CHANGING HUMAN BEHAVIOR: DEVELOPING EFFECTIVE TREATMENTS FOR CHILDREN WITH SEVERE AGGRESSIVE AND ANTISOCIAL BEHAVIOR

Alan E. Kazdin

In this essay, I wish to highlight my professional trajectory and with that a line of research that has dominated my career. The paths leading to that work seem indirect and fortuitous, and I will convey key influences along the way and changes in that work over time. I would love to have a childhood story that explains the path and make early connections to what I do professionally fascinating and cohesive with the overall trajectory. Noting I was raised by feral wolves before I swam to an island only to find Mel Crusoe (Robinson Crusoe's neglected younger brother), who raised me without language. Unfortunately, dear reader, none of that is true and hence you may have already read the most interesting part of this essay. My roots were modest in many ways and sparse in terms of any academic stimulation. Consequently, I use the occasion of this essay to chart much of my professional work, including key foci, what influenced me along the way, and how my career has unfolded.

As background, I was born in 1945 in Cincinnati, Ohio, and after six months moved to Michigan for a couple of years. Then my family (mother, father, older sister) moved to Los Angeles, California, where I spent the rest of my childhood and adolescence. In school, I was an unremarkable student. I have an older sister (four years older) who was and is brilliant academically and that was wonderful for me. Somehow her accomplishments lifted all pressure on me in my home life. I must have conveyed to my parents early that I was not likely to achieve very much. At school, teachers initially were very impressed with me when they called roll for the very first time. As each teacher noted my last name, there often was a pause in the roll call to ask if I was related to Frances, my sister. At that moment, but sadly for that moment only, each teacher

---

Alan E. Kazdin is Sterling Professor Emeritus of Psychology and Child Psychiatry at Yale University. Before coming to Yale, he was on the faculty of the Pennsylvania State University and the University of Pittsburgh School of Medicine. At Yale, he has been chair of the Psychology department, director and chair of the Yale Child Study Center at the School of Medicine, and director of Child Psychiatric Services at Yale New Haven Hospital. His research has focused primarily on the treatment of aggressive and antisocial behavior in children and adolescents. In 2008 he was president of the American Psychological Association. His 800+ publications include fifty books that focus on research methodology, psychosocial interventions for children and adolescents, parenting and child rearing, and interpersonal violence. His work on parenting and childrearing has been featured on NPR, PBS, BBC, and CNN, and he has appeared on the *Today Show, Good Morning America, ABC News, 20/20,* and *Dr. Phil.*

was pregnant with hope that yet another flawless, determined, and stellar student was in their class. In a very short time (typically after the first quiz), one teacher after another fell into line as they realized that my sister and I apparently only shared last names and perhaps not even the same gene pool. Consequently, both at home and at school no one expected very much, and I am happy to say I met everyone's low expectations without failure.

Child-rearing at the time, at least for my lower-income family, seemed to be laissez-faire. I was left alone, and the most frequent directive probably was approval in the form, "Yes, you can go out and play." I was not intensively prepared for any future in a strategic way via assorted lessons or experience that would accord special appreciation (e.g., literature) or develop talents (e.g., music, dance, gymnastics). There was always music in my home, and my mother was a singer and pianist who did not turn to these talents until I was already deep into adolescence. Nevertheless, music was core to my upbringing and still is a huge part of my life. In general, my parents and those of my peers often left well enough alone unless a child had a problem in the neighborhood or with the law. I did not have those so I just lived with a well-run home (meals, cleanliness, regular family meals, and celebration of holidays) and with caring when I needed it, but without much attention to guide my path. I am grateful for all that was provided, especially because in my professional work, many families I worked with rarely had what I considered to be basics. My parents worked hard, and one could readily discern that by the jobs and hours they put in and the absence of habits or leisure that would use resources (money or time).

Without pressure and no strong academic background in the home, I strategically established a pattern of academic mediocrity that continued through my glorious high school years. In high school, happiness and superficiality blossomed as I spent my time: working on my car so it would be drivable, spending time with my girlfriend, working at a part-time job to pay for my car (but not for my girlfriend), and playing basketball. Indeed, practicing basketball probably occupied most of my time, certainly my weekends. In high school, I even had individual physical education classes (just me and a coach) that would allow me to maintain that intensity in the off-seasons.

I went to college (San José State University) to continue to play basketball, but some circumstances (prolonged mononucleosis) limited that and something else grasped my attention. In college, I studied psychology and philosophy and had a very influential and close adviser in each area. I went from a carefree high school student to someone who absolutely loved the learning opportunities and work of college. I focused on learning all I could from classes and began to do very well.

My mentors gave me remarkable time. As an illustration, on two occasions within my four years, my philosophy adviser noted that if I cared about this or that topic (something which seemed to pique my interest) I should take a course on something else that was related. In both instances, in separate years, there was no such course, so he developed one in which I was the only student. In his office, where each class (three days a week for a full semester) was conducted, he stood most of the time with his hands in his pockets, lectured, and paced a little within the constraints of a small office. I had readings from the required texts. I took notes and wrote multiple brief term papers to turn in and so on. I took these courses for granted — it all seemed so natural, but it was remarkable individual tutoring. Also, the courses were not a funfest or informal. In one of them, he returned five short papers to me and asked me to completely rewrite them with a different and more critical focus. I remember him breaking this news to me. We were in his office and he had the five graded papers in hand. He put them down and said, "Let's go for a walk." On that walk, a typical cloudless sunny California day, he told me about the kind of paper I ought to write, the kind of thinking it ought to reflect, and why the papers I had turned in were not suitable. We went back to the office, he handed me the papers and noted, "No lecture today, just work on the papers." What an influence that was. His guidance, orientation, and thrust actively guided my work on those papers, of course, but even now on research papers I write. I kept in contact with him past graduate school and deep into my mid-career to make sure he was aware concretely of what his impact was on me.

My psychology adviser gave me similar time, and I often shadowed him on his daily activities (evening presentations to mental health groups, chatting about his research with professional athletic teams, walking with him through campus, playing basketball at lunch with the psychology faculty, and asking an endless stream of questions about his work or psychology more generally). Our personalities were very much alike — so much so that before I met him, a handful of peers encouraged me to take his course precisely because we seemed to share so much in our personalities. Whatever the verdict on that, it was easy and enjoyable being with and learning from him. I ended up doing a senior thesis (research investigation) with him, and that fostered even more frequent contact and exposure to infectious enthusiasm for research and his professional work.

In my senior year, I had an academic identity crisis about whether to go to graduate school in philosophy or psychology but finally decided to pursue psychology. I was unsure what I wanted to do for a career, and psychology, when contrasted with philosophy, seemed to have vastly greater career options (e.g., in colleges and universities, hospitals, schools, private practice, industry, the military, and more). I selected psychology and ended up going to Northwestern University and began the Ph.D. program in clinical psychology. This followed the exact path of my psychology adviser.

Chronology: Research and Career Development

Graduate School (Northwestern University, 1967–70)

Background. Graduate school included the expected course work, clinical work to learn and practice selected psychotherapy techniques, and research experiences, but the unique feature was great emphasis on research methodology. This included understanding the underpinnings of various types of investigations as well as the concrete techniques derived from them to conduct investigations. Whereas students in current doctoral programs might get a course on how to conduct research, we had multiple courses on statistics and research design (e.g., quasi-experimental designs, factor analysis, novel types of measurement). When my peers and I saw the light at the end of the methodology tunnel, it turned out to be a white neon lighted sign that said, "Turn left here for your next set of methodology courses." There was a handful of areas of psychology and students in their Ph.D. programs, but the clinical psychology group required more science methods and related courses. We were baffled. Yet, the rationale was that clinical research (clinic settings, patient samples) can be methodologically messy (e.g., challenges of recruitment, missing data, endless sources of bias to address, dropouts, nonexperimental designs). Clinical faculty told us that knowing a broad range of methodological (design, statistical) options was especially important. Much of psychology is based on running experiments with college students who show up for experiments, receive course credit, and are done. In clinical psychology, or so we were told, our work would be much more onerous, and drawing valid scientific inferences required much more background, nuanced thinking, and nuanced methods. At the risk of conceding yet another "I told you so" in my life, this turned out to be quite true.

The emphasis on methodology and statistics was very much in keeping with my interests and temperament. Two among a handful of enduring lessons from philosophy related to methodology. First, scrutinize the question and key constructs before even thinking about the answer. The question and its formulation may include all sorts of assumptions and beginning points that greatly influence, and in some cases dictate, the answer. Second, there are multiple ways of knowing (e.g., intuitionism, induction and deduction, faith); they begin at different places, have different goals, and usually lead to different answers. Even within one way of knowing (empirical knowledge), methodological practices (design, measures, data analyses) can influence the findings and conclusions. Keeping this in mind has helped me retain mild skepticism about scientific findings in general and those from my own work in particular and advocate for methodological diversity in graduate training and our research programs. And from psychology, and now with endless statistics and methodology, one could look at ways of obtaining answers to more carefully formulated questions and how to recognize the many threats to drawing clear conclusions and how to prevent them from being very plausible when the study was completed.

