Why Did American Psychiatry Abandon Psychoanalysis?

Authority and the Production of Knowledge in Twentieth Century Science

by

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Abstract

Dynamic psychiatry—that is, the model of psychiatry grounded in Freudian psychoanalysis—was the dominant mode in American psychiatry from World War II until around the late 1970s. Most psychiatric departments were headed by dynamic psychiatrists, and psychiatry residents, even those who did not intend to become psychoanalysts, received training in psychoanalytic concepts as part of their basic education. Furthermore, dynamic psychiatrists expanded psychiatric treatment to include all types of mental distress except schizophrenia and bipolar disorder—and sometimes even those. By the 1980s, though, dynamic psychiatry's fortunes had changed. Dynamic psychiatry was abandoned in favor of the "diagnostic" model, which viewed mental distress as a group of discrete medical illnesses. Psychoanalysis was increasingly seen as unscientific, and dynamic psychiatrists no longer had the same presence in the faculty of medical schools and hospitals.

Why did dynamic psychiatry fall from its position as the most influential model in American psychiatry, and why did it fall so fast? In this thesis, I argue that psychiatry as a whole, a branch of medicine that was under intense scrutiny from both the lay public and the rest of the medical world, was under pressure to prove its legitimacy as a science and as medicine. And in order to establish its scientific authority, psychiatry needed to prove that it was capable of generating scientific knowledge. Dynamic psychiatry struggled to do this, and certainly did not do it as well as diagnostic psychiatry and other models. Thus, because psychoanalysis was not useful for solving the political problems of American psychiatry as an *institution*, it fell out of favor in psychiatry as a *science.*

To argue this point, I examine the ways in which dynamic psychiatry either did not or could not produce scientific knowledge. In my first chapter, I examine the uncontrolled growth of competing theories and schools of thought in psychoanalysis to show how, even when seemingly producing knowledge prolifically, psychoanalysis was in fact a highly fractured field that was incapable of testing or sorting through newly produced ideas. I also examine how dynamic psychiatry, unlike other sciences, was stunted in its ability to innovate freely due to the inescapable influence of its founder, Freud. In my second chapter, I examine how changing notions of scientific objectivity during this time tarnished the standing of psychoanalytic knowledge, which lacked the labels of "scientific" and "empirical." In my third chapter, to argue my point that dynamic psychiatry fell out of favor because it was unable to produce scientific knowledge, I show how the model that replaced it, diagnostic psychiatry, facilitated the production of knowledge very well.

Since Foucault's revolutionary work on the mutually reinforcing relationship of power and knowledge, there has been a great body of scholarship furthering our understanding of the intersection of the two. However, most of the scholarship on the concept of power/knowledge (including Foucault's) has focused on the way that knowledge of a particular person helps one gain power over that person (as seen in the relationship between the doctor and the patient, the state and the prisoner, etc). In contrast, the case of dynamic psychiatry shows that, in the sciences at least, the production of knowledge itself—regardless of its relationship to its subject—functions as both a prerequisite and source of authority. Thus, the fall of dynamic psychiatry demonstrates both the pressure on science to produce knowledge and how social and political factors often underlie scientific change and progress.

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INTRODUCTION

A Field in Decline

Freud's 1909 lectures at Clark University, which occurred during his first and only visit to the United States, sowed the seeds for psychoanalysis' dominance in American psychiatry for the middle part of the twentieth century. The quick success of Freud's ideas in America was due partly to strategy and partly to serendipity: strategy, in that Freud presented his ideas as simplistically and optimistically as possible in order to appeal to the well-known pragmatism of the Americans; serendipity, in that Freud's theories blamed sexual repression as a prime cause of neuroses at a time when Americans were fiercely engaged with changing sexual mores.[1] As sociologist Michael Strand notes, though, perhaps the most important factor in psychoanalysis' success in America is that psychoanalysis offered the possibility of establishing an etiology for "everyday" mental distress, or at least mental distress less severe than the extreme mental illness of asylum patients; thus, American psychiatry found just the tool it needed to shift from its "lowly duty of managing the insane" in asylums to a much larger role in regulating mental health in the United States.[2] This "shift from the asylum to the office," as historian Allen Horwitz puts it, is well-supported by the statistics: in 1917, the proportion of American psychiatrists in private practice was 8 percent; by 1941, it had risen to 38 percent; in 1970, it reached 66 percent.[3]

Psychoanalysis also rose to influence in training centers and academic positions: by the mid-1960s, psychoanalysts chaired 58 percent of all psychiatry departments in the

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[1] Nathan G. Hale, *The Rise And Crisis of Psychoanalysis In the United States: Freud And the Americans, 1917-1985* (New York: Oxford University Press, 1995), 5.
[2] Michael Strand, "Where Do Classifications Come From? The DSM-III, the Transformation of American Psychiatry, and the Problem of Origins in the Sociology of Knowledge," *Theory and Society 40* (2011): 276.
[3] Allan V. Horwitz, *Creating Mental Illness* (Chicago: University of Chicago Press, 2002), 51.

United States, and in 1954, a study of fourteen psychiatric resident training centers discovered that most of the training centers were psychoanalytically oriented, and that individual residents were likely to be psychoanalytically oriented even when their training center was not.[4]

However, despite psychoanalysis' dominance in psychiatry in the middle of the twentieth century, during the 1970s and 1980s its popularity in psychiatry suddenly declined as rapidly as it had risen. At UCLA, for instance, the percentage of psychiatric residents in psychoanalytic training centers in 1966 was 50 percent; by 1975, it had already dropped to 27 percent, almost half that number.[5] Today, there are virtually no psychiatry departments headed by psychoanalytic psychiatrists.[6] What was it that led to the death of psychoanalytic psychiatry in the 1970s and 1980s?

Some authors, such as Robert Whitaker, argue that the introduction of a handful of new psychopharmacological medications raised hopes for a "magic bullet" for various psychiatric conditions, and the demonstrable initial success of these medications spurred a "biological revolution" in psychiatry that was comparable to the introduction of penicillin in the rest of medicine.[7,8] This new biological perspective, it is argued, initiated the movement away from the neuroses of the dynamic model (that is, the model of psychiatric practice grounded in the tenets of psychoanalysis), which were held to be inseparable and continuous with normal mental life, and towards its successor, the diagnostic model, where mental illness was conceived as a series of discrete medical illnesses. The growing

[4] Strand, 277.
[5] Hale, *Rise and Crisis,* 302.
[6] Horwitz, 54.
[7] Edward Shorter, *A History of Psychiatry: From the Era of the Asylum to the Age of Prozac* (New York: John Wiley & Sons, Inc.: 1997), 255.
[8] Robert Whitaker, *Anatomy of an Epidemic* (New York: Random House, 2010).

attention to biology in psychiatry, and the subsequent effects of this attention on society's conception of human mental life and human subjectivity, is an important topic worthy of critical attention. However, it alone does not explain the historical shift from the dynamic model to the diagnostic model, because while diagnostic psychiatry conceived of mental distress as composed of a series of separate illnesses, the individual medications produced by the biological approach typically proved effective in treating many of these supposedly different illnesses at once.[9] That is, while American psychiatry increasingly thought of mental distress in terms of discrete and distinguishable syndromes, pharmaceuticals did not. By extension, the growth in popularity of the biological approach to psychiatry cannot alone explain why the dynamic model was abandoned in favor of the diagnostic model.

In this thesis, I will argue instead that American psychiatry abandoned psychoanalysis during this time because psychoanalysis increasingly appeared to both laymen and scientists to be unable to generate scientific knowledge, and that psychiatry, a relatively new and increasingly controversial branch of medicine and science, needed to demonstrate publicly that it was capable of producing knowledge in order to maintain authority and legitimacy in the eyes of the other branches of medicine and with the public. By the time of its decline, psychoanalysis appeared stagnant, and the legitimacy of its methods had been called into question; psychiatry needed other tools by which to carve out a niche for itself in science and medicine. Therefore, I will examine psychoanalysis with reference to the changing practices of science and scientific research in the United States to identify the sociohistorical factors that led American psychiatrists to turn to other models of psychiatry during the 1970s and 1980s. In doing so, I hope to shed light on the cultural

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[9] Mitchell Wilson, "DSM-III and the Transformation of American Psychiatry: A History," *The American Journal of Psychiatry* 150 (1993): 403-404.

pressures on scientific disciplines to produce scientific knowledge, even as the societies within which they operate change what qualifies as knowledge over time.

In my first chapter, I will substantiate and expand upon my claim that psychoanalysis failed to grow as a science and discipline since its introduction to the United States by Freud in 1909. By this claim, I do not mean to say that psychoanalysis did not produce new theories and other innovations. In fact, new theories proliferated during this time. That was part of the problem, though: the new theories produced by psychoanalysis often resembled divergent and contradicting schools of thought, and these new theories had the effect of dividing rather than unifying psychoanalysts. Furthermore, psychoanalysis seemed to lack the epistemic tools to test and reject some portion of these new theories, and so the creation of these new, competing schools of thought went unchecked. The proliferation of new ideas in psychoanalysis did not lead to psychoanalysis' development or to the unification of psychoanalysts, but rather to what historian of science Paul Stepansky terms the "fractionation" of psychoanalysis during this period.[10] I will substantiate this claim with a brief analysis of publishing trends of psychoanalytic books and journals.

