

CHAPTER 2

Three Myths and Truths about Beck's Early Years

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On September 29, 2006, Aaron Beck received the Albert Lasker Award for Clinical Medical Research, the U.S. equivalent of the Nobel Prize in Medicine. Sitting in the audience (and recognized in Beck's acceptance speech) was Dr. Marvin Stein of New York City. Stein was an old friend of Beck, from his early years in the psychiatry department at the University of Pennsylvania. Stein had been his best friend in those days. They were on the junior faculty at Penn together as well as candidates at the Institute of the Philadelphia Psychoanalytic Society. They lived within walking distance of each other, and their wives and children were friends. They played tennis and golf together. They spoke on the phone every day.

Stein was also the golden child of Penn psychiatry. Unlike anyone else in his cohort, Stein had begun his career as a scientist. Stein had come to Penn in 1953 just as he was catching the enormous wave of postwar government funding for medical science and with the promise to transform Penn's psychiatry department into a world-class scientific enterprise. With the blessing of his chairman, Kenneth E. Appel, in 1954 Stein built the university's first psychiatry laboratory, up in the turrets of the old Hare Building, where he studied the psychosomatics of induced asthma in guinea pigs. Stein was equally brilliant in inspiring his colleagues—all of whom were clinicians, like Beck, with no prior experience in the laboratory—to try science themselves. Stein encouraged Beck to undertake a large-scale

experimental study of depression. Beck admired and emulated Stein so much that he described Stein to me as his “Steerforth,” the streetwise older boy in Charles Dickens’s *David Copperfield* who took the orphaned and naive David under his wing.¹

One might imagine that Stein would play a role somewhere in our histories of cognitive therapy (CT). But Stein’s name is unknown to cognitive therapists. Why? The answer is because in 1962 Stein suddenly dropped out of the picture. In fact, many of Beck’s colleagues from his early years at Penn suddenly dropped out of the picture. Even the generation of clinicians and researchers with whom Beck collaborated in the 1960s—the formative decade of cognitive therapy—are not part of the CT origin story. They, too, remain unfamiliar names today.

This is a curious historical situation. Beck, at the age of 42, was not a young man when he published his first article on thinking and depression (Beck, 1963). And he was 55 when his first book on CT—*Cognitive Therapy and the Emotional Disorders*—came out (Beck, 1976). Surely something must have happened, even larger than his discovery of the role of cognitions in depression, to cause such an astonishing “historical amnesia.”² As a historian, I have spent almost two decades exploring both the known and the (vast) unknown territories of Beck’s early years to try to paint a fuller and more nuanced picture of the origins and contours of the model.

The answer to the question “What happened in and around 1962 to generate this odd kind of amnesia?” lies in Beck’s complicated break with psychoanalysis. The standard origin story of CT is that in the late 1950s Beck undertook a major study of the manifest dreams of depressed patients, funded by the National Institute of Mental Health (NIMH), to prove the psychoanalytic hypothesis that depression is a form of inverted hostility. As Beck has recounted many times, the results were sufficiently equivocal to cause him in the early 1960s to reconsider not only the psychoanalytic theory of depression but indeed all of the postulates of psychoanalysis. By 1963, he had completely dismantled psychoanalytic theory, was creating a new cognitive theory, and was refusing to look back. When Richard Suinn asked Beck in 1991 who his professional influences were, he answered: “I had a number of psychoanalytic advisors but none of them influenced my work as it eventually developed. I’m afraid I am a ‘dead end.’”³ Beck’s

¹Author interview with Aaron T. Beck, November 3, 2010; see also Beck’s acceptance speech, Lasker Award Ceremony, September 26, 2006, Center for Cognitive Therapy, University of Pennsylvania.

²I am borrowing the phrase “historical amnesia” from historian Russell Jacoby, whose *Historical Amnesia: A Critique of Contemporary Psychology* (1975) focused on the rise of ego psychology.

³Letter from ATB to Richard Suinn, October 29, 1991. Personal Collection, Dr. Aaron T. Beck.

colleagues are familiar with this story. It is also completely in line with the historical record and even accounts for the fact that many of the people with whom he had previously been associated dropped off the map.

