
The Return of the Repressed

Psychology's Problematic Relations With Psychoanalysis, 1909–1960

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When psychoanalysis first arrived in the United States, most psychologists ignored it. By the 1920s, however, psychoanalysis had so captured the public imagination that it threatened to eclipse experimental psychology entirely. This article analyzes the complex nature of this threat and the myriad ways that psychologists responded to it. Because psychoanalysis entailed precisely the sort of radical subjectivity that psychologists had renounced as unscientific, core assumptions about the meaning of science were at stake. Psychologists' initial response was to retreat into positivism, thereby further limiting psychology's relevance and scope. By the 1950s, a new strategy had emerged: Psychoanalytic concepts would be put to experimental test, and those that qualified as "scientific" would be retained. This reinstated psychologists as arbiters of the mental world and restored "objective" criteria as the basis for making claims. A later tactic—co-opting psychoanalytic concepts into mainstream psychology—had the ironic effect of helping make psychology a more flexible and broad-based discipline.

Freud and Jung were having dinner in Bremen. It was the evening before they set sail for the Clark conference, the occasion of Freud's only visit to America. Jung started talking about certain mummies in the lead cellars of the city. Freud became visibly disturbed. "Why are you so concerned with these corpses?" he asked several times. Jung went on talking. Suddenly, without warning, Freud fell to the floor in a faint. When he recovered, he accused Jung of harboring death wishes against him. But it was not Jung who wanted Freud dead. Had Freud only known what American psychologists were about to do to psychoanalysis, he might never have gotten up off the floor.

There is no easy way to talk about psychology's relations with psychoanalysis.¹ It is a story dense with disillusionment and the shapeless anger of rejection. Each side behaved badly, and then compounded its insensitivity with disdain. Their fates bound together like Romulus and Remus, psychology and psychoanalysis struggled to find their separate spheres, only to end up pitted against one another at every turn. Too much was at stake—property lines, areas of influence, and a deeper question: Which field would ultimately dictate the ground rules for a science of the mind?

In the 1890s, when this struggle began, there was little sign that it would become another Hundred Years' War. Psychologists had just begun to apply experimental methods to some of the classic problems of metaphysics,

with the hope of answering questions that had bedeviled philosophers for centuries. By systematically organizing the psychological world into a set of discrete variables, these methods brought the unruly phenomena of mind within the purview of science. It was a heady time, a time of possibility and change and the reckless felicity of the new. American psychologists raced around founding laboratories at every college that would let them, in closets, basements or wherever they could snatch a little space, setting up apparatus in their own homes if necessary. They invented new forms of measurement, odd devices, tests of all sorts. Reports of their findings poured into the journals that sprang up suddenly to fill the need. The *new psychology*, as they liked to call it, seemed destined even in its infancy to do what had been declared since Kant to be impossible—to create a truly scientific approach to mind.

Psychoanalysts thrust themselves directly into the middle of this scene, brazenly trying to supplant the new psychology at the moment of its greatest promise. At first psychologists stood aside, astonished, as the analysts, bursting with self-importance and an almost frightening zealotry, pronounced themselves the real scientists of the mind. By the time psychologists began to take this threat seriously, psychoanalysis had so captured the public imagination that even its pretensions could not be ignored.²

The question was how to define science. To the analysts, science had nothing to do with method, with con-

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¹ The standard reference on this whole topic is Shakow and Rapaport (1964). Their study remains invaluable as a thoughtful, systematic review of much of what psychologists have had to say about psychoanalysis. However, because their goal was to document Freud's influence on American psychology, they focused more on positive effects than on negative ones. My goal is to characterize psychologists' attitudes toward psychoanalysis. Many psychologists saw psychoanalysis as a threat and not as a positive influence, and thus my version of the story is inevitably more conflicted than Shakow and Rapaport's.

² A discussion of the popular reception of psychoanalysis in America is beyond the scope of this article. See Hale (1971, 1978) and Burnham (1968, 1978, 1979, 1987) for detailed treatments of this issue.

trolling variables or counting things. What made something scientific was that it was true. Constructing a science of the mind could mean only one thing—finding some way to peer through the watery murk of consciousness to the subaquean reality that lay beyond. The efforts of psychologists, with their bulky equipment and piles of charts and graphs, seemed superficial and largely irrelevant to this goal.³

For their part, psychologists initially saw psychoanalysis as just another of the “mind cures” that flashed across the American landscape in the 1890s—like Christian Science or the Emmanuel movement—a popular craze that had nothing to do with the scientific study of mind. Most psychologists who attended Freud’s Clark lectures in 1909 saw his speculations about dreams and sex as a pleasant diversion, about as relevant to their work as Mrs. Eddy’s epistles. The occasional articles about psychoanalysis that appeared in psychology journals before 1910 (e.g., Putnam, 1906; Scott, 1908) made it seem mildly interesting, but not essentially different from related methods like suggestion.