I am not writing a science or philosophy of science thesis, so I will not argue, and do not purport to know, whether the new theories produced by psychoanalysts during this time are true or false, or scientific or unscientific; rather, I am performing a historical analysis of what sociocultural factors stood in the way of the long-term development of psychoanalysis as a science in the United States. In addition to the seeming inability on the part of psychoanalysis to reject new theories as they were created, a related factor responsible for the discipline's failure to entrench itself, I will argue, is that even as new

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[10] Paul E. Stepansky, *Psychoanalysis at the Margins* (New York: Other Press, 2009), xvii.

theories sprang up uncontrolled, psychoanalytic thought, unlike other sciences, demonstrated an unusual inability to move past the ideas of its founder, Freud; furthermore, psychoanalysts who did so ran the risk of incurring stigma from other psychoanalysts. Whether psychoanalysts during this time would have thought of their reliance on Freud as a bad thing is, of course, debatable. My argument, though, is that, whether psychoanalysts considered it a drawback or not, their reliance on Freud hindered psychoanalysis' ability to develop and adapt as a science, according to the criteria for scientific knowledge that were coming to be increasingly formalized and applied during the first half of the twentieth century.

In my second chapter, I will investigate changes in the philosophy and practice of science during the twentieth century (e.g. the importance increasingly placed on reproducibility and falsifiability, and the growing role of double-blind trials) to show how psychoanalysis as a means of producing knowledge began to fall out of favor. These historical changes were not good for the reputation of psychiatry, which was dominated by psychoanalytic thought, in the judgment of other branches of science and the public. Changing opinions on what was considered "legitimate" science or what constituted valid scientific knowledge during this time led to psychoanalysis' decline in psychiatry. Specific topics addressed in this chapter include the use and reputation of case studies, which for a long time constituted the backbone of psychoanalytic research, but declined in medicine and science as a whole during the twentieth century. They were replaced by methods such as the double blind trial, which purported to eliminate subjectivity in research. I will show how the privileging of knowledge considered "objective," gathered by certain technical methods but not others, eventually led to psychoanalysis' declining reputation and use.

In the third chapter, I will compare psychoanalytic psychiatry with its successor, diagnostic psychiatry, in order to highlight the sociohistorical factors that led psychiatrists to abandon psychoanalysis in the 1970s and 80s. By showing what made diagnostic psychiatry successful, I hope to demonstrate why dynamic psychiatry was not: the shift to the diagnosis in understanding mental illness, which reached its apex with the publication of the third edition of *Diagnostic and Statistical Manual of Mental Disorders* (DSM-III), constitutes a major event in the history of American psychiatry, and by analyzing its rise and its eventual supplanting of psychoanalysis, it will be easier to understand why psychoanalysis fell out of favor. In particular, I will argue that the medical diagnoses of the DSM, in addition to increasing the reputation of psychiatry by emphasizing its "medicalness," filled a void in American psychiatry by creating a system by which to study mental distress: a nosology of mental illnesses composed of a tremendous number of discrete conditions which made it easy to organize and accumulate knowledge about mental distress. Conversely, the neuroses of psychoanalysis were not discrete, but rather continuous with the "healthy" mind, and psychoanalysts placed little emphasis on diagnoses, if they were used at all: instead of distinguishing between a finite set of identified conditions, psychoanalysis viewed mental health as a spectrum. The continuous nature of psychoanalytic neuroses with normal mental life, I will argue, made it difficult to organize and produce a body of medical knowledge about them, because they made categorization difficult and because medicine is based on a distinction between the normal and the pathological.

The transition to diagnostic psychiatry made the study and treatment of mental distress financially as well as epistemologically possible. The growing tendency of health insurance companies to cover part of the costs of psychotherapy necessitated the creation of some sort of categories for billing purposes, and the medical diagnoses of the DSM filled this role. Furthermore, these companies required some measurement and prediction of progress and outcomes in mentally ill patients, and the neat diagnoses of the DSM enabled bodies of research to be created for individual conditions. The creation of the National Institute of Mental Health (NIMH) to fund and direct research on mental illnesses during this time also created an incentive for a movement towards a formal and organized nosology of mental illness. Furthermore, though diagnostic psychiatry replaced the dynamic model, in which neurosis was continuous with normal mental life, the sheer amount of diagnoses that it proposed ensured that psychiatrists would have no shortage of patients to treat.

In concluding my Introduction, I would like to lay out particular definitions that I will use in my thesis. *Psychiatry* is the study and treatment of mental illnesses or mental suffering by individuals with medical training, specifically those with MD degrees or the international equivalent. *Psychoanalysis* is the name for the general psychological theory (or theories) devised by Freud, emphasizing unconscious drives and early childhood experiences. *Psychodynamic* or *dynamic psychiatry* was psychiatry that used psychoanalytic theory in the treatment of the mentally ill. It contrasts with *diagnostic psychiatry*, which used a “medical model” consisting of discrete diagnoses in interpreting and treating mental illness.

CHAPTER 1

The Limits of Psychoanalytic Knowledge

To say that psychoanalysis has grown stagnant as a scientific field may at first seem a sweeping and completely unwarranted claim. After all, in terms of producing new branches of thought, psychoanalytic theory has undoubtedly proven an expansive and fruitful domain; to argue that psychoanalytic progress suddenly stopped after Freud would require answering to object relations theory, ego psychology, self psychology, and Lacanian psychoanalysis, to name but a few. Furthermore, rather than dying with Freud in 1939, psychoanalysis produced these subfields through a variety of different thinkers—the role of Melanie Klein, Anna Freud, Heinz Kohut, and Jacques Lacan in their respective theories above seem to demonstrate that psychoanalysis was not a one-man show after all. In fact, it was during the decades immediately following Freud's death that dynamic psychiatry was at the peak of its influence in the United States.[11,12] Given the proliferation of new models and theories of psychoanalytic thought under an equally diverse group of psychoanalysts, on what grounds could the argument that psychoanalysis failed to produce visible and useful knowledge possibly possess any merit?

The answer is in the question: it was precisely the sheer amount and diversity of psychoanalytic subfields that delegitimized psychoanalysis as a whole: the presence of such diversity of opinion within the same field undermined the authority of any one subfield. Rather than adding to a collective fund of psychoanalytic knowledge, each of these different

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[11] Horwitz, 52.
[12] Nathan G. Hale, "American Psychoanalysis Since World War II," in *American Psychiatry after World War II, 1944-1994,* ed. Roy W. Menninger and John C. Nemiah (Washington, D.C.: American Psychiatric Press, 2000), 96.

subfields took a different approach to psychoanalytic theory and practice. Former American Psychiatric Association president Alan Stone said:

> Today, at least in my opinion, and I am not entirely alone in thinking this, neither Anna Freud's Ego Psychology nor Melanie Klein's Object Relations Theory seem like systematic advances on Freud's ideas. Rather they seem like *divergent schools of thought*, no closer to Freud than Karen Horney who rebelled against Freudian orthodoxy.[13]

The frequent emergence of these competing "divergent schools of thought" and their dissenting followers, then, made any developments in psychoanalysis seem to other scientists less like legitimate scientific discoveries and more like competing hypotheses. In contrast with more established fields like biology, innovations in psychoanalysis often seemed to contradict earlier psychoanalytic ideas as well as one another, frequently forming branches and sub-branches without regard to maintaining any sort of continuity or internal consistency in psychoanalysis as a whole.[14,15]

In fact, many of these developments were reactionary in nature, responding to other trends in psychoanalysis rather than to new clinical data. This is the case of Heinz Kohut's development of self psychology, which was a reaction against the subfields of ego psychology and classical drive theory. The revival of American interest in the work of Melanie Klein in the second half of the twentieth century has also been described as a

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[13] Alan A. Stone, "Where Will Psychoanalysis Survive?" (Keynote address to the American Academy of Psychoanalysis, December 9, 1995). http://harvardmagazine.com/1997/01/original.html. Accessed 21 March 2014.
[14] Stepansky, 103.
[15] Thomas Shapiro, "Editorial: Our Changing Science," *Journal of the American Psychoanalytic Association* 37 (1989): 4.

reaction against ego psychology.[16] Furthermore, never did one of these new theories thoroughly abrogate and replace a previous one in the way that, for example, Einstein's theory of general relativity transformed Newtonian physics.

This is not to say that a new idea in psychoanalysis would not have been met with resistance upon its introduction; however, it soon proved that psychoanalysis on the whole lacked the tools that other disciplines had to debunk or prove new theories. By what criteria could psychoanalysts reject or accept a new hypothesis? In physics, a new model was expected to be compatible with currently available data, as well as able to make predictions to be confirmed by observation[17]; similarly, a new pharmaceutical drug was expected to prove itself by beating a control in a double-blind trial. But such criteria, even if psychoanalysts wanted to use them, were not as conveniently applied to unconscious phenomena proposed by psychoanalysis.

Even the gathering of data from clinical psychotherapy was typically unable to resolve the conflict between two competing subfields; problematically, any clinical data that could potentially prove the efficacy of one psychoanalytic school could be interpreted to support others as well.[18] In an article for *Psychoanalytic Psychology,* psychologist Robert Holt, even as he argued for the validity of psychoanalysis as a "testable scientific theory,"[19] admitted the difficulty of producing data that could settle disputes between psychoanalytic and non-psychoanalytic theories, let alone between schools within psychoanalysis:

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[16] Hale, "American Psychoanalysis," 94-95.
[17] Newton's laws of motion and gravitation, for example, enabled astronomers to predict that unexpected measurements of Uranus' orbit were not due to observational error, but to a large body affecting its orbit gravitationally; this was later confirmed to be the planet Neptune.
[18] Hale, *Rise and Crisis,* 362.
[19] Robert R. Holt, "The Current Status of Psychoanalytic Theory," *Psychoanalytic Psychology* 2 (1985), 289.