The problem with this story is not that it lacks truthfulness but rather that it is an incomplete telling of a larger story. Beck's break with psychoanalysis was actually one of the most protracted and convoluted in American history. From 1962 to 1976, he simultaneously broke from and sought an audience with psychoanalysts. A salient example of this phenomenon is the fact that even as he was training first-generation cognitive therapists—Jim Stinnette, Dean Schuyler, Martin Seligman, John Rush, Steve Hollon, Maria Kovacs, and others—he was simultaneously courting psychoanalysts and was even a fellow of a psychoanalytic organization. The tradition of dichotomous (dare I say black-and-white) thinking that has characterized most historical accounts of CT simply isn't adequate to the task of penetrating these complicated truths.

There are three aspects ("truths") to Beck's protracted break with psychoanalysis that challenge long-held assumptions about the origins of CT. Each has a corresponding "myth" which I have so named not because it is false but because it has had the effect of misdirecting the community's attention and unwittingly generating a false understanding of where the boundaries lie between the two schools.

MYTH #1: BECK'S DISCOVERY OF A COGNITIVE ASPECT OF DEPRESSION WAS *SUI GENERIS*

Beck has often told the story of an epiphany he experienced in 1956: while in session with a depressed patient, he suddenly intuited that there is a preconscious stream of thinking, accessible to awareness, in which we are constantly evaluating ourselves and our world. As he tells the story, he had been employing the standard psychoanalytic postulate that depression was a form of inverted hostility, and he had tried to convince his patient that she was really suffering from hostility. To his surprise, she rejected his interpretation and instead unloaded a complex of worries that he didn't like her, that he found her boring, and so on. This insight eventually led Beck to study experimentally the psychological correlates of depression (e.g., Bloch, 2004). While no hard data exist to confirm Beck's Martin Luther-like epiphany-in-a-thunderstorm, there is also no reason to doubt its truthfulness. Beck's memories nearly always map accurately onto facts I have found in the archival record.

There is more to this story, though, that helps cast light on the context of his early work. Beck's epiphany did not emerge fully formed out of nowhere. It was rather the result of having trained in the early 1950s with a Hungarian-born émigré and psychoanalytic psychologist named

David Rapaport. From 1950 to 1952, Beck was a psychiatry fellow at a small but influential private mental hospital in western Massachusetts called Austen Riggs. Rapaport was the dominant intellectual force there at that time. Rapaport and the other clinicians at Riggs were exemplars of a particular branch of psychoanalysis that flourished in America in the postwar years known as *ego psychology*. As a group, these Rapaport-affiliated ego psychologists were building on the work of Anna Freud, Heinz Hartmann, Ernst Kris, and others in studying what would now be called patients' metacognitions—how patients evaluate and make meaning out of their world.⁴ Beck's mentors at Riggs conceptualized those beliefs as "the reality-testing capacities of the ego," or the patient's "ego strength." Beck learned to understand patients who were anxious, depressed, phobic, or overwhelmed as suffering from "defective ego-structures."

Rapaport was especially interested in systematizing the psychoanalytic theory of *thinking*. In 1950, the year Beck arrived, Rapaport had just completed a massive tome called *The Organization and Pathology of Thought* (Rapaport, 1951). This book was his life's work (he died prematurely in 1960). Rapaport argued that thinking—a category that included cognition, attention, perception, learning, and memory—was the bridge that connected psychoanalytic theory with experimental psychology. To be more specific, Rapaport was convinced that the *ego*, the hypothesized region of the mind that mediates between primitive urges and the demands of reality, was also the location in which the "conflict-free" mental functions of normal psychology operated. I have written elsewhere that Rapaport's passion for thinking and the ego dominated the Riggs conversations. Psychiatry fellows had frequent contact with him (Rosner, 2012, 2014).

Beck's epiphany occurred only four years after he had left Riggs (and two years after he completed a tour of duty as a psychiatrist in the Korean War). Given the close proximity between his 2 years with Rapaport and this insight into patients' metacognitions, it is highly likely that the influence of Rapaport and ego psychology were still strong. Indeed, we must assume this to have been so. The historical record is brimming with evidence that young clinicians who came under Rapaport's influence walked away with his cognitive stamp. Everyone who studied with him in the late 1940s and 1950s—ranging from those who hewed most closely to ego psychology (notably George Klein, Robert Holt, and Roy Schafer; see Friedman, 1991) to Nobel Prize-winning psychologist Daniel Kahneman (who studied with Rapaport in 1959) to master historian of American psychoanalysis John Burnham (who was also at Riggs in 1959)—has admitted Rapaport's influence.⁵ Seen in this context, Beck surely would have been

⁴Author conversation with Dr. Jeremy Ridenour, Austen Riggs, October 5, 2015.