By 1915, readers of a publication like *The Journal of Abnormal Psychology* had an opportunity for more varied exposure to psychoanalytic ideas.⁴ Books by Freud, Jung, and A. A. Brill were regularly reviewed. Articles demonstrating the therapeutic effectiveness of psychoanalytic techniques began to appear, along with some discussion of the theory itself (see, e.g., Coriat, 1910; Emerson, 1912–1913; Gordon, 1917; MacCurdy, 1913; Maeder, 1910; Putnam, 1909–1910). Criticisms, when made, were fair-minded and well within the spirit of scientific repartee. Donley (1911), for example, suggested that anxiety neurosis might have other causes beyond those considered by Freud. Bellamy (1915a) argued that dreams fulfill fears or states of anger just as often as they represent wishes. Taylor (1911) noted that there were cases of neurosis in which patients recovered without having had their childhood or sexual life dissected. Even critics with a broader focus expressed little ire. Wells (1913) was concerned about “looseness in the formulation of psychoanalytic theories” (p. 227). Solomon (1916) argued that the term *sexual* was used inconsistently by analytic writers. The psychiatrist Morton Prince (1910) expressed the common view that psychoanalysts “fit the facts to the universal concepts which dominate the school” (p. 349).

There were occasional writers who became exasperated and called psychoanalysis “weird” (Donley, 1911), “esoteric” (Carrington, 1914), or “grotesque” (Bellamy, 1915a), its assumptions “fantastic” or “sheer nonsense” (Humphrey, 1920b), but these imprecations were unusual in the early years. The sexual nature of psychoanalytic interpretation was a problem for some; Bellamy (1915b), for example, in reviewing a book by Coriat, made plain his relief that “there is not a word or sentence in this book that a precise maiden lady need hesitate to read to her Sunday school class or at a pink tea” (p. 434). On the whole, however, psychologists were initially so supportive of psychoanalysis that when Roback reviewed Dunlap’s (1920) *Mysticism, Freudianism and Scientific*

Psychology, he felt he had to defend its critical tone on grounds of balance: “Freud has had so many warm advocates of his views in this country and so few systematic critics among the psychologists that Dunlap’s discussion is both timely and important” (Roback, 1921, p. 406).

These positive attitudes might well have resulted from more than psychologists’ open-mindedness. Analysts, ever worried about their public image, left little to chance. Soon after the Clark conference they embarked on a systematic campaign to win Americans to their cause. A. A. Brill, the founder of the New York Psychoanalytic Society, was charged with disseminating information about psychoanalysis in that city; Ernest Jones, Freud’s scrappy lieutenant, took the rest of the country for himself (Burnham, 1967, pp. 134–137). Psychologists were among the major recipients of Jones’s educational largess; by 1916, they had been treated to 20 of his articles, abstracts, reviews, and comments in the *Journal of Abnormal Psychology* alone. Most of these pieces were patient expositions of psychoanalytic concepts, designed to lead the uninitiated to a correct understanding of the theory. But Jones also maintained a vigilant watch over what psychologists were writing about psychoanalysis, and shot back a tart riposte whenever he encountered an “erroneous” statement (see also Tannenbaum, 1916, 1917).

Neither Jones nor his colleagues gave serious attention to the careful criticisms that psychologists leveled against psychoanalysis in the early years. Acutely aware of the tenuous status of their own new field, psychologists found this highly disconcerting. After all, they were constantly obliged to defend their science against attacks from philosophy and biology; what gave analysts the right not only to ignore legitimate criticism but to patronize their opponents? Who knows what might have happened had analysts been more responsive; what did happen was that psychologists sharpened their pencils and began to fight.

The first skirmish actually occurred as early as 1916, when the Princeton philosopher Warner Fite reviewed Jung’s *Psychology of the Unconscious* for *The Nation* (Fite, 1916). His surprisingly nasty tone incited a riot of response from psychologists. In her letter to the editor, Christine Ladd-Franklin, the eminent experimentalist, characterized psychoanalysis as a product of the “undeveloped . . . German mind” (hardly a compliment in 1916), and concluded ominously that “unless means can speedily be found to prevent its spread . . . the prognosis

³ Psychologists were not alone in having to struggle with competing definitions of science. Kuklick’s (1980) analysis of boundary maintenance in sociology offers a general model for understanding how each of the social sciences resolved this dilemma.

⁴ Of all major psychology journals of the period, the *Journal of Abnormal Psychology* was the one with the greatest number of articles relevant to psychoanalysis (both pro and con). Not all were written by psychologists, but they were clearly intended for this audience. G. Stanley Hall published the text of Freud’s, Jung’s, and Ferenczi’s Clark lectures in his *American Journal of Psychology* in 1910, but from then on that journal concentrated primarily on reviews of the psychoanalytic literature (both German and English) and carried very few original articles by psychologists.

for civilization is unfavorable" (Ladd-Franklin, 1916, p. 374). R. S. Woodworth of Columbia (1916), a bit more circumspect, called psychoanalysis an "uncanny religion" (probably not the psychologist's highest accolade) that led "even apparently sane individuals" to absurd associations and nonsensical conclusions. In a telling illustration, he showed how the words *Freudian principles* led to a train of thought that revealed his own "deep-seated wish . . . for a career of unbridled lust" (p. 396).