> All too often, the full set of data used to confirm a particular clinical hypothesis (and which therefore partly confirm the special clinical hypotheses entailed in it) are equally compatible with *another* set of general hypotheses. For example, we are familiar with the fact that followers of non-Freudian schools of analysis or of nonpsychoanalytic clinical theories are ready with their own explanations of our cases. It is commonplace that most of these theories, with incompatible general hypotheses, are about equally capable of accommodating one another's data. All of them seem to be confirmed in clinical practice, but they cannot all be true.[20]

Observation and case studies, mainstays in the production of psychoanalytic knowledge, did not have the capability of rejecting new branches of psychoanalysis: observation might vary from analyst to analyst,[21] and the narrative nature of case studies made them more likely to *respond* to changing fashions in psychoanalytic theory than to refute or support them.[22] Because psychoanalysis lacked the scientific means or even the scientific criteria to identify a new theory as false and repudiate it, these branches went unpruned, until psychoanalysis was filled with them. Thus, the abundance of new schools of thought in psychoanalysis after Freud was, in fact, an *over*abundance: this vast proliferation of new ideas and models can be said to demonstrate the fruitfulness of Freud's original theory only in that it publicly showcased an apparent epistemic inability on the part of psychoanalysis to reject any new hypotheses—except, perhaps, by means of partisan polemic.

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[20] Ibid., 302.
[21] For a comprehensive argument against the validity of data gathered from clinical encounters, see Grünbaum's *The Foundations of Psychoanalysis.*
[22] Hale, *Rise and Crisis,* 39.

It might also be argued that the continuing creation of psychoanalytic literature could function as proof of psychoanalysis' development as a field, that the shelves and shelves of clinical journals were more than sufficient evidence of psychoanalysis' growth and expansion. Certainly, psychoanalysis' ability to inspire new and endlessly original books and journal articles could not be denied; however, far from demonstrating psychoanalysis' capability of producing new information that pushes forward the field as a whole, the actual publication of these books and journals provides an interesting case study in evidence of the contrary.[23] Historian of medicine Paul Stepansky claims that "journals such as [*Journal of the American Psychoanalytic Foundation*] and *Psychoanalytic Quarterly* now accept the reality of theoretical pluralism" and publish accordingly, accepting papers from a range of different subfields of psychoanalysis.[24] As even Theodore Shapiro, a psychoanalyst and former editor of the *Journal of the American Psychoanalytic Foundation,* conceded in a 1989 editorial in that very journal, "Even within analysis 'not-so-complementary explanations' abound. We may now be said to variously espouse ego psychology, self psychology, separation-individuation, and object-relations psychology. Many say that these are simply viewpoints, but too often they seem to be alternative explanations."[25] Given the role of journals in the dissemination of a field's knowledge, it is difficult to ignore such a declaration of pluralism in psychoanalysis when it comes from an editor of the most prominent journal in the field.

Even as psychoanalytic literature expanded (and expands today) to accommodate the huge range of these "alternative explanations," in terms of print runs the publications

[23] Stepansky, 107.
[24] Ibid., 106.
[25] Shapiro, 4.

themselves were and are dying out. Stepansky, also the former Managing Director of the publishing company The Analytic Press, Inc., writes:

> When I arrived at The Analytic Press in 1983, my default print run for authored and edited books alike was 1,500 copies. That is, I could reasonably expect to sell at least 1,500 copies of everything we published. Between 1983 and 2006, this default print run fell successively to 1,200 copies, then 1,000 copies, and ended up at 700 to 800 copies for many authored books.[26]

Stepansky attributes this to the "fractionation" of psychoanalysis into an assortment of competing, rather than supporting, schools of thoughts, and the consequent inability of publishing firms to move enough copies of books to continue publishing them. Stepansky argues that "the very idea of a big psychoanalytic book no longer exists, for the simple reason that the field is neither big enough nor cohesive enough nor influential enough to yield indigenously big books"; thus, psychoanalysis in its "dispiritedly pseudoentirety" lacks both the influence and self-coherence necessary to attract nonanalytic therapists to its books.[27] Stepansky concludes:

> Psychoanalytic publishing is not failing because it cannot produce big books that pull together the various theoretical islands into which the field has drifted. It is failing because, owning to this selfsame fractionation and the simultaneous contraction of the field, its good-enough books are no longer good enough to keep small professional firms in business.[28]

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[26] Stepansky, 74.
[27] Ibid., 67.
[28] Ibid., 75.

Thus, it is difficult to argue that the creation of new texts is evidence of psychoanalysis' ability to grow as a discipline when psychoanalytic literature is in a state of fractionation and disappearance.[29]

Even as psychoanalysis splintered to the point where theoretical pluralism became the norm for psychoanalytic books and journals, its progress was hindered, paradoxically, by a certain lack of originality: even as new theories sprang up prolifically, these ideas exhibited a difficulty in moving past Freud. An analysis of these ideas reveals that this difficulty was not due to a mere lack of creativity or effort on the part of psychoanalysts; rather, it was because Freud had taken on a function that exceeded his role as the founder of psychoanalysis.

In many cases, this meant that fidelity to Freud became a criterion for acceptance within the psychoanalytic community. This can be shown by the case of Otto Rank. Rank was an early disciple of Freud's; Freud affectionately referred to him as a "bright and honest boy."[30] Rank even sat on Freud's secret committee, an "inner circle" of the six psychoanalysts closest to Freud, which lasted in its initial form from 1912 to 1924. This committee, which formed in the wake of Alfred Adler's and Wilhelm Stekel's departures from Freud's theories as well as Carl Jung's anticipated defection, was specifically dedicated to helping Freud respond to critics. The central agreements of this committee ensured loyalty in Freud's closest followers; as Sulloway notes, "No member of the committee was to depart publicly from any of the fundamental tenets of psychoanalysis without first

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[29] Ibid., 106.
[30] Letter from Sigmund Freud to Ernest Jones, February 24, 1912. *The Complete Correspondence of Sigmund Freud and Ernest Jones, 1908-1939*, ed. Andrew R. Paskauskas (Cambridge, Mass.: Belknap Press of Harvard University Press, 1993), 133.

discussing his views with the others."[31] Freud even distributed ceremonial rings to this secret committee.[32]

Thus, it came as a surprise to Freud and the committee when Rank published *The Trauma of Birth* in 1924. In this book, Rank deviated from Freud's idea of the Oedipus complex as the source of all neuroses as well as art, religion, and philosophy, instead proposing the traumatic pain of being born as the "prototype of all later attacks of fear."[33] This was not a small deviation: Freud had made the Oedipus complex the basis of psychoanalysis, and Rank was asserting that Oedipal conflicts with the father were just "a mask for the essential ones concerning birth" and the mother.[34] Rank, even by coining the term "pre-Oedipal,"[35] had committed heresy. Freud's reaction to birth trauma was initially mixed but soon turned hostile. Ernest Jones, biographer of Freud and member of the secret committee himself, quotes Freud as saying, "I believe it will 'fall flat' if one does not criticize it too sharply, and then Rank, whom I value for his gifts and the great service he has rendered, will have learned a useful lesson."[36]

In the meantime, Rank visited America, and shared the news that "the 'old' psychoanalysis had been quite superseded by his new discoveries."[37] This claim met with a cool reception. Abraham Brill, for example, an influential psychoanalyst whose

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[31] Frank J. Sulloway, "Reassessing Freud's Case Histories: The Social Construction of Psychoanalysis," *Isis* 82 (1991), 245-275, 269.
[32] Sherry Turkle, *Psychoanalytic Politics: Freud's French Revolution* (New York: Basic Books Inc., 1978), 120.
[33] Ernest Jones, *The Life and Work of Sigmund Freud,* ed. and abridged by Lionel Trilling and Steven Marcus (New York: Basic Books, Inc., 1961), 418.
[34] Ibid.
[35] Otto Rank, *The Psychology of Difference: The American Lectures*, ed. by Robert Kramer (Princeton, NJ: Princeton University Press, 1996), 43.
[36] Ibid., 425.
[37] Ibid., 425-426.

achievements included founding the New York Psychoanalytic Society, translating Freud into English for the first time, and being the first practicing psychoanalyst in the United States, had one reaction: he simply "wanted to know what Freud had to say about it all."[38]

Rank's reception in America was dimmed by further ostracization from Freud's committee back in Europe. After a short period of rather cold correspondence, the ideological tension between Freud and Rank soon turned personal: Freud said, "When he comes to his senses it will of course be the time… to forgive him all his divagations. I dare not hope for that, however; experience shows that once the devil is loose he goes his way to the very end."[39] This strong reaction caused Sándor Ferenczi, a member of the committee who was very close to Rank, who had coauthored a book with Rank the same year that Rank released *The Trauma of Birth*, and who had originally extolled the improved clinical outcomes of psychoanalysis based on Rank's birth trauma theory, to think that he had placed himself on the "losing" team. Jones writes, "[Ferenczi] had been on the edge of a precipice, and he now drew himself back in an unmistakable fashion. He announced to Freud after reading Rank's rude letter that he had definitely turned his back on him."[40]

Rank returned to Vienna to say goodbye to Freud and share with the committee his intention to move to and work in America semi-permanently. Rank, who had been diagnosed with what is now called bipolar disorder, almost immediately fell into a depressive state, not making it past Paris on his journey back to America. Rank delivered a wholehearted apology to both the committee and Freud, who received his apology gladly.

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[38] Ibid., 426.
[39] Ibid., 427.
[40] Ibid., 427.

However, this state of affairs did not last long, and in 1926, Rank once again said goodbye to Freud and departed Vienna.