⁵For observations on Rapaport's influence on Kahneman, see "Daniel Kahneman—Biographical." *Nobelprize.org*. For influence on Burnham, see interview with author, November 22, 2013.

primed in the mid-1950s to be looking for how his patients evaluated themselves and their world. Beck himself has credited his “interest in cognition, which fitted under the umbrella term of ego psychology . . . (to) his contacts at that time with David Rapaport.”⁶

What was *sui generis* about Beck's insights about thinking and depression, however—what set him apart from the ego psychologists—was that he eventually concluded that cognitions *were themselves* the psychopathology rather than the *sequelae* of the psychopathology. Rapaport had primed all of his students to look for cognitive patterns and even to postulate the existence of cognitive structures. But George Klein, Roy Schafer, and others who publicly followed Rapaport still believed that psychopathology was the result of a conflict between primitive wishes and the press of reality. For them, the locus of psychopathology was unresolved conflicts within the ego. Any changes in a patient's thinking were consequences of the ego's inability to manage those conflicts.

Beck flipped the situation around. He rejected the ideas of primitive wishes and conflict and held instead that *the structures themselves were cognitive in nature such that the psychopathology resided directly within them*. Now *faulty cognitive structures*, not defective ego structures in a motivational system, were the *locus of psychopathology*. In the early to mid-1960s, during a self-imposed five-year sabbatical from his department, Beck began fleshing out the contours of these cognitive structures (which he called “schemas”). He proposed that schemas develop in childhood and that in their primitive, childhood condition, they have the qualities of being fixed, rigid, dichotomous, and closely tied to emotions (good/bad, black/white, happy/sad etc.). As the child matures, the schemas take on the qualities of flexibility and distance from emotions such that the individual gains the capacity to evaluate situations rationally and resists the pull of strong emotions. Psychopathology results from a failure of primitive schemas to attain these mature qualities. Repeated exposures to situations that trigger a primitive schema can lead to a buildup of energy (Beck imagined a threshold-activation model) and a hyperactivation of the extreme ends of the structure. If enough energy builds up, the energy can then spill over into the extreme ends of neighboring schemas. In sum, these immature and poorly functioning schematic structures not only produce the primitive thinking typical of psychopathology but also create the cascade effect that leads people to generalize and extend their faulty thinking beyond the immediate situation (see Rosner, 2012, for a full exposition on this subject).

One of Beck's closest psychoanalytic collaborators at Penn in the late 1950s, Marvin Hurvich—who later became an ego psychologist himself—read a draft of one of Beck's papers in which he made public parts of this new theory and recognized immediately that

⁶Aaron T. Beck, draft of biographical sketch, extended version, Box 2 ff: Biographical write-ups—CV, 9/27/89. Personal Collection, Dr. Aaron T. Beck.

the finding of consistent cognitive structures in depressives apparently has not been noticed by the psychological testers [ego psychologists like Roy Schafer], who are interested especially in cognitive structures. For example, Schafer maintains . . . that the diagnosis of depression is not based on any particular characterological picture . . . but rather on indications of speed, efficiency & variability of thought and action. Said another way, it appears that Schafer has only been impressed by the “speed,” “efficiency,” & “variability” aspects of the thinking of depressives as clues to differentiating the thought processes of depressives from the thought processes of other groups. Your work goes considerably beyond this.⁷

Seymour Feshbach, another psychoanalytically-oriented psychologist who was a consultant on Beck’s depression study, agreed that “cognitive theorists will be very pleased by this paper. . . . My own view is that it will serve as an important and necessary corrective to certain motivational accounts of depression.”⁸

In sum, one of Beck’s earliest influences was ego psychology, particularly Rapaport’s cognitive strain. Interestingly, Beck never divorced himself fully from ego psychology, even after he allied with behavior therapists in the 1970s. One could even make the case that his first book on CT, *Cognitive Therapy and the Emotional Disorders* (Beck, 1976), was actually an ego psychology text. The only publisher who took the manuscript was International Universities Press (IUP). For decades, IUP had been the main publishing house for ego psychology texts, including an edited volume from Austen Riggs (in which one of Beck’s papers was included; Beck, 1952; Knight & Friedman, 1954), the ego psychology monograph series *Psychological Issues*, and George Klein’s 1976 book on psychoanalysis (Klein, 1976).

