

Clinical Dysfunction and Psychosocial Interventions: The Interplay of Research, Methods, and Conceptualization of Challenges



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A handwritten signature in blue ink that reads "Alan E. Kazdin". The signature is written in a cursive style.

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Abstract

I describe the development and course of my research in studying clinical dysfunction among children, adolescents, and adults. This is an autobiographical account that highlights programs of research, career moves, and experiences along the way that were particularly influential.

Research on specific topics and the methods to study them were inherently fascinating but invariably led me to broader issues well beyond what I was studying. The research alerted me to how and why current methods, assumptions, and research practices might be constraining and perhaps slightly misguided. My research and specific findings in a given area were not necessarily part of any particular breakthrough but rather helped me see how more, different, and better work was needed. Collaborations with a diverse set of colleagues and models from other disciplines than psychology helped me conceptualize the goals of research on a given topic (e.g., developing evidence-based treatments, reducing the burden of mental illness, promoting a sustainable environment to mitigate climate change) and propose a shift from current practices as a means to obtain them.

INTRODUCTION

It is an honor to have this opportunity to discuss my work. In this article, I highlight key themes, provide samples of my research, and convey the larger issues to which these have led. Although I emphasize research, in keeping with the priorities of our science, I highlight my nonempirical writings as well. Research has led me to think about methodology, challenges, and conceptual issues that relate generally to areas in which I am working but go well beyond the narrow focus of my own studies. I also highlight a few critical career experiences, collaborations, and other influences that shaped my thinking and priorities. The article is autobiographical and selective in what is presented. As psychologists, we specialize in understanding memory in its various forms. Consequently, I happily concede at the outset that the overarching themes in this article, the orderly sequence and accretion of research and findings, and endless stream of putative insights that surmount Himalayan heights might be cognitive constructions or what would be called in literature, fiction.

RESEARCH THEMES AND PROGRAMS

A few themes characterize my work. I count as a theme any area that includes two or more programs or lines of research, i.e., multiple studies on a topic that have spanned many years.

Intervention and Behavior Change

Intervention research has dominated my work and is illustrated by four programs of research. First, much of my work has consisted of treatment outcome studies and has included children, adolescents, and adults. Work with adults focused on imagery-based treatment (covert modeling or imagined rehearsal) to develop social skills and reduce anxiety. Work with children and adolescents focused on interventions derived from applied behavior analysis and cognitive-behavioral treatment to reduce severe aggressive and antisocial behavior.

Second, I have been interested in moderators and treatment processes. Our group has focused on current and past child, parent, family, and contextual factors that influence therapeutic change in the treatment of children. Also, we have looked at processes that emerge during treatment (e.g., parent-therapist and child-therapist alliances, parent perceptions of treatment), mediators of these processes, and how to explain therapeutic change.

Third, we have studied participation in treatment including who drops out of treatment and why, who cancels and does not show up for treatment sessions, and who among dropouts benefits from treatment. Our work began as crassly empirical by merely identifying predictors until we developed and tested a model of why individuals drop out, as noted later.

Finally, we have conducted studies on treatment acceptability or the extent to which treatment techniques seem reasonable and appropriate for the clinical problems to which they are applied. Acceptability of treatment procedures (among clients, parents, teachers, therapists) has direct implications for remaining in and adhering to treatment among clients and recommending and using treatment among professionals. We have evaluated the acceptability of different treatments and determinants and predictors of acceptability.

Clinical Dysfunction and Its Characteristics

Evaluation of child and adolescent clinical dysfunction is another theme and encompasses three research programs. First, we have conducted studies of childhood depression and focused on characterizing the disorder and its core features among children referred for inpatient hospitalization. This work has included how to define and diagnose depression in children, cognitive (e.g., hopelessness) and emotional (e.g., limited experience of pleasure or anhedonia) processes, overt behavior during everyday and laboratory situations, and predictors of suicidal

ideation and attempt. Second, our work with conduct disorder examined different symptom patterns (covert and overt; reactive and proactive) and salient symptoms (match play and firesetting) and characteristics associated with them. The disorder, as designated in current psychiatric diagnosis, is heterogeneous, and breaking this down in various ways (e.g., subtypes, salient symptoms) was an effort to move to a better understanding of its features. Finally, through multiple ongoing collaborations I have been fortunate to work with diverse clinical populations and individuals with various challenges or disabilities. These collaborations have included research on child anxiety disorders and psychological functioning and socialization among adults with intellectual disabilities or visual impairment.

