 1960s.
Fanny Victoria L.S.W.	Licensed social worker working at DIMH during the 1960s.
William Jax M.D.	Community psychiatry director at DIMH from the 1960s, through the 1980s.
Harold Johnson M.D.	Psychiatry fellow at DIMH during the 1960s. Training analyst at DPI throughout the 1970s and 1980s.
Victor Rotelle M.D.	DIMH faculty in the 1960s.
Chris Homer M.D.	DIMH faculty member throughout the 1950s and 1960s.
Martin Thompson M.D.	DIMH faculty and Chief of Children's Services throughout the 1950s and 1960s.
Dorian Hasselback	DIMH administrator in the 1950s and 1960s.
Mathew James M.D.	DIMH resident in the 1950s, and faculty in the 1960s. DPI director through the 1970s.
Zelda Lucile Ph.D.	UDub psychology student in the 1950s. DPI research track student during the 1960s.
Billing Jaspers Ph.D.	UDub's liberal arts dean throughout the 1920s and 1930s.
Teresa Florid Ph.D.	UDub psychology faculty and director of the training clinic from the 1930s, through the 1950s.
Daniel Welter Ph.D.	UDub psychology department chair from the mid-1940s through the mid-1950s
Zachary Edmund Ph.D.	A graduate student in UDub's psychology program in the late 1960s and 1970s.

Suzy Elisa Ph.D.	Graduate student in UDub's psychology program during the 1950s.
Max Musselman Ph.D.	UDub psychology department faculty member from the 1960s, through the 1990s.
Lance Davidson Ph.D.	UDub graduate student during the 1930s. UDub faculty member from the 1950s through the 1990s.
Georgia Davidson Ph.D.	UDub faculty member during the 1960s
Mary Packer L.S.W.	A DIMH social worker during the late 1960s, 1970s, and early 1980s.
Loran Peters Ph.D.	UDub research faculty during the 1960s.
Derik Teo M.D.	DIMH's Director through the 1970s.
Randall Aaranson Ph.D.	DCGC clinical director in the 1960s.
Ned Regent Ph.D.	UDub graduate student in the 1960s.
Frank Gogle Ph.D.	Chair of UDub's school of education in the 1960s and 1970s.
Fontina Ebert Ph.D.	DIMH research faculty through 1980s until the present.
Kevin Dershowitz M.D.	DIMH director from the early 1980s into the 1990s. UDub psychiatry department chair through the present.
Boogle and Alter	UDub faculty in the 1950s and 1960s.
Chris Wilkins M.A.	Private practice psychologist in Dubville throughout the 1970s and into the present.
Nathan Scully M.D.	Dubstate mental health administrator
Hal Trafford Ph.D.	UDub psychology department graduate student, and then psychology faculty in the community psychiatry division of DIMH in the 1970s.
Beverly Sharon Ph.D.	UDub school of education Ph.D. student.

Justine Patterson	Paraprofessional in Dubville.
Bob Fairbanks M.D.	Esteemed researcher in neuropsychiatry.
Barbra Anitas Ph.D.	UDub graduate student in cognitive neurology who researched at DIMH during the 2000s.
Madeline Verity M.D.	DIMH child psychiatry faculty.
Carl Witner	Dubstate's Secretary of Public Welfare through the mid-1980s
Woody Levy L.C.S.W.	A licensed social worker working at DIMH from the 1980s through till the present.
Merle Mondrian L.C.S.W.	A licensed social worker working at DIMH from the 1970s through till the present.
Frank Motley Ph.D.	Director of St. Augustus' addictions clinic.
Sam Banders L.C.S.W.	A licensed social worker working at DIMH during the 2000s.
James Aarons L.C.S.W.	A licensed social worker working at DIMH during the 2000s.
Greg Fantolli Ph.D.	A DIMH administrator in the 2000s.
Ford Hoover M.D.	DPI president in the mid-2000s.
Christy Collingsworth Ph.D.	DPI's first psychologist director
Mendel Reardon Ph.D.	Dubville's first psychologist in private practice for psychotherapy.
Carrie Sufax Ph.D.	UDub psychology department faculty from the 1980s through to the present.
Sally Dilthy M.S.	UDub graduate student.
Geert Adalwin Ph.D.	UDub philosophy professor

REFERENCES

Catalogue for the Psychiatry Program at the University of Dubville and the Dubville Institute of Mental Health. (1964). University of Dubville.