Clearly, the editors at IUP put Beck in that camp. And so did Beck himself. In 1981, he wrote to John Bowlby that

it might be a point of curiosity therefore for you to know that my psychiatric training was completely and exclusively psychoanalytic . . . I would consider my theoretical work as derivative from ego psychology rather than from cognitive psychology or learning theory. At the present time in fact I am trying to reformulate many of the basic psychoanalytic assumptions into cognitive terms.⁹

⁷Letter from Marvin Hurvich to ATB, February 25, 1963. Personal Collection, Dr. Aaron T. Beck.

⁸Letter from Seymour Feshbach to ATB, January 17, 1963. Personal Collection, Dr. Aaron T. Beck.

⁹Letter from ATB to JB, July 29, 1981. Personal Collection, Dr. Aaron T. Beck.

And he acknowledged to Paul Salkovskis in 1990 that “first I called [cognitive therapy] ego psychology, [and then I felt that this was] the psychoanalysis of the ‘60s, this is neo-analysis. What I am saying is that [cognitive therapy] is consistent to this day with Adler and Horney and so on.”¹⁰ And so the full truth about Beck’s interest in cognitions is that it dates to 1950 when he came under the influence of David Rapaport and ego psychology. Rapaport’s theory of thinking is the missing link between Beck’s formative years as a psychiatrist, his expertise in cognitions, and his admission that CT is a derivative of ego psychology.

MYTH #2: COGNITIVE THERAPY IS BASED ON SCIENCE WHILE PSYCHOANALYSIS IS NOT

By the late 1960s, just as he was going public with CT, Beck set terms that would define the mission: CT would champion experimentalism in contradistinction to psychoanalysis, which relied on dogma and faith. It is true that Beck had felt enormous pressure to take what amounted to a loyalty oath to psychoanalytic theory. In 1968, he reflected on this pressure in a letter to Paul Meehl: “As time went on, I realized that support for [the psychoanalytic] postulate was ultimately derived from the declarative statements of the psychoanalytic authorities rather than from evidence; I began to quaver in my belief that ‘20,000 analysts can’t be wrong.’”¹¹ He spoke more bluntly with his biographer, Marjorie Weishaar, in 1991:

The personal element . . . that got me out of the whole psychoanalytic framework is the whole notion that authorities don’t have to be taken at their face value and my own data seemed to contradict the authorities; that my own data can be trusted. . . . And there were no authorities that are more powerful in this world except maybe priests—the Pope—but no authority is more powerful than analysts because they know everything. They have the word.¹²

It might seem, therefore, that Myth #2 is really the whole truth. Indeed, it does convey the expectation of organized psychoanalysis—by which I mean the American Psychoanalytic Association and its local institutes—of loyalty to their interpretation of the model. For them, experimental science was anathema because to operationalize and standardize psychoanalytic

¹⁰Transcript of an interview with Aaron T. Beck/Interviewer Paul M. Salkovskis, November 3, 1990. Personal Collection, Dr. Aaron T. Beck.

¹¹Letter from ATB to Paul Meehl, March 13, 1968. Personal Collection, Dr. Aaron T. Beck.

¹²Transcript of an interview with Aaron T. Beck/Interviewer Marjorie Weishaar, August 4, 1991, pp. 17–18. Personal Collection, Dr. Marjorie Weishaar; Weishaar, 1993.

constructs meant to observe and quantify them. And yet a creed of the psychoanalytic model was that the presence of any observing body in the therapy room would necessarily violate the mechanism of treatment, the transference. So from an epistemological point of view, they held, it was foolhardy to study psychoanalysis experimentally (see Rubenstein & Parloff, 1959; Rosner, 2005, for discussion of this particular epistemological position).

Regardless, Myth #2 is part of a more complicated story about the challenge of reconciling the epistemologies of psychoanalysis and experimentalism. Long before Beck trained as a psychoanalyst, a small but influential minority of psychoanalysts was exploring experimentalist approaches. Rapaport was one of them. So was Franz Alexander, yet another Hungarian-born émigré who had recently come to the U.S. from Berlin to lead the Chicago school of psychoanalysis. Alexander was in constant conflict with the American Psychoanalytic Association over his innovations with shorter treatments, behavioral exercises, psychological tests, and quantitative studies linking manifest dream themes with presumed psychosomatic illnesses such as hypertension, asthma, menstrual disorders, and others (Rosner, 1999).