Woodworth went on to publish an extensive critique of "Freudism" in the 1917 volume of the *Journal of Abnormal Psychology*. Adopting the peevish tone that soon became commonplace in these sorts of articles, he complained that analysts disregarded psychological research, contemptuously dismissed it as superficial, and treated psychologists "shabbily" (Woodworth, 1917, p. 175). What most annoyed Woodworth was the analysts' slippery dodge, their way of attributing any criticism of psychoanalysis to unconscious resistance on the part of the critic.

Other writers echoed these complaints, often with less poignancy and considerably more pique than Woodworth. But what soon emerged as the real irritant for psychologists was the analysts' insistence, at times moralistic, at times snide, that only those who had themselves undergone a personal psychoanalysis were qualified to evaluate the theory. To an experimental psychology whose *raison d'être* was to differentiate itself from religion, this talk of initiation rites and secret knowledge was anathema. Such a rule also conveniently disenfranchised just about every psychologist from serving as a potential critic; even those Americans who sought analysis had a hard time finding it in this country before 1920. Of course the real issue here was not who had been analyzed and who had not (a good thing, since Freud and his closest colleagues would have had to disqualify themselves); what was at stake was the fundamental question of subjectivity in science.

For experimental psychologists, being scientific meant creating distance. It meant opening up a space, a "no man's land," between themselves and the things they studied, a place whose boundary could be patrolled so that needs or desires or feelings could never infiltrate the work itself. Every aspect of the experimental situation was bent toward this goal—the "blind subjects," the mechanized recording devices, the quantified measures, and statistically represented results (Danziger, 1990; Hornstein, 1988; Morawski, 1988). What united experimental psychologists more than anything else was a distrust of personal experience, a sense that feelings in particular were dangerous and had to be held carefully in check lest they flood in and destroy the very foundations of the work. They were willing to make a number of sacrifices to protect psychology from this threat, including a radical narrowing of the field to include only phenomena that could be studied "objectively."

Having gone to these lengths, psychologists found it profoundly disquieting to have analysts claim that being psychoanalyzed was what made someone a credible sci-

entist. This implied that science was subjective, that it was ultimately about personal experience rather than rigorous method. Even worse, it suggested that the unconscious was so powerful a part of mind that its force had to be experienced directly, in one's own life, in order to understand the psychology of others.

Such a view could not go unchallenged. "Voodooism," Watson (1927, p. 502) called it. "A delusion," echoed Jastrow (1932, p. 285). The very idea of an unconscious conjured up the chaos and irrationality that psychologists had banded together to escape. If analysts wanted to plunge into that nightmare world and call it science, so be it, but they could not be allowed to drag everyone else down with them.

The technique of free association came in for particular scorn (Heidbreder, 1933). It struck psychologists as an elaborate subterfuge, a way for analysts to appear not to influence patients when of course they did. Interpretation, they argued, was nothing but a new name for suggestion; that patients were gullible enough to mistake it for truth was hardly proof of its scientific status. Analysts were "free," all right—free to define as evidence whatever would meet their needs, free to label any challenge "resistance," free to pretend that they were doing nothing of the sort.

Heidbreder (1933), in her typically fair-minded way, struggled to make these practices sound reasonable. But even she could muster only this faint defense: Just because "psychoanalysts offer a different kind of evidence from that accepted by science . . . does not mean that they offer *no* evidence" (p. 402). To most psychologists, calling an analyst's retrospective musings about events that occurred in the secrecy of the consulting room evidence was an insult to science. Even first-year students knew that the cardinal rule of scientific proof was publicly verifiable data. Knight Dunlap (1920, p. 8) put it bluntly: "psychoanalysis attempts to creep in wearing the uniform of science, and to strangle it from the inside."⁵

By the mid-1920s, psychologists seem to have decided that the best way to defend science was simply to do it. Critiques of psychoanalysis began to be displaced in the literature by enthusiastic works like *Great Experiments in Psychology* (Garrett, 1930). Any remaining aggressive tendencies were easily absorbed by the interminable debates over behaviorism and Gestalt psychology.⁶ Psychologists did not need psychoanalysis, and it surely did not need them.

Or so it seemed, until one day in the fall of 1934 when the rumor got out that Edwin Garrigues Boring,

⁵ With characteristic irony, Dunlap (1920) concluded that psychoanalysis might ultimately prove beneficial to psychology: "Just as Christian Science has tremendously accelerated the progress of Scientific Medicine, so Psychoanalysis, by compelling psychology to put its house in order, will eventually help in the development of the Scientific Psychology it aims to thrust aside" (p. 9).