Aarons, J. (2007). Aarons Interview. In D. Warner (Ed.). Dubville.

Abbott, A. D. (1988). The System Of Professions: An Essay On The Division Of Expert Labor. Chicago: University of Chicago Press.

Abbott, A. D. (2001). Chaos Of Disciplines. Chicago: University of Chicago Press.

Adalwin, G. (2008). Adalwin Inteview with. D. Warner (Ed.). Dubville.

Alexander, F. (1963). Fundamentals of Psychoanalysis (2nd ed.). New York: Norton and Co.

Alexander, F. G., & Ross, H. (1952). Dynamic Psychiatry. [Chicago]: University of Chicago Press.

Alter, B. a. (1958). Martial Patterns of Alcoholic Women. Psychiatric Communications, 1(2).

Andreasen, N. C. (1984). The Broken Brain: The Biological Revolution In Psychiatry (1st ed.). New York: Harper & Row.

Anitas, B. (2005). Anitas Interview. In D. Warner (Ed.). Dubville.

Arlen, M. (1958). Physiologic, Psychologic, and Social Determinants in the Etiology of Duodenal Ulcer. American Journal of Digestive Diseases, 3, 285-314.

Baars, B. J. (1986). The Cognitive Revolution In Psychology. New York: Guilford Press.

Banders, S. (2005). Banders Interview. In D. Warner (Ed.). Dubville.

Bateson, G. (1972). Steps To An Ecology Of Mind; Collected Essays In Anthropology, Psychiatry, Evolution, And Epistemology. San Francisco,: Chandler Pub. Co.

Beck, A. T. (1976). Cognitive Therapy And The Emotional Disorders. New York: International Universities Press.

Beck, A. T. (1979). Cognitive Therapy Of Depression. New York: Guilford Press.

Benjamin, D. B. B. a. L. T. (2005). Creating a Profession: National Institute of Mental Health and the Training of Psychologists, 1946–1954. In W. E. Pickren, S. F. Schneider & American Psychological Association. (Eds.), Psychology and the National Institute of Mental Health : a Historical Analysis Of Science, Practice, And Policy (1st ed., pp. xi, 348 p.). Washington, DC: American Psychological Association.

Berg, D. N., & Smith, K. K. (1985). Exploring Clinical Methods For Social Research. Beverly Hills, Calif.: Sage.

Bernstein, S. (2007). Bernstein Interview. In D. Warner (Ed.). Dubville.

Bieber, I. (1973). On Behavior Therapy: A Critique. Journal of the American Academy of Psychoanalysis, 1(1), 39-52.

Breggin, P. R. (1994). Toxic Psychiatry. New York: St. Martin's Press.

Brosin, H. W. (1961). Lectures on experimental psychiatry. [Pittsburgh]: Univ. of Pittsburgh Press.

Bruner, J. S. (1983). In Search Of Mind : Essays In Autobiography (1st ed.). New York: Harper & Row.

Burston, D. (1996). The Wing Of Madness : The Life And Work Of R.D. Laing. Cambridge, Mass.: Harvard University Press.

Burston, D. (2007). Erik Erikson and the American Psyche : Ego, Ethics, And Evolution. Lanham, Md.: Jason Aronson.

Burston, D. (2007). An Invisible Plague: Polypharmacy and the Chemical Colonization of Childhood. In S. Olfman (Ed.), Bipolar Children. Westport, CT: Praeger.

Caplan, G. (1961). An Approach To Community Mental Health. [New York]: Grune & Stratton.

Chavern, H. (1959). Survey of Psychiatrists Work Time. Psychiatric Communications, 2(3).

Craig, A. B. (1990). One Woman's Dream: A Brief History Of The Staunton Farm Foundation. Unpublished manuscript.