One of Alexander's protégés was Leon J. Saul, who became Beck's training analyst in Philadelphia. Saul was even more of a renegade than Alexander. Saul regularly gave his patients homework assignments and conducted treatment over the telephone, in his back yard, and even in his car. Saul also championed experimentalism and assembled a team of scientists, even though he himself was not expert in the laboratory. In 1956 Saul offered a seminar on the quantification of hostility in manifest dreams. That seminar was Beck's introduction to scientific research. That same year Saul had published a "hostility scale" (for measuring the presence of hostility in manifest dreams) after which Beck modeled his own first scale (with the assistance of Marvin Hurvich), known as the "masochism scale." This was the beginning of what became Beck's NIMH-funded study of depression (Rosner, 1999).

As experimentalists, Saul and Beck joined the ranks of influential psychoanalytic psychologists like Lester Luborsky (first of the Menninger Clinic and later of the Department of Psychiatry at Penn), David Shakow (of the Intramural Psychology Laboratory at NIMH), and George Klein (Rapaport's protégé at New York University). They were also in the company of sympathetic psychoanalytic psychiatrists, especially Roy Grinker of Chicago who was editor of the *Archives of General Psychiatry*. Grinker's *Archives* published many of the papers that came out of Beck's first NIMH depression study, including the paper on the Depression Inventory (e.g., see Beck & Ward, 1961; Beck, Ward, Mendelson, Mock, & Erbaugh, 1961; Beck, Sethi, & Tuthill, 1963; Ward, Beck, & Rascoe, 1961, 1962). Beck modeled his first book, *Depression: Clinical, Experimental, and Theoretical Aspects* (Beck, 1967) on Grinker's 1961 monograph on depression

(Grinker, Miller, Sabshin, Nunn, & Nunnally, 1961). In 1972, Beck publicly acknowledged his debt to Grinker by contributing an essay to a *fest-schrift* in his honor (Beck, 1972). Finally, in 1968 Beck joined an organization that Grinker, Saul, and others had founded called the American Academy of Psychoanalysis. The mission of the Academy was to combat the “antiscientific” attitude of the American Psychoanalytic Association (Grinker, 1958). The Academy was actually the first organization Beck joined after he built CT. In other words, the very first national community to which Beck turned with CT was not behavior therapists but scientifically inclined psychoanalysts. Beck became a fellow in 1969, chaired sessions at meetings, and published articles in their journal. He stayed through 1976 (Rosner, 1999). So the truth is that a subgroup of American psychoanalysts was actively bridging psychoanalysis and experimental science, and Beck positioned the cognitive model in their camp, at least at first.

But why then was he simultaneously writing to Meehl disparaging comments about the antiscientific attitudes of psychoanalysts? Here a more sobering aspect of experimentalism and psychoanalysis becomes clear. The hard truth all of these analysts had to face was that to stay true to the epistemology of experimentalism they had to be willing to modify psychoanalytic theory should the data call for it—and few were willing to do so. I have described elsewhere the “epistemic frame” through which Saul and his cohort viewed scientific practice, which I have dubbed a “theory-trumps-data” mentality. For them, scientific data were valuable only as long as they supported psychoanalytic theories. Loyalty to the theory was paramount (Rosner, 2014).

They were therefore in an epistemological bind, and most of their efforts failed. Edith Sheppard, one of Saul's protégées and a close colleague of Beck in these early years, actually threw away data that didn't support her analytic hypotheses.¹³ Rapaport's closest followers found his dizzying psychoanalytic constructs impossible to operationalize, but rather than question psychoanalysis, they revolted instead against Rapaport. David Shakow of the NIMH invested millions of government dollars in an effort to obtain objective knowledge about psychoanalysis through filming an entire course of treatment. But he abandoned the project with the sobering conclusion that it was simply too difficult to study psychoanalysis experimentally (Rosner, 2005). Even Roy Grinker lamented that the American Academy had failed in its mission to promote science in psychoanalysis.¹⁴

When viewed from the perspective of these failures and frustrations,

¹³Author Interview with Dr. Robert Daroff, Cleveland, Ohio, December 2012.