Assessment

I have worked on measures and measurement and measurement issues as part of my research. Two lines of work can be delineated. The first area has been in devising measures and has occurred in the narrow context of wanting to address a specific question and for which no measure or suitable measure was available. We developed the:

1. Hopelessness Scale for Children—to measure pessimism and risk for suicidality;
2. Interview for Antisocial Behavior—to measure a full range of symptoms of conduct disorder, with particular delineation to overt and covert behaviors;
3. Children's Hostility-Guilt Inventory—to distinguish overt actions from grudges and internal anger;
4. Barriers to Participation in Treatment Scale—to test a model of why people drop out of treatment;
5. Treatment Acceptability Inventory—to measure the extent to which therapists, parents, and children consider a given intervention reasonable or appropriate;
6. Management of Children's Behavior Scale—to assess child-rearing practices, with particular emphasis on punishment;
7. Children's Pleasure Scale—to measure anhedonia or the experience of pleasure among children; and
8. Parent Expectancies for Child Therapy Scale—to measure the extent to which parent expectancies for treatment corresponded to the actual demands of treatment.

We completed a basic study or two to prune items and evaluate reliability and validity and then went on to address the original questions we had in mind. More formal and extensive scale development (e.g., normative work, special scale analyses) was not part of the agenda and was not completed. Even so, others have used some of the measures (e.g., items #1 through #5) well beyond our research program.

A second area of assessment work focused on correspondence among measures designed to assess the same construct. This work included examining child dysfunction as evaluated by children, parents, teachers, and clinicians and the factors that relate to agreement. In addition, we have looked at the implications of poor interrater agreement for diagnostic issues and have examined cross-modality assessment relations (e.g., ratings, overt behavior in naturalistic and lab situations).

CHRONOLOGY: RESEARCH AND CAREER DEVELOPMENT

Below I illustrate the programs of research as they emerged in my career and provide some of the specific findings to convey more concretely what we have actually done. **Table 1** summarizes my education and academic appointments and could obviate the need to plod chronologically through my work. Yet, there are consistencies, inconsistencies, and building that all occurred in a timeline, and this is an opportunity to test the reader's stamina to slog through that.

Table 1 The author's education, academic history, and primary appointments at a glance

University	Years	Area of study/department	Position/primary appointment
San Jose' State University	1963–1967	Psychology/philosophy	Undergraduate
Northwestern University	1968–1970	Experimental psychopathology/ clinical psychology	Graduate student
	1970–1971	Psychology	Assistant professor
Pennsylvania State University	1971–1980	Psychology	Assistant to full professor
University of Pittsburgh School of Medicine	1980–1989	Psychiatry	Professor of Child Psychiatry
Yale University	1989–present	Psychology	Professor of Psychology and Child Psychiatry
	2000–present	Psychology	John M. Musser Professor
Yale University School of Medicine	2002–2006	Child psychiatry (Child Study Center) Yale-New Haven Hospital	Director and Chair Director, Child Psychiatric Services

As I mentioned, studies I have completed contributed to my nonempirical works that have attempted to raise issues and set the agenda in a given area. By studying a given topic over time and by using standard practices (e.g., experimental designs, treatments, data evaluation),

problems and issues that I viewed as critical for psychological research more generally emerged. The identification of these difficulties occurred in the context of various time periods and positions associated with them. For that reason, the tracing of my work chronologically extends beyond mere stylistic convenience.

Pre-Career Years: Graduate School at Northwestern University (1967–1970)

Background. Graduate school included the expected coursework, clinical work, and research experiences, but the unique feature was the great emphasis on methodology. Where students in current doctoral programs might get a lecture, chapter, or unit (e.g., quasi-experimental designs, factor analysis), we had courses. 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.” The rationale was that clinical research (clinic settings, patient samples) can be methodologically messy (e.g., missing data, dropouts, nonexperimental designs). Clinical faculty told us that knowing a broad range of methodological (design, statistical) options was especially important. 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. My undergraduate studies were in philosophy and psychology. 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 actually 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 (research designs, measures, data analyses) can be a moderator of the findings (for examples, see Kazdin 2015b). 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.

Transformative experience. Twice in my career, I have been in a position where there is a need to change the behavior of children or adults who are not functioning well in everyday life; they have difficulties in getting along with others, meeting the demands of school or work, and managing to stay out of trouble. The first occasion, the one that is relevant here, occurred in graduate school when I took a part-time job at a facility for children, adolescents, and adults with a range of emotional and behavioral problems and intellectual disabilities. At the job interview, the director (Stanley Cabanski), a clinical psychologist, told me the job would require me to intervene to make therapeutic changes that would help individual clients, beginning with the adolescents and adults in the sheltered workshop part of the facility who might be placed in jobs

in the community. I weepily confessed and threw myself on the mercy of the court by admitting I knew nothing (possibly even less) about the clientele, let alone how to have any influence on them. Even so, he invited me to take the job and sit in an office (I had an office!) for as many weeks or months as it took to learn what I needed and, when ready, to try to change behavior.