Transformative Experience. At three points in my career, I have been in a position where there is a need to change behavior and adaptive functioning of individuals who were having difficulties in getting along with others, were completely unmanageable at home or at school, and who could not stay out of trouble (e.g., at school, with the law). The first occasion occurred in graduate school. Changes were occurring in psychotherapy research with increased attention to the psychology of learning and how findings from basic laboratory research might be applied to enhance treatment. A class of psychological treatments, referred to as behavior therapy and later cognitive behavior therapy, were emerging along with a keen interest in placing psychotherapeutic interventions on firm empirical footing. Much of psychotherapy that is used in clinical practice had not been evaluated in research, so claims about whether they were effective were a matter of speculation and mostly anecdotes. By the mid- and late 1960s new scientific journals emerged to publish and foster rigorous studies that evaluated treatment for a variety of disorders, with special emphasis on anxiety and depression among adults. The research focused on careful evaluations of treatment and developing measures to demonstrate that in fact people changed, beyond merely reporting that they did, as a function of the treatment. It is not that self-report is irrelevant or unimportant. Rather it is often easier to have people say that they have changed on some questionnaire than having changed in how they function or behave in everyday life. By the end of the 1960s I was in graduate school and part of the emphasis at that time was placing interventions on firmer scientific footing than had been the case in prior generations.

Within this context, my adviser, noting my budding interest in behavior change, suggested that I explore a part-time job opportunity at a nearby facility for children, adolescents, and adults with a range of emotional and behavioral problems and intellectual disabilities. Perhaps there was a useful experience here to complement my training. I arranged an appointment. At the job interview, the director, a clinical psychologist, asked about my interests and then described the setting, clientele, and goals. He then asked me to work at the facility part-time and make therapeutic changes that would help individual clients, beginning with adolescents and adults in the sheltered workshop part of the facility, so they could be placed in jobs in the community. I weepily confessed and threw myself on the mercy of the court by admitting I was merely a graduate student and that I knew nothing about the clientele or the problems they evinced, let alone how to have any influence on them. His reply was stunning, discomforting, and welcoming. He noted that the facility included expert professionals from all the mental-health professions as well as counseling, rehabilitation, education, and nursing, but no one seemed to know what to do to have concrete impact on problem behaviors or skills that the clients needed. I again reiterated that what I brought to the setting would be ignorance, heavily seasoned with a thorough lack of expertise, which only seemed to strengthen his resolve.

After a little more discussion, he walked me to an empty office far away from his but near the portion of the facility where he wanted me to work. He pointed to the empty chair in a fully equipped but plainly unoccupied office and asked me to take the job and sit there for as many weeks or months as it took to learn what I needed; and then, when ready, to change behaviors of the clients on the “floor” (a vast room with many individuals, staff, work stations), putting them in a position for community placement. I took the job part-time, which accommodated my graduate school schedule, and spent the time in the office for several weeks. Eventually, I did what the director asked.

Many promising interventions were emerging in behavior therapy. From my office, I wrote to key leaders at the time, read their articles on behavior change, and began to learn the emerging literature in an area of learning referred to as applied behavior analysis. This area drew on experimental laboratory research (operant conditioning) and principles and techniques that could be derived and used to change human behavior. I also called some of these leaders to ask for more details than their articles provided about implementation. It was during this time that I began to study intensively both the substantive approach (principles and techniques that can be derived from them) and methods (assessment and single-case experimental designs) that were often used to evaluate change. As an example, positive reinforcement as a general principle (ways of providing consequences) has an enormous range of options that can be converted to techniques that form part of an intervention.

After a few months, I made some modest efforts to work with the staff directly in charge of the clients. We intervened and evaluated programs with individual clients to address their specific problem areas (e.g., wildly explosive tantrums, extreme social withdrawal, seeming inability to accept change in activities or tasks, loud and constant negative comments and complaining). This required the methods of behavior analysis (e.g., developing reliable objective measures, establishing measurement reliability, conducting observations) and evaluating the impact of using the rigorous yet flexible methods of single-case designs (e.g., continuous assessment over time, alteration of conditions to test for causal relations, iteration of interventions when interventions were not having the desired effect).

Very soon I was working daily with the staff on the floor who implemented the procedures, and I was infrequently in my office. We started to make changes in the behaviors of many clients such as reducing or eliminating tantrums, improving socialization with others and social skills, following instructions regarding the work, and shifting tasks without a stress reaction. Some of the modest projects and demonstrations even saw light of day in various journal publications. I began to explore some procedures and their nuances to understand how they worked and how to improve their impact. This eventually led to various review articles on some of the techniques and a few books as well.[1]

This job lasted for three years and had enormous influence on my later and present work. The challenge of that job: What could be done to effect change in a reliable and marked fashion? For example, when staff were called upon to change the behaviors of a sixteen-year-old male who constantly swore out loud, said nasty things to women all day, and had outbursts from what seems the slightest or no real provocation, what could be done? Operant conditioning techniques and the methods used to evaluate their impact proved extremely helpful and often provided many options to achieve therapeutic change. To be sure, I had my failures as well, and understanding these generated ideas and a few articles on what might be done differently to improve treatment effectiveness.

A second instance that contributed to my focus on interventions was an extension of the previous experience. A superintendent of a local school district learned of the work I was doing and asked me to implement and evaluate similar interventions on a larger scale in several school classrooms. Here too there was some accountability with the job where the metric was behavior change (disruptive behaviors, performance of academic tasks) of "problem students" in classrooms. The most dramatic introduction was with the first challenge presented to me. This student was a brilliant (IQ allegedly over 130 or 140) ten-year-old boy who was reported to routinely walk on top of all the desks (in a U-shaped arrangement in class) during a lesson, stepped on the papers and classwork of his classmates in the process, spoke very loudly while he was walking as the teacher was trying to present a lesson, and then went to the front of the room and "fondled the teacher." I believed none of this so I observed by sitting quietly in the back of the room for a couple of days. Everything was true exactly as stated. What could be done beyond the ineffective reminders, reprimands, and trips to the principal's office? A lot; and beginning with subtle reinforcement schedules (differential reinforcement for other behavior), contingent attention, shaping, and ignoring, we eventually had the child seated and doing his work. No magic—the techniques have variations that can be adapted and change behavior.

Beyond any one child, the overall mandate for this position was more accountability. What could be done to effect change? Beyond workshops for teachers on some of the uplifting usual cliché themes (e.g., empowerment, setting limits), could the teachers and I work together in ways that had direct impact on classroom and academic child behavior, and could I demonstrate that? Here too, variations of procedures I had been working with and the methodological demands (e.g., operational definitions of the goals, ensuring they were reliably assessed, evaluating intervention effects over time) were valuable. I worked in several classrooms helping to design and implement interventions for individual students as well as for entire classrooms.

The work in the schools allowed me to design and complete research for my dissertation. I conducted a project comparing several variations of intervention programs on classroom performance of the students. This of course could never be done without the support of the school superintendent of the schools, along with small monetary support to carry out the project. Research in applied settings, rather than say in a laboratory, has many challenges, and implementation can raise a series of headaches. Even so, I completed the study and requirements for my Ph.D. sooner than expected (August of one year rather than June of the next). Consequently, I had not gone on the job market in time for a fall job. Northwestern graciously placed me on the junior faculty for one year during which I taught a variety of basic psychology courses. I kept my part-time day job and started to write up some of the findings from that work and to have the papers accepted in various scientific journals. Then I finally went on the job market and started my next position.

Early Career: The Pennsylvania State University (1971–80)

I accepted a position in the department of psychology at Penn State. After some start-up time and mini cul-de-sacs in research, I began three programs of research. First, I began to study an imagery-based therapy (covert modeling) for the treatment of adults and focused on two problem domains (social withdrawal or anxiety). Imagery united several different schools of therapy, mostly outside of the United States, and was an interesting bridge between traditional nonbehavioral therapy and an emerging set of behavior therapy techniques referred to as covert conditioning. Also, modeling (also called observational learning) had been studied extensively in basic and applied (e.g., therapy) research. A key of modeling is in the coding of experience (e.g., cognitive, neural), usually by observing a live or filmed model. But it is the coding or representation that is likely to be critical, rather than presentation of material visually. Having individuals systematically imagine models (referred to as covert modeling) might be a way to effect change without direct observation.

I began a series of studies to evaluate whether covert modeling would in fact lead to change and then variations of modeling imagery to examine similarities in the effects of observed and imagined models. The studies were randomized controlled trials (RCTs) and included imagined rehearsal by a model, with imagery (but no model) control and no-treatment control conditions, self-report and behavioral outcome measures, evaluation of treatment integrity, control for expectancy and credibility of treatment, tests for generalization of treatment effects beyond training materials, evaluation of clinical significance of change (bringing individuals within the range of individuals functioning well on the target domain), and brief follow-up assessment (e.g., from weeks to six months). These design features helped my later work in much more complex clinical research situations. We completed several studies of covert modeling over a period of more than a decade, all with adults as clients, and demonstrated the therapeutic impact of the intervention in randomized controlled as well as esoteric variations and experimental manipulations that influenced the impact of treatment.