Rank died a little more than ten years later, and his post-Freud efforts did not prove particularly fruitful.[41] Freud rarely mentioned him, and when he did, it was not usually in positive ways. For the most part, "All that mattered to Freud was that [his] work should be clearly differentiated from psychoanalysis."[42] Thus, Rank's theory of birth trauma, which was originally received with great enthusiasm by many members of the committee, proved to signal the end of his career when Freud's opinion of it shifted from undecided to negative, thereby changing its status from psychoanalytic to non-psychoanalytic.

It is easy to use an example like Rank, a contemporary and disciple of Freud, to show how Freud governed psychoanalytic discourse in his lifetime. But what of psychoanalysis *after* Freud? In fact, post-Freudian psychoanalysts operated no more independently from Freud's influence than Rank did. This is perfectly illustrated in the work of Jacques Lacan. Though he produced much original work and gathered his own crowd of followers, Lacan began his career not with a move forward, but a move back, in that his most notable idea was that of a "return to Freud": a rereading of Freud's works through the lens of linguistics, mathematics, and contemporary philosophy. It is not mere coincidence that one of the most notable ideas from one of the most notable thinkers in the field of psychoanalysis should be defined primarily by his relationship with its founder; in fact, Lacan's case is quite characteristic of the nature of progress in psychoanalytic thought in that it is constantly mediated by Freud's original work.

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[41] Ibid., 430.
[42] Ibid.

The relationship of Lacan's work to Freud's is complex. Though it has been argued that Lacan remains the only psychoanalyst whose ideas bear serious comparison to Freud's own,[43] his work was produced—and received—not independently, but as part of a perpetual conversation with his predecessor. Thus, to say that Lacan work was unable to operate independently from Freud is not to say that Lacan was merely rehashing old Freudian ideas—Malcolm Bowie argues that even his "return to Freud" was "conducted on Freud's behalf, and at the same time, against him."[44] It is merely to say that, whether he is assenting to or dissenting with Freud, he is always operating within the sphere of his influence: Freud remains an essential reference.

Sometimes, this involved Lacan deliberately working Freud's authority to his advantage, attributing, for rhetorical purposes, his own ideas to Freud;[45] Dany Nobus argues that in order for the ideas in Lacan's "return to Freud" to succeed, he needed to justify the necessity of his project by showing how the French psychoanalytic establishment had been misinterpreting Freud.[46] But the authority that came with Freud's name proved a double-edged sword for Lacan: on the occasions when Lacan truly did break away from Freud, it often proved detrimental to both his career and the reception of his work. This is seen most notably in Lacan's prime clinical contribution: the "variable-length" therapy session. For Lacan, it seemed that the therapy session should not end after a "fifty

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[43] See Malcolm Bowie, *Lacan*. (Cambridge: Harvard University Press, 1991).
[44] Ibid., 7
[45] Élisabeth Roudinesco, *Jacques Lacan,* trans. Barbara Bray. (New York: Columbia University Press, 1997), 440.
[46] Dany Nobus, "Knowledge in Failure: On the Crises of Legitimacy in Lacanian Psychoanalysis," *Who Owns Psychoanalysis?,* ed. by Ann Casement. (London: Karnac Books, 1994), 210.

minute hour," the norm for psychoanalysis at the time,[47] but in order to punctuate a conversation between analyst and analysand. End the session was itself intended to function as an analytic technique, enabling the analyst to highlight a significant breakthrough had been made (or simply to prevent an analysand from wasting time). Rather than bury such breakthroughs with further talk until the requisite fifty minutes had elapsed, he argued, the timing of the session should be determined by analytic progress. For Lacan, this meant that sessions sometimes ran just a few minutes. Predictably, this innovation was highly upsetting to the psychoanalytic establishment. Lacan's critics alleged a financial motive: the ability of an analyst to end sessions when they wanted meant that they could see more patients in an hour, and "variable-length sessions" typically meant "short sessions," as Lacan's opponents rechristened them.[48,49] As Sherry Turkle notes, "[O]f course, patients want to know why the Lacanian analyst never wants to 'shake up' the routine by keeping them for more rather than less time."[50]

More important, though, Lacan had committed a fatal error: he had broken from Freud. Though Freud did not always meet for hour-long sessions, when he diverged from this pattern, it was usually for longer sessions, not shorter.[51] In any case, Lacan had taken the authority to make a radical change in his clinical technique without grounding it in Freud. Nobus writes, "With the introduction of the variable-length sessions, Lacan, of course, favored a technical principle that had not featured as such within Freud's original

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[47] Ibid., 212.
[48] Bruce Fink, *A Clinical Introduction to Lacanian Psychoanalysis: Theory and Technique.* (Cambridge: Harvard University Press, 1997), 17.
[49] Ibid.
[50] Turkle, 204.
[51] Richard Friedman, "Shrinking Hours," *New York Times*, October 12, 2013. http://www.nytimes.com/2013/10/13/opinion/sunday/shrinking-hours.html?_r=0. Accessed 21 March 2014.

discourse, and this lack of Freudian justification no doubt contributed to his being perceived as deviating dangerously from a central aspect of psychoanalytic practice."[52]

Lacan tried to establish a Freudian basis for his innovation retroactively,[53] but the damage was done; for other psychoanalysts, this innovation was too much to bear. Lacan, under intense fire from the other members of the Société Parisienne de Psychanalyse, the dominant psychoanalytic group in France at this time, left the group and founded his own with other estranged analysts in 1953.[54] This new institution, though, due to its affiliation with Lacan, was unable to join the International Psychoanalytical Association (IPA), and ten years later, when the IPA offered the group the ability to join their ranks on the condition that Lacan be stripped of his status as a training analyst, they accepted.[55]

Foucault's seminal essay "What is an Author?" offers a critical standpoint in examining the inability of analysts like Rank and Lacan fully to move past Freud. According to Foucault, authors are not merely the creators of texts; they constitute a function by which such texts are organized. When these authors become what Foucault terms "founders of discursivity," they "become more than just the authors of their own works. They have produced something else: the possibilities and the rules for the formation of other texts."[56] However inspired Rank or Lacan's ideas may have been, in order to qualify as psychoanalytic they needed to situate themselves within a Freudian discourse, which is to say that their texts were governed by the rules initiated by its founder.

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52 Nobus, 220.
53 Ibid.
54 Ibid., 213.
55 Ibid., 214-215.
56 Michel Foucault, "What is an Author?" *The Essential Foucault*, ed. by Paul Rabinow and Nikolas Rose (The New Press: New York, 2003), 389.

Thus, when such authors become the organizing principle for texts in a particular discourse, it is impossible to distance a work fully from the founder of that discourse if one is to remain within that discourse; Foucault writes of "the inevitable necessity, within these fields of discursivity, for a 'return to the origin.'"[57] This was certainly the case in post-Freudian psychoanalysis: in fact, Foucault's mention of the "return to the origin" is a direct reference to Lacan, who was present at Foucault's reading of the paper and participated with interest in the discussion that followed.[58] Foucault continues, "Reexamination of Galileo's text may well change our understanding of the history of mechanics, but it will never be able to change mechanics itself. On the other hand, reexamining Freud's texts modifies psychoanalysis itself…"[59] That is, though a discourse may change over time, in some cases (Foucault mentions Freud and Marx) it is still fundamentally bound to the works of that discourse's founder, which generate criteria for judging whether or not a subsequent work actually belongs to the discourse in question. Jean-Michel Rabaté, in the *Cambridge Companion to Lacan*, summarizes thusly: "[I]f Marxism and psychoanalysis do not have the status of hard sciences, it is because they are still in debt to the texts of a founder…"[60] Unlike other disciplines (Foucault uses the example of physics), progress in psychoanalysis is mediated through and restricted by its founder.

In his address to the American Academy of Psychoanalysis, Stone asked, "What is there about Freud's vision that has made his monumental work a limiting factor rather

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[57] Ibid. 389.
[58] Jean-Michel Rabaté, "Lacan's Turn to Freud," *The Cambridge Companion to Lacan*. (Cambridge: Cambridge University Press, 2003), 7.
[59] Foucault, 389.
[60] Rabaté, 8.

than a scaffolding on which others can stand?"[61] Foucault states that, though the future of such discourses is likely (or even necessarily) characterized by divergences from their founders, they may never directly contradict them. One can avoid statements by a founder of discursivity if they can be "deemed inessential" or "derived from another type of discursivity," but, crucially, "one does not declare certain propositions in the work of these founders to be false…"[62] In this, we find the answer to Stone's question. Like the ideological fractionation that plagued psychoanalysis through the 20th century, Freud's authority and inescapable influence imposed constraints on psychoanalysis that stunted its ability to achieve meaningful intellectual expansion and develop itself as a scientific discipline.

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[61] Stone.
[62] Foucault, 388.