⁶ See, for example, a classic work like *Psychologies of 1925* (Murchison, 1926), which allots four chapters to behaviorism, three to Gestalt, and even three to the dying gasps of structuralism, but none to psychoanalysis.

the self-acknowledged dean of experimental psychology, had entered analytic treatment. To preserve his reputation, he told colleagues that he was studying the relation between the two fields; actually, he was depressed, frightened, and unable to work. The strange saga of Boring's analysis gives a glimpse into psychologists' continuing ambivalence about psychoanalysis.

Boring chose as his analyst the emigré Berliner, Hanns Sachs, who had been a member of Freud's inner circle and was therefore above reproach. Despite his depression, Boring embarked on the analysis with customary gusto, quickly absorbing the daily analytic sessions into the swirl of his 80-hour work week.

Boring struggled to make the analysis a success. He missed no sessions. He wept. He threw things. He made enough of a financial sacrifice to demonstrate the seriousness of his commitment. He discussed his childhood, explored his dreams, and scrutinized the motivations for his actions. Then, at the end of 10 months, he ran out of money, time, and desire. He had completed 168 sessions, for which he had paid \$1,680, more than a fifth of his yearly salary. But his efforts brought little relief:

[A]ll that happened was that the analysis petered out in an uneventful session on June 21st and my analyst went abroad! . . . I was distraught. I had tried a last resource, and it had failed. Yet, unwilling to accept so bitter a conclusion, I found myself seizing on the analyst's casual statement that I ought to wait a month. I waited anxiously, hoping for a new personality by July 21st. None came. Finally I sought out my psychologist-friends who believe in psychoanalysis, and we sat in conference discussing this sad immutability of my personality—on August 21st, as I suddenly realized. Their advice was patience, the less haste the more speed; wait at least until December 21st, they urged. So I waited. . . . And finally I ceased to expect a miracle. (Boring, 1940, pp. 9–10)⁷

How could a man like Boring, whose name was practically synonymous with hard-nosed experimentation, have such childlike faith in psychoanalysis? He actually seemed to expect that he would wake up a new man, that “a light from heaven” would change him “from Saul to Paul” (p. 9). There are certainly no hints of these hopes in his published writings. In the first edition of his classic *History of Experimental Psychology* (Boring, 1929), published just five years before the analysis, there were only four brief mentions of Freud in almost 700 pages. Psychoanalysis did not even appear in the index of *Psychology: A Factual Textbook*, the text Boring published with Langfeld and Weld in 1935, the same year he saw Sachs.

Yet in his own life, Boring kept searching for some sign that the analysis might have worked. Five years passed. Still no light. In 1940, he tried a new strategy. He proposed to the *Journal of Abnormal and Social Psychology* that it locate other well-known psychologists who had been analyzed, solicit reports of their experiences, and publish them in a special issue. Perhaps they would reveal something that he had missed. Leaving nothing to chance, Boring even persuaded Sachs to write a compan-

ion piece to his own account, evaluating the analysis from the analyst's perspective.

Psychologists turned out to be surprisingly excited by the prospect of reading about their colleagues' adventures on the couch. The American Psychological Association even reprinted the articles and sold them as a set, exhausting the entire edition within a few months. Boring, ever hopeful, titled his piece “Was This Analysis a Success?” Sachs (1940) replied with a tactful “no.” Wistful and perplexed by the whole experience, Boring struggled to come to terms with his sense of loss: “There is so much about this personality of mine that would be better if different, so much that analysis might have done and did not!” (Boring, 1940, p. 10). Yet he refrained from attacking psychoanalysis directly. His colleagues, however, knew where to lay the blame for their own failed attempts. Carney Landis of Columbia parodied his experience with a statistical analysis of how much time he had allocated to each of eight topics during free association. To Landis, analysts were scientific illiterates who did little but mouth received dogma in order to make themselves rich. Hinting that his “neurosis” was created by the analysis itself, Landis (1940) concluded his tirade by warning that psychoanalysis was safe only when used by experimental psychologists to produce psychopathic phenomena in the laboratory.

The editor of the *Journal of Abnormal and Social Psychology*, apparently concerned about the lack of balance in these articles, invited the eminent analyst Franz Alexander to contribute a rejoinder. Instead of critiquing the other papers, Alexander (1940) made a parable of his own life. Like his readers, he had spent his youth as a devotee of laboratory science. When he first tried to read Freud's work, he found its “vague and ambiguous mental excursions . . . equal almost to physical pain” (p. 312). He turned to psychoanalysis only when the evidence in support of it became undeniable. This meant sacrificing his promising academic career, enduring the opprobrium of his colleagues, and being forced from home by his irate philosopher father, who considered psychoanalysis a “spiritual gutter.” But for Alexander, there was no choice—having committed himself to empiricism, he had to adopt whatever view had the most evidence, regardless of how distasteful it might be on other grounds. Of course, in the end, his quest for truth was vindicated when his father, near death, gave up his own lifelong belief in the superiority of natural science to express the fervent wish that “psychoanalysis will enthrone again real understanding in place of fumbling—the rule of thought in place of that of the gadget” (p. 314).