A second line of work focused on evaluation of applied behavior analysis interventions in the classroom and was an extension of the previously mentioned part-time job and application in the schools. At Penn State, I had access to a laboratory school for elementary school students with moderate to mild intellectual disabilities. Collaborators (classroom teachers) knew applied behavior analysis very well, which allowed some of our work to look at nuanced influences that required careful execution of the interventions. The physical facility (e.g., a mirrored back wall behind which multiple observers could code the behavior of individuals or the entire class) was conducive to research. We completed several studies that relied on single-case experimental designs (e.g., ABAB, multiple baseline, simultaneous treatment), focused on small numbers of individuals, and based on direct observation of overt behaviors as the key dependent variable. This work built on my classroom applications in graduate school but was much more systematic and evaluated all sorts of interventions designed to change child behavior, to improve the impact of interventions, and to maintain these changes over time.

Third, my work on acceptability of treatment began during this period. Intervention research, including my own, focused primarily on symptom or target behavior change and overt behavioral measures. This focus developed as part of the move toward more direct and "objective" indices of change beyond the heavily emphasized subjective experience and self-reports of internal states and symptoms. Yet it became clear that it was important to extend evaluation of treatment beyond outcome or symptom change. One dimension was the extent to which nonprofessionals, laypersons, and other potential consumers of treatment view the treatment procedures as reasonable, fair, and appropriate, i.e., treatment acceptability. The rationale is obvious in the sense that developing effective interventions may not be very helpful clinically if patients do not also find them acceptable and agree to participate in them.

We began a series of studies, beginning with college students, to evaluate alternative treatments that were effective for a given problem. Once the basic paradigm was worked out, we used patient samples and expanded the evaluators to children, parents, hospital staff, and outpatient therapists. We evaluated different treatments (e.g., psychotherapy, medications, hospitalization, time-out from reinforcement) and treatment characteristics (e.g., restrictiveness, effectiveness, side effects) that might influence acceptability. Several studies were completed showing that treatment acceptability could be readily distinguished among treatments, with more restrictive treatments (e.g., hospitalization) or aversive treatments (punishment) and medication being less acceptable than psychotherapy and behavioral interventions (e.g., positive reinforcement of incompatible behaviors to eliminate behavioral problems).

Assessment of treatment acceptability of children, parents, and therapists was routinely included in our later treatment outcome studies and occasionally was evaluated to see its relation to therapeutic change or some other facet of treatment. As importantly, specific findings (e.g., the acceptability of positive reinforcement of incompatible behavior) influenced the actual interventions we used with clinical populations.

Mid-career: University of Pittsburgh School of Medicine (1980–89)

By the late 1970s, my research programs were in place and collaborators, students, and resources were excellent. All was going quite well. For no clear reason I could identify, I felt a slight dissatisfaction; I wanted to work more with clinical populations and to focus exclusively on children. I was invited to interview at Western Psychiatric Institute and Clinic (WPIC), the psychiatry department at the University of Pittsburgh School of Medicine. Somewhat embarrassingly, I began annual job interviews over a period of a few years because I was unsure of the shift in lifestyle from a pastoral academic psychology department to the pace of a medical school department. I finally took a leave of absence from Penn State to try the job at WPIC and after an enormously stimulating and high-paced year remained in Pittsburgh. The pace was marvelous and included learning more than ever before.

When I arrived, I was given the luxury to float for a few months to discern the vast array of research programs on psychiatric disorders as approached from multiple perspectives and types of research (e.g., wet-lab bench, patient-care, community-wide research). I was asked to decide what I wanted to do and then to let the department chair know. After a couple of months, I mentioned I would like to do research on the children's inpatient service (Children's Psychiatric Intensive Care Service). The department chair was enthusiastic and immediately noted that I would be placed in charge of that service. Understanding very little, I thought I had not been clear in my initial statement. I respecfully noted that the position I wanted was one in which I could do research (research director would be the formal title) and that I had no interest in being in charge of everything and everyone (program director). With equal respect, the department chair noted that to do what I wanted required complete authority over how the operation was run. This was not at all what I wanted. A psychologist administratively over medical and nursing staff, teachers, ward staff, and others was rare in a medical school to say the least and completely against my better judgment, not to mention interest. In keeping with my already well established streak of being wrong in such matters, the appointment as program director turned out to be essential to mobilize the service to address research. Literally overnight I was placed in charge of the inpatient service, a locked ward inpatient service for youths 5–12 years of age. I was also informed that my first task (first thing the next morning) was to inform the current director, a child psychiatrist, that he was no longer in charge; and my second task was to stop all data collection that was not being used clinically or as part of a productive research program. The first day completed, learning the routines and activities of the service and the role of each person began on the second day.

As I settled in, I began two programs of research, each funded by a set of grants and each in collaboration with a research team on the clinical service and nursing and medical staff. First, a line of work focused on elaborating key features of childhood depression. The department had become a clinical research center for depression and had enormous resources and diverse disciplines. Also, depression and suicidality were frequent bases for clinical referral to the child inpatient service I directed.

Resources including child, mother, and father evaluations of child dysfunction and the accumulation of cases reflected a rare research opportunity. We completed several studies that evaluated cognitive and emotional features, overt behavior (e.g., social activity, affect-related facial expressions) in naturalistic observations, and nonverbal behavior (during interviews and storytelling), and that addressed diagnostic issues and challenges. We also evaluated depression and its relation to early child experiences (e.g., child abuse) and in relation to other disorders (e.g., anxiety disorders, developmental disabilities). A main focus was on suicidality and how to predict who would be likely to attempt or complete suicide.

Second, we began a series of studies on developing treatments for children with severe aggressive and antisocial behavior (e.g., extensive fighting, property destruction, stealing, setting fires) and who were unmanageable at home and at school. Most of the children on the inpatient service were referred for these problems. As a psychiatric disorder, this is referred to as Conduct Disorder and is one of the most costly of mental disorders in the United States. The disorder encompasses:

- academic and social problems,
- psychiatric disorder and physical health problems in adulthood,
- substance use and abuse,
- domestic violence and other criminal behavior, and
- an earlier than expected death in adulthood from cancer, cardiovascular disease, and chronic respiratory disease.

Previously I mentioned two positions in graduate school in which I was asked to develop effective ways of changing behavior and demonstrating the impact in palpable ways that made a genuine difference. The hospital position now provided a third opportunity with the same thrust, but in this case to develop interventions to reduce aggressive and antisocial behaviors among severely impaired children. In the inpatient service, we tried many reasonable treatment options (e.g., various medications, social skills training, traditional individual therapy sessions with a therapist, group therapy, and a structured milieu) and occasionally even allowed parents to try options they viewed as reasonable (e.g., exorcism) and with no less evidence than interventions we were trying. We decided to develop and evaluate treatments for the children. That led to a three-decade research program with a central stream and many tributaries.

The goals of our interventions were to improve child functioning at home, at school, and in the community. We began with parent management training (PMT), an intervention that draws heavily on principles and techniques from applied behavior analysis. These are the principles and techniques I drew on in my prior work when in a position to change behavior. Individual treatment sessions rely on extensive role-playing of parent-child interactions (usually with the therapist and no child present); repeated practice and shaping of parent behavior; and modeling, feedback, and praise on the part of the therapist. The therapist alters parent behaviors; the parents apply the skills trained in the sessions to change child behavior. We drew on a variety of behavior-change techniques drawn from research—these are fairly esoteric, even within psychology (e.g., elimination of behaviors through reinforcement; use of a range of antecedents; use of simulations, response priming, shaping, and special ways of delivering praise)—many of which I had been studying in my prior job.

Consider an illustration of one technique to alter one of many problems the children brought to treatment. Many parents come to us and note their child has explosive tantrums. Explosive tantrums usually mean something like this. The child will scream, hit, break things when just told no on something. Typically, I would ask the parent if the child ever had a tantrum that was not so explosive. (If that were the case, we could work from those and gradually develop much less explosive tantrums.) For this example, let us assume my usual case, namely, all the tantrums seem to be explosive. As part of a larger set of techniques we would train the parent to use, for this we would focus on simulation. The goal of this intervention, as other techniques of PMT, is to foster repeated practice of the behavior we want. Simulation is something you, as a reader, already support, rely on, and understand well. Airline pilots do not learn how to handle crashes in real situations; they work in a simulator in which they repeatedly practice the behaviors under diverse conditions. In any case, for the tantrum, we worked with parents in a special way to foster repeated practice. The simulation includes subtle techniques that foster the practice. We begin by working with the parent and asking them to play a game with the child. The parent is instructed to go to the child at home when everyone is calm and suggest playing something called a "tantrum game." The name is not important here. Now the parent explains the game to the child. The explanation might go approximately like this, as the parent says:

"Let me explain a game to you that I think you will like—it is called 'the tantrum game.' What happens is this: I'm going to say 'no' and say you can't do something. This is just pretend, and you really can do what I say. After I say that, you are going to have a tantrum, and this is pretend too. When you have a tantrum, you're going to say 'no,' you're going to fold your arms—maybe like this [parent demonstrates], and you're going to stamp your feet, but you're not going to hit mommy and you're not going to use that obscene gesture that grandma taught you [one or two components that the child does are selected, so this part is individualized]. If you can have a 'good tantrum' without those words and gestures, you can earn a point (an X) on this chart. [The parent can have a chart ready to place an X for each good tantrum.] These points can be used for some toys" [or something the child might like—more television or video time, some activity with a parent]. As an important aside, the points are not needed. The point helps the parent do this well. The most critical part is praise after the good tantrum as I describe in a moment.