Cummings, E. M., Davies, P., & Sufax, C. B. (2000). Family Process And Developmental Psychopathology: Theory, Research, And Clinical Implications / E. Mark Cummings, Patrick T. Davies, Susan B. Campbell ; foreword by Dante Cicchetti. New York: Guilford Press.

Curtis, A. (Writer) (2002). The Century of the Self [DVD]. In L. K. Adam Curtis (Producer). UK: BBC Four.

Davidson, G. (2008). Davidson Interview. In D. Warner (Ed.). Dubville.

Davidson, L., & Peters, L. (1963). Experimental Desensitization of a Phobia. Journal of Abnormal and Social Psychology, 66, 519-525.

Derrida, J., & State University of New York. Research Foundation. (1985). Difference in translation (J. F. Graham, Trans.). Ithaca: Cornell University Press.

Dershowitz, K. (2003). Dubville Institute of Mental Health Research and Training Programs 2002-2003. Dubville: Dubville Institute of Mental Health.

Dershowitz, K. (2006). Dershowitz Interview. In D. Warner (Ed.). Dubville.

Dewsbury, D. A. (2005). Comparative Psychology: A Case Study of Development of Support for Basic Research by a Federal Agency With an Applied Mission, 1948–1963. In W. E. Pickren, S. F. Schneider & American Psychological Association. (Eds.), Psychology and the National Institute of Mental Health : A Historical Analysis Of Science, Practice, And Policy (1st ed., pp. xi, 348 p.). Washington, DC: American Psychological Association.

Dilthy, K. (2008). Dilthy Interview. In D. Warner (Ed.). Dubville.

DIMH. (1951). Dubville Institute of Mental Health Annual Report 1951- 1952.

Ebert, F. (2005). Treating Bipolar Disorder: A Clinician's Guide To Interpersonal Rhythm Therapy. New York: Guilford Press.

Ebert, F. (2006). Ebert Interview. In D. Warner (Ed.). Dubville.

Edmund, Z. (2008). Edumund Interview. In D. Warner (Ed.). Dubville.

Elisa, S. (2007).Elisa Interview. In D. Warner (Ed.). Dubville.

Ellenberger, H. F. (1970). The Discovery Of The Unconscious; The History And Evolution Of Dynamic Psychiatry. New York,: Basic Books.

Ellison DL, H. R., Hitchcock J, Wolford JA. (1971). Problems In Developing A Community-Based Research Component For A Mental Health Center. Mental Hygiene, 55(3), 312-317.

Fairbanks, B. (1992). Modern Psychiatric Reflections. In K. Dershowitz (Ed.), (pp. xv, 143 p.). Washington, DC: American Psychiatric Press.

Fancher, R. E. (1996). Pioneers of Psychology (3rd ed.). New York: Norton.

Fantolli, G. (2007). Fantoli Interview. In D. Warner (Ed.). Dubville.

Farreras, I. G. (2005). The Historical Context for National Institute of Mental Health Support of American Psychological Association Training and Accreditation Efforts. In W. E. Pickren, S. F. Schneider & American Psychological Association. (Eds.), Psychology and the National Institute of Mental Health : A Historical Analysis Of Science, Practice, And Policy (1st ed., pp. xi, 348 p.). Washington, DC: American Psychological Association.

Flexner, A., Carnegie Foundation for the Advancement of Teaching., & Pritchett, H. S. (1910). Medical education in the United States and Canada; a report to the Carnegie Foundation for the Advancement of Teaching. New York City.

Florid, T. M. (1946). Child Psychology For Professional Workers (Rev. ed.). New York,: Prentice-Hall.

Foucault, M. (1973). Madness And Civilization; A History Of Insanity In The Age Of Reason. New York: Vintage Books.

Freud, S. (1947). The Questions Of Lay Analysis ([1st English ed.). London,: Imago Pub. Co.

Freud, S., Bance, A., & Phillips, A. (2002). Wild Analysis. London, England ; New York, N.Y., USA: Penguin Books.

Freud, S., Breuer, J., & Luckhurst, N. (2004). Studies in Hysteria. London ; New York: Penguin Books.

Glover, L. (July 26, 2002). St. Augustus Set To Close Doors-medical Center To Become Children's New Home. Dubville Business Times.