⁷ Among those Boring consulted was his colleague Henry Murray, who advised him to let Sachs have it “right between his eyes. . . . give him the works—don't omit a single grievance, not one.” (H. Murray to E. G. Boring [n. d., August 1935?], Box 43, Folder 919, E. G. Boring Papers, Harvard University Archives quoted by permission.) There is no evidence that Boring took this advice: He and Sachs maintained a cordial relationship for some time thereafter, dining together at the Harvard Club and exchanging papers and letters on professional topics.

Alexander's inspiring tale fell on closed ears. Distrusting subjectivity in all its forms, psychologists put little stock in personal testimony, even that of fellow scientists. This series of articles clearly had less to do with evaluating psychoanalysis than it did with assuaging the anxiety of its contributors, many of whom were worried, like Boring, that their analyses had failed. What they needed was reassurance. But the tangible benefits of this kind of therapy are always elusive. Recall Janet Malcolm's (1984) sardonic comment: "The crowning paradox of psychoanalysis is the near-uselessness of its insights. To 'make the unconscious conscious' . . . is to pour water into a sieve. The moisture that remains on the surface of the mesh is the benefit of analysis" (p. 25). Ultimately, these articles were exercises in self-persuasion, attempts by the contributors to convince themselves that psychoanalysis was too ridiculous or too ineffectual to be taken seriously. If they managed in the process to warn off colleagues who might have been tempted to try the thing themselves, so much the better.

By the early 1940s, the situation had reached a critical stage. Psychoanalysis was becoming so popular that it threatened to eclipse psychology entirely. Journalists seemed oblivious to the differences between the two fields, and exasperated psychologists often found their discipline being portrayed as if it were nothing but a branch of psychoanalytic inquiry. This was especially galling because most psychologists assumed that psychoanalytic claims were not even true. But how could they prove this? The critiques of the early years had not worked. Attacking psychoanalysis from the couch had simply allowed Alexander to make psychologists look foolish. There had to be a better way.

The solution turned out to be so obvious that it is hard to believe it took until the mid-1940s to appear. Psychologists would set themselves the job of determining through carefully controlled experiments which, if any, psychoanalytic concepts were valid. This reinstated psychologists as arbiters of the mental world, able to make the final judgment about what would and would not count as psychological knowledge. It allowed them to evaluate psychoanalysis, rather than be overshadowed or absorbed by it. Most important, it restored the objective criterion of the experiment as the basis for making claims and settling disputes, undermining the analysts' attempts to substitute a new, subjective standard for psychological truth.

Psychologists took to their new role with a vengeance. Every conceivable psychoanalytic concept was put to the test, in hundreds of studies whose creativity was matched only by the uselessness of their findings. Mowrer (1940) demonstrated that regression and reaction formation could be produced in rats. Blum and Miller (1952) found that children who were categorized as having an "oral character" ate significantly more ice cream than did other children. Scodel (1957) showed that "high-dependency" men did not manifest the predicted preference for women with large breasts. Schwartz (1956) found more castration anxiety among men than women, with homosexual men

scoring the highest of all. Sarnoff and Corwin (1959) reported that "high castration anxious" men showed a greater increase in fear of death than did "low anxious" men after being exposed to photographs of nude women. And Friedman (1952) found that when children were shown a picture of a father and a child near some stairs, more girls than boys fantasized that the father would mount the stairs and enter the room.

Topics like oedipal relations and anal personality had their aficionados, but it was *perceptual defense* that really captured the imagination of psychological researchers. Their hypothesis was a simple one: If the mind did defend against forbidden material, then words with disturbing or salacious associations should be recalled less easily than more neutral stimuli. Fresh-faced graduate students spent hours making certain that items like *whore* and *bugger* were matched in length and salience with their sexless counterparts. Controversies erupted left and right: Were taboo words difficult to recognize just because they were not used very frequently? Wiener's (1955) famous "pussy-balls" study dispatched that idea by demonstrating that the context, not the words themselves, made certain stimuli threatening. But was exposure to a list of scatological words really analogous to the sort of trauma that necessitated repression? Blum (1954) addressed that problem with a new methodology based on the Blacky Pictures, a set of cartoon images of a dog depicted in various psychoanalytically relevant poses (licking his genitals, observing his parents having sex, defecating outside their kennel). When studies with Blacky were found to support the earlier word-item findings, repression gained the sort of empirical reality that only psychologists could give it.

By the 1950s, research on psychoanalysis had become so popular that psychologists were drowning in it. No one could possibly read all the studies that were being published, much less keep track of their results.⁸ A new cottage industry was born of this need, with workers who did nothing but summarize and evaluate these studies. Robert Sears had been the first such laborer, commissioned in 1943 by the Social Science Research Council to write an objective review of the scientific literature on psychoanalytic theory. Sears's approach, used by all subsequent evaluators, was straightforward: Having first divided the literature into topic categories (fixation, sexuality, object choice), he then counted how many studies in each area supported Freud's claims. The larger the number, the more scientific the claim. Taken together, these individual scores were supposed to provide an answer to the overall question of whether psychoanalytic theory was valid.