The parent continues, "So let us try a practice, and don't worry, I am going to help you." Now the parent leans over to the child with a smile and says, "Okay, Billy, you can't watch TV tonight." She quickly whispers for this first practice, "You really can watch TV tonight, we are just pretending" and then continues, "Okay, now your turn—try to have a good tantrum, remember no hitting mommy and no gestures." The child does the tantrum and the mom gives very special praise that includes three components: great enthusiasm (that was fabulous!), specifies exactly what she is praising (you had the tantrum and …), and now adds nonverbal approval (e.g., pat on the back, affectionate touch to the head, or something equivalent). And, she notes, "You earned a point on this chart—this was just practice but it was so good, let us add a point to the chart." Now the mom displays a mischievous playful look and says to the child, "I'll bet you can't do two in a row, no six-year-old can do it. When you're bigger you'll be able to do two in a row." It is very likely now that the child is smiling, beaming actually, and says, "No, no, I could do two, I could do two." The mom says, "Why don't we wait and try it another day." The child is usually a bit more insistent but still smiling. The mom yields and says, "Okay, okay we'll do it one more time." They do it again. Over the next several days, they play the tantrum game once or twice as convenient. The goal of all of this is to have practice trials so that repeating the behavior is the most central part of the intervention. Typically, between one and three weeks what usually happens is the child's regular tantrums outside the game change. They get a little bit better, and now one of the parents runs over and says, "We weren't even playing the game and you did this and this and that [and specifies features of the 'good' tantrum]. That is super wonderful" (and say what was super wonderful, provide praise, and something nonverbal but on point). As the tantrum game continues, one can further craft the tantrum, based on eliminating or reducing components or intensity that had made this explosive. In about two or three weeks, the regular tantrums become fine and take over. Parents praise those for a while, but the game can be stopped and the praise can be stopped and the child's behaviors continue.

The above has playful and informal features, but the technical features are critical to note because they involve research on several features of the procedures. First, the game uses antecedents, namely multiple factors that increase the likelihood of getting the behavior. In the game, the use of a calm time, the mother leaning over and smiling, the tone of the voice, and the game-like context all greatly increase the likelihood of getting the good tantrum. Second, asking initially for a small change in the tantrum compared to the usual (referred to as shaping) is critical. Third, the consequences, primarily praise, are critical too, and the usual praise parents provide in everyday life are not close to what is needed to effect repeated practice and change. The components of praise make a difference. And the challenge to the child ("I'll bet you can't do two in a row") is another antecedent that greatly increases the likelihood that the child will do this again and enthusiastically.

Recall that we are doing the simulation game and using various techniques as part of that to have repeated practice. We know that repeated practice builds habits and changes the brain as there is more and more practice. The approach is about building enduring changes and habits, and the programs are temporary ways to accomplish permanent change. There is a strong research basis for all of this and there is no magic. Indeed, the reader may recall the distinction: what's the difference between a psychologist and a magician? A psychologist pulls habits out of rats.

I have provided a sliver of what is done in PMT. Parents come to sessions (rather than the child) and extensively practice carrying new behaviors. Sessions involve role play, praise and feedback by the trainer, and developing highly skilled parents who can carry out technique. Trainers enact tantrums, refuse to get into hypothetical care, delay getting ready for school, and so on. Parents are trained what to do. Occasionally the child is brought into the session so the therapist can watch the parent and child in action. This is a way to refine parent performance but is more often used to assess how well the parent is at executing the techniques.

PMT was not a viable option for many children on the inpatient service because parents experienced some limiting condition (e.g., major psychiatric disorder, in and out of prison, selling illicit drugs, engaging in prostitution). For these children, we needed a treatment with the child that did not require parent participation.

Children with aggressive and antisocial behavior have deficits in how they identify social cues, in how they consider their options in responding, and then of course in how they respond. We selected cognitive problem-solving skills training (PSST) to redress specific deficits in relation to social behavior. In PSST, the child is trained to engage in a sequence of steps or self-statements designed to help him or her look carefully at the demands of the situation, consider what might be alternative positive (rather than aggressive) ways of responding, consider the consequences of different actions, select one of those responses, and act out the solution in a role-play situation in the treatment session. The therapist models how to apply the self-statements to situations (e.g., being bullied, being threatened, being asked to steal something by a friend) the child may encounter and how to complete the sequence of steps. PSST sessions include intensive practice in using the steps and in responding to increasingly more complex and provocative social situations in the sessions but also at home, at school, and other settings.

After pilot testing and modifying the various treatments for many months, we began a series of RCTs to compare variations of PMT and PSST to the usual hospital care or usual hospital care plus individual sessions of play and relationship-based therapy. Treatment studies with seriously impaired clinical samples, unlike those with adult volunteers (covert modeling), took much longer to mount. Indeed, typically a study required three to five years to complete (e.g., twelve or more months to pilot treatment variations; three to six months of therapist training including supervision of "practice" cases, recruitment, and treatment of cases; and pre-, post-, and repeated follow-up assessments of one to two years).

When I was in charge of the children's inpatient unit, treatment began while the child was hospitalized and then continued after hospitalization on a weekly and then more tapered basis. Eventually, I moved from the inpatient unit and began a specialty outpatient clinic devoted exclusively to the treatment of conduct disorder. From our studies, we had many findings, but the main summary to note here is that the techniques were effective in reducing aggressive and antisocial behavior and increased prosocial behavior among children referred for inpatient or outpatient treatment. These changes were evident at home, at school, and in the community immediately after treatment and up to one to two years of follow-up. The treatments produced additional changes not specifically targeted including child improvement in peer relations at school, parent decreases in depression, decreases in stress at home, and improvements in relations among family members.

Beyond treatment, we studied diverse facets of conduct disorder related to parent and family characteristics, diagnostic issues, and nuances of various symptom patterns. Apart from my own lines of research, the setting was rich in collaborative opportunities that led me to work with a variety of populations including individuals with visual impairment, intellectual disabilities, anxiety, all in the context of psychiatric disorders. Impetus for going to WPIC was to become involved in research with clinical populations. The opportunities proved to be without peer. I became steeped in child psychiatry, routinely attended and presented papers at meetings, became more deeply involved in psychiatric diagnosis in research and clinical work, and collaborated on projects using methods I would not have used on my own.

Mid- and Later Career: Yale University (1989–Present)

After ten years in a medical school, a couple of job opportunities emerged and it was time to return to academic psychology. I moved to Yale University and transplanted my outpatient clinic and various grants. At Yale, I began a clinic (Yale Child Conduct Clinic) on campus and received referrals for children with severe aggressive and antisocial behavior from child psychiatry at the School of Medicine and eventually from clinics and services throughout the region. We continued to evaluate the effects of treatment with more in-depth analyses of the treatment, the range of factors on which it depended, and characteristics of children, families, and the context in which they lived. We completed several studies of such topics as stress in the home, severity of parent and child psychiatric disorder, the physical abuse of *parents* by their children, adverse child-rearing practices, dropping out of treatment, barriers to participation in treatment predicted worse treatment outcomes, responsiveness to treatment, the effects of online treatment, the role of the therapeutic relationship in treatment, and more.

The research on PMT for conduct problems has led to an enormous (and international) demand from parents to our clinical service to help with the "normal" challenges of child-rearing. To paraphrase more than one parent, "you can get children to stop beating up their peers, destroying property, or setting fires, but can you get my child to go to bed on time or to eat vegetables, or can you have my adolescent ditch the 'attitude,' nasty comments, and sigh of disgust when I just ask her 'how was school'?" For our treatment team, addressing these challenges was a therapeutic oasis compared to our daily fare with clinically referred children. Consequently, we divided our time to provide PMT for parents interested in this focus. With the expanded focus, we wanted to eliminate the notion of "clinic" to reduce any stigma parents might perceive in bringing children without problems to our service. The new name, Yale Parenting Center, accommodated families whose children experienced significant mental disorder (conduct disorder but other disorders along with that) as well as other families whose children presented no clinical problems. Parents just wanted help with the normal challenges of raising children and adolescents. Unfortunately, we could not sustain providing services for the normal challenges of parenting. The costs could not be reimbursed at the level needed, and grants were not available for that purpose. Consequently, after a couple of years, we continued treating only those families with severely impaired children. I provided multiple resources for parents, as an alternative, as I mention further below.