Many promising interventions were emerging in behavior therapy. I wrote to key leaders at the time, read their articles, and began to learn the budding literature in applied behavior analysis, i.e., the application of operant conditioning to therapeutic and other applied ends. After a few months, I made some modest efforts to intervene and evaluate programs with individual clients. A superintendent of schools learned of this work and invited me to implement and evaluate similar interventions on a larger scale in several school classrooms, an experiment that turned out to be my dissertation. In short, the part-time job guided me to and then down a path to develop and study interventions. With enormous pride (because of the continuity of this interest now spanning decades) and an equal amount of embarrassment (because I may be a one-trick pony), I am still on that path.

I “accidentally” completed my PhD much sooner than expected (August of one year rather than June of the next; see **Table 1**). 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 (for three years total) and was able to start writing up some of the findings from that work (e.g., Kazdin 1973, Kazdin & Polster 1973) as well as reviews of interventions I had used (e.g., Kazdin 1972, Kazdin & Bootzin 1972).

Early Career: Pennsylvania State University (1971–1980)

I accepted a position in the department of psychology at Penn State. After some start-up time and mini cul-de-sacs in my investigations, I began three programs of research. First, I began to study an imagery-based therapy (covert modeling) for the treatment of adults; I focused on two problem domains, social withdrawal and 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 behavioral therapy techniques referred to as covert conditioning. 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 the 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). All of 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 (e.g., Hersen et al. 1979; Kazdin 1975, 1979b; Kazdin & Mascitelli 1982)¹. As a sample of findings:

- Imagined rehearsal (covert modeling) was effective with each of the problem domains in which we worked;
- Many facets of overt modeling [e.g., use of multiple models, reinforcement of model behavior, use of homework (in vivo) practice] behaved similarly and enhanced the effects of covert modeling;
- Novel features of covert modeling (e.g., elaboration of imagery content, verbal coding of imagined material) also could be used to improve treatment outcome; and
- Overt modeling and covert modeling were equally effective in changing behavior among non clinically referred samples and hospitalized psychiatric patients.

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. At Penn State, I had access to a laboratory school for elementary school students with moderate-to-mild intellectual disabilities. Our 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 included features (e.g., a mirrored back wall behind which multiple observers could code the behavior of individuals or the entire class) that were conducive to research. Our studies relied on single-case experimental designs (e.g., ABAB, multiple baseline, simultaneous treatment), focused on small numbers of individuals, and were based on direct observation of overt behaviors as the key dependent variable. We completed several studies that evaluated vicarious reinforcement, discriminative stimuli, nonverbal reinforcement, ways of improving the effectiveness of token economies, integrating peers into reinforcement contingencies, and maintaining behavior after programs are ended (e.g., Jones &

¹ Throughout this article, I provide only a selection of the studies for a given research program, yet the findings that are summarized draw from the full set of studies.

Kazdin 1975, Kazdin 1977b, Kazdin & Geesey 1977, Kazdin & Mascitelli 1980, Kazdin et al. 1975). As a sample of key findings:

- The effectiveness of token reinforcement could be improved by altering several facets of the contingencies, such as sharing reinforcers with one's peers and being able to wean oneself off token reinforcement contingencies;
- Providing reinforcement (praise) to one individual in a classroom leads to improvement in the behavior of adjacent individuals as well (vicarious reinforcement), and this is primarily due to the cueing (discriminative stimulus) function of the reinforcement rather than modeling (imitation) effects;
- Nonverbal teacher approval makes its own contribution to behavior change (e.g., beyond verbal praise) in the classroom; and
- Reinforcement effects can be maintained after a behavior change program is ended by gradually fading the contingencies and moving to increasingly intermittent schedules.

This research was helpful in programs used later in my own work (parent management training) by elaborating the effects of antecedents and consequences, identifying alternatives to punishment to reduce or eliminate behavior, and determining how to maintain behavior after an intervention program was terminated.

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 (e.g., Kazdin 1977a, Kazdin & Wilson 1978a, Wolf 1978). One dimension was the extent to which non professionals, laypersons, and other potential consumers of treatment viewed 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 find the interventions acceptable and agree to participate in them.