¹⁴Letter from Roy Grinker to Henry Laughlin, June 25, 1969. American College of Psychoanalysis Collection, Box 2, ff. 3. Courtesy of the New York Hospital and Cornell University Medical College, Oskar Diethelm Library, History of Psychiatry Section, Department of Psychiatry.

Beck's solution appears radical. His tactic was to switch out the "theory-trumps-data" mentality for "data-trumps-theory," and then to modify psychoanalytic theory with abandon (Rosner, 2014). Already in 1962, he had turned away from drive theory. This decision relieved him of the obligation to hold any other part of the theory sacrosanct. It freed him to design a study in the early 1960s that tested the hypothesis that depressed patients wish to suffer (he found they do not wish to suffer) (Loeb, Feshbach, Beck, & Wolf, 1964). In sum, in chucking drive theory Beck was free to forge ahead with the agenda of his early psychoanalytic mentors, namely, to explore the "conflict-free" cognitive aspects of the "ego"—which accounts for his comfort in identifying himself as an ego psychologist. It may sound heretical, but Beck's trajectory with CT was arguably the most successful adaptation of ego psychology to the demands of experimentalism. Still, the price Beck paid for doing so (abandoning drive theory) was too high for most psychoanalysts to pay.

MYTH #3: AARON BECK BROKE WITH PSYCHOANALYSIS

This myth seems so patently true that readers may be astonished I even question any aspect of its truthfulness. Didn't Beck admit that he broke with drive theory in 1962? Didn't he tell Paul Meehl, Marjorie Weishaar, and so many others over the course of decades that he rejected psychoanalysis?

The problem with this myth is not the actuality of a break but rather the assumptions that Beck was the one who did the breaking and that the break was clean. The truth is that Beck never actively sought to break with *organized* psychoanalysis. The opposite was true. Since 1950, he had diligently and thoroughly mastered the craft, jumped through the hoops, and become a fellow of the Philadelphia Psychoanalytic Society. He began formulating the cognitive model of depression while he was still an active member of the psychoanalytic community—and viewed the model as a reformulation of psychodynamic theory. He wrote to Leon Saul in 1961 that he believed the time had come to rethink the psychodynamics of depression because his data suggested that drives might not be involved.¹⁵ Surely it was bold to suggest that drives were not involved anywhere in depression—but there was precedent. Karen Horney had made a similar argument, as had other neo-analysts who followed her. And still, Beck, as was true of them, did not originally plan to break with psychodynamics.

Instead, the psychoanalytic establishment broke with him. The establishment couldn't have done more than it actually did to kick him out. The

¹⁵ATB to Leon Saul, September 28, 1961. Personal Collection, Dr. Aaron T. Beck.

context of his break is the missing piece of the puzzle. Beck has never publicly spoken about the fact that in the early 1960s both he and his cohort became pawns in two different power plays, one by the American Psychoanalytic Association and the other by the senior faculty at Penn. These power plays occurred just as Beck was cresting with his new model. The crises took an enormous personal toll on him, and ultimately he had no choice but to walk away.

The first crisis was his failed attempt to join the American Psychoanalytic Association. Membership in the national organization was a given for any graduate of a local institute, and Beck followed the required procedures, including agreeing to a two-year waiting period after graduating from the Philadelphia Psychoanalytic Institute before applying. He finally applied in 1960. But the American Psychoanalytic Association nonetheless deferred his application, claiming Beck had insufficient training. Beck's patients had only needed two years of analysis, and the committee did not believe they could have been "symptom free" and "improve(d) . . . after such comparatively brief periods of analytic work." The committee advised him to undertake "additional supervisory work on the advanced or termination phases of a suitable analytic case, preferably a female, for about one year."¹⁶ Although Beck did not undertake additional training, he did reapply in the fall of 1961 with a detailed description of his four control cases. The American Psychoanalytic Association again deferred his application: "I couldn't even get mad at something like that," he later told me. "It's like [when] a hallucinating schizophrenic starts calling you names. I was really quite disillusioned now. I can't say necessarily with psychoanalysis but with the people. They were so dumb, they really were dumb."¹⁷ He did not reapply.¹⁸

What Beck couldn't have known was that his deferment was a warning from the American Psychoanalytic Association to Leon Saul that he was taking too many liberties with the orthodox model. Historian Nathan Hale has shown that the American Psychoanalytic Association used strategies like this—punishing the student as a slap on the wrist of the training analyst—to curb innovation and force loyalty (Hale, 1995). Beck was not Saul's only student to be deferred. Regardless, after 1961 Beck faced an uncertain future. He refused to undertake additional training, and yet without membership in the national organization his professional options

¹⁶Letter from Gerhart Piers to ATB, December 16, 1960. Personal Collection, Dr. Aaron T. Beck.