Broader Topics, Themes, and Issues

The empirical investigations have led me to broader issues including methodological and conceptual challenges, strategies commonly used in scientific research that I considered limited or misguided, and lines of work intended to improve the impact of our research on clinical dysfunction. Here I highlight topics I have addressed in multiple papers spanning an extended period.

Research Methodology

Much of my writing has focused on research design and scientific methodology. In the context of the dominant research practices of null hypothesis statistical testing, I have written on artifact and bias in measurement; unobtrusive measures; evaluation of treatment integrity, diverse control group options and what they can and cannot accomplish; moderators, mediators, and mechanisms of change and how they might be better studied; statistical power; and meta-analysis. Some of the method papers began by questioning key assumptions, a carryover from an undergraduate influence I mentioned already. To provide just three examples, colleagues and I have argued that the commonly used measures in treatment studies are not all that relevant to patients who come for treatment; that the evidence base for evidence-based treatment has some surprising and pervasive sources of bias and is readily challenged; and that so-called premature termination from treatment cannot be assumed to be premature at all in light of the great changes many patients make that serve as a basis for their departure. Of course, invariably it is important to go beyond complaining and challenging, as enjoyable as those parts are, and to point constructively to how new directions and research strategies could mitigate current practices and what a different research agenda might look like.

Standard scientific research across most disciplines focuses on group research in the tradition of null hypothesis testing. I have elaborated another approach, single-case experimental designs, their strengths and limitations, and how they could improve evaluation, especially in the context of clinical work. I have written about qualitative research, yet another approach that is infrequently trained in graduate work. The overall message is that multiple methodologies are all viable and give different information about the phenomena we wish to study. Letting one methodological approach dominate, however beneficial its products, is unnecessarily limiting.

Substantive Topics

There are substantive topics that have been enduring in my work as well. I note three here that are programmatic and go beyond one or two papers on a topic. First, child and adolescent psychotherapy has occupied many of my writings. This has included status reports of where we are now in relation to progress in research, what the theoretical and research agenda ought to be to move forward, and special issues in relation to ethnic and cultural diversity. Second, use of punishment in child-rearing, especially corporal punishment, has been of enduring concern largely because the topic has emerged repeatedly in my clinical work with conduct disorder and more importantly because the topic is so well studied in research. Among the topics are the deleterious consequences of corporal punishment (greatly increases risk for mental and physical disorders, early death, criminal behavior), mild punishment options that can be used, and effective behavior-change interventions to eliminate behavior (e.g., functional analysis, differential reinforcement schedules) that do not require punishment at all. Third, corporal punishment led me to the broader concern of interpersonal violence (e.g., child and spouse/partner abuse, rape, exposure to violence). Violence affects all ages, is a huge concern internationally, and has implications for mental and physical health. Also, it perpetuates cycles that are not frequently noted. Perpetration of one type of violence (child abuse) predicts perpetration of another type of violence (date violence, spouse abuse). Victimization from one type of violence is a predictor of victimization from another type. In short, violence is linked to deleterious mental and physical health problems as well as social woes.

Finally, I have written on the token economy (point systems), which might well be one of the most extensively applied psychosocial interventions in terms of range of ages (e.g., toddlers through the elderly), clinical problems (e.g., individuals with schizophrenia, anxiety, conduct disorder), behaviors in everyday life (e.g., conserving fuel, carpooling), and samples (e.g., school students, the military, amateur and professional athletic teams). Among the topics are how to make programs more effective, what to do when programs fail, and how to maintain behavior after programs are terminated. Many of the findings from this literature entered into the development of PMT and our clinical work.

Bridging Research and Clinical Practice

An enduring question in clinical psychology is whether the findings from well-controlled treatment studies generalize to clinical practice, given their very different priorities and conditions (e.g., patients, therapists, and monitoring and supervision of treatment). Many of my articles focused on this gap including an empirical evaluation and comparison of the characteristics of research and practice of child psychotherapy. One effort with this emphasis was a contract with a state within the United States to evaluate empirically the impact of child therapy as administered in clinical practice throughout that state. (The contract for this latter project forbids any comments or sharing of results, which was a wise stipulation given the findings, which showed that the treatment paid for by the state and federal government was not having much if any effect on the children who were receiving treatment. Nothing changed after these data became available.) For a variety of reasons, it is difficult to transfer research findings about what might be effective to clinical practice; and even when the transfer is made, the interventions get diluted in their delivery and do not produce the scope of benefits evident in research. Bridging the research practice gap occupies the careers of many, so perhaps there may be hope.

In retrospect, I see my writings and efforts to bridge research and practice as naive and wish I could unpublish most of them. Many influences (e.g., clinical training and accreditation, professional license requirements, professional and consumer satisfaction with and acceptability of non-evidence-based treatments, no formal quality control of delivery of services) provide wide marble pillars that firmly support the status quo and are not stressed by soft verbal breezes suggesting change. I recognize (finally) that just because research and practice can be bridged, that does not mean there is a strong interest, incentive, or pressure to do so, at least at this point in time.

Psychology and Other Disciplines

My primary interest is addressing clinical and social problems beyond the questions of my own research and certainly beyond the constraints of any one discipline. Examples of such problems include providing clinical services, fostering societal-wide lifestyle changes (e.g., exercise, walks in nature, community opportunities for mindfulness exercises) to support mental and physical health, redressing mental health disparities. Theories, models, and methods from other disciplines have helped me conceptualize problems as well as potential solutions. As one example, psychological science can make an enormous contribution to promote a sustainable environment and mitigate climate change. The concepts of “wicked problem” from policy making, social planning, and business and the concept of life-cycle analysis from manufacturing and ecology have helped me better understand the nature of the challenge, the inherent complexities in identifying and implementing solutions, and the processes through which solutions might be sought and evaluated.

As another example, clinical psychology has made remarkable progress in developing evidence-based psychotherapies, with one government agency, the U.S. Department of Health and Human Services, listing hundreds of treatments. Yet most people in need of services in the United States and worldwide receive no treatment at all. As I have argued and tried to show, disseminating evidence-based psychotherapies to clinical practice cannot help very much in changing this situation or reducing the burdens of mental illness, given the continued emphasis on one-to-one therapy delivered by a mental health professional.

Outside of psychology, there are many models for delivering interventions and products in novel ways that can be scaled up to reach large numbers where professional resources are constrained. Delivery models from economics, public health, social policy, business, entertainment, computer science (e.g., social robotics), some already tested in the context of physical health care, provide viable options and often with empirical evidence. This has led me to suggest a shift in treatment research to focus on models of delivery rather than on, or of course in addition to, intervention. Many other areas of study outside of psychology (e.g., operations research, the physical internet) are not off-the-shelf ready to apply to spread treatments to those in need, but they include models for decision-making, scaling interventions to reach large swaths of individuals, and identifying the effects and side effects of alternative courses of action, and are quite relevant. Drawing on other disciplines has been extremely helpful in clarifying the limits of my own work and in identifying paths to help reach some larger goal. Solutions for complex clinical and social problems have clearer paths when one draws on the richness of models and their evidence from multiple disciplines.

Dissemination of Research Findings through Public Media

For two decades or so, I have been increasingly interested in disseminating findings of psychological research to the public at large. In our science, we speak to ourselves too often and are silent on issues that could help with daily life. Without scientists speaking up, the dominant voice often is web-based opinions, self-anointed gurus (I hope I am not one), and statements that often violate what is known.

My media work focuses on issues of public concern related to areas in which I work and includes such topics as child-rearing, day care effects, bullying, violence in the media, monitoring of children and adolescents, child and parental stress, the relation of physical and mental health, creating a nurturing home environment, and helping children cope with trauma. I devote a few hours each week to television, radio, magazines, newspapers, and various internet media. Occasionally these include call-in radio talk shows in cities whose locations sometimes escape me as I listen to ads for barbeque sauce or a two-day sale for lawn chairs during the commercial breaks. More visible radio and television shows include NPR, PBS, BBC, CNN, the *Today* show, *Good Morning America, ABC News, 20/20,* and *Dr. Phil.* I have prepared several articles (e.g., Slate.com, *TIME*) designed for public consumption on many of the media topics I noted previously as well as two trade books designed to help parents with the challenges of child-rearing. I have also prepared a free online parenting course based on the collaboration of Yale and Coursera that provides practices that can help parents with the normal challenges of child-rearing (www.coursera.org/learn/everyday-parenting). The techniques are scientifically based and readily transferable to concrete things parents can do. In the same spirit, I have a web page (www.alankazdin.com) for parents that also makes an effort to translate scientific findings and child-rearing into practices that can help in daily life.

On the Lighter Side

Occasionally, I have taken breaks from the usual writing and seriousness of our scientific and clinical work to attempt humor. Publication of these attempts began accidentally. On two separate and unrelated occasions, I sent brief papers mocking some scientific practice or statistical method to friends that I thought they would find amusing.