We initiated 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 (e.g., Kazdin 1980, 1986a; Kazdin et al. 1981a). We evaluated different treatments (e.g., psychotherapy, medication, hospitalization, time-out from reinforcement) and treatment characteristics (e.g., restrictiveness, effectiveness, side effects) that might influence acceptability. Among the main findings:

- 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);
- Acceptability of a given treatment bears little or no relation to how effective the treatment is when effectiveness is manipulated experimentally in a lab paradigm or when observed naturalistically as part of a clinical trial;
- Children and parents view treatments differently. Stronger and more restrictive treatments are viewed more positively by parents and more negatively by children;
- How a treatment is presented to the child (giving the child a participatory role in selecting the treatment) increases acceptability; and
- For a given treatment that families actually receive, barriers that parents experience influence how acceptable they find that treatment to be.

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

Mid-Career: University of Pittsburgh School of Medicine (1980–1989)

By the late 1970s, my research programs were in place and my collaborators, students, and resources were excellent. Yet 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), which is the psychiatry department at the University of Pittsburgh School of Medicine. I began annual job interviews because I was unsure of the shift in lifestyle from a mellow academic psychology department to the intense pace of a medical school department. Finally I took a leave of absence from Penn State to try the job at WPIC and after a year decided to remain in Pittsburgh.

I was given the luxury to float for a few months to decide what I wanted to do and then to let the administration (Department Chair Thomas Detre and Research Director David Kupfer) know. After a couple of months I mentioned I would like to do research on the children's inpatient service. Literally overnight I was placed in charge of the Children's Psychiatric Intensive Care Service, a locked ward inpatient service for youth 5–12 years of age. The rationale was that to do the work I wanted required complete authority over how the operation was run. This was not at all what I wanted. Appointment of a psychologist to administer over

medical and nursing staff, teachers, ward staff, and others was rare in a medical school (to say the least) and I felt that such a position went completely against my better judgment, not to mention interest. In keeping with my already remarkable streak of being wrong in such matters, the appointment as program director turned out to be essential to mobilize the service to address research.

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 (Karen Esveldt-Dawson, Deborah Colbus, Todd Siegel, Antoinette Rodgers, and Roseann Sherick) and nursing and medical staff (Nancy French, Michael Rancurello, Chris Thomas, and Alan Unis). The first line of work focused on elaborating key features of childhood depression. The department had become a clinical research center for depression and represented 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 (e.g., $N > 200$ in some of our studies) 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 (e.g., Kazdin, 1989, 1990; Kazdin et al. 1983d, 1985a, 1986b). We also evaluated depression and its relation to early child experiences (e.g., child abuse) and to other disorders (e.g., anxiety disorders, developmental disabilities) (e.g., Kazdin et al. 1985b, Matson et al. 1983). As a sample of our results, we found that:

- Inpatient children could readily report on their depression;
- Children with suicidal ideation and attempt were high in depression and hopelessness and lower in self-esteem compared to nonsuicidal control patients;
- Depression in children was evident in direct observations of social behavior, solitary play, and affect-related facial expressions during free time and when depressed and nondepressed children were compared;
- Nonverbal behaviors (e.g., facial expressions, gestures, and body movements) could distinguish depressed and nondepressed children in a laboratory paradigm;
- Child anhedonia (reduced experience of pleasure) predicted clinical depression and was also related to a broader cognitive attributional style (e.g., predicting reward seeking and outcome expectations); and
- Separate criteria commonly used in research to delineate children as depressed yielded groups with relatively little overlap.

Findings critical to our work were not invariably replicated. The most salient instance is

that we found hopelessness to be a better predictor of suicidality than depression in one study but the opposite (depression was a better predictor and hopelessness did not contribute) in another study. We did not have a theoretical explanation or hypotheses about likely moderators that might explain the effect and hence did not seek to resolve this discrepancy in light of other priorities. Also, others began to study hopelessness in children and made much further progress using the measure we devised for our one or two studies.

The second program of research we initiated was a series of studies on conduct disorder and its treatment. This treatment focus derived almost from necessity and represented the second time (graduate school was the first) I felt there was a need to do something that made a difference in patient care. Most of the youth were referred to the service for severe aggressive and antisocial behavior (e.g., extensive fighting, property destruction, stealing, firesetting) and usually were unmanageable at home and at school. We tried many reasonable treatment options (e.g., various medications, social skills training, traditional individual therapy sessions with a therapist, and a structured milieu) and occasionally even allowed parents to try other options they viewed as reasonable (e.g., exorcism).