Grob, G. N. (1994). The Mad Among Us : A History Of The Care Of America's Mentally Ill. New York Toronto: Free Press ; Maxwell Macmillan Canada ; Maxwell Macmillan International.

Hanks, R. (1952). 1951-1952 Dubville Institute of Mental Health Annual Report.

Hanks, R. (1953). 1952-1953 Dubville Institute of Mental Health Annual Report.

Hanks, R. (1955). 1954-1955 Dubville Institute of Mental Health Annual Report.

Hanks, R. (1958). Introductory Statement Psychiatric Communications, 1(1), 3.

Hanks, R. (1961). 1960-1961 Dubville Institute of Mental Health Annual Report.

Hanks, R. (1966). 1965-1966 Dubville Institute of Mental Health Annual Report.

Healy, D. (1997). The Antidepressant Era. Cambridge, Mass.: Harvard University Press.

Healy, D. (2002). The Creation Of Psychopharmacology. Cambridge, MA: Harvard University Press.

Healy, D. (2004). Let Them Eat Prozac : The Unhealthy Relationship Between The Pharmaceutical Industry And Depression. New York: New York University Press.

Healy, D. (2009). Psychiatric Drugs Explained (5th ed.). Edinburgh ; New York: Churchill Livingstone Elsevier.

Homer, C. (1958). Tranquilizers—Their Use and Abuse. Psychiatric Communications, 1(3).

Hoover, F. (2006). Hoover Interview. In D. Warner (Ed.). Dubville.

Inhoff, G. (2008). Inhoff Interview. In D. Warner (Ed.). Dubville.

James, M. (2005). James Interview. In D. Warner (Ed.). Dubville.

James, M., & Wilkins, C. (2005). The Healer's Bent: Solitude And Dialogue In The Clinical Encounter. Hillsdale, NJ: Analytic Press.

Johnson, D. M., & Erneling, C. E. (1997). The Future Of The Cognitive Revolution. New York: Oxford University Press.

Johnson, H. (2006). Johnson Interview. In D. Warner (Ed.). Dubville.

Kirsner, D. (2000). Unfree Associations : Inside Psychoanalytic Institutes. London: Process Press.

Kubie, L. (1961). Research in Psychiatry: Problems in Training, Experience, and Strategy. In H. W. Brosin (Ed.), Lectures On Experimental Psychiatry (pp. vi, 361 p.). [Dubville]: Univ. of Dubville Press.

Kuhn, T. S. (1962/1996). The Structure Of Scientific Revolutions (3rd ed.). Chicago, IL: University of Chicago Press.

Leavy, W. (2005). Levy Interview. In D. Warner (Ed.). Dubville.

Levin, S. (2005a). *DIMH* A Perfect Place To Start. Dubville Post-Gazette Retrieved November 25, 2008, December 25, 2005, from http://www.post-gazette.com/pg/05359/627795.stm

Levin, S. (2005b). Empire Building: Starzl's Success A Model For Growth At UDMC. Dubville Post-Gazette Retrieved November 25, 2008, December 26, 2005, from http://www.post-gazette.com/pg/05359/627795.stm

Levin, S. (2005c). Empire Building: Consolidation and Controversy at UDMC. Dubville Post-Gazette Retrieved November 25, 2008, December 27, 2005, from http://www.post-gazette.com/pg/05359/627795.stm

Levin, S. (2005d). Empire Building: Clash of the Titans. Dubville Post-Gazette Retrieved November 25, 2008, December 28, 2005, from http://www.post-gazette.com/pg/05359/627795.stm

Levin, S. (2005e). Empire Building: Expansions and Departures. Dubville Post-Gazette. Retrieved November 25, 2008, December 29, 2005, from http://www.post-gazette.com/pg/05362/628745.stm

Lucile, Z. (1979). Our New Mathology. The American Psychoanalytic Association Newsletter, 13(4), 1, 4.

Lucile, Z. (2006). Interview. In D. Warner (Ed.). Dubville.