In so doing, I lost track of the fact that each friend also happened to be a journal editor. Unbeknownst to me, my brief manuscripts intended for their eyes alone were revised and then published under a pseudonym. Other more direct efforts at spoofs have also found their way into professional journals that usually are very serious. Ponder the irony—my best research manuscripts, by my assessment at least, are occasionally, often actually, rejected by scientific journals. Now I have manuscripts I did not submit for publication accepted and published. Whenever I can, I insert something light (e.g., a limerick, a Mad Libs™ guide to research design, "new" optical illusions). Production editors vary in how stridently they object to such passages. I remind them that no one really reads my work and hence there is no real reason for concern.

Select Activities and Transformative Experiences

Over the years, several projects, panels, and consultations with various agencies and organizations have greatly shaped my work, largely by expanding my views well beyond what might be the perspectives and methods of psychology. As I have mentioned, my research, as research normally does, focuses on narrow questions and topics. Yet I am as or even more interested in the larger picture of an area in which I am working, and a few experiences have fed that very well.

Activities and Projects

History of Behavior Modification Project. In the early 1970s, I was invited by a panel (Committee on Brain Sciences) of the National Academy of Sciences to interview for a task. The task was to prepare a detailed document (a book) that traced how experimental research in psychology has led to psychological interventions. A prior history of science scholar had completed the task, but the final product was rejected by the committee (not a good sign). At this point in my career I already was submitting my research to journals and did not need new sources of rejection. Nevertheless, I made the trip from State College, Pennsylvania (I was then at Penn State University), to Washington, D.C. I was interviewed in person by the panel of senior eminent scholars in psychology and psychiatry. Deep into the interview, a pointed but fair and gently delivered challenge was presented by one member, "What makes you think you could write this history?" Apparently, I replied immediately, "Nothing, in fact I do not like history very much. Moreover, I think these three people [I gave names of mid-career people very visible in the field] would be perfect for this; they know so much more about this and actually are part of the emerging history." After a few hours of the interview and a lunch, I returned to my home university. In a few days, I was informed that the panel would be very pleased if I took on the task, which I eagerly accepted. Over the course of the project, I had occasional contact with the panel or members again in Washington, D.C. I learned a couple of years into the project that my answer to the question about what made me think I could do the job, and my rapid recommendation of three others in my place, sealed the committee's choice. The group felt that the answer meant I had no particular point of view or agenda in developing the history. Of course, a more parsimonious interpretation to my answer might be the lack of social skills.

This was a four-year project that required hiding in a library cubicle, endlessly searching historical documents and rare books, and interviewing living leaders involved in basic learning and applied psychological research (the nonliving leaders who seemed to have the better information proved to be less responsive). I was endlessly on the phone with others internationally but also visited others in person. Many people who might otherwise be reluctant to meet or chat were extremely open once they learned this was a history project for the National Academy of Sciences.

From that project I not only met many people but also developed enduring friendships (e.g., B.F. Skinner, Joseph Wolpe, Andrew Salter) that lasted decades beyond the project. Overall, the history was extremely educational, fascinating, and humbling. Many contemporary intervention procedures had been well anticipated long before they emerged in contemporary work, there were historical rivalries, scandals of yesteryear (affairs and divorces), multiple incompatible claims among the living about who invented what and when, and rather explicit contradictions galore. Most of this material was not central to the charge of the committee and did not make it into the product. And yet it was essential to confirm putative facts and seek convergence where none seemed to exist. The book was completed and the NAS selected a publisher (before they became their own publisher), an organization that promised contractually that this book would never go out of print. And it did not, until the company went bankrupt and went out of business!

Center for Advanced Study in the Behavioral Sciences. In the mid-1970s, I was invited to spend a year at the center, then an independent think tank (now part of Stanford University). The invitation was "suspicious." Initially by telephone, it spelled out too-good-to-be-true conditions. I was invited to go to the center for one year, receive my salary, and was not required to do anything but have lunch (rather than hide and work) with the other people (approximately forty) from psychology and mostly other disciplines. Let me think—lunch outdoors in the sun year-round, overlooking Stanford's campus, and with fascinating people from multiple disciplines—yes, I could do that. I was not sure what to dig in my heels for to bargain for a much better offer. In addition, a few others working in some facet of behavior change and closely aligned topics were invited that year, and we were asked to synthesize and evaluate behavior therapy with no demand necessarily of any formal product. They included W. Stewart Agras, Nathan Azrin, Walter Mischel, Jack Rachman, and G. Terence Wilson. Weekly meetings during the year and endless discussions led to novel ideas, enduring friendships, and for me a few books and articles.

The time at the center was the best and most intellectually stimulating year of my career. The daily regimen included a little work in a private office, long lunches, followed by long volleyball games, a quick shower, some iced tea afterwards (if no sherry hour had been scheduled), some miscellaneous conversations with others in residence, then a quick peek at my mail, and — my God, it was getting close to the time go home — I better wrap up. Nevertheless, this was my most productive year (e.g., four books and a handful of journal articles), even though it was interrupted with monthly trips to Penn State to see if my grant research was still going on and if my office had been turned into a storage closet. The work with colleagues at the center helped consolidate my interest in shifting my research to work with clinical dysfunction and patient populations. A few years after my time at the center, I moved to my job at the University of Pittsburgh School of Medicine.

John D. and Catherine T. MacArthur Foundation Research Network on Psychopathology and Development (1994–98). A truly transformative experience was participation in this network (chaired by David Kupfer). The group included leaders from several disciplines: Ronald Kessler (sociology, epidemiology), Charles Nelson (neurobiology), Kathleen Merikangas (genetics and epidemiology), Ellen Frank, Jerome Kagan, Laurence Steinberg (psychology), Helena Kraemer (biostatistics and behavioral sciences), Richard Harrington, Peter Jensen, Dan Offord (child psychiatry), Tomas Boyce (pediatrics), Marilyn Essex (sociology/psychiatry), and others over time. The group met multiple times a year, each time with a few all-day meetings, discussion, and agenda. The overall goal was to advance understanding and develop novel assessments of child psychopathology evident in the early years of life. Critical issues were discussed and debated, and actual advances were made in conceptualizing contexts and their influences on development, new ways to look at mediation and moderation, clarification of risk and how to evaluate risk, and trade-offs in using various prevention strategies for childhood psychopathology, among other topics. Collaborations of members in various combinations produced scores of papers that would not have emerged without the intense and prolonged interactions. The influence on my work went well beyond assorted publications. The network provided intensive educational experiences on content, methodologies, and conceptual views of diverse disciplines and enduring collaborations. As I write this, two decades after that group dissolved, I continue some of the collaborative relations, and my work has changed for the better.

Medical School Jobs

Approximately one-third of my career has been working in psychiatry in medical schools, including ten years at the University of Pittsburgh and four years at Yale. At Yale, I was asked (by the university president and the dean of the School of Medicine) to serve as department chair and director of the Child Study Center, an interdisciplinary department of mostly child psychiatrists but with other disciplines and professions (e.g., neuroscience, genetics, education, law, social policy) as well. With this position also came serving as director of Child Psychiatric Services at Yale New Haven Hospital. The position required very frequent interactions with chairs of other medical school departments: formal and informal meetings, lunches, dinners, and the like. This was an intellectual and, through new friendships, a personal high and brought many special educational experiences (e.g., interacting with PhRMA [Pharmaceutical Research and Manufacturers of America], watching different types of surgery while being at the surgeon's side, understanding hospital administration and safety practices, collaborating with a few other chairs to craft medical school faculty ethics guidelines). Also, as part of the hospital leadership, larger pictures (health care in general, health care disparities, reimbursement, postmortem evaluations, psychiatric crises in the emergency room) placed my own work in a much better context. My work—directed toward evidence-based treatments—is a puzzle piece of providing services, but the puzzle is huge and requires broader understanding of where and how to exert impact. (My professional goal had always been to be a small fish in a large pond. I could see now that this had been achieved.) Seeing the larger picture planted seeds for later changes in my career. The piece of improved health care on which I had been working might not have the impact on helping people that I had envisioned. I left that thought and returned to my regular job in the psychology department. Throughout it all, I had continued my funded research at my parenting center on the Yale campus.

Work with Professional Organizations

From early in my career to the present, I have been involved in various offices, boards, and committees of professional organizations including the American Psychological Association (APA), Association for Behavioral and Cognitive Therapies (ABCT), Society for the Experimental Analysis of Behavior, and Association for Psychological Science. Often my activities related to journal editorships and the committees they required, but for APA and ABCT I was fortunate to serve as president.

The APA is the largest organization of psychologists in the world. The presidency (2008) is a three-year period (elect, current, and past). This was an excellent experience on all counts but certainly in getting a perspective on the many areas of psychology and psychology from an international perspective. The job entailed many activities including conveying psychology's interests directly to members of the U.S. Congress, meeting with leaders of other scientific organizations and disciplines, helping to craft position statements on a variety of topics, and addressing crises and legal issues, all competently spearheaded by senior executives in the organization.