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 (Kazdin 2005a). Individual treatment sessions use 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. PMT had been used with children who engage in antisocial behavior in pioneering work by Gerald Patterson and his colleagues (1992). We expanded on the intervention procedures to apply them to a severely impaired population and drew on a broad range of behavior change techniques (e.g., elimination of behaviors through reinforcement; use of a range of antecedents; and use of simulations, response priming, shaping, and special ways of delivering praise), many of which I had been studying in my prior job. PMT was not a viable option for many children on the inpatient service because their 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 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 actually 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, and 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 in other settings.

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 (e.g., Kazdin et al. 1987a,b, 1989, 1992). Treatment studies with seriously impaired clinical samples, compared with those with adult volunteers (covert modeling), took much longer to mount (e.g., 12+ months to pilot treatment variations; three to six months of therapist training including supervision of “practice” cases; and pre, post, and repeated follow-up assessments for one to two years). Treatment began intensively on an inpatient basis 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.

As a sample of the findings, we established that PMT and PSST alone and eventually their combination:

- Reduced aggressive and antisocial behavior and increased prosocial behavior among children referred for inpatient or outpatient treatment;
- Yielded gains that surpassed the effects of other treatment conditions (e.g., treatment as usual, play and relationship therapy);
- Produced changes that were evident at home, at school, and in the community immediately after treatment and at one to two years of follow-up;
- Led to changes that were clinically significant, i.e., after treatment, the symptoms and social competence of many children fell within levels of normative functioning of same-sex and same-age peers from community samples; and
- 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. This included developing measures to evaluate covert (e.g., stealing, firesetting) versus overt (fighting) conduct problem symptoms and another to evaluate internal nonexpressed aggression (hostility, grudges) and acts of aggression. We also evaluated the ability of children with conduct disorder to control

and improve their prosocial behavior when asked to do so (e.g., Kazdin et al. 1981b, 1983b, 1987c).

Our work with conduct disorder and depression underscored the salience of an emerging topic, namely, the lack of correspondence of raters in evaluating childhood psychopathology, and that led to a separate line of studies. Evaluation of child symptoms and social adaptive functioning varied as a function of the source of information (e.g., mothers, fathers, teachers, and children). We conducted convergent and discriminant (multitrait-multimethod matrix) studies that consistently found poor interrater agreement (validity). Measures completed by the same rater correlated more highly with each other when rating different constructs than did measures completed by different raters when evaluating the same construct (e.g., Kazdin et al. 1983c,d). The evaluation of different raters on standardized checklists raised broader assessment issues we studied including the correspondence of checklist data, observed performance, and psychiatric diagnoses (Kazdin & Heidish 1984; Kazdin et al. 1983a, 1985a). We also gave some attention to child ratings of clinical dysfunction and how they evaluated different disorders and their causes, outcomes, and remedies (Kazdin et al. 1984).

WPIC was rich with opportunities for collaborative research. I became involved in research programs of others. A few of us were working on socialization and social skills but with different populations: Johnny Matson and Vince Senatore (children and adults with intellectual disabilities, respectively) and Vince Van Hasselt (children who were visually impaired). Our research intertwined and led to many collaborative studies (e.g., Senatore et al. 1982, Van Hasselt et al. 1983). David Kolko led another program of research on firesetting and match play among inpatient children on the unit I was directing. This led to several studies on the characteristics of individuals who engaged in firesetting or match play (e.g., Kolko & Kazdin 1988, Kolko et al. 1985). Finally, another program, directed by Cynthia Last, focused on child anxiety disorders. This led to several studies that delineated subtypes of anxiety among children, parent psychiatric dysfunction associated with child anxiety disorders, and the relation of child anxiety and social competence, including the prospective study of children (e.g., Last et al. 1991, 1996).

The 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 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 (e.g., electromyography, electroencephalography, medication blood levels).

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

After 10 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) 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.² The research agenda on the effects of treatment moved from emphasis on outcome to moderators and mediators of treatment outcome (e.g., Kazdin & Crowley 1997; Kazdin & Wassell 2000; Kazdin & Whitley 2006a,b). As a sample of findings:

- Among child predictors, greater severity and breadth of child psychiatric dysfunction (comorbidity or number of symptoms across multiple disorders), and academic dysfunction (e.g., academic delays, failing at school) predicted worse treatment outcomes;
- Among parent and family predictors, socioeconomic disadvantage, degree of parental stress, severity of parent psychopathology, adverse child-rearing practices, and parent experience of barriers to participation in treatment predicted worse treatment outcomes; Parent-perceived barriers to participation in treatment moderated outcome, but even parents with the highest barriers still profited from treatment; and
- In separate samples of oppositional defiant disorder and conduct disorder, degree of comorbidity was related to initial standing (more severe dysfunction) at pretreatment, but comorbidity did not relate to level of dysfunction by the end of treatment.