CHAPTER 2

Psychoanalysis and the Criteria of Objectivity

In his 1995 keynote address to the American Academy of Psychoanalysis, Alan Stone, a graduate of the Boston Psychoanalytic Institute and former president of the American Psychiatric Association, made a surprising declaration: he and the other "faithful" had become disillusioned with psychoanalysis and its critics appeared correct. He claimed that psychoanalysis was not an "adequate form of treatment" and psychoanalysis' future lay in the arts and humanities. He went on to suggest that psychoanalysis should be used therapeutically only after a patient's actual symptoms had been dealt with. At the root of psychoanalysis' failure as a medical science, Stone argued, was the fact that psychoanalysis did not function as a "cumulative discipline."[63]

Though the occasion for Stone's declaration was surprising, its timing was not. Psychoanalysis had been consistently declining in popularity with the public and within science for several decades. By 1990, a survey by the American Psychoanalytic Association found that its analysts were seeing, on average, only two patients for analysis a week.[64] Stone, in arguing for the persistence of psychoanalysis in the arts and humanities but not in psychiatry or clinical treatment, and in deeming Freud an "artist/subjectivist/philosopher" rather than a "physician/objectivist/scientist," highlighted one of the most salient factors in psychoanalysis' perceived failure as a psychiatric tool: Freud, though a genius, was a thinker, an interpreter, not a scientist: he had created a field of study that was simply not objective enough to function as a science. Stone, giving the example of Freud's explanation

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[63] Stone.
[64] Jonathan Engel, *American Therapy: The Rise of Psychotherapy in the United States.* (Penguin Group Inc.: New York, 2008), 218.

of religious asceticism, said, "A marvelous subjective speculation—I find it persuasive, but is it empirical? Is it based on objective data?"

These questions had haunted psychoanalysis long before Stone's speech, but Stone's phrasing highlights psychoanalysis' struggles perfectly. Stone claims that Freud's methods are "not recognizable as science," but in the same sentence admits that he knows of "no other work in psychiatry or psychology… so immediately convincing." What psychoanalysis suffered from, in other words, was a shift in the culture's prime concern from "Is it useful?" or even "Is it true?" to "Is it empirical? Is it based on objective data?" The historical rise of the concept of objectivity and scientific empiricism to a position of dominance in medicine was, for psychoanalysis, simply poor timing.

One of the most significant ways in which psychoanalysis was unable to adapt to changing standards of medicine was its inability to move past the case study as the primary means of gathering and disseminating psychoanalytic discoveries. The case study was of great use in the dynamic model because it allowed the sort of in-depth analysis of a patient that was necessary to meet the conditions of psychoanalytic knowledge; when exploring the complexities of the unconscious, texts that focused on thoroughly uncovering the psychological state of one patient would be more informative than studies of multiple patients at once.

As with many trademarks of psychoanalysis, the prevalence of the case study in dynamic psychiatry can be traced back to Freud, who used six detailed case studies after developing psychoanalytic therapy. Freud used these case studies as ways of entering into a particular diagnosis or to demonstrate a particular technique—treatment via dream interpretation, for example, was demonstrated in the case study of "Dora," a teenager

Freud diagnosed with hysteria, in *Fragments of an Analysis of a Case of Hysteria*. He even used his own experiences as the subject of case studies, such as his analysis of the dream he titled "Irma's Injection" in *The Interpretation of Dreams*.

Despite the curious fact that these case studies involved mostly patients whose therapy was, by Freud's own admission, unsuccessful,[65] the central role of the case study in dynamic psychiatry persisted well after Freud's death in 1939. In 1952, for example, the influential dynamic psychiatrist Karl Menninger published his *Manual for Psychiatric Case Study*, which was warmly received.[66] And in 1965, a dedicated Freudian named Kurt Eissler declared case studies "the pillars on which psychoanalysis as an empirical science rests."[67]

In fact, the case study as a means of presenting research was originally a standard and accepted practice in all branches of medicine, not just psychiatry.[68] However, it began to fall out of favor in medicine in the middle of the twentieth century, largely due to the development of research techniques that were designed to produce truth free from subjective error in a manner that the case study could not. One such development was the double-blind trial, in which neither the test subjects nor the experimenters are aware of which subjects are receiving treatment and which are receiving the placebo. The development of this revolutionary new technique coincided very closely with dynamic psychiatry's decline. Ken Alder writes, "Shortly after the start of the Cold War… double-blind reviews became the norm for conducting scientific medical research, as well as the

[65] Sulloway, 251.
[66] E.g., William B. Terhune, "A Manual for Psychiatric Case Study," *American Journal of Psychiatry*, 111 (1954), 76-77.
[67] Kurt R. Eissler, *Medical Orthodoxy and the Future of Psychoanalysis* (New York: International Universities Press, 1965), 395.
[68] Horwitz, 58.

means by which peers evaluated scholarship."[69] Other scholars confirm this dating.[70] (Marcia Meldrum links the growth in popularity of double-blind trials at this time to the need of the booming pharmaceutical industry to establish the credibility with the public.[71]) The double-blind trial's conscious rejection of the role of the professional stands in stark contrast to the case study, which by its very nature is inseparable from the experience, knowledge, and authority—which is to say, subjectivity—of the professional. In the creation of a case study, the subject speaks face-to-face with the professional, who is the same one to record information gathered from sessions, and is the same one to interpret it.

The large role of the professional in the production of the case study opens it up to more than just the mere threat of bias; the subjectivity of the professional is, in fact, an integral component of the case study. Stepansky makes this point by contrasting the interpretation of the mind in the case study to the interpretation of heart sounds:

> The sounds heard by the analyst are very different: They are verbal expressions of complex mental "productions," which are themselves embedded in dense life narratives. How does the analyst hear these sounds? He listens through a theoretical filter that translates sounds into meaningful, narratively embedded utterances. This filter… comprises not only a theory

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[69] Ken Alder, "The History of Science, Or, an Oxymoronic Theory of Relativistic Objectivity," *A Companion to Western Historical Thought*, ed. by Lloyd Kramer and Sara Mazar (London: Blackwell, 2002), 307.
[70] Frederick C. Strong III, "Guest Editorial: The History of the Double Blind Test and the Placebo.," *Journal of Pharmacology and Pharmacotherapeutics* 51 (1997): 237.
[71] Marcia L. Meldrum, "A Brief History of the Randomized Controlled Trial: From Oranges and Lemons to the Gold Standard," *Hematology/Oncology Clinics of North America* 14 (2000): 754.

> (or theoretical sensibility) but also the analyst's own subjective personhood, his or her own unique subjectivity.[72]

Thus, the creation of the case study is not merely vulnerable to the subjectivity of the therapist; it is predicated on it.

Far from meeting the criteria for objectivity that was growing ever more important to science in the 1960s, the case history began to seem less like a research method and more like a literary form. Interestingly, though Freud never won the Nobel Prize in medicine, Thomas Mann and other literary notables of the day publicly advocated for him to be awarded the Nobel Prize in literature,[73] and he did win the Goethe Prize, a literary award, in 1930.[74] Freud observed, "[I]t still strikes me myself as strange that the case histories I write should read like short stories and that, as one might say, they lack the serious stamp of science. I must console myself with the reflection that the nature of the subject is evidently responsible for this, rather than any preference of my own."[75]

The last sentence of this quotation is particularly interesting: Freud not only recognized that case studies had scientific shortcomings in the eyes of others; he considered these shortcomings to be intrinsic to psychoanalysis. There is no way to write a psychoanalytic case study, Freud implied, that is free of the subjective influence of the therapist. In this sense, the case study is to the therapist as the short story is to the fiction author; the creative role and talent (or idiosyncrasy) of the therapist cannot be divorced

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[72] Stepansky, 161-162.
[73] Stone.
[74] Sulloway, 265.
[75] Joseph Breuer and Sigmund Freud, *Studies on Hysteria,* trans. and ed. by James Strachey. (London: The Hogarth Press and the Institute of Psycho-Analysis, 2000), 160.

from the creation of the case study. On the inescapable influence of the individual therapist in psychoanalytic research, Horwitz writes:

> [H]ow could anyone be shown *not* to have an Oedipal complex when protestations that one had no such desires were taken as evidence of resistance to admitting its presence [Hale 1995]? Freud, for example, interpreted his patients' refusal to accept his interpretations of their symptoms as confirmations of his theory of repression.[76]

In short, the therapist-as-author has the authority to interpret anything the subject says or does, which allows the therapist to make the subject's testimony fit the therapist's overall narrative of the case study. Therefore, though Freud was rather dismissive of the literary rather than scientific appearance of the case study, attributing it not to his own talent as a writer but to the nature of psychoanalysis itself, it is clear in the quotation above (originally published in 1895 in *Studies on Hysteria*) that Freud did not anticipate the extent to which allegations of not being scientific would eventually come to haunt psychoanalysis: by the criteria of scientific objectivity as it became conceptualized in the second half of the twentieth century, case studies simply could not accurately represent their subjects, so the knowledge they produced—if they could be considered to produce any at all—could not be given the label "scientific."

The role of this apparent subjectivity in the decline of psychoanalysis is reflected by the difficulty of translating case studies into the cumulative knowledge that Stone felt psychoanalysis needed. Data gathered from surveys can be added to, and experiments can be replicated, but each case study is a standalone endeavor as unique as the individual

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[76] Horwitz, 60.

subject. Furthermore, any psychoanalyst who wishes to "cross-examine" the patient in another psychoanalyst's case study is likely to be prevented due to confidentiality restrictions, and so even individual case studies are unable to be replicated or added to (except once they are published, when individual psychoanalysts can dispute interpretations or cite them as evidence for their own hypotheses).

One important criterion for scientific objectivity that increasingly came into play during the twentieth century was that data gathered from separate studies should theoretically be consistent across studies, given certain conditions (e.g. the experiment is performed correctly, the sample size is large enough, etc.); that is, the experiment would be *reproducible*. Reproducibility was increasingly considered a prerequisite for knowledge to count as "science." In *The Logic of Scientific Discovery,* Karl Popper famously declared, "Non-reproducible single occurrences are of no significance to science."[77] In addition to its growing role as a criterion of objectivity, reproducibility also opens up the possibility of repeating the inquiry with certain variations to explore a subject thoroughly. In short, reproducibility means that knowledge can be cumulative: because experiments can be repeated with everything held the same except a single manipulated variable, the conclusions drawn from the study can be relatively easily added to and integrated with previous knowledge on the subject.