The APA president has access to funds to address themes and priorities. I elected three themes: Interpersonal Violence, Anxiety and Trauma in Children, and Psychological Science's Contributions to the Grand Challenges of Society. The first theme included the development of an international organization (National Partnership to End Interpersonal Violence; http://www.npeiv.org) under the remarkable leadership of Robert Geffner and Jacquelyn White, a new APA journal (*Psychology of Violence*), and a two-volume set of books that brought together multiple disciplines to identify priorities for research and services. The second theme, on anxiety and trauma in children, mounted a special task force (chaired by Annette La Greca) with a charge to summarize and disseminate what is known and what treatments were available, and to develop and circulate user-friendly materials for the public, caregivers, policy makers, and agencies serving children and adolescents.

My final theme on grand challenges began by convening a conference of scientists from diverse areas of psychology to prioritize and identify three challenges that would serve as a focus. Those adopted included climate change and promoting a sustainable environment, prolonging vitality in aging, and delivering health services to diverse populations. Booklets were developed and circulated (e.g., among policy makers, the public, students, and teachers) to bring the science together in readable formats for diverse audiences and to circulate these (e.g., to U.S. Congress). I used my presidential address to convey how psychology might contribute to countering climate change. Each of the themes included meetings of various groups, think tanks, and programming in the annual convention and drew on the resources and collaborations of various organizational units within APA.

Editing

For most of my career, I have been editing something, usually a scientific journal. This began early in my career with two associate editorships (*Behavior Therapy,* 1975–78; *Journal of Applied Behavior Analysis,* 1975–76) and moved to editorships. By now I have been fortunate to have served as editor-in-chief of six journals (*Behavior Therapy,* 1979–83; *Journal of Consulting and Clinical Psychology,* 1985–90; *Psychological Assessment,* 1989–91; *Clinical Psychology: Science and Practice,* 1994–98; *Current Directions in Psychological* Science (1999–2004); and *Clinical Psychological Science,* 2012–17). As editor, one reads on topics one would never look at otherwise, sees new methods and measures, new theories—that part is richly rewarding. Editing can influence research and researchers, and in my own career, remarkable (but not always gentle) reviews of my papers improved subsequent studies and even generated ideas for novel studies I would not have otherwise pursued.

I have served as editor of four book series. First, Benjamin Lahey and I coedited an annual series called Advances in Clinical Child Psychology (1977–92). Each year we invited a diverse set of authors to write on research topics. The series produced fourteen volumes, after which we turned this over to others. Second, I edited a series entitled Developmental Clinical Psychology and Psychiatry for Sage Publications (1983–99). This series produced forty-five individually authored books on diverse topics of child and adolescent psychopathology. Third, I edited a series, Current Perspectives in Psychology, for Yale University Press, 2000–2008. I was chair of the Publications Committee at Yale Press and a member of the board, and it became clear that the psychology collection was weak. I was asked to build a small series on topics and invite authors to prepare books; thirteen individually authored books were published before the series ended. Finally, I had a brief three-year run (2004–7) as a coeditor (with

Susan Fiske and Daniel Schacter) of the *Annual Review of Psychology,* a privilege and yet another opportunity to try to grasp the scope of our science. I resigned to go on to other tasks.

I have edited a few books of my own, one of which warrants mention. I edited the *Encyclopedia of Psychology,* a joint project of the APA and Oxford University Press. This introduced a qualitatively different set of challenges from the usual edited book and involved layers of editors and contributors throughout the world. The project involved conceptualizing the scope of psychology and how psychology connects with much of the rest of science. And the goal was to fully represent a widely diverse field that spans diverse topics (e.g., psychology and sports, the military, law, industry), multiple levels of analyses (molecules, families, social networks, and culture), and endless connections with other disciplines (e.g., medicine and health, education, psychiatry). This was a seven-year, eight-volume project and the longest book project on which I have worked.

Grant Funding

Most of the work I have done has required large research grants in light of the costs of running a clinical service, providing treatment, retaining a stable cadre of trained staff, obtaining time-consuming assessments, and retaining clinic families. Support has been primarily from the National Institute of Mental Health (NIMH) through various grant mechanisms (e.g., Research Project Grants [R01], Research Scientist Development and Research Scientist Awards, and MERIT [Method to Extend Research in Time] Awards) spanning over thirty-five years as a principal investigator (PI). The challenges of procuring such funding have increased over the years. Funding agencies themselves are under pressure. For example, in mid-career I was told not to apply to renew one line of federal grants for my work because the funds were going to be used for young investigators. Later, pressure increased to overcome the lack of diversity among those who were funded. And throughout all of this, competition has increased and the proportion of applications that are funded has decreased. I was fortunate in my medical and nonmedical school positions that my salary never depended on grants, but that was a minor consolation. The funds of all my staff, their positions, and my research program were completely dependent on funding.

Although my primary work was with children with aggressive and antisocial behavior, my grants outside of this domain provided opportunities to evaluate the impact of psychiatric hospitalization on children, the effectiveness of treatment for children in clinical practice settings, characteristics of child and adolescent therapy research, characteristics of clinical practice in the United States, animal-assisted therapies, and social robotics. Sources of funds came from the Blue Guitar Foundation, John D. and Catherine T. MacArthur Foundation, Laura J. Niles Foundation, Leon Lowenstein Foundation, Morris Animal Foundation, Human-Animal Bond Research Initiative Foundation, Rivendell Foundation of America, the Robert Wood Johnson Foundation, the William T. Grant Foundation, and the Jack Parker Corporation.

In Connecticut where I reside, funds were provided by the State of Connecticut, Department of Social Services and Community Foundation of New Haven, and Yale University. Usually one does not learn in graduate school how pivotal funding might be and the ins and outs of the full process of grant application from letter of intent to antidepressant medication (when the full proposal is rejected after months of waiting). Almost none of my work could have been completed without grants, and I am extremely grateful for that support.

Collaborations

I end this section with collaboration because of its pervasive role and impact in all that I have stated to this point. I learned as a teenager in competitive sports that one should practice and play regularly with individuals who are much better—vastly better is even more desirable. I stumbled on this principle accidentally, but the lesson was stark and guided me in academic life as well. Collaboration is great for all sorts of reasons, but collaborate with people who are much better, smarter, and more knowledgeable and skilled. (But as one of my high school teachers emphasized, in my case especially, finding more competent people intellectually would be very easy—this did not sound like a compliment at the time, nor even now.)

I cannot list all the collaborations. For the sample listed here and as an arbitrary cutoff, I include individuals with whom I have worked for many years and/or shared multiple publications. Each added brilliance and sparkle to any contribution I made in our joint work and corroborated the high school lesson I mentioned previously. Among the collaborators, I have been privileged to work with W. Stewart Agras, Alan S. Bellack, Richard Bootzin, W. Edward Craighead, Susan Fiske, Richard Harrington, Donald Hartmann, Michel Hersen, Peter Jensen, Jerome Kagan, Ronald Kessler, David Kolko, Mary Koss, Helena Kraemer, David Kupfer, Benjamin Lahey, Cynthia Last, Michael Mahoney, Johnny Matson, Dan Offord, Theodore Petti, Jack Rachman, Carlo Rotella, John Weisz, Jacquelyn White, and G. Terence Wilson. Some of the collaborators turned out to be students and have moved to very successful careers, including Stacey Blase, Mark Celano, Andres De Los Reyes, Benjamin Johnson, Russell T. Jones, Paul Marciano, Matthew Nock, Francheska Perepletchikova, Sarah Rabbitt, Rebecca Schrag Hershberg, and Linda Wilcoxon Craighead; or members of my clinical research team (e.g., Debra Bass [Colbus], Erin Carrubba, Michael Crowley, Karen Esveldt-Dawson, Bernadette Lecza, Jennifer Mazurick, Molly McDonald, Antoinette Rodgers, Rosanna Sherick, Todd Siegel, Gloria Wassell, Moira Whitley, and many more).

In contemporary research, more work now has to be collaborative because of complex assessments that require outside-of-discipline expertise (e.g., neuroimaging, optogenetics), novel and esoteric statistics (e.g., dynamic statistics, finite mixture modeling), and emerging situations and opportunities (e.g., "big data"). There is great value in collaborating even when the research does not require it. The mix of views can enrich or change a conceptual model beyond what one would accomplish on one's own.

Shifting the Focus to a Broader Problem

My Work in Context

My main line of research achieved the original goals my group had intended, namely, to develop evidence-based interventions for children and adolescents with conduct problems. My work fits into a larger body of research with investigators of my cohort who have developed effective treatments for a variety of psychiatric disorders. Treatments for depression and anxiety are among the most well studied, but the interventions for diverse problems (e.g., eating disorders, substance abuse disorders, post-traumatic stress, schizophrenia, bipolar disorder, and many others) have advanced remarkably. With an abundance of evidence-based interventions, perhaps there is much to celebrate. Yet the interventions are not widely adopted or even routinely included in the training of mental health professionals. Even more than that, there is a broader problem.