¹⁷Author interview with ATB, July 17, 1997.

¹⁸Undated, unsigned handwritten document, American Psychoanalytic Association Papers, RG11 Committees, Series 10, Subseries 1 (Committee on Membership 1961–1971), Folder: 1962–1964, Oskar Diethelm Library, Institute for the History of Psychiatry, Weill Cornell Medical College, New York.

were limited. An unfortunate truth about the American Psychoanalytic Association during this period in American history is that it had unchecked power and did not use that power wisely. It couldn't have done more to turn a creative psychoanalyst away.

The second crisis, which had been fulminating for 2 years, erupted in the summer of 1962, about six months after Beck learned of his second deferment. The crisis was a pitched battle between the psychiatry department and the university administration over who would succeed the retiring chairman of psychiatry, Kenneth E. Appel (Rosner, 2014). The battle became a referendum on the future of psychoanalysis. The senior faculty championed Marvin Stein, Beck's best friend and the brilliant scientist who promised to preserve old traditions like psychoanalysis and psychosomatics. The university administration, in contrast, wanted Eli Robins, a biological and experimental psychiatrist from Washington University who was vocally antagonistic toward psychoanalysis. The senior faculty, in their desperation to hold onto long-standing traditions, employed less-than-honorable tactics to secure Stein's nomination and pillory Robins.

Beck was caught in the crossfire. Appel (along with other senior faculty like Leon Saul) put tremendous pressure on Beck and other junior faculty to support Stein. Stein himself used pressure tactics to secure his nomination. Beck had originally supported Stein but eventually felt that the wisest political position was neutrality. He urged, indeed pleaded with, Stein to do the same—but the pressure was too much. Stein pushed hard for the chairmanship, Beck resisted being drawn into factions, and the result was a breach in their friendship and a split among the junior faculty in their loyalties. The crisis over Appel's successor nearly tore the department apart and irreparably damaged Beck's friendship with Stein. In the end, the administration chose someone else entirely, another junior faculty member named Albert J. (Mickey) Stunkard. But the damage was done. Stein grew enraged with Beck. Stein no longer saw a future for himself at Penn and within a year would leave for New York City.

Within weeks of Stunkard assuming the chairmanship, Beck requested a one-year sabbatical—which Stunkard granted. Stunkard's impression was that Beck needed time to heal from the break with Stein. One year turned to five. Between 1962 and 1967, Beck worked from his home-office (and saw patients in his office at the Girard Bank Building at 133 S. 36th Street at the corner of Walnut Street) in a self-imposed isolation from Penn psychiatry. It was during this "splendid isolation" that he composed the two foundational articles on thinking and depression, completed his first book (Figure 2.1 shows Beck ca. 1968 sitting in his home office looking at his new depression book), and built his new "cognitive" therapy. Other major events in Beck's life, by unfortunate coincidence, also dated to late 1961 and 1962, including the death of his mother, the NIMH's decision not to renew funding for his large depression project, and his family's move to

a more affluent suburb. All of these major events converged to make 1962 a particularly difficult year and the prospect of a sabbatical even more appealing (Rosner, 2014).

One might imagine that by now Beck would have become allergic to organized psychoanalysis. So it is surprising that he immediately brought his cognitive model to the Philadelphia Psychoanalytic Society when he returned to active departmental life in 1967. In other words, he tried yet again to *find fellowship* with psychoanalysts. And yet again, psychoanalysts *broke with him*. In 1997, Beck reminisced about the moment he presented CT to the Philadelphia Psychoanalytic Society: “When I presented this material before the local analytic society, I said, ‘this is really neo-analysis.’ They said, ‘Well, Beck, this is no longer analysis. You better stop calling yourself an analyst.’”¹⁹ So Beck turned instead to the American Academy of Psychoanalysis. The Academy did not reject Beck, but, in failing to cultivate science in psychoanalysis, the Academy couldn’t really take his ideas very far either. In 1970, Beck decided to court behavior therapists. But the behavior therapists didn’t accept him either, at least initially:

I had to find a new name for this approach. At that time I was attracted to behavior therapy, so I thought maybe I’d call myself a behavior therapist.