The ability of reproducibility to create cumulative knowledge did not work for the case study. The inescapable possibility that two psychoanalysts could draw two different conclusions from the same patient frustrated the possibility of reliably building upon the knowledge gained from a case study. Thus, despite Eissler's claim for the fundamental role

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[77] Karl R. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959), 203.

of the case study in psychoanalysis' status as an empirical science, this method, unlike other research methods, was not useful to psychiatry in its quest for scientific legitimacy: the case study was incapable of producing a body of knowledge that met the criteria of being "scientific" according to the standards of the era.

In 1982, the growing tension between the theoretical approach of psychoanalysis on the one hand and the drive toward objectivity and empiricism in the rest of science on the other culminated in a major public embarrassment for psychoanalysis: Rafael Osheroff's lawsuit against the Chestnut Lodge. Osheroff, a 42-year-old nephrologist from Virginia, was admitted to the psychoanalytically-inclined Chestnut Lodge in Baltimore for severe depression in 1979. During his seven-month stay, Osheroff engaged in regular psychotherapy sessions, but, despite his requests and despite some success with medications prior to his admission to the Lodge,[78] he was denied medication. If Osheroff wanted to make genuine and permanent progress, his caretakers argued, he must regress to the point in his childhood from which his symptoms sprung;[79] relieving his symptoms with medications would, therefore, only impede progress.

Dr. Osheroff's life began to unravel. During his stay, he lost forty pounds, developed severe insomnia,[80] and his feet began to bleed from feverish pacing.[81] He eventually managed to transfer to another clinic, the Silver Hill Foundation, where he was given medication. His symptoms improved after three weeks, and he was discharged after only

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[78] Stepansky, 9.
[79] Shorter, 309.
[80] Stepansky, 9.
[81] David Healy, *Let Them Eat Prozac: The Unhealthy Relationship between the Pharmaceutical Industry and Depression* (New York: New York University Press, 2004), 234.

three months (with no relapse in the next decade).[82] However, the damage had been done: Osheroff returned home to find his wife had left him, he had lost custody of his children, and he had been forced out of his joint practice by his partner.

Osheroff sued the Chestnut Lodge for malpractice in 1982, claiming that, by being denied medications whose efficacy was well established in favor of a psychotherapeutic regime that, if anything, made him worse, he was denied state-of-the-art medical treatment. Osheroff was awarded $250,000 by an arbitration panel, but both plaintiff and defendant appealed. Eventually a settlement for an undisclosed sum was agreed upon.[83]

The Osheroff case certainly constituted a humiliating moment for psychoanalysis: it appeared that psychoanalysis was so ineffective in treatment of depression that its therapeutic failure had become a legal issue. Perhaps more significantly, though, the case seemed to highlight what many felt was an inescapable shortcoming of psychoanalysis: it simply did not subject itself to the new techniques of objectivity that other sciences were undergoing. In an article titled "The Psychiatric Patient's Right to Effective Treatment" for the *American Journal of Psychiatry,* Gerald Klerman, a prominent Harvard psychiatrist who had testified on behalf of Osheroff, claimed:[84]

> With regard to all kinds of therapeutics…the most scientifically valid evidence as to the safety and efficacy of a treatment comes from randomized controlled trials when these are available. Although there may be other

[82] Septansky, 9.
[83] Ibid., 10.
[84] This article took place in the context of a debate with Alan Stone in the same journal and issue, several years before Stone's keynote address to the American Association of Psychoanalysis. See Alan A. Stone, "Law, Science, and Psychiatric Medicine: A Response to Klerman's Indictment of Psychoanalytic Psychiatry," *American Journal of Psychiatry*, 147 (1990): 419-427.

> methods of generating evidence, such as naturalistic and follow-up studies, the most convincing evidence comes from randomized controlled trials.[85]

For Klerman and others, it was that simple: the best, most scientific kind of evidence was that which came from controlled trials, and controlled trials supported the efficacy of antidepressants while controlled trials supporting psychoanalysis were lacking; ergo, withholding medication from Osheroff was, objectively, the wrong choice.[86]

Through the Chestnut Lodge trial, psychoanalysis suffered a major blow to its credibility with the public, within psychiatry, and with other physicians and scientists.[87] Though no legal precedent was set by the case, the result was that psychiatrists were given the impression that treating serious mental illness with psychoanalysis could potentially constitute malpractice.[88] More importantly, though, the Chestnut Lodge incident marked the climax of the growing pressure on psychoanalysis to be objective. The title of Klerman's article (not to mention the fact that psychoanalysis' legal status was called into question) was just one part of a much broader trend in public and scientific opinion: psychoanalysis' lack of objectivity was not just unscientific—it was dangerous.

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85 Gerald L. Klerman, "The Psychiatric Patient's Right to Effective Treatment: Implications of *Osheroff vs. Chestnut Lodge*," *American Journal of Psychiatry*, 147 (1990): 412.
86 For a continued discussion of the Osheroff case and psychiatric authority, see Michael Robertson, "Power and knowledge in psychiatry and the troubling case of Dr Osheroff," *Australian Psychiatry*, 13 (2005): 343-350.
87 Robertson, 347.
88 Shorter, 310.

CHAPTER 3

The Rise of Diagnostic Psychiatry

Dynamic psychiatry was not practiced in a vacuum, and it is difficult to analyze how psychoanalysis disappeared from psychiatry without comparing psychoanalysis to competing ideologies and practices. Perhaps the most important of these competing ideologies was diagnostic psychiatry. As the name suggests, the shift to diagnostic psychiatry consisted in a tremendous proliferation and utilization of medical diagnoses in psychiatry. Diagnoses had been used in American psychiatry for centuries, if only in categorizing the seriously ill patients in mental asylums. But the "diagnostic revolution" led to a tremendous increase in both the number and use of such diagnoses, such that there was an 800% increase in psychiatric diagnoses in the last fifty years of the twentieth century.[89] The possibilities created by the formation of a set of expansive but discrete diagnoses were too tempting for the discipline of psychiatry to ignore. Diagnostic psychiatry offered something that psychoanalysis could not—a way to organize and systemize (and, as I will argue later, fund) the production of knowledge.

Dynamic and diagnostic psychiatry were, to a large extent, two models that contradicted each other. Diagnostic psychiatrists were more likely to use medications in treatment; dynamic psychiatrists, despite having medical training, preferred to use analysis and eschewed the pharmacological approach.[90] There was one similarity, however: both models treated domains of conditions that encompassed a broad range of behaviors and emotions. Mayes and Horwitz write, "By the 1970s, the clients of dynamic psychiatrists

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[89] Arthur C. Houts, "Fifty Years of Psychiatric Nomenclature: Reflections on the 1943 War Department Technical Bulletin, Medical 203," *Journal of Clinical Psychology* 56(2000): 935.
[90] Jonathan Engel, *American Therapy: The Rise of Psychotherapy in the United States* (Penguin Group Inc.: New York, 2008), 219.

were people with poor marriages, troubled children, failed ambitions, general nervousness, and diffuse anxiety."[91] Horwitz argues that the broad range of conditions treated by diagnostic psychiatry was directly inherited from dynamic psychiatry.[92] That said, psychoanalysis as a theory did not neatly distinguish between the pathological and the normal in the way that diagnostic psychiatry did; rather, the healthy and the neurotic were simply ends of a continuum.[93]

Thus, the emergence of the diagnostic model in psychiatry constituted nothing less than the formation of an entirely new discourse, one that combined the implicit authority of a medical diagnosis with a framework for building up scientific knowledge in psychiatry. The Kuhnian model of scientific revolution proves useful in describing the significance of this change: the movement to diagnostic psychiatry represents a paradigm shift in American psychiatry. Horwitz writes:

> In Kuhn's view, a transformation from one thought community to another rarely arises out of the development of new knowledge; instead, such change is only undertaken in order to resolve a state of crisis in the previously dominant paradigm. The new model gains acceptance not so much because it more accurately characterizes the natural world as because it is better able to justify the social practices of the relevant discipline.[94].

This very accurately describes the case of the shift from psychodynamic to diagnostic psychiatry in American psychiatry in the second half of the twentieth century. As

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[91] Rick Mayes and Allan V. Horwitz. "DSM-III and the Revolution in the Classification of Mental Illness," *Journal of the History of the Behavioral Sciences* 41 (2005): 251.
[92] Horwitz, 2.
[93] Ibid., 1.
[94] Ibid., 57.

I will argue later in this chapter, the shift to the diagnosis in American psychiatry was not based on growing evidence for the validity of the diagnosis, the medical model, or the understanding of mental suffering in terms of discrete illness analogous to biological illnesses; rather, the diagnosis became popular in American psychiatry because it addressed the perceived shortcomings of psychoanalysis. The "crisis in the previous dominant paradigm," in the case of American dynamic psychiatry, was, in short, that psychoanalysis faced difficulties in meeting the sociocultural pressure to produce knowledge. The new model that was "better able to justify the social practices of the relevant discipline" was diagnostic psychiatry, which became popular because it enabled a more systemized (and hence more easily funded) way of studying mental distress.

Before the tension between psychoanalysis and diagnostic psychiatry can be examined, it is important to establish a brief history of the diagnosis in American psychiatry. This history is most easily told through the creation and evolution of texts offering up diagnostic systems, in particular the *Diagnostic and Statistical Manual of Mental Disorders* (DSM). The DSM, published by the American Psychiatric Association, is a text containing a collection of mental illnesses and their diagnostic criteria. It has been published in several editions and revisions since the DSM's first publication in 1952 (DSM-I), with the DSM-V being the most recent edition, released in May 2013.