Sears (1943) hedged, saying that some of it was, and some of it was not. Such caution soon vanished. The self-appointed judges whose reports appeared up through the

⁸ Fisher and Greenberg's (1977) review includes more than 400 studies from the 1940s and 1950s alone. By the mid-1970s, there were at least 1,000 more.

early 1970s placed themselves squarely on one side of the debate or the other. Evaluation studies quickly became as difficult to sort out as research on psychoanalysis itself, and much less fun to read (see, for example, Fisher & Greenberg, 1977; Kline, 1972). Each report took a tone yet more strident than the last, and the original goal of providing an objective review was lost entirely. This was nowhere more evident than in Eysenck and Wilson's (1973) polemic. Every shred of evidence seeming to support psychoanalysis was scrutinized for methodological flaws, whereas studies opposing the theory were flaunted as examples of good science.

No one especially cared that the evaluation literature was becoming debased. It made little difference what the findings were; as long as psychoanalytic phenomena were made subservient to empirical test, empiricism was vindicated.⁹ That much of this research supported Freud's theory was an irony appreciated by few. It was the act of doing these studies, of piling them up and sorting them out and arguing about them that was important, not what they revealed about psychoanalysis. Some psychologists found these activities so salubrious that they recommended them even to analysts. As Albert Ellis (1950) cheerfully noted, "sociologists, who but a decade or two ago were mostly concerned with pure theory, now frequently design and execute crucial experiments which enable them to support or discredit hypotheses. There is no basic reason why psychoanalysts cannot do likewise" (p. 190).

Analysts were in no position to point out that the content of these psychological studies had only the dimmest relation to Freud's theory. "Every country creates the psychoanalysis it [unconsciously] needs," said Kurzweil (1989, p. 1), and disciplines surely do the same. Research on psychoanalysis was invigorating because it gave psychologists a sense of mastery: They had ventured onto the battlefield of the unconscious and returned, triumphant, with a set of dependent variables. Some psychologists even managed to convince themselves that the danger had been exaggerated all along, that they had really been in control. They scoffed that psychoanalysis had never been much more than an inflated way of talking about conditioning, one of psychology's oldest topics. By the time Dollard and Miller (1950) actually began translating every psychoanalytic concept into its learning theory equivalent, their efforts were almost redundant.

These behaviorist reworkings of Freud, although often clumsy, did signal a new strategy in dealing with psychoanalysis—co-optation. More satisfying than silence, with none of the pitfalls of criticism, the appropriation of psychoanalytic concepts into mainstream psychology seemed an ideal compromise. Like the Christianizing of paganism, the dangerous parts were still there somewhere, but in such diluted form as to pose no real threat.¹⁰

Watson had tried to move in this direction as early as the 1920s. By relabeling the *unconscious* as the *unverbalized*, he could sweep most psychoanalytic phenomena into the neat piles of behaviorist theory. Emotions became sets of habits; neurosis was conditioning; therapy, uncon-

ditioning. Watson never denied the reality of Freud's findings; he simply cast them in his own terms (e.g., when he warned [1928, p. 80] that sexual frustration made mothers want to kiss rather than shake hands with their children). At times, Watson even took to calling himself an analyst, as if, like some ancient warrior, he could magically disarm his enemy by assuming his name.¹¹

Other behaviorists continued where Watson left off. Humphrey (1920a), following Holt's (1915) earlier lead, dissolved wishes into conditioned reflexes. Keller and Schoenfeld (1950) laid claim to such psychoanalytic staples as the slip of the tongue (yet another reflex) and the oedipal complex (a consequence of early conditioning). But it was Skinner who took the task of appropriating Freud most seriously. In *Science and Human Behavior* (1953), he systematically redefined each of the defense mechanisms in operant terms (*repression*: a "response which is successful in avoiding the conditioned aversive stimulation generated by punishment," p. 292; *reaction formation*: "an extension of a technique of self-control in which the environment is altered so that it becomes less likely to generate punished behavior," p. 365). By the end of the book, even symbols and dreams had taken on the veneer of conditioned responses. Artful as these efforts were, they did not really solve the problem. Freud was still there. His new operant outfit gave him a natty American look, but there was no mistaking that sardonic smile. As long as psychoanalytic concepts remained identifiable as such, they were potential rivals to psychology's own constructs.

Help with this problem came from an unlikely source—introductory textbook writers. Typically dismissed as nothing but purveyors of pabulum for college students, these authors, many of them prominent psychologists, played a major role in advancing the co-optation of psychoanalytic theory. This is not so surprising. As Morawski (1992, this issue) shows, introductory texts exist in a liminal space, neither popular nor professional,

⁹ Hilgard (1952) was the only evaluator who seemed willing to grant this point. He chastised psychologists for doing experiments that "give merely trivial illustrations of what psychoanalysts have demonstrated . . . in clinical work," and argued that although "such illustrations may be useful as propaganda," they "do not really do much for science." In his view, psychoanalytic research "ought to *advance* our understanding, not merely *confirm* or *deny* the theories that someone [else] has stated" (p. 43).