The effects were not always replicated. In some cases, the moderators predicted change in one context (child performance at home but not at school). In other cases, moderators (e.g., parent, family, and contextual factors) contributed to outcome in one study but not in another. Resolving inconsistencies is important to be sure, but the challenge is prioritizing what can be done in a given amount of time. We continued to focus on the more reliable predictors (e.g., comorbidity, barriers to participation in treatment) and moved to other questions before us.

One of those “questions” was actually a related line of work on the therapeutic alliance with its own set of studies. The therapeutic alliance had been investigated (thousands of studies) with adults but had received almost no attention in child therapy. The novel opportunity in our situation was to examine child-therapist (PSST) and parent-therapist (PMT) alliances and their

² Over time, parental demand for assistance with parenting and child rearing increased among nonclinical cases. Families wished help to handle the “normal” challenges of parenting (e.g., toilet training, getting children to eat vegetables, getting ready for school on time without coercion and nagging, altering teen “attitude”). We renamed the service the Yale Parenting Center to remove concerns about coming to a “clinic” among families that were otherwise doing fine. The service sees both severe aggressive and antisocial behaviors (conduct disorder) but also works with families to address other typical parenting challenges.

relation to outcome as well as to consider what pretreatment factors may contribute to the formation of a strong therapeutic alliance (e.g., Kazdin & Durbin 2012; Kazdin & Whitley 2006b; Kazdin et al. 2005, 2006).

As a sample of our results, we found that:

- Parent-therapist alliance predicts improved parenting practices at the end of PMT; the better the quality of the alliance, the greater the changes among the parents;
- Pretreatment social relations of the parent predict the quality of the parent-therapist alliance and partially explain (mediate) the alliance-improvement relationship;
- Child-therapist alliance also predicts improvements in child dysfunction over the course of treatment; and
- Beyond treatment outcome, both parent-therapist and child-therapist alliances predict parent perceived barriers to treatment and treatment acceptability; better alliances are associated with fewer perceived barriers and views that treatment is more acceptable.

Moderator and process work began to blur with our controlled trials of treatment outcome. For example, in one study we identified parental stress as a moderator of participation in treatment and common to both European American and African American families (Kazdin et al. 1995), but we also drew from that what seemed evident clinically, namely, that stress interrupts critical processes of treatment and constrains the benefits of treatment. We developed and piloted an intervention to mitigate parental stress and showed in an RCT that reducing parental stress during treatment enhanced treatment outcome of the children (Kazdin & Whitley 2003).

A separate line of work focused on attrition or dropping out of treatment. The initial goal merely was to evaluate whether we could predict dropping out, but this ended up being an extended series of studies. We could predict dropping out (~80% accuracy), but we then moved the agenda to identify nuances among who and when people dropped out, characterize families who dropped out but were doing very well, develop and test a model of why people dropped out, and intervene to improve participation in treatment (e.g., Kazdin & Mazurick 1994; Kazdin & Wassell 1998; Kazdin et al. 1994, 1997; Nock & Kazdin 2005). Among the major findings:

- Families that drop out of treatment are characterized by children with greater severity of conduct disorder and delinquent behavior, higher levels of parental stress (in role functioning as well as life events) and symptoms of psychopathology, and socioeconomic disadvantage;
- Predictors of dropping out vary as a function of when families drop out of treatment (early versus late in treatment). Multiple predictors related to the parent(s) and family (e.g., single- parent family, younger mother, poor living accommodations, adverse

child-rearing practices) are associated more with dropping out of treatment early; child dysfunctions (e.g., history of antisocial behavior, lower child IQ, poor social functioning at school) are associated more with dropping out of treatment later;

- European American and African American families in our sample share many predictors of dropping out (e.g., parental stress, adverse child-rearing practices, child and parent history of antisocial behavior) but vary on several other factors (e.g., socioeconomic status, child IQ);
- Among individuals who drop out “prematurely,” 34% indicate that their children significantly improved as compared with 79% among children who completed treatment. Improvement is moderated by similar variables whether or not individuals dropped out or completed treatment;
- Treatment outcomes vary as a function of dropping out of treatment. Those who drop out do less well, a finding usually interpreted to mean that the more therapy, the better. However, when pretreatment severity of dysfunction is controlled, outcomes between dropouts and completers no longer differ; and
- Of all the variables we have studied, barriers to participation in treatment were the best predictor of cancelling and not showing up for treatment sessions and dropping out of treatment (controlling for other variables such as child and parent dysfunction).