There were attempts to establish classificatory systems well before the publication of the first edition of the DSM. One such attempt was the 1918 *Statistical Manual for the use of Institutions for the Insane,* a joint effort by the American Medico-Psychological Association (which later became the APA) and the National Committee for Mental Hygiene.

The purpose of this manual was to standardize the data reports made annually by mental hospitals in the United States.[95]

The publication of the DSM-I was another significant step in the shift to diagnostic psychiatry. Published in 1952, the DSM-I was a direct result of the large demand for and changing responsibilities of psychiatry in the wake of World War II; the demand on psychiatry to provide assessment and treatment of soldiers created the necessary conditions for the formation of a text like the DSM-I.[96] Though containing an impressive 106 diagnoses,[97] the DSM-I did not conflict with psychoanalysis in the way that diagnostic psychiatry eventually did. In fact, the DSM-I came out of a classificatory scheme called Medical 203 created for the Army by a prominent psychodynamic psychiatrist named William Menninger, and the DSM-I is consequently highly inflected by psychodynamic thought.[98]

The first revision of the DSM took the form of the DSM-II, published in 1968. The DSM-II no longer characterized mental disorders using the psychoanalytic term "reaction"

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[95] American Medico-Psychological Association and the National Committee for Mental Hygiene, *Statistical Manual for the use of Institutions for the Insane Prepared by the Committee on Statistics of the American Medico-Psychological Association in Collaboration with the Bureau of Statistics of the National Committee for Mental Hygiene* (New York: 1918), 3-4.

[96] For a thorough assessment of the cultural and historical context in which the DSM-I was created, see Gerald N. Grob, "Origins of *DSM-I*: A Study in Appearance and Reality," *The American Journal of Psychiatry* 148 (1991): 421-431.

[97] American Psychiatric Association. *Diagnostic and statistical manual of mental disorders: DSM-I* (Washington, DC: American Psychiatric Association, 1952).

[98] Mitchell Wilson, in his "DSM-III and the Transformation of American Psychiatry: A History," writes, "The hegemony of the psychosocial theory in which individual psychological conflict and environmental circumstance collide to produce psychopathology, was nowhere better exemplified than in DSM-I…" (401).

(although it retained the term "neurosis")[99], but like its predecessor, it reflected the psychodynamic thought dominant in American psychiatry at that time.[100]

The turning point of the "diagnostic revolution," is widely held to be the publication of the DSM-III in 1980.[101,102] Similar to its predecessors but larger in scope, the DSM-III was at least partially a reaction to growing concerns that the United States and other countries were diagnosing mental disorders in different ways—it seemed, for example, that English psychiatrists diagnosed bipolar disorder significantly more often, and schizophrenia significantly less often, than American psychiatrists.[103] This seemed at least partially due to the disproportionate influence of psychoanalysis in American psychiatry compared with other countries at this time.[104] This led to increased pressure for standardizing diagnostic criteria internationally. The DSM-III was published in 1980 and contained 265 diagnoses.[105]

Table 1. Expansion of DSM. Reproduced from Rick Mayes and Allan V. Horwitz. "DSM-III and the Revolution in the Classification of Mental Illness," Journal of the History of the Behavioral Sciences. 41 (Summer 2005): 249–267.

Version	Year	Total Number of Diagnoses	Total Number of Pages
I	1952	106	130
II	1968	182	134
III	1980	265	494
III-R	1987	292	567
IV	1994	297	886

99 Houts, 947.
100 Mayes and Horwitz, 249–267.
101 Wilson, 399-410.
102 Mayes and Horwitz, 249-267.
103 See, for example, Ronald Gelfand and Frank Kline, "Differences in Diagnostic Patterns in Britain and America," *Comprehensive Psychiatry* 19 (1978): 551-555.
104 Ibid., 552.
105 American Psychiatric Association. *Diagnostic and statistical manual of mental disorders: DSM-III* (Washington, DC: American Psychiatric Association, 1980).

It was in the publication of the DSM-III that the conflict between dynamic and diagnostic psychiatry reached its apex. Despite being created by a committee chaired by Robert Spitzer, a psychiatrist with dynamic leanings, the DSM-III, unlike the DSM-I or DSM–II, did not attempt to incorporate psychoanalytic theory almost at all.[106]The DSM-III created its own problems for psychiatry—it was widely criticized for pathologizing normal human behaviors, such as tobacco dependence and poor attention span in children[107](in a 2007 interview, Spitzer himself estimated that, after the DSM-III was published, 20 or 30 percent of the population were misdiagnosed as having a mental disorder[108]). However, the shift to the diagnostic model, despite the issues in public relations it presented, proved beneficial for psychiatry's authority on the whole. The rigorous laying out of specific criteria in the various editions of the DSM and its precursors was a direct attempt to standardize the classification of mental distress, and thereby to solve the problems posed to research by the psychoanalyst's subjectivity that are outlined in Chapter 2.

The clash between the psychoanalytic community and the creators of the DSM-III was, in fact, strongly symbolic of the shift from the dynamic to the diagnostic perspective as a whole. Despite critiques that it neither added anything new to our understanding of mental illness nor correctly represented its nature, the diagnostic perspective prevailed by virtue of its implicit scientific authority. Classificatory systems, though a tool of "objective" science, are not always innocent, as Foucault established in *The Order of Things*, nor are

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[106] Houts, 935.

[107] Hannah S. Decker, *The Making of DSM-III: a Diagnostic Manual's Conquest of American Psychiatry* (New York: Oxford University Press, 2013), 263, 272.

[108] "The Lonely Robot." *The Trap: What Happened to Our Dream of Freedom*. By Adam Curtis. BBC Two. 18 March 2007. Television. Accessed via YouTube.com 11 January 2014.

they free from subjectivity: Gerald Grob, for one, in reference to the development of psychiatric nosology, writes:

> Classification systems are neither inherently self-evident nor given. On the contrary, they emerge from the crucible of human experience; change and variability, not immutability, are characteristic. Indeed, the ways in which data are organized at various times reflect specific historical circumstances. Empirical data, after all, can be presented and analyzed in endless varieties of ways.[109]

Though the objectivity of classificatory systems is itself unreliable, it is certainly true that it was *viewed* as objective, and therefore psychiatry was able to ground its authority in a classificatory model, like the rest of biology and medicine.

Revisiting the Kuhnian approach to the shift to the diagnostic model, it is not difficult to see the underlying social pressures predicted by Kuhn's model: the movement to classification greatly augmented the scientific credibility of psychiatry as a whole. The complement to the idea that scientific change is motivated by social factors, though, is that it is *not* necessarily motivated by scientific developments. This is demonstrably true in the history of the DSM-III. The DSM-III caused controversy for a number of reasons, but particularly for its alleged emphasis on *reliability* (that is, defining diagnoses in such a way that multiple psychiatrists would diagnose the same patient the same way) over *validity* (defining diagnoses to actually describe the mental illness as accurately as possible)—in short, an emphasis on *consistent* diagnosis rather than *meaningful* diagnosis. Creating and defining mental illnesses in a way that prioritized reliable diagnosis became a priority of

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[109] Grob, 421.

the creators of the DSM-III;[110] Decker says simply, "For Spitzer, reliability trumped validity."[111]

Perhaps unsurprisingly, on the other side of the "reliability versus validity" debate were the dynamic psychiatrists.[112]These psychoanalytically trained psychiatrists commonly complained that the deliberately "atheoretical" approach taken by the creators of the DSM-III in the name of objectivity completely eschewed the etiological insights offered by the psychodynamic perspective, which was still the dominant view in psychiatry at that time.[113] Many psychoanalysts felt that this snub was inherent in the nature of the project, as neither classification nor diagnosis had strong roots or obvious usefulness in psychoanalysis. (Freud himself rejected the idea of a taxonomy of mental illness, in part because he felt that such a system could only offer a superficial understanding of mental distress, and that this would directly inhibit psychoanalysis' ability to explain a patient comprehensively, and partly because psychoanalytic theory held, at its core, the belief that each individual subject was unique.[114])

Like the growth of classification in psychiatry, the relative emphasis on reliability over validity represented a conscious effort on the part of diagnostic psychiatry to display objectivity and empiricism, regardless of whether its diagnoses were accurate or beneficial: while the validity of the diagnoses might be hard to assess, their reliability could be easily

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[110] Horwitz, 69.
[111] Decker, 132.
[112] Andrew E. Skodol, "Diagnosis and Classification of Mental Disorders," in *American Psychiatry after World War II, 1944-1994,* ed. Roy W. Menninger and John C. Nemiah (Washington, D.C.: American Psychiatric Press, 2000), 440.
[113] Ibid. 441.
[114] Decker, 132.

measured (and they were, with encouraging results[115]). True, just because the diagnoses of the DSM-III were reliable did not necessarily mean they were valid. But with proving the validity of its diagnoses once and for all an ambitious and monumentally difficult project, diagnostic psychiatry gained credibility for itself in the way that it could: it established that its diagnoses were *reliable*. Proving reliability did not prove validity, but in terms of establishing social legitimacy, it was nearly as useful. First, it gave the diagnoses of the diagnostic psychiatry the appearance of internal coherency. Second, though reliability does not entail validity, it is logical that validity entails reliability.[116] Therefore, the DSM-III's reliability seemed to suggest, at least superficially, that the diagnostic model met the prerequisites for validity in a way that psychoanalysis did not—this reliability in diagnosis, after all, is directly analogous to the concept of reproducibility that, as I argued in Chapter 2, was nearly impossible in dynamic psychiatry and increasingly becoming a fundamental criterion for the scientific objectivity of knowledge. In this way, the "reliability versus validity" debate demonstrates that the classificatory system propounded by the DSM-III was motivated more by social pressures than new discoveries in science, just as Kuhn described.