Ludmerer, K. M. (1999). Time To Heal : American Medical Education From The Turn Of The Century To The Era Of Managed Care. Oxford ; New York: Oxford University Press.

Luhrmann, T. M. (2000). Of Two Minds: The Growing Disorder In American Psychiatry (1st ed.). New York: Knopf.

McLeod, J. (2003). An Introduction To Counselling (3rd ed.). Philadelphia: Open University Press.

Melton, S. (2006). Interview. In D. Warner (Ed.). Dubville.

Miller, G. (1956). The Magical Number Seven Plus or Minus Two: Some Limits on Our Capacity for Processing Information. Psychological Review, 63, 81-97.

Mondrian, M. (2006). Mondrian Interview. In D. Warner (Ed.). Dubville.

Motley, F. (2007). Motley Interview. In D. Warner (Ed.). Dubville.

Musselman, M. (2008). Musselman Interview. In D. Warner (Ed.). Dubville

Musselman, M. J. (1999). History of the University of Dubville Psychology Department, Part Two. 2, 1 - 57.

Musselman, M. J. (2001). History of the University of Dubville Psychology Department, Part Three. 3, 1 - 38.

Musselman, M. J. (2007). A Brief History of University of Dubville Psychology Department. Dubville: University of Dubville Psychology Department.

Neisser, U. (1967). Cognitive Psychology. New York,: Appleton-Century-Crofts.

Olfman, S. (2006). No Child Left Different. Westport, Conn.: Praeger Publishers.

Olfman, S. (2007). Bipolar Children : Cutting-Edge Controversy, Insights, And Research. Westport, Conn.: Praeger Publishers.

Packer, M. (2006). Packer Interview. In D. Warner (Ed.). Dubville.

Patterson, J. (2007). Patterson Interview. In D. Warner (Ed.). Dubville.

Paull, B. I. (1986). A Century Of Medical Excellence: The History Of The University of Dubville School of Medicine. [Pittsburgh, PA]: University of Pittsburgh, Medical Alumni Association.

Pearson, G. B. (1943). Psychology at the Western State Psychiatric Hospital. UDub News.

Pickren, W. E., Schneider, S. F., & American Psychological Association. (2005). Psychology and the National Institute of Mental Health : A Historical Analysis Of Science, Practice, And Policy (1st ed.). Washington, DC: American Psychological Association.

Reardon, M. (2006). Reardon Interview. In D. Warner (Ed.). Dubville.

Reardon, M. (2008). Reardon Interview. In D. Warner (Ed.). Dubville.

Regent, N. (1970). Client-Centered And Behavior Therapies: Their Peaceful Coexistence: A Case Study. Journal of Abnormal Psychology, 76(1), 155-160.

Regent, N. (2007). Regent Interview. In D. Warner (Ed.). Dubville.

Richter, A. (2005). Richter Interview. In D. Warner (Ed.). Dubville

Rogers, C. R. (1961). On Becoming A Person; A Therapist's View Of Psychotherapy. Boston,: Houghton Mifflin.

Rogers, C. R., & Carmichael, L. (1939). The Clinical Treatment Of The Problem Child. Boston, New York, etc.,: Houghton Mifflin company.

Rogers, C. R., & Carmichael, L. (1942). Counseling and Psychotherapy; Newer Concepts In Practice. Boston, New York [etc.]: Houghton Mifflin company.

Rogers, C. R., & Russell, D. E. (2002). Carl Rogers: The Quiet Revolutionary, An Oral History. Roseville, CA: Penmarin Books.

Roland, A. (1958). Dynamics of Total Behavior in Man—the Concept of Disease. Psychiatric communications, 1(1), 11.

Rorty, R. (1991). Objectivity, Relativism, And Truth. Cambridge ; New York: Cambridge University Press.

Rorty, R. (1999). Philosophy and Social Hope. New York: Penguin Books.

Rotelle, V. (1958). Some Remarks on Psychotherapy as Reflected in Hospital Charts. Psychiatric Communications, 1(4).

Schachner, M. K. (1984). Western Psychiatric Institute and Clinic of the University of Pittsburgh : Its Years Of Research, Teaching And Service. Unpublished Thesis (Ph D), University of Pittsburgh, 1984.