I continue to work on treatment for conduct disorder. We have moved from effectiveness, moderators, and processes and now explore novel ways to deliver the program so that it can be extended more broadly. Currently we provide online treatment, “face-to-face” by encrypted computer programs anywhere in the world where there is Internet. Our use of technology to administer evidence-based treatment is part of a broader thrust in treatment to use technology and social media (e.g., smartphones, tablets, apps) as a means of providing treatments.

BROADER TOPICS, THEMES, AND ISSUES

In the previous sections I highlighted findings from many of our studies. Any individual study often yields many different findings. As a general rule, the main findings of most studies are lost unless something stark and possibly counterintuitive is obtained or accumulated as part of many findings in a review (e.g., meta-analysis). I do not know that any one or more of my findings has yielded a stark result to stand out. Our accumulated work may support a general conclusion, such as the effectiveness of treatment of conduct disorder, but even here the nuances of individual studies are important to provide proper qualification, and my research now is hardly needed to draw that overall conclusion.

The research has led me to broader issues including methodological and conceptual challenges, commonly used research strategies that I considered limited or misguided, and lines of work

intended to improve the impact of our research on clinical dysfunction. One cannot discern if these broader topics have any impact, but they at least transcend the specificity of a given program of research and hence may be relevant to a larger audience. Here I highlight “programs of issues” analogous to programs of research, i.e., topics I have addressed in multiple papers spanning an extended period. As with the research mentioned previously, I include a few applicable citations rather than list all of the articles.

Research Methodology

Much of my writing has focused on methodological topics. In the context 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; how and why comparative treatment outcome studies are likely to find no differences; statistical power; and meta-analysis (e.g., Kazdin 1986b, 2001b; Kazdin & Bass 1989; Perepletchikova et al. 2007). I have given special emphasis to expectancy and nonspecific (common) factors as an explanation of therapeutic change (e.g., Kazdin 1979c, 2005b; Kazdin & Wilcoxon 1976) and have used this as a base for urging work on mechanisms (not just mediators) of change (e.g., Kazdin 2007, 2014b; Kazdin & Nock 2003).

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 to assess “clinically significant change” do not really provide evidence that the changes have made any difference to the clients (e.g., Kazdin 1999, 2001a);
- The evidence base for evidence-based treatment has some surprising and pervasive sources of bias (De Los Reyes & Kazdin 2006, Kazdin 2014a); and
- 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 (e.g., Kazdin & Wassell 1998).

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 (e.g., De Los Reyes & Kazdin 2005; Kazdin, 2000a, 2001b, 2011b, 2015b; Perepletchikova & Kazdin 2005).

In relation to single-case designs, my method papers have elaborated novel design options (e.g., simultaneous-treatment designs, withdrawal designs) including applications (e.g., quasi-experimental single-case designs) that could improve clinical work (e.g., Kazdin 1981, Kazdin & Hartmann 1978, Rusch & Kazdin 1981). Also, I have discussed sources of artifact and bias in the assessment of overt behavior, the strengths and limitations of visual inspection, and the special (i.e., nonmainstream) statistics that can be used to evaluate data from $N = 1$ research (e.g., Hartmann et al. 1980; Kazdin 1978c, 1979a, 2011c; White et al. 1989).

Substantive Topics

There are substantive topics that have been enduring in my work as well. I note three here that are programmatic and leave aside one- or two-shot reviews of other topics. 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, what the agenda ought to be to move forward, and special issues in relation to ethnic and cultural diversity (Kazdin 2000c, 2008a, 2009c, 2011b; Kazdin & Weisz 1998). Second, the 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. Among the topics are the deleterious consequences of corporal punishment, mild punishment options, and effective behavior-change interventions (e.g., functional analysis, differential reinforcement schedules) to eliminate behavior that does not require punishment at all (e.g., Benjet & Kazdin 2003, Kazdin 2013). Third, corporal punishment led to the broader concern of interpersonal violence (e.g., child abuse 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 (Kazdin 2011a, Koss et al. 2011, White et al. 2011). Also, perpetration of one type of violence predicts perpetration of another type of violence. Victimization from one type also is a predictor of victimization of another type. In short, violence is at the core of several health and social woes.

Finally, I have written on the token economy, which might well be one of the most extensively applied psychosocial interventions in terms of range of ages (i.e., 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) (Kazdin 1977c, 1982; Kazdin & Bootzin 1972). 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. Many of my articles focused on this question, including an empirical evaluation and comparison of the characteristics of research and practice of child psychotherapy (Kazdin et al. 1990a,b) and a statewide evaluation of the impact of child therapy as administered in clinical practice. [The contract for this latter project forbids any comments or sharing of results, which was a wise stipulation given the findings (shhh, you did not read this).] Other writings focused on identifying multiple dimensions that distinguish research and practice and how they can be studied, using systematic assessment and quasi-experimental single-case designs to improve clinical work, and identifying reciprocal influences that research and clinical practice exert (e.g., Goodheart et al. 2006; Kazdin 1978a, 2008b, 2009a; Kazdin et al. 1986a).