While treatment research has advanced, so has work on psychiatric epidemiology and the description of the rates and distribution of mental illness in the community. For example, we now know that 26 percent of the U.S. population met criteria for at least one psychiatric disorder within the past twelve months. This increases to 46 percent of the population over the course of life. For ease of computation, consider that approximately 25 percent of the U.S. population experiences a psychiatric disorder during a given year and 50 percent during their lifetime. From a U.S. population of approximately 326 million, this translates to more than 80 million and 160 million people, respectively. Important to add is that the estimates (percentages) are conservative because some disorders (e.g., schizophrenia), subsyndromal (subclinical) disorders, and institutionalized populations with disorders often are omitted from the surveys.

Separate lines of research have addressed the extent to which individuals in need of services receive them. In relation to adults, in the United States, approximately 70 percent of individuals in need of services do not receive any services. Ethnic minority groups (e.g., African, Hispanic, and Native Americans) have even less access to care than do European Americans.

From work on prevalence and treatment, we know now that there is a huge treatment gap. The treatment gap refers to the difference in the proportion of people who have disorders (prevalence) and the proportion of those individuals who receive care. The treatment gap is not unique to the United States. The World Health Organization (WHO) provided extensive data on the treatment gap from surveys of over 60,000 adults in fourteen countries in the Americas, Europe, Middle East, Africa, and Asia. Receiving services consisted of any contact (even just one) with a health professional (e.g., psychiatrist, psychologist), general medical or other professionals (e.g., general practitioner, occupational therapist), religious counselors (e.g., minister, sheikh), and traditional healers (e.g., herbalist, spiritualist). The proportion of respondents who received treatment for emotional or substance-use disorders during the previous twelve months ranged from a low of 0.8 percent (Nigeria) to a high of 15.3 percent (United States). These numbers convey that the vast majority of individuals in need of services (99.2 percent and 84.7 percent, respectively, by subtracting the above percentages from 100 percent) did not receive treatment. A useful summary of the situation in the United States and worldwide is that the most commonly received intervention for mental disorders is nothing, that is, no treatment.

There are all sorts of barriers to receiving professional care (e.g., stigma, lack of reimbursement for services, mental health illiteracy). Yet a key one is the dominant model of delivering psychosocial interventions, and this applies to evidence- and non-evidence-based interventions. Most psychosocial interventions for children, adolescents, and adults are delivered with a model that has three interrelated characteristics:

1. treatment sessions are provided in person and one-to-one with a client (child, couple, family);
2. treatment is administered by a highly trained (e.g., master's or doctoral level) mental health professional; and
3. sessions are held at a clinic, private office, or health care facility.

These characteristics inherently limit the scalability of treatment and the reach to special groups especially unlikely to receive care.

Consider just one facet of the model, namely, the model's reliance on mental health professionals. In the United States, for example, there are too few trained mental health professionals. They are heavily concentrated in urban areas rather than in many areas with few or no services, and are infrequently trained to accommodate many populations for which there is great need (e.g., children, adolescents, older adults). Also, the ethnic and cultural diversity of mental health professionals does not come close to matching the diversity of the population in need of treatment, and this has direct implications for clients seeking, remaining in, and profiting from treatment. In relation to treatment for psychological dysfunction, the situation of too few professionals in relation to the need is projected to become much worse through 2025.

From Interventions to Models of Delivery

If most people in need receive nothing, merely designing more effective treatments that cannot be delivered is of little use, outside of tests of principle or of procedures that might one day be delivered in novel ways. Consider for a moment that a single pill was devised that was effective for the treatment of multiple cancers. This would be a marvelous scientific breakthrough. But consider for a moment that the pill was the size of a beach ball and had to be swallowed whole. An "effective" treatment in this case requires a way of being delivered that in fact could be utilized by people in need. We need effective interventions as a first step, but also ways of ensuring they can be delivered. And so it is with evidence-based psychotherapies. To the vast majority of people in need, they are beach balls, not consumable, and hence of little use in their lifetimes. I find that hard to swallow so am now out of the treatment development and evaluation business.

My own shift in focus now is on models of delivery, that is, how the interventions are provided that can be scaled to reach individuals in need and to demonstrate impact. There are several such models with names unfamiliar within psychology; they include task shifting, best-buy interventions, disruptive innovations, Entertainment-Education, social networking, and others. These models draw on many different disciplines, including public health, business, entertainment, economics, robotics and artificial intelligence, and law and social policy. Their characteristics permit scaling interventions to reach large numbers of individuals in need, to reach many special groups least likely to receive services, and in varying degrees to sidestep some of the barriers (e.g., stigma, cost, lack of mental health professionals) that contribute to the treatment gap. Moreover, some of the models draw on the now extensive evidence that lay individuals can be trained to administer evidence-based treatments and are just as effective in administering psychological treatments to patients with significant mental disorders as are mental health professionals.

My intervention research began with the naive view that our main if not sole task was developing effective treatments for serious mental disorders and related psychological slings and arrows of life whether or not they rose to the level of a disorder. To be sure, we do need evidence-based treatments. And, within clinical psychology and psychiatry, the flow of RCTs of such treatments continues to gush from our best journals on a monthly or bimonthly basis. Yet now that we are armed with many evidence-based treatments, we must look to whether they are reaching people, whether they are having impact, and whether they reduce the burdens of mental illness. Currently, it is a very small and elite group that receives individual therapy, evidence-based or not. The problem is not with the interventions per se but rather the manner in which they are delivered, namely, the dominant model to which I referred.

Our research now must look at scalability (improving the number and proportion of individuals who receive treatment) and reach (connecting with special populations that are routinely neglected.) Currently, we have the means (models of delivery) that could have enormous impact on reducing the treatment gap. Invariably, there is more to know, and we always want better and more science. Yet it is important, at least for me, to keep in mind that the most common intervention for mental disorders among those people in need the United States and worldwide is no treatment, i.e., nothing formal or informal from any health professional or healer. It does not need to be that way, based on what we know. Perhaps understandably, even if rather delayed, I am shifting my own priorities to ways of reaching people in need of services and practices that can be scaled. At the end of my career, I have closed my labs (two on campus) to shift emphasis to what I consider to be much more important than the work I have been doing.

The research and career I have highlighted perhaps can be characterized by the dialectic process described by the German philosopher Georg Wilhelm Friedrich Hegel (1770–1831). This process includes the familiar three stages consisting of thesis, antithesis, and synthesis. Developing and establishing evidence-based interventions might be considered the thesis or beginning stage for me. I thought these treatments would automatically be adopted and help many people. In light of the treatment gap I have mentioned, my work moved to the antithesis stage. This stage, a bit too extreme, but where I am now, reflects my view that in principle and practice, evidence-based treatments cannot have much impact. The dominant method of establishing and evaluating evidence-based treatments (meta-analyses) is of little help in deciding what really has palpable impact on anyone. And, the dominant model of treatment delivery precludes reaching the masses in need of services. On the horizon, I see use of novel models of delivery as a synthesis stage that combines or integrates the truths of both prior stages. That is, we need evidence-based treatments but with empirically established assurances that they can be delivered, reach people in need, and have impact in ways that reduce the individual, family, and social burdens of mental disorders. Of course, the issues are not about "me" and what I see but rather about a neglected crisis. In the wings in the United States (and worldwide), there are desperate individuals and their families who will receive no treatment for their mental disorders. That is antithetical to the goals we treatment researchers probably began with as we started our programs of research.

Closing Comment

I have focused on highlighting my professional work and main lines of research. I have refrained from mentioning my personal life, or even my impersonal life now that I think of it. That is a style preference. I have kept personal and professional private as a matter of lifestyle and do so here with a major exception. As I write this, my fiftieth book has just been published, on my career change and new emphasis I highlighted previously. In the wings is a massive support system that made it all possible. It is fitting to end by noting that none of my work would have happened without good fortune and a remarkable family. Joann (diDonato) Kazdin and our daughters (Nicole and Michelle) provided so many enabling conditions, and my gratitude is at once deeply felt and, as they well know, beyond words. My older sister, Frances, has guided and helped my life in so many ways and deserves much credit for anything I have accomplished. Much of academia focuses on the impact one has on others (e.g., students, other researchers). A personal note is a useful place to end as a way of underscoring the impact that others both in personal and professional spheres have had on me. I have a special kind of gratitude for that.

Notes

1 For the purposes and format of this publication, I have omitted all citations to my work and the work of others. The findings from my research, journal articles and books, and citations to the works of others to support claims (e.g., about the treatment gap, effectiveness of interventions) are available in full detail in other publications that provide more in-depth treatment of these topics. Three sources in particular provide such documentation: Alan E. Kazdin, “Clinical Dysfunction and Psychosocial Interventions: The Interplay of Research, Methods, and Conceptualization of Challenges,” *Annual Review of Clinical Psychology* 11 (2015): 25–52; “Developing Treatments for Antisocial Behavior among Children: Controlled Trials and Uncontrolled Tribulations,” *Perspectives on Psychological Science* 13 (2018): 634–50; and *Innovations in Psychosocial Interventions and Their Delivery: Leveraging Cutting-Edge Science to Improve the World’s Mental Health* (New York: Oxford University Press, 2018).