Scully, N. (2007). Scully Interview. In D. Warner (Ed.). Dubville.

Shakow, D. (1946). The Nature Of Deterioration In Schizophrenic Conditions. Unpublished "Presented In Substantially The Present Form As A Thesis For The Ph D Degree At Harvard University ", Nervous and mental disease monographs, Coolidge foundation, Pref., New York,.

Shorter, E. (1992). From Paralysis To Fatigue : A History Of Psychosomatic Illness In The Modern Era. New York, Toronto: Free Press ; Maxwell Macmillan Canada ; Maxwell Macmillan International.

Shorter, E. (1997). A History Of Psychiatry : From The Era Of The Asylum To The Age of Prozac. New York: John Wiley & Sons.

Starr, P. (1982). The Social Transformation Of American Medicine. New York: Basic Books.

Sufax, C. (2000). Developmental Perspectives On Attention Deficit Disorder. In L. M, A. Sameroff & S. Miller (Eds.), Handook of Developmental Psychopathology (Vol. 1, pp. 383-401). New York: Kluver/Plenum Press.

Sufax, C. (2006). Sufax Interview. In D. Warner (Ed.). Dubville.

Sufax, C. B. (1990). Behavior Problems In Preschool Children. New York: Guilford Press.

Teo, D. (1977). 19767-197 Dubville Institute of Mental Health Annual Report.

Teo, D. (1981). 1980-1981 Dubville Institute of Mental Health Annual Report.

Teo, D. (1982). 1981-1982 Dubville Institute of Mental Health Annual Report.

Teo, D. (1983). 1983-1984 Dubville Institute of Mental Health Annual Report.

Teo, D. (1984). 1983-1984 Dubville Institute of Mental Health Annual Report.

Teo, D. (1992). Modern Psychiatric Reflections. In K. Dershowitz (Ed.), (pp. xv, 143 p.). Washington, DC: American Psychiatric Press.

Teo, D. (2007). Teo Interview. In D. Warner (Ed.). Dubville.

Teo, D., & Dershowitz, K. (1975). New Psychiatric Frontiers. In D. A. Hamburg & H. K. H. Brodie (Eds.), American Handbook Of Psychiatry (2d ed., pp. xi, 1092 p.). New York: Basic Books.

Teo, D., & Jarnowski, H. G. (1971). Modern Treatments in Psychiatry. Philadelphia,: Lippincott.

Trafford, H. (2005). Trafford Interview. In D. Warner (Ed.). Dubville

Verity, M. (2007). Verity Interview. In D. Warner (Ed.). Dubville.

Victoria, F. (2005). Interview. In D. Warner (Ed.). Dubville.

Wallerstein, R. S. (1998). Lay Analysis : Life Inside The Controversy. Hillsdale, NJ: The Analytic Press.

Watson, J. B. (1913). Psychology as the Behaviorist Views it. Psychological Review, 20, 158-177.

Weiner, H., Thaler, M., Reiser, M.F., and Arlen, I.M. (1957). Etiology of Duodenal Ulcer. "Relation of Specific Psychological Characteristics to Rate of Gastric Secretion (Serum Pepsinogen). Psychosomatic Medicine, 19, 1-10.

West, L. J., Stein, M., Addis, M., & Mirsky, I. A. (1982). Critical Issues In Behavioral Medicine. Philadelphia: Lippincott.

Wilkins, C. (1996). Capitalism in the Consulting Room.

Wilkins, C. (2007). Wilkins Interview. In D. Warner. Dubville.

Witmer, L. (1907). Clinical Psychology. Psychological Clinic, 1(1), 1-9.

Witner, C. (2006). Witner Interview. In D. Warner, Dubville.

Wolpe, J. (1958). Psychotherapy by reciprocal inhibition. Stanford, Calif.,: Stanford University Press.

Zaretsky, E. (2004). Secrets Of The Soul : A Social And Cultural History Of Psychoanalysis (1st ed.). New York: Alfred A. Knopf.

Zilboorg, G., & Henry, G. W. (1941). A History Of Medical Psychology. New York,: W. W. Norton & company.