¹⁰ Precisely the same thing was done with Gestalt psychology. At first, the philosophic assumptions of the theory were seen as a challenge to American (behaviorist) psychology, and Gestalt was explicitly opposed. Then the dangerous aspects were simply stripped away, making it appear as if the principles of organization were empirical observations that had arisen out of nowhere. A contemporary student of perceptual psychology would have no idea that these principles were originally formulated in opposition to behaviorist thought.

¹¹ "I venture to predict that 20 years from now an analyst using Freudian concepts and Freudian terminology will be placed upon the same plane as a phrenologist. *And yet analysis based upon behavioristic principles is here to stay and is a necessary profession in society—to be placed upon a par with internal medicine and surgery*" (Watson, 1925, p. 243). The comparison of psychoanalysis to phrenology was a favorite among psychologists; Dallenbach (1955) later wrote an entire article on this theme.

yet somehow both. They function simultaneously as translators of standard doctrine and contributors to it. Because new texts constantly supplant older ones, they become disciplinary artifacts, frozen moments of taken-for-granted knowledge, X rays of the uncontroversial.

Textbook writers took advantage of their role by assimilating psychoanalytic concepts into mainstream psychology without mentioning their origins. An early example was Walter Hunter's 1923 text, *General Psychology*, in which the various defense mechanisms were stripped of any connection to the unconscious, much the way bagels now appear in the frozen-food sections of Peoria supermarkets. Other writers soon adopted this practice, sometimes using the term *adjustment mechanisms* to expunge any remaining whiff of psychodynamics (Guthrie & Edwards, 1949; Kimble, 1956).

These appropriations took place amidst a general silence in these texts about psychoanalytic theory itself. Many writers ignored the topic entirely: Robinson and Robinson's 665-page *Readings in General Psychology* (1923) included the contributions of every conceivable psychologist, even Helen Keller and the Lord Archbishop of York, but had nothing by Freud or any other psychoanalyst (the section titled "Dreams as a Vehicle of Wish Fulfillment" was written by Watson). Readers of well-known texts like Seashore's (1923) *Introduction to Psychology* or Warren and Carmichael's (1930) *Elements of Human Psychology* would never have known that psychoanalysis existed. Even as late as 1958, a classic like Hebb's *Textbook of Psychology* barely mentioned the topic. When Freud did make an appearance, it was more likely to be in the section on punishment or motivation—topics dear to the heart of experimentalists—than in expected places like the chapter on abnormality.

Of course some textbook writers did discuss psychoanalysis in more depth, but few besides Hilgard (1953) did so sympathetically.¹² Kimble (1956) went to the trouble of including a special section in his introduction warning readers not to make the common error of confusing psychology with psychoanalysis. It was not that Freud had no value: Kimble called his work "one of the great milestones in the history of human thought" with "insights [that] have never been equaled" (pp. 369–370). Psychoanalysis just happened to be "entirely literary and not worth discussion" in a scientific text (p. 370).

In 1956, Gardner Murphy was asked to determine the extent of Freud's impact on the various subfields of psychology. He likened the overall effect to the erosion of the rocky coastline in Maine, but admitted that some areas had remained untouched by the psychoanalytic current. His results, on a numerical scale, of course, constitute what one might call an *index of introgression*, ranging from 0, Freud never had a chance, to 6, he made it all the way in. Here are Murphy's ratings: intelligence and physiological = 0; comparative, learning, thinking, perception, and vocational = 1; memory, drive and emotion, child and adolescent = 2; social and industrial = 3; imagination = 4; abnormal = 5; personality and clinical = 6.

What is surprising about these results is that there are any high scores at all. How could a discipline that had spent 50 years protecting its chastity end up seduced by a ladykiller like Freud? Of course the problem was really only with the clinicians, but there were thousands of them, and more every year (Gilgen, 1982; Kelly, 1947). When the American Psychological Association surveyed a sample of its members in 1954, asking who had influenced them to enter the field, Freud, of all people, got the greatest number of mentions (Clark, 1957, pp. 17–18). True, by that time, 37% of APA members were clinicians (p. 116), but how had that happened? Why were so many psychologists fleeing the laboratory?

Perhaps it was just the money. Or the effects of the war. But what if this exodus had a more ominous meaning?

Repression is a perverse process. It appears to efface the offending material, but this is an illusion—the contents of the unconscious are indestructible. Repressed material, like radioactive waste, lies there in leaky canisters, never losing potency, eternally dangerous. What is worse, it actively presses for expression, constantly threatening to erupt into consciousness. No one can control these forces; the best we can do is try to deflect them. It is a sign of health if we can accomplish this with a few judiciously used defenses. We know we're in trouble when we have to resort to the rigidity of symptoms.