Certain growing demands on psychiatric research also facilitated the shift from the dynamic to the diagnostic model. The rise of partial reimbursement for psychotherapy by medical insurance companies during the 1960s added further pressure for the development of a "categorical, rather than continuous, model of illness."[117] Medical insurance plans paid, on average, one quarter of outpatient psychotherapy treatment; this

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[115] R.L. Spitzer, J.B. Forman, J. Nee, "DSM-III fields trials: I. Initial interrater diagnostic reliability." *American Journal of Psychiatry*, 1979 136:(6) 815-817.
[116] Skodol, 438.
[117] Horwitz, 75.

proportion grew steadily throughout the 1970s, and the insurers grew to include Medicaid.[118] Insurance companies required that treatment effectiveness have some way of being objectively assessed. Mayes and Horwitz write:

> To objectively determine what the optimal treatment was for a given mental disorder, the critics called for new and stringent standards for demonstrating effectiveness, such as those used by the FDA to test the efficacy of drugs: quantitative and comparative studies based on matched samples of patients uniformly diagnosed, randomly assigned, and treated with standardized procedures, with outcomes judged not only by clinicians but by impartial observers not involved in the treatment.[119]

The economic pressures of third-party payers to formalize the study of mental distress necessitated the creation of discrete mental illnesses with specific criteria delineated in a text agreed upon by consensus (i.e. the DSM or *International Classification of Diseases* [ICD], the international equivalent of the DSM).

It is inaccurate to characterize the shift to the diagnostic model as simply a matter of greed, because the market for psychotherapy, including psychoanalysis, was "lucrative and growing" at this time.[120]But it would be equally inaccurate to ignore the financial incentives for shifting to the new diagnostic paradigm. Though the continuous nature of neuroses according to the dynamic model made it possible for nearly anyone to be a patient, the sheer number and variety of mental illnesses in the diagnostic model ensured there would be no dearth of patients in that model either. More importantly, though, while the *practice*

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118 Ibid.
119 Mayes and Horwitz, 256.
120 Horwitz, 59.

of psychiatry may still have been lucrative under the dynamic model, *research* thrived best under the funding enabled by the classificatory diagnostic model. The founding of the National Institute of Mental Health (NIMH) in 1946 was an important moment for psychiatry, as it became an important source of funding for mental health research in the United States. Initially focused on providing mental health services for communities across the nation, during the 1970s growing pressure from research psychiatrists (and, Horwitz suggests, an increasingly conservative Congress that opposed the "sweeping social agendas" of the NIMH's community centers[121]) led to a gradual shift in focus and funding to the research of the diagnostic entities of the DSM.

It was fundamentally this advantage that led to the success of the diagnostic model of psychiatry: the classification of mental distress enabled more organized and (seemingly, at least) more empirical research. This organization and empiricism seem to lend classificatory systems like taxonomy and nosology an implicit authority as a scientific endeavor; Hannah Decker, in her account of the DSM-III's creation, states, "Classification is a necessary endeavor that human beings automatically carry out from early infancy on in order to comprehend the world they live in."[122] Though whether a tendency toward classification is an essential characteristic of human nature is certainly up for debate, Decker does manage to convey the historical significance of classification as a scientific technique. It is classification's implicit authority that gave the field of psychiatry, when it shifted to the diagnostic model, both an immediate boost in credibility and, more importantly, the means by which to create and organize new scientific knowledge: the

[121] Ibid., 77.
[122] Decker, 129. See also Geoffrey C. Bowker and Susan Leigh Star, *Sorting Things Out: Classification and its Consequences* (Cambridge, Mass: MIT Press, 1999).

creation of a system of distinct diseases allowed mental distress to be defined and rendered researchable by modern scientific methods.[123]As Grob puts it, "In modern medicine, as in modern society, classification systems play a crucial role, for without such systems the collection and analysis of data are all but impossible."[124]

And this, more than lucrative psychotherapy sessions, was what psychiatry needed; psychotherapy could, in theory, be practiced without a medical degree, but in order for psychiatry to maintain its legitimacy as a science and branch of medicine, it needed to find a way to produce knowledge. Thus, the shift to the diagnostic model seems to have been motivated by certain political pressures on psychiatry, from within and outside the discipline, to develop new ways of producing scientific knowledge. This, in short, is why psychiatry rejected psychoanalysis toward the end of the twentieth century: psychiatry found itself in the middle of a culture where psychiatry's adherence to psychoanalysis jeopardized its existence.

[123] Horwitz, 58.
[124] Grob, 421.

CONCLUSION

Between Society and Science

I am a pre-medical student, and when my fellow pre-med students find out I am concentrating in English, the second thing they say is a question: why did I choose a degree so different from my career aspirations? I always tell them the same thing: Nothing happens in a vacuum. Science does not happen in a vacuum, and medicine certainly does not happen in a vacuum. I study society and culture not in spite of my medical ambitions, but because of them. This response is satisfactory to my fellow English majors, but my pre-med friends tend to get even more confused. I believe this is explained by the *first* thing they usually say when they find out I'm an English major: "Oh, I could never do that! I'm no good at that subjective stuff. I prefer science and math, where there are right and wrong answers."

To many, science and medicine are havens of objectivity and empiricism. For these people, the idea that society, politics, and culture could pervade the sanctity of these havens is entirely unheard of. Yet, this is undoubtedly the case—even if the objects of study themselves are indifferent to human society, the way that we study them is not. How do we define what counts as "science?" Does it matter? When is something empirical? Are non-empirical research methods to be discounted entirely? What are the societal implications of scientific discoveries?

Perhaps the most undeniable example of the inevitable mingling of society and scientific research is the question of what gets researched.[125] And this question is in part

[125] Complementarily, the question is often *who* gets researched. See Steven Epstein, *Inclusion: The Politics of Difference in Medical Research* (Chicago: University of Chicago Press, 2007).

answered by the concept of "publish-or-perish:" that is, the institutional pressure on researchers and academics to produce knowledge in quantity even at the expense of quality—and, implicitly, to do so without questioning the definition of knowledge.[126] In this thesis, I apply the concept of publish-or-perish not just to a single researcher or laboratory, but an entire discipline. Psychiatry's abandonment of psychoanalysis, I have argued, was essentially due to psychoanalysis' failure to produce knowledge, or at least knowledge considered "scientific." The diagnostic model performed extremely well in this regard, and it is mostly due to this that we now operate under the diagnostic model rather than the dynamic.

In his book *Impure Science: AIDS, Activism, and the Politics of Knowledge,* Steven Epstein analyzes the example of protesters from ACT UP (the AIDS Coalition to Unleash Power) demonstrating outside Harvard Medical School on the first day of classes in fall 1988. The protesters passed out fliers for an "AIDS 101" course with an outline of topics like, "AZT—Why does it consume 90 percent of all research when it's highly toxic and is not a cure?" and "Medical elitism—Is the pursuit of elegant science leading to the destruction of our community?" Epstein writes, "These protesters were not rejecting medical science. They were, however, denouncing some variety of scientific practice—'elegant' science, 'what Harvard calls "good science"—as not conducive to medical progress and the health and welfare of their constituency."[127] That is, the chemical properties and clinical efficacy of AZT might not be a social construction, but how the research gets carried out, and whether it gets carried out at all, certainly is determined by society. Thus, understanding the

[126] Ushma S. Neil, "Publish or perish, but at what cost?", *Nature* 467 (2010): 252.
[127] Steven Epstein, *Impure Science: AIDS, Activism, and the Politics of Knowledge* (Berkeley and Los Angeles: University of California Press, 1996), 2.

intersection of society and science is indispensible to anyone interested in practicing science and medicine.

To understand better the social politics underlying our concepts of mental illness, biological illness, and medicine more generally is ultimately the purpose of this thesis. The diagnostic model is one way of organizing and understanding mental illness; it has its advantages and disadvantages, and may certainly be more useful to particular ends than other models, but ultimately, the discrete mental illnesses of the diagnostic model are still just tools that we use to understand the same phenomena (e.g. sadness, anxiety, etc.) that Freud was attempting to explain through psychoanalysis. Furthermore, as argued in Chapter 2, the concept of objectivity in science has changed over time; even if it is not entirely a social construction, the fact that it has changed even as recently as the late twentieth century implies that our current conception of objectivity—or, at least, our supposedly objective practices and techniques—are still flawed or incomplete. Thus, scientists and medical professionals who acknowledge this may gain perspective in their own research and practice.

Regardless of whether the subject matter of science and medicine is indifferent to societal factors, science and medicine are both conducted in a social and political world. Paul Starr, in *The Social Transformation of American Medicine,* writes, "[Physicians] serve as intermediaries between science and private experience, interpreting personal troubles in the abstract language of scientific knowledge."[128] If this is true, then science is only half of the equation; to carry out their job, physicians must familiarize themselves with both the social origins and social consequences of the scientific knowledge they utilize. In this way,

[128] Paul Starr, *The Social Transformation of American Medicine* (New York: Basic Books, Inc., 1982), 4.

my analysis of the death of psychoanalysis in American psychiatry is more or less my extremely long response to those pre-med friends who are confused about why I majored in English. To borrow a term from medicine, this thesis is an autopsy—a case study of an organism that is now dead, performed in order that we might better understand what is still alive.

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