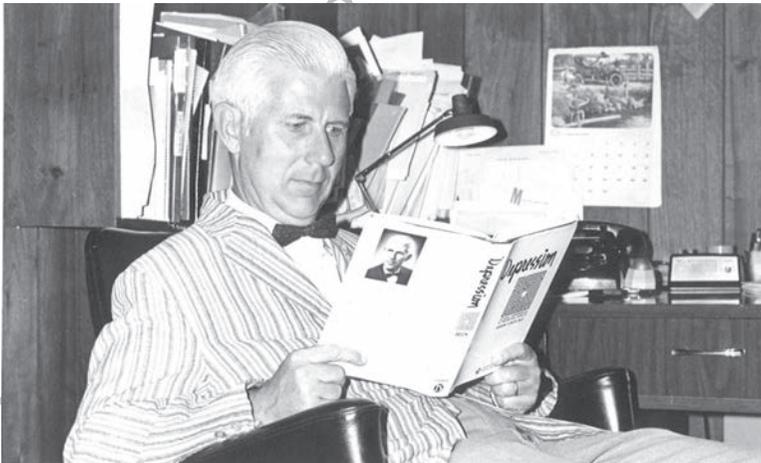


FIGURE 2.1. Aaron Beck ca. 1968 sitting in his home office looking at his new *Depression: Clinical, Experimental and Theoretical Aspects*. Used with permission of Aaron T. Beck.

¹⁹Aaron T. Beck, “The past and future of cognitive therapy,” Unpublished manuscript, University of Pennsylvania School of Medicine, 1997, p. 7.

I spoke to Dr. Wolpe about some of my ideas and he said, “Well, you’re not a behavior therapist at all.” So, I ended up with the idea of calling my approach cognitive therapy.²⁰

It wasn’t until Beck attracted a critical mass of residents and postdoctoral fellows in the mid-1970s—a full generation younger than he with no memory of the old psychoanalytic culture—that CT gained traction. They and successive generations catapulted Beck into a leadership role in the burgeoning cognitive-behavioral therapy movement.

These examples paint a picture, then, not of someone proactively breaking with psychoanalysis but of someone with whom the psychoanalytic establishment kept breaking. And despite his intense dislike of the culture of faith and loyalty in psychoanalysis, he continued seeking fellowship with like-minded analysts wherever he could find them. Beck emerges as a highly creative analyst intensely frustrated with a psychoanalytic establishment that kept curtailing innovation. He turned away from drive theory in an effort to break out of those restrictions. It was not in his nature, however, to rebel with a flourish but rather to plot a course that would maximize his chances of transforming psychiatry into something closer to his own image—with whomever was eager to join in (see also Bloch, 2004, p. 860, where Beck admits a “fuzzy” break with psychoanalysis and his rebellion against the autocracy of the psychoanalytic establishment).

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Beck and Stein did not resume contact, with the exception of a few phone calls and letters, until the Lasker Award ceremony in 2006, forty-four years after the breakup of their friendship. In his acceptance speech, Beck acknowledged Stein’s crucial role in helping him become a scientist. It is tempting to speculate why Beck invited Stein to the Lasker Awards. Perhaps he still yearned for Stein’s approval, keen to show him that he had made it as a scientist. He and Stein used to joke that they should create a school of psychosynthesis to put back together all of the people analyzed apart by psychoanalysis. Maybe he wanted to celebrate with Stein the realization of this vision. It’s likely, too, that Beck felt the political situation in psychiatry had changed enough and they were old enough now to mend fences. I first made contact with Stein (by serendipity) in 1997, almost a decade before Beck’s Lasker Award, and even then Stein was eager to share his story and help with my historical research on Beck. Clearly Stein, too, was eager to mend fences.

Whatever his motives, Stein’s presence at the Lasker Award ceremony brought Beck’s journey with CT full circle: to the time before the political

²⁰Ibid.

fallout when he believed his new ideas about thinking and depression might revolutionize psychodynamics. A constant theme in Beck's work has been a quest for common ground. Neither he nor Stein had asked for the political crisis that destroyed their friendship, soured (even more) their feelings toward psychoanalysis, and launched Beck on the road to cognitive-behavioral therapy. In the case of telling his history, it may well be that a détente with psychoanalysis is in order if only because it facilitates a fuller remembering of the complicated truths of Beck's early years.

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