Experimentalists took a calculated risk in trying to create a psychology in which subjective phenomena were banned from study. They knew that this would be difficult, that it would require erecting a set of defenses (the experimental method and all its appurtenances) and being vigilant about their use. But subjectivity creeps through every crevice and finds its way around even the strongest barricade. In the early years, this threat was manageable and psychology was willing to tolerate some narrowing of its operations in exchange for the reduction of anxiety its defenses allowed. Psychoanalysis tore this fragile equilibrium to pieces. By embracing subjectivity—sometimes even reveling in it—while still proclaiming itself a science, psychoanalysis forced psychology to define itself in ever more positivist terms. This was no ordinary battle over intellectual turf. It was more like a nightmare, in which psychologists watched, horrified, as the very phenomena they had sought to banish now returned to haunt them. They did what they could to contain the threat, but each new tactic only made things worse. Co-opting analytic concepts proved to be especially disastrous because it let the banned phenomena inside psychology itself. Even in scientific disguise, they were still dangerous, like a well-dressed hitchhiker who pulls a knife after getting into the car. With the threat now internal as well as external, ex-

¹² Buys (1976) has argued that it was only in the 1970s that positive portrayals of psychoanalysis became common in introductory texts. See also Herma, Kris, & Shor (1943), whose study focused on how Freud's theory of dreams was presented in such texts. They found such a high degree of criticism that they were forced to make separate tallies for *ridicule, rejection on moral grounds, and sheer denial*.

perimental psychology was forced to harden itself still further. What had once been science became scientism, the neurotic symptom of a frightened discipline.

In retrospect, we might say that this was all to the good. The psychology that emerged from these wrenching experiences was stronger and more resilient, able to tolerate a degree of diversity among its members that would once have been unthinkable. The past 30 years have been a time of exponential growth, as older areas like learning have reorganized and newer ones like clinical have matured. The "cognitive revolution" that brought the mind back to psychology transformed even the most hard-core behaviorist, and terms like *self-perception* are now banded about the laboratory as if they had been there all along. The rigid experimentalism of the 1940s now seems vaguely embarrassing, one of those righteous crusades of adolescence that pales before the complex realities of middle age.

There were many reasons for these changes, and certainly the threat from psychoanalysis was only one of a host of factors pushing psychology toward greater flexibility. But, as Burnham (1978) has argued, psychoanalysis did represent an extreme position against which more conservative disciplines like psychology and psychiatry had to define themselves. The willingness of analysts to occupy the radical frontiers of subjectivity gave psychologists room to maneuver, to create a middle ground in which previously excluded phenomena could enter without threatening the scientific standards psychologists had fought so hard to establish.

Equally important were the changes in psychoanalysis itself. During the period from 1940 to 1960, internecine warfare reached new heights among American analysts. The purges in the New York Psychoanalytic Institute were only the most visible sign that the field had become increasingly intolerant of dissent, and the huge influx of candidates after the war accelerated this slide toward conformity and conservatism (Hale, 1978; Jacoby, 1983). Psychoanalysis in 1950 was fundamentally different from what it had been in 1920, and its new mainstream mentality made it far easier for psychologists to accept.

The Second World War also played a significant role in these dynamics. Psychologists made substantive contributions to the diagnosis and treatment of war-related disturbances, as well as to myriad other problems from personnel selection to instrument design. These efforts enhanced the reputation of professional psychology and stimulated a massive increase in funding for psychological research. The war also brought to America European refugee psychologists, many of whom saw psychoanalytic ideas as part of the psychological canon. Psychologists began to spend less time worrying about whether analysts were eroding the fragile boundary between legitimate and popular psychology (Morawski & Hornstein, 1991) and took advantage of opportunities to get some favorable press of their own.¹³

American psychology has always been distinguished by an uncanny ability to adapt itself to cultural trends

as quickly as they emerge. Once it became clear that the public found psychoanalysis irresistible, psychologists found ways of accommodating to it. Instead of concentrating all their efforts on criticism, they identified those parts of the theory that were potentially useful to their own ends and incorporated them. As psychoanalysis became less threatening, psychologists were able to notice that the two fields actually shared many of the same basic assumptions: a commitment to psychic determinism, a belief in the cardinal importance of childhood experience, and an optimistic outlook about the possibility of change.

It has been only 70 years since James McKeen Cattell rose from his seat at the annual meeting of the American Psychological Association to castigate a colleague for having mentioned Freud's name at a gathering of scientists (Dallenbach, 1955, p. 523). Today that same APA celebrates the success of its lawsuit against the psychoanalytic establishment, a suit which gave psychologists the right to become bona fide candidates at the analytic institute of their choice (Buie, 1988). As the moribund institutes prepare to be enlivened by a rush of eager psychologists, perhaps it is not too much to suggest that psychology itself has benefited from having had the psychoanalytic wolf at its door.

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¹³ See, for example, Gengerelli's (1957) rhetorical romp in the *Saturday Review*, which painted psychologists as tireless laborers in the "scientific vineyard" and analysts as "muddle-headed, sob-sisters" (p. 11) who are the cause of every social ill from delinquency to early marriage.

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