In retrospect, I see my writings and efforts to bridge research and practice as naïve and wish I could unpublish some or 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. Examples of such problems include providing clinical services, helping people make lifestyle changes to support mental and physical health, and 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 (see Kazdin 2009b). The concept 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 listing over 330 such treatments (Natl. Regist. Evid.-Based Programs Pract. 2014). 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, and 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 techniques (Kazdin 2015a, Kazdin & Blase 2011, Kazdin & Rabbitt 2013). Many other areas of study outside of psychology (e.g., operations research, the physical Internet) are not off-the-shelf ready to spread treatments to those in need, but they are quite relevant and include models for decision making, scaling interventions to reach large segments of the population, and identifying the effects and side effects of alternative courses of action. 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 the past decade 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 voices often are web-based opinions, self-anointed gurus (I hope I am not one), and statements that often violate what is known.

My media work usually focuses on issues of public concern related to areas in which I work and include such topics as child rearing, day care, 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 TV, 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 barbecue sauce or a two-day sale for lawn chairs during the commercial breaks. The more visible radio and TV networks and shows include NPR, PBS, BBC, CNN, *Today* show, *Good Morning America*, *ABC News*, *20/20*, and *Dr. Phil*. A colleague and I have prepared several articles (for Slate.com) 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 (Kazdin & Rotella 2008, 2013).

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 to friends that I thought they would find amusing. In so doing, I did not focus on 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, I was given a pseudonym (“Kazrin”—a separate story), and then the “article” was published, one on meta- meta-analysis and another from one of my daughters (a toddler at the time) insisting on a greater role and voice for children in the Association for Advancement of Behavior Therapy [Kazrin, Durac & Agteros (AKA Alan Kazdin, Jack Rachman and G. Terence Wilson) 1979; M.J. Kazdin 1980]. (Ponder the irony—my best manuscripts, such as they are, seem to be routinely rejected by journals, sometimes it seems before the editor gets past the title of the article, yet manuscripts I did not submit for publication have been accepted and published.) Other instances of note include developing and promoting a group penile plethysmograph (bad taste courtesy of Kazrin & Durac 1983) and my tightly reasoned and devastating refutation of an article claiming that I had accomplished something in my career (Kazdin 1997). 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 helped and 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 (the 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 interventions. A prior history of science scholar had completed the task,

but the committee rejected the final product (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. I was interviewed in person by the panel of eminent scholars. Among the many questions directed to me was a fair, gently phrased challenge, “What makes you think you could write this history?” I replied, “Nothing, in fact I do not like history very much. Moreover, I think these three people (I gave names I omit here) would be perfect for this; they know so much more about this and actually are part of that history.” I learned a couple of years into the project that my answer to that question sealed the job. The group felt that the answer meant I had no particular point of view or agenda (or social skills).

This was a four-year project that required hiding in a library cubicle, endlessly searching of 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). From that project I met many people and developed 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, there were historical rivalries (e.g., Pavlov and Bechterev), scandals of yesteryear (e.g., Watson’s affair and divorce that now would be mere fodder for an animated TV show), and multiple incompatible claims among the living about who invented what and when. None of this material was central to the charge of the committee and did not make it into the product (Kazdin 1978b).

Center for Advanced Study in the Behavioral Sciences. In the mid-1970s, I was invited to spend a year at the Center for Advanced Study in the Behavioral Sciences, then an independent think tank (now part of Stanford University). The invitation was “suspicious.” I was to go to the Center for a year, receive my salary, and was not required to do anything with one exception, have lunch (rather than hide and work) with the other people (~40) from psychology and mostly other disciplines. Let me think—lunch outdoors in the sun, overlooking Stanford campus, and with fascinating people (e.g., in psychology alone, Center Director Gardner Lindzey, Solomon Asch, Sandra Scar, and Elliot Valenstein)—yes, I could probably do that. In addition, others working in behavior therapy and closely aligned topics were invited that year, and we were asked to synthesize and evaluate behavior therapy, with no demand necessarily of a formal product. The other invitees included W. Stewart Agras, Nathan Azrin, Walter Mischel, Jack Rachman, and G. Terence Wilson. Weekly meetings during the year, collaborative papers, