

PSYCHOANALYSIS AND THE MODEL OF HOMOSEXUALITY AS PSYCHOPATHOLOGY: A HISTORICAL OVERVIEW

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As the founder of psychoanalysis, Freud entertained complex and inconsistent views about homosexuality. In a famous letter to the mother of a homosexual, he stated that homosexuality is neither a vice nor an illness (Freud, 1935). He spoke out against persecution of homosexual people and pointed out that many great men, among them Plato, Michelangelo, and Leonardo DaVinci, were homosexual. He also observed that it was generally not possible to change sexual orientation with psychoanalysis. In a letter to Ernest Jones, he affirmed the right of homosexual people to become psychoanalysts (Freud, 1921).

In other writings, however, he took a different stance that critics have suggested stemmed from a number of biases (Friedman, 1988; Lewes, 1988; Fisher and Greenberg, 1985; Domenici and Lesser, 1995). For example, Freud based generalizations about all people on psychoanalytic case studies of individual patients. This led him to discuss "primitive" mental mechanisms found in "inverts" and to suggest that they compulsively fled from passionate relationships with people of the opposite sex (1905).

Freud's giftedness as a writer lent weight to ideas which, had they been expressed by someone of less stature, might not have been so influential. Most psychoanalysts, until quite recently, accepted that generalizations about human behavior could validly be based on psychoanalytic studies of a few individuals. Most seemed to agree with Freud's tendency to regard psychoanalysis as a tool that allowed clinicians to study the structure of the psyche in similar fashion as histologists used microscopes to observe the structure of single cells.

As many critics have pointed out, Freud also had a phallogocentric anti-female bias. He felt that women were more "primitive" than men. He

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spoke of "the greater proneness of women to neuroses and especially to hysteria." Freud attributed this to a model of female development that has long been abandoned (Freud, 1933; Horney, 1939; Tyson, 1992). Some critics have suggested that Freud's formulation of pathological dynamics in male "inverts" was similar to those he believed generally characteristic of women (Lewes, 1988).

Freud lacked terminology to express important distinctions that are useful in understanding sexuality in the context of development. The concepts of gender identity (the psychological sense of maleness or femaleness, not primarily based on the genital) and gender role (the expression of gender identity by one's behavior in social situations) did not exist during Freud's lifetime. Thus, he did not differentiate erotic from other gender-based activity. Similarly, he did not always make the distinction between erotic fantasy and erotic behavior. As a result, Freud proposed condensations such as "passive-feminine-homosexual (man)" that were not based on fact and have proven to be incorrect (Freud, 1905; Friedman, 1988).

Freud also believed that the Oedipus complex was biologically determined and universal (Freud, 1924, 1940). Failure to resolve oedipal conflicts led, in his view, to psychopathology. It was impossible for countless psychoanalytic clinicians to imagine how a man who was sexually attracted to his *mother* during childhood could become predominantly or exclusively homosexual during adulthood and still be—in some sense—"normal." Many psychoanalysts today still retain this view.

Another of Freud's cherished concepts—his theory of constitutional bisexuality—proposed that no matter what conscious mental experience was like, same-sex and opposite-sex sexual objects were universally experienced on an unconscious level. In 1915 he wrote: "By studying sexual excitations other than those that are manifestly displayed, it has been found that all human beings are capable of making a homosexual object choice and in fact have made one in their unconscious. Indeed libidinal attachments to persons of the same sex play no less a part as factors in normal mental life, and a *greater part as a motive force for illness*, than do similar attachments to the opposite sex" (Freud, 1915, italics added by authors). Freud's ideas about unconscious homosexuality influenced his speculation that paranoid delusions were pathological defenses against unconscious homosexual wishes (Freud, 1911). This hypothesis has not withstood the test of time (Friedman, 1988). His theory that unconscious heterosexual wishes were universal led to the erroneous belief that the sex of the consciously experienced erotic object could be altered by psychoanalysis (Socarides, 1978). We discuss this in more detail later in this chapter.

Freud suggested a number of theories about the *etiology* of homosexuality. These included castration terror stimulated by the sight of the

mother's genitals and the alteration of the sexual object from the mother to a female with a penis; prolonged and excessive early closeness to the mother, leading to pathological identification with her and therefore the wish to love others who are like oneself; the "negative" Oedipus complex in which, for a number of possible reasons, the child identifies with the opposite-sex rather than the same-sex parent; the turning of rage toward same-sex siblings—and presumably father—into homosexual love via the defense of reaction formation (Freud, 1905, 1910, 1918, 1922; Lewes, 1988).

HOMOSEXUALITY AS PSYCHOPATHOLOGY

In the United States during World War II and the postwar years, most psychoanalytic writings about homosexuality utilized Freud's drive theory and his model of childhood development. The emphasis in most articles was to put forth a model directed at explaining the *etiology* of homosexuality and relating this to specific types of psychopathology in homosexual patients (Bayer, 1981; Lewes, 1988).

This line of speculation found in the writings of such analysts as Rado, Bergler, and Socarides (and many others) rested on the notion that "normal" development always resulted in heterosexuality. Derailment from a "normal" developmental track was most serious when it occurred during early childhood years, prior to age 5 or so. Such derailment, due to traumatic experiences of various types, not only produced what Socarides termed "obligatory" homosexuality but also led to global impairment in personality functioning. Two types of such impairments were stressed: In one, patients experienced severe anxiety, irritability, identity disturbances, and were prone to impulsive-compulsive activities and poor judgment. Patients in this group would be called borderline today. In the other category of pathology, patients were dominated by the pursuit of pleasure, had impaired frustration tolerance, and poor self-esteem regulation. Patients in this group would be called narcissistic today (Socarides, 1978). This way of thinking about homosexuality influenced the architects of DSM-I in which homosexuality was listed as a sociopathic personality disorder (1957) and DSM-II in which it was listed as a sexual deviation such as sexual sadism or fetishism (1968). Psychodynamically oriented practitioners were offered no way of conceptualizing homosexuality except in the framework of developmental derailment, character pathology, and sexual pathology. These psychoanalytic models of homosexuality were accepted not only by psychoanalysis and psychiatry, but by the other mental health professions as well.

The pathological model dominated psychiatric and psychoanalytic

thinking about homosexuality until quite recently. Scholarly commentary has suggested that this occurred because of antihomosexual bias (Lewes, 1988; Young-Bruehl, 1996; GAP monograph, in press). We agree but note that a biased attitude toward homosexual individuals was only one example of a widespread belief system that influenced psychoanalytic attitudes toward all behavior at that time. We comment below on a number of specific psychoanalytic attitudes and assumptions that, acting in concert, contributed to bias against homosexual people in particular. We discuss this from a historical perspective beginning with the relationship between American psychiatry and psychoanalysis in the 1940s.

PSYCHOANALYSIS AND WORLD WAR II

Psychiatric disturbances of combat soldiers were a significant problem during the Second World War. In major campaigns, more than 20% of all casualties were primarily behavioral reactions to combat (Hale, 1995).

At the beginning of the war, American psychiatrists were divided into two groups: organically oriented and psychoanalytic. The former tended to be institutionally based and to specialize in the treatment of the severe psychoses with electric convulsive therapy (ECT), insulin coma therapy, sedation, and rarely psychosurgery (Redlich and Freedman, 1966). To the degree that they counseled their patients, they tended to be directive and advice-giving. In contrast, the psychoanalytically oriented psychiatrists were younger and conveyed the enthusiasm of intellectual explorers. They were convinced that they were at the cutting edge of modern advances in the behavioral sciences. A few had been psychoanalyzed by Freud and others had studied with other pioneer psychoanalysts. The patients who they worked with had a much wider range of psychopathology than those treated by the organic psychiatrists. Some psychoanalytically oriented psychiatrists such as Harry Stack Sullivan (1962), Silvano Arieti (1955), and Freida Fromm Reichmann (1948) were committed to the treatment of psychotic patients. Others, following Franz Alexander's lead, specialized in psychosomatic disorders (Alexander, 1950). Many others worked with neurotic patients and/or those who experienced traumatic reactions to immediate stressors. As a group, the psychoanalytically oriented psychiatrists emphasized outpatient more than inpatient treatment, and unlike the other psychiatrists, were convinced that their approach was directed not only at symptom relief but at correcting the underlying causes of mental illness.

When the war came, the organic psychiatrists had relatively little to offer, either by way of understanding etiology or delivering effective treatment to traumatized combat troops. The psychoanalytically oriented psy-

chiatrists did, however. Training programs were set up in the Armed Forces and in civilian psychoanalytically oriented psychiatric centers temporarily working for the military.

The type of psychodynamic approach that was actually utilized so effectively with soldiers during World War II was not, in fact, strictly "psychoanalytic"—as the term came to be defined in the postwar years. Major psychoanalytic ideas were accepted such as that of the role of unconscious conflict in symptom formation and the significance of early childhood in shaping adult motivation. Psychoanalytic jargon and technical concepts were avoided. Psychiatrists working with combat soldiers applied therapeutic methods that were practical, flexible, and driven by the necessities of war. Hypnosis, abreaction, and catharsis were widely used, as were mixtures of organic and psychodynamic therapies such as the use of pentathol to assist in recovering repressed memories (Grinker and Spiegel, 1945). In the military, time-limited therapy was the rule, not the exception, and psychoanalysis as a treatment technique was obviously impractical. The willingness of the wartime psychoanalytically oriented psychiatrists to choose approaches to treatment based on their effectiveness and to apply psychoanalytic wisdom to pressing immediate problems led to respect for psychoanalysis throughout the Armed Forces and American society. Psychoanalysts achieved high status and became role models for most younger physicians who aspired to become psychiatrists.

Meanwhile, prior to and during the war, mainly because of Nazism, waves of European psychoanalysts migrated to the United States, England, and Latin America. A partial list of these includes many of the most dazzlingly brilliant contributors to the field: Franz Alexander, Therese Benedek, Bruno Bettelheim, Grete Bibring, Helen Deutsch, Erik Erikson, Paul Federn, Otto Fenichel, Heinz Hartmann, Karen Horney, Edith Jacobson, Margaret Mahler, Sandor Rado, David Rapaport, and Theodor Reik.

The mixture of largely native American military psychiatrists with émigrés led to the context within which specific psychoanalytic ideas and ideals were expressed in America during the postwar years. One cannot help but observe that the group is somewhat intimidating. Imagine the attitude of a psychoanalytic candidate in the early 1950s. Too young to have fought in the war, he or she might in fact have never even traveled abroad. From the list of émigrés and ex-military psychoanalysts, any combination of personal analyst, supervisors, and classroom teachers would yield an imposing array of life-experienced, energetic, effective people. This in itself may have led to a sense of awe at what one's elders had accomplished. Not only had they created psychoanalysis but they had also defeated the Nazis and the Japanese. It would have taken an unusual young person

indeed, who—in such company—had the courage to adopt the type of skeptical posture that challenges old assumptions and leads to scientific progress. Those who were able to accomplish this were in the minority.

In the mid-1950s, about one-third of American psychiatrists were psychodynamically oriented—a figure that was to grow to one-half during the next 10 years (Grob, 1980). By the 1960s, a majority of professorial positions in the most respected departments of psychiatry in the United States were held by psychoanalysts and/or candidates at psychoanalytic institutes. A psychoanalytic perspective about normality and psychopathology was taught to medical students and psychiatric residents at the nation's premier medical schools (*GAP Report*, 1962). In 1962, 52 of the 89 psychiatric departmental chairpeople were members of psychoanalytic organizations (*American Psychoanalytic Newsletter*, 1962).

The American Psychoanalytic Association was responsible for setting and enforcing standards for the training of psychoanalysts at most psychoanalytic institutes throughout the United States. In general, applicants were to be psychiatrically trained physicians. In 1955 only 6% of the 600 members of the American Psychoanalytic Association were not medical doctors (Hale, 1995). Because psychoanalytic educators believed that it was necessary for someone who sought to practice psychoanalysis to have been psychoanalyzed, a candidate was required to complete a didactic analysis of at least 300 hours, four times per week, conducted by a training analyst. Although training analyses lasted, on average, about three years in the 1950s, their duration extended subsequently. By the 1960s, it was not uncommon for training analyses to have lasted for five or more years.

As an additional requirement for graduation from a psychoanalytic institute, candidates were to have psychoanalyzed at least two patients completely—from beginning to successful termination. Other psychoanalyses were to be in progress as well. There was also a substantial didactic requirement. As a result of classroom study—usually at least six hours per week for a number of years—the candidate was expected to become thoroughly familiar with the psychoanalytic literature. The candidate's progress through this arduous course was determined by his or her training analyst and an education committee. The approval of both was required at key points along the way, such as psychoanalyzing patients after the initial phases of psychoanalytic education, or working with a patient with a specific type of psychiatric disorder.

These standards were in place by the 1960s and 1970s and were to be found with some modification in institutes affiliated with the American Psychoanalytic Association. This phase of psychoanalytic history exactly coincided with the publications of most of the influential psychoanalytic articles on the pathological model of homosexuality. Charles Socarides, for

example, is the most outspoken American psychoanalyst still advocating the general validity of the pathological model. The first papers on homosexuality that he published in the *Journal of the American Psychoanalytic Association* appeared in 1963—on female homosexuality—and in 1968—on male homosexuality. In these articles, in keeping with the conventions of the day, he used his clinical experience with patients as the exclusive database to support generalizations about *all homosexual people*.

Except during wartime, medical school took four years to complete, internship an additional year, and psychiatric residency an additional three years. Medical school was expensive and wages during internship and residency were low. At the beginning of psychoanalytic training, therefore, candidates tended to be in their early 30s, and either of independent means or struggling to make a living and repay debts. Those who did not apply for admission directly after completion of their residencies were even older. Their teachers, mostly men, were almost always full-time psychoanalytic practitioners who taught at and administered the psychoanalytic institutes on a part-time basis. Thus, the contributions of eminent women notwithstanding, the psychoanalytic institutes tended to be run by middle-aged and elderly men in private psychoanalytic practice (Hale, 1995).

During this period there was increasing emphasis on what today would be termed “classical Freudian” psychoanalysis, as opposed to psychodynamically oriented psychotherapy. This led in turn to a narrowing of the patient base appropriate for treatment. The demands on patient time and financial resources were considerable, even extraordinary. The typical psychoanalytic patient had to be able to afford to pay for four sessions per week and to fit these sessions into his or her schedule. Most patients had to attend analytic sessions during the day. The analytic session took 45 or 50 minutes; at least one-half hour travel time to get there and an additional one-half hour to return to work was necessary. The patient had to have control over his or her work schedule or be of independent means so that he or she was not required to work at all. The bias toward wealthy and highly successful patients was built into the organization of treatment.

The technical dimensions of the psychoanalytic treatment itself also led to a selection bias—toward reasonably functional people—and away from those with severe psychiatric illnesses. The patient was required to have adequate “observing ego” to be able to participate in the psychoanalytic process and simultaneously learn to understand himself or herself. Patients with psychoses, impulse disorders, addictions, and/or severe identity problems were usually deemed “unanalyzable,” although not necessarily without the capacity to profit from less intensive psychotherapy. This occurred despite the ongoing interest of some psychoanalysts, such as Harold Searles, in working with such patients (Searles, 1965).

This type of approach to behavior was different than that which had been found useful to assist soldiers in combat. It also signified a radically different attitude toward patients. Military psychodynamically oriented psychiatrists were interested in helping all who needed assistance. The post-war psychoanalysts, however, became primarily committed to helping patients who could be treated with a specific technique: classical psychoanalysis.

Within an astonishingly short time after the war, organized psychoanalysis became centralized, bureaucratic, resistant to change, intolerant of criticism either from within or without, and devoted to the training of elite professionals. This occurred despite vigorous opposition from many outspoken and energetic psychoanalysts. For example, the historian Nathan Hale, having interviewed Franz Alexander, commented:

By 1960 Alexander had come to believe that American psychoanalysis was overly conservative, slavishly upholding everything for which Freud stood in theory and practise, . . . Alexander feared the field was in danger of being frozen and that organizational fervor was taking the upper hand. The trend was towards standardization and uniformity. (Hale, 1995, p. 133)

Despite the outspoken efforts of psychoanalysts such as those, including the dissidents who formed the American Academy of Psychoanalysis in 1956, the American Psychoanalytic Association maintained dominance over psychoanalysis in the United States.

Historians have speculated that the extreme conservatism of psychoanalysis during the 1950s and 1960s may well have been a reaction to the intense struggles occurring throughout American society over such issues as communism, the Cold War, segregation, and the war in Vietnam. It is understandable that an organization whose roots were in prewar Europe with many leaders who had endured profound social chaos could have wanted to construct an oasis of stability—an island of reason and professional expertise in an unstable, unexpectedly threatening environment. Even psychoanalytic leaders who were not émigrés were aware of the hostile reactions to Freud's work experienced by early psychoanalysts. Perhaps they wished to protect psychoanalysis by cloaking it in a conservative uniform.

The institutional structures that psychoanalysis created in the United States and the intellectual isolation these engendered eventually led to diminished influence of psychoanalysis on psychiatry and academic medicine in general. A great deal of attention has been devoted to the defects of organized psychoanalysis in the 1950s, 1960s, and even later (Fisher and Greenberg, 1977; Grunbaum, 1984; Edelson, 1984). One that we feel re-

quires particular emphasis, even more than has already been expressed, was the virtual abandonment of the scientific method by the institutes of the American Psychoanalytic Association.

Although the psychoanalysts affiliated with the American were, by and large, physicians, their psychoanalytic institute training heavily emphasized the *clinical case method*. Psychoanalysis was the only medical specialty whose therapeutic endeavors were not rooted in science. The scientific method was ignored, and even frequently proclaimed irrelevant, by many psychoanalytic educators. Consider the differences between psychoanalysis and medicine and surgery for the purposes of discussion. Medicine and surgery are based on the sciences of biochemistry, physiology, anatomy, and pathology. In contrast, in the postwar years, psychoanalysis was based on the unsystematic insights of its only genius—Freud—and the *observations* of his followers. Freud's own career had begun in the sciences but inexorably moved toward the purely clinical and then the purely theoretical as he concentrated more and more on the manifestations of "mind" as opposed to the relationships between "mind and brain." This was reasonable enough, since during Freud's lifetime, techniques for studying the function of the nervous system were extremely limited.

When the scientific method is used, observation and inference are meticulously distinguished. Care is taken to be sure that different observers agree on fundamental observations. Inductive logic is applied to these to create general hypotheses. These are used to make predictions about *potential* outcomes. These potential observations are then actually made—either in experiments, in which conditions for making them are imposed from without—or naturalistically. In science there is a recognized need to compare events that occur as a result of specific experimental manipulations to those that might occur without such manipulations. For example, if a patient is treated by psychoanalysis and improves, how do we know that the improvement is due to the treatment rather than to an *aging* effect? Psychoanalysis takes years to complete—perhaps people "naturally" get better over time without treatment. Another possible reason for improvement could be related to life events. Consider a patient who improved following psychoanalysis but who inherited a fortune during the treatment. Was his improvement the result of psychoanalysis or an unexpected accumulation of wealth? Problems such as these are routine in the sciences and are the reason that the scientific method was invented during the Renaissance.

As a result of the scientific method, empirical knowledge has rapidly grown in chemistry, physics, and biology. Modern medicine and surgery are based on such knowledge. For example, the treatment of diabetes mellitus is informed by understanding the relationship between glucose

metabolism and insulin secretion. The treatment does not rest entirely on the cumulative insights of clinicians—no matter how penetrating—about the utility of insulin.

During the post-World War II years this was not the case with psychoanalysis. The theory of treatment was based entirely on ideas about psychology derived from the treatment of patients (or observations about sociocultural behavior) made primarily by Freud and to a much lesser extent by his followers.

If inferences are made on the basis of the clinical case method, they must be tested via application of the scientific method before general principles can be assumed to be true. The clinical case method is basically illustrative—not demonstrative. Thus, the clinical case method cannot in itself be the basis for general principles of either human motivation or treatment of psychological disorders. In rejecting the scientific method, the psychoanalytic institutes turned their back on the accepted route to accretion of knowledge since the Renaissance. This led to their becoming more and more isolated and peripheralized in the world of medicine.

Although the psychoanalytic institutes carried out little research (and most of what was done did not test the validity of its basic tenets), scientists in allied fields did. It was largely because of their efforts that psychoanalysis radically modified its ideas about the psychological development and functioning of women. For example, Freud's theory about vaginal-clitoral relationships in development and in sexual functioning was not abandoned because of progress within psychoanalysis but rather because of the psychophysiological observations of William Masters, a gynecologist, and his associate, Virginia Johnson (Masters and Johnson, 1966; Fisher and Greenberg, 1977). The sexologists demonstrated that there was no physiological difference between vaginal and clitoral orgasms and that the most common sensory source of erotic stimulation was the clitoris.

Freud's theories about women had been earlier challenged from within the psychoanalytic movement, most notably by Karen Horney (1939). Neither she nor her followers influenced the psychoanalytic mainstream to modify its view of women, however.

Modern psychoanalytic ideas about the psychological development of girls and women were also influenced by research on sexual differentiation and gender identity carried out by many nonpsychoanalytic investigators (Money and Ehrhardt, 1972; McEwen, 1983). These basic scientists made it possible for the psychoanalyst Robert Stoller (1968) and others to propose alternative models for the development of gender identity than those suggested by Freud.

These two examples illustrate that the most far-reaching paradigm changes that organized psychoanalysis made in the 1950s, 1960s, and

1970s came from work done by nonpsychoanalysts whose discoveries were based on the scientific method. This is not to suggest that all psychoanalysts were antiscientific. Important investigations were carried out by psychoanalytically trained investigators at many academic medical centers and universities. Psychoanalysts carried out systematic research in psychobiology and psychosomatic medicine, sleep and dreaming, child development, diverse psychiatric disorders, outcome of treatment, and other areas (Benedek and Rubenstein, 1942, op. cit.; Engel, 1962; Kernberg et al., 1972; Holzman, 1976; Wallerstein, 1986). However, much of this research did not take place at psychoanalytic institutes. When it did, it tended to be peripheralized and not given high status. Rare exceptions to this general trend did occur, however. Benedek and Rubenstein's study demonstrating cyclic changes in fantasy associated with the hormonal changes of the menstrual cycle, at the Chicago Institute, achieved immediate acclaim locally and nationally. Nonetheless, this research has not been widely cited in the more recent psychoanalytic literature.

The need for psychoanalytic institutes to more adequately integrate research into their culture was recognized by many scholars (Holzman, 1976; Holt, 1989). It never adequately occurred, however.

It is interesting to compare the impact of the Benedek and Rubenstein study on psychoanalysis to the fate of studies performed by investigators whose *culture* retained the scientific method. We mention two biological studies in the area of male homosexuality to illustrate the importance of replication in science.

In the late 1970s, Robert Kolodny, a psychoendocrinologist working with Masters and Johnson, reported that serum testosterone was inversely related to the degree of homosexuality in men. Serum testosterone was highest in heterosexuals, lowest in exclusive homosexuals, and of intermediate value in bisexuals. More recently, Simon LeVay observed that the microstructure of parts of the brain of male homosexuals was different from that of heterosexuals, and similar, in certain ways, to that of women (LeVay, 1991). These findings immediately generated intense and widespread public interest. What is their status today?

Many investigators tried and failed to replicate Kolodny's observations (Friedman, 1988). The theoretical speculation stimulated by Kolodny's research was therefore discontinued. Attempts to replicate Simon LeVay's study are presently underway. The apparent disinterest by modern psychoanalysts in Benedek and Rubenstein's research, in contrast to that of nonpsychoanalysts in Kolodny's research, has been due to its lack of *popularity*, not its lack of *validity*. In fact, no psychoanalytic institute attempted a replication study.

Interestingly, the single attempt to carry out a scientific study of homo-

sexuality during the postwar years was that of the Committee of Medical Psychoanalysts (Bieber et al., 1962). This group was not affiliated with the American Psychoanalytic Association but rather a newer psychoanalytic group: the American Academy of Psychoanalysis. Irving Bieber, the principal investigator of the study, had resigned from the American Psychoanalytic Association because he believed that their institutes discouraged free inquiry (Bieber, 1989). Long after the results of the investigation he directed were published, Bieber became controversial because he opposed the decision by the American Psychiatric Association to remove homosexuality from the DSM. It is important to distinguish between Bieber, the investigator of the 1950s and 1960s, and Bieber, the political advocate of the 1970s. We have critically reviewed the Bieber study elsewhere (Friedman, 1988). Although the investigation by the Committee of Medical Psychoanalysts has often been cited to support the pathological paradigm of homosexuality, this was, in fact, not studied by the investigators. They assumed homosexuality was pathological and directed their attention to familial and developmental variables.

ATTITUDES OF PSYCHOANALYTIC INSTITUTES

In the psychoanalytic institutes, candidates were expected to conform to the attitudes, values, and expectations of their teachers. It was generally believed that full-time devotion to psychoanalytic clinical work left no time to carry out research. Most supervisors and training analysts themselves were not researchers and many did not think it was possible to investigate the basic tenets of psychoanalysis by application of scientific methods. They became the role models for candidates at the training institutes—the psychoanalysts of the future.

Many prominent academicians and researchers were psychoanalysts. Despite their efforts, however, they were not usually presented by *the institutes* as appropriate figures for idealization and identification. They tended to be viewed as having *applied* psychoanalytic psychology in order to advance extrapsychanalytic aims. The professional identities of most psychoanalytic candidates incorporated mostly clinical attitudes and values. By this we mean that they valued clinical “process” but, by and large, not objective assessment of outcome, critical analysis of basic assumptions, or experimental testing of hypotheses. They were therefore not protected by their procedural methods from systematic error. Such error occurred in their generally accepted psychoanalytic theories of homosexuality. Thus, theories about the psychodynamics and psychopathology of homosexuality were accepted because they were approved of by psychoanalytic authorities, not because they were based on validated research studies.

In the field of psychiatry, advances in psychopharmacology during the 1950s and 1960s provide an interesting contrast to those in psychoanalytic psychology.

PSYCHOPHARMACOLGY AND PSYCHOANALYSIS IN THE 1950s

In Freud's last work, *The Outline of Psycho-Analysis*, he commented:

We are here concerned with therapy only in so far as it works by psychological methods; and for the time being we have no other. The future may teach us how to exercise a direct influence by means of particular chemical substances, upon the amounts of energy and their distribution in the apparatus of the mind. It may be that there are other undreamed of possibilities of therapy. But for the moment we have nothing better at our disposal than the technique of psychoanalysis and for that reason, in spite of its limitations, it is not to be despised. (Freud, 1940, p. 79)

Although accurate in its day, the psychopharmacological revolution has rendered this observation of Freud's no longer timely. The "talking cure(s)" have certainly not been abandoned, but discovery of powerful and effective drugs for the treatments of psychiatric disorders has vastly expanded modern therapeutics.

During the very same years as organized psychoanalysis in America was becoming codified, important observations were being made by pharmacologists about the effects of drugs on psychiatric disorders. The first report of the treatment of mania with lithium was published in 1949 (Cade, 1949). Chlorpromazine was synthesized in 1950 and first used to treat psychiatric patients in 1952 (Laborit et al., 1952). The antidepressant effects of imipramine and iproniazid were both recognized in 1958 (Kuhn, 1958; Kline, 1958).

Scientific advances led to the stepwise increase in psychopharmacological knowledge until small knowledge cascades were followed by larger ones. Today the leaders of psychiatry are psychopharmacologists and bio-descriptive psychiatrists. Medical psychoanalysts, at least in the United States, have declined in status and influence.

Young psychiatrists today—even those who are psychoanalytically oriented—often have difficulty believing that the response by many psychoanalysts to the then novel psychopharmacological treatments was unenthusiastic, at best. In fact, psychoanalysts were frequently concerned that the new drugs would contaminate the psychoanalytic process and interfere with the orderly unfolding of transference phenomena. Myrna Weissman, a researcher specializing in the investigation of mood disorders, put it this way:

The literature on drugs and psychotherapy of the early to mid 60s was not concerned with the efficacy of psychotherapy compared to drugs, but mainly questioned how and when, if at all, drugs should be introduced into the psychotherapeutic treatment of depressed patients. The questions were a reflection of ideological dominance of psychoanalysis in the USA and Canada at that time. . . . With few exceptions little attention was paid in their clinical writings to diagnosis, type or quantity of drugs or psychotherapy, length of treatment, or the differentiation of possible outcomes under the different treatments. (Weissman, 1978, pp. 1313–1321)

This situation has changed today, of course. Psychoanalysts everywhere recognize the importance of research in the areas discussed by Weissman, and on the psychoanalytic process itself.

We have stressed the general intellectual climate of American psychoanalysis in the 1950s and 1960s because it was during those years that the influential articles on the pathological model of homosexuality were published. Although we have taken a critical stance, we do not seek to convey the impression that psychoanalytic education was entirely arid. In fact, inspiring teachers discussed Freud's work and the subsequent psychoanalytic literature in institute classrooms. Many patients were helped by psychoanalysis. The insights of their analysts proved helpful and the cumulative wisdom of the field was usefully applied to ameliorate their suffering. Others, however, were misunderstood because of biases of which the field of psychoanalysis as a whole seemed unaware. The reason that we have stressed the negative dimension of the psychoanalytic institutes to such a degree is that the rigidity with which the pathological model of homosexuality was believed and the tenacity with which it was defended when under attack in the 1970s were anchored in an overall "zeitgeist" as it were; that of the authoritarian, clinically focused psychoanalytic institute. Let us now return to consider the area of homosexuality itself.

SANDOR RADO'S THEORY OF SEXUAL ORIENTATION

Of the distinguished émigré psychoanalysts, the most important with regard to psychoanalytic ideas about homosexuality during the postwar years was Sandor Rado. Rado was brought from Berlin to become the first director of the New York Psychoanalytic Institute in 1931. Following psychoanalysis by Karl Abraham, Rado had become director of training of the Berlin Psychoanalytic Institute. Initially enamored of Freud, by the time Rado came to New York he had become critical of the sycophancy, intolerance of criticism, and grandiosity that flawed the psychoanalytic movement. He was committed to building bridges between science and

academic medicine, and in the mid-1940s he left the New York Psychoanalytic Institute to become first director of the Columbia Psychoanalytic Institute, a university-affiliated institute in contrast to the New York Psychoanalytic Institute (Hale, 1995, p. 250).

Rado believed that psychoanalysis was a science of human behavior. Its cumulative insights about the mind were, to him, analogous to the observations of neuroscientists about the structure and function of the brain (Redo, 1949).

Rado was a formidable, charismatic teacher whose students included Irving Bieber, Lionel Ovesey, and Charles Socarides, all of whom disseminated and elaborated on his ideas about homosexuality. He was probably the most influential psychoanalyst to conclude that homosexuality is inherently pathological. Rejecting Freud's theory of constitutional bisexuality, Rado concluded that all people were primarily heterosexual. Those who were erotically attracted to others of the same sex were unconsciously attracted to those of the opposite sex. Their primary heteroerotic wishes were irrationally experienced as dangerous. Unconscious fears led to repression of heterosexual wishes and to the replacement in the conscious mind of the heterosexual erotic object by one of the same sex (Rado, 1940).

THE PSYCHOANALYTIC TREATMENT OF HOMOSEXUAL PATIENTS PRIOR TO THE APA REFERENDUM IN 1973

Sandor Rado's theory that homosexuality was due to an unconscious irrational fear of heterosexuality was accepted as valid by most American psychoanalysts (Bayer, 1981). It fostered the widespread conviction that the sex of the consciously perceived object of desire could usually be altered with psychoanalysis. Pathological defenses could be undone—the argument went—resulting in the emergence into awareness of previously repressed heterosexual desires (Rado, 1940; Ovesey et al., 1963; Ovesey and Gaylin, 1965; Ovesey, 1969). This was proposed despite the fact that Freud himself, in his letter to the mother of a homosexual, pointed out that this could not be accomplished (Freud, 1935).

During this period (the 1940s to 1970s), the treatment goal for most homosexual patients was conversion to heterosexuality. There were no adequately designed studies indicating how frequently this occurred, but it seems to have happened rarely. For example, in the investigation of homosexual men carried out by the Committee of Medical Psychoanalysts, data concerning outcome were collected from the patients' psychoanalysts—not independent evaluators. These data were restricted to sexual orientation change that occurred during the analysis, with the result that no fol-

low-up information was obtained. Of patients who were predominantly or exclusively homosexual, 20% were reported to change from homo- to heterosexual, while 80% did not (Bieber et al., 1962).

Complications of treatment undertaken to convert homosexual individuals to heterosexual were more the rule than the exception during this phase of the history of psychoanalysis. Such complications included loss of self-esteem resulting from the many instances of—what was then considered—treatment failure as well as the wasting of financial resources on treatment deemed unsuccessful because the replacement of homosexual by heterosexual fantasies was never accomplished. A sense of betrayal and mistrust was experienced by many in the homosexual community toward organized psychoanalysis (Duberman, 1991; Isay, 1996; Goldman, 1995).

A vivid description of psychoanalytic treatment during the postwar years has been provided by Martin Duberman (1991), a historian, and presently director of the Center for Lesbian and Gay Studies of the City University, New York. The title of Duberman's book, *Cures*, belied his experiences as a patient. Here he describes his experience with Dr. Albert Igen (a pseudonym), professor of psychiatry at the Yale Medical School:

I liked him immediately. Low-keyed and compassionate, he refrained from pompous pronouncements and optimistic predictions alike, telling me that for the sake of "sounder adjustment" we should indeed work to change my sexual orientation, but that there was no guarantee of success. "It will largely depend" he solemnly told me "'on your wish to change.' The ultimate rewards will be great, but since the process of change is fraught with frustration and difficulty, you must greatly want that change in order for it to happen." (Duberman, 1991, p. 44)

The analysis proceeded at three sessions per week. Dr. Igen interpreted his patient's homosexual activities as "acting out" a need for immediate gratification. When Duberman complained of feeling that his professional life was unfulfilling, Dr. Igen commented:

The cure for feelings of professional sterility was not a half-baked leap into emotional turmoil. My current concentration should be on therapy; until I resolved my conflicts, I could not hope to improve either my professional or my personal life. (Duberman, 1991, p. 50)

Dr. Igen subtly encouraged his patient to give up homosexual relationships and take up heterosexual ones in their place.

After nearly a year and a half of therapy he said it was time for me to take hold. The failure of the affair with Billy should have conclusively demonstrated

to me, Igen said, the futility of investing my hopes for sustained intimacy in a homosexual relationship. It was time to look elsewhere, time to close the "escape hatch," confront my underlying anxiety, move toward a heterosexual adjustment. (Duberman, 1991, p. 52)

The patient obediently courted a young woman for whom he felt no sexual desire.

Dr. Igen kept the focus steady. He encouraged me to speculate about the source of my "unconscious resistance" to physical love with a woman. Though rarely theoretical he made reference to a possible "breast complex" and asked me if I had reacted violently to being weaned. When I laughed and said I couldn't remember as far back as yesterday's movie, let alone my experiences in the crib, he replied, with a pained expression that that too was part of the resistance! . . . "Is it any wonder you have had difficulty ever since in entrusting yourself to a female? You're chronically angry, angry at women and refuse to get it up for them. To enter a vagina is for you to risk being swallowed alive." (Duberman, 1991, pp. 58–59)

About 1½ years later Duberman broke off the treatment since his work took him to another city. His analyst assured him he seemed "on the verge of a cure." His sexual orientation, however, had not changed.

Later in his life, Duberman, having moved to another city and again desirous of becoming heterosexual, tried psychodynamically oriented individual and group therapy, with "Karl," who commented to his patient:

"What would cause you to defend your mother so vigorously as 'a good woman' other than guilt?—which in turn is a mask for incestuous wishes you never worked through as a child. You and your mother are not separate people and that is why your gender identity remains faulty. Until you pass through a separation phase, you will never be your own person—and none of your relationships can become adult ones, which is to say post-familial." (Duberman, 1991, p. 104)

Karl remarked about Duberman's persistent homosexual orientation:

"There's a deep refusal to change yourself, a nihilistic insistence that you can't change. . . . Way down beneath the rhetoric of struggle, you think change is one of two things—easy or impossible. What this all adds up to is that you don't want to do the hard work: you don't want to control your artificial appetites in the name of allowing a deeper self to emerge." (Duberman, 1991, p. 190)

The goal of sexual orientation change as a result of psychoanalysis was generally unattainable. The conviction that this endpoint was even possible

to reach was unsupported by research. How do we understand this? Was the grandiosity of prediction limited to homosexuality, or were psychoanalysts *generally* promising behavioral change beyond anything that could reasonably be accepted?

There was a general belief in the field that psychoanalysis could lead to total personality reorganization and repair damaged mental structures, thus minimizing the likelihood of future psychopathology (Knight, 1941; Rado, 1956). Karl Menninger put it this way:

Psychoanalysis remains . . . the supreme paradigm of psychotherapy . . . the most thoroughgoing, the most penetrative, the most intensive and persistent. (Menninger, 1963)

Numerous instances of prolonged treatment, assurances of progress, and failure to improve occurred in the psychoanalyses of patients who were *not* homosexual. In a well-known book about his own disappointing psychoanalysis, Dan Wakefield reported that after five years he still suffered from agonizing symptoms. According to Wakefield, he was assured that psychoanalysis was a medically effective treatment, as certain to work as setting a broken leg (Hale, 1995). Wakefield terminated analysis in dismay, ultimately achieving help through other means (Wakefield, 1988).

Thorough discussion of the reasons for the trend toward excessive therapeutic optimism is beyond the scope of this book. Complications and inadequate results are only part of the story, however.

There is no question that for many patients, psychoanalysis was invaluable. Many found the way to meaningful lives because of it, and some credit psychoanalysis with saving them from suicide.

Physicians are constantly placed in the situation of having the responsibility to care for suffering patients despite the fact that scientific knowledge of illness and therapeutics is sparse or even absent. Surgery prior to the discovery of anesthesia and the treatment of bacterial infections prior to the discovery of antibiotics illustrate this. Patients *needed* surgery long before modern surgical techniques existed. Patients with pneumonia were admitted to hospitals before the introduction of penicillin.

It is understandable that well-intentioned practitioners, given what appeared to be a powerful new technique, tried to apply it whenever possible to diminish suffering. In the instance of psychoanalysis, results often were positive. Most experienced psychoanalysts had reason to believe that their methods often helped people.

We suspect that this occurred because most patients did not manifest a type of behavior whose origins were misunderstood and whose treatment plan was invalid because of systematic errors built into the basic structure

of psychoanalytic organization. Thus, many patients who suffered from depression were no doubt greatly helped by psychoanalysis. This is because (later progress in psychopharmacology notwithstanding) the generally accepted psychoanalytic model of the mind *informed* their treatment. This was not the case with regard to change from homosexual to heterosexual orientation.

CONCLUSION

During the entire 20th century psychoanalytic ideas have been responsive to local geohistorical influences to a greater degree than generally acknowledged. Although Lewes (1988) has discussed these influences on models of homosexuality to some degree, we have elaborated upon his findings and extended observations here, and have also reviewed issues not previously discussed by Lewes or other intellectual historians. In considering the clinical consequences of the pathological model of homosexuality, it should be noted that these were not restricted to patients but were experienced by homosexual analysts as well.

During the postwar years, otherwise well-qualified applicants to psychoanalytic training institutes were routinely rejected solely because of their homosexual orientation. Ralph Roughton, a prominent contemporary psychoanalytic educator, recently observed:

Applicants were told they were unanalyzable, or to "reapply when you're cured." Several gay psychiatrists have told me their stories of being advised not to apply for analytic training or of being rejected when they did. One respected leader in psychiatry, now retired, was rejected by two institutes when he acknowledged his homosexual orientation but was accepted by a third when he did not reveal it to interviewers. However, after telling his training analyst in the first session, the analyst reported it and he was immediately dropped from the institute. (*The American Psychoanalyst*, Vol. 29, No. 4, pp. 15–16, 1995)

Some homosexual psychiatrists, determined to become psychoanalysts, completed psychoanalytic training, despite its hazards. Usually they assumed a heterosexual lifestyle during this phase of their lives and often married (Isay, 1996). By systematically rejecting for psychoanalytic training candidates who were gay or lesbian, the psychoanalytic institutes eliminated a potential source of clinical data that could cast doubt on the prevailing theory of homosexuality.

Contemporary psychoanalysis is attempting to base its theories on empirically validated databases whenever possible. This process is associated with anxiety as old ideas are abandoned and new ones often raise more questions than they answer.

The history of psychoanalytic ideas about homosexuality illustrates many strengths and limitations of the adaptive capacity of organized psychoanalysis in the United States. Hopefully, critical discussion of its intellectual history will strengthen the recent tendency of psychoanalysis to foster the type of collaboration between clinicians and researchers that is necessary for its future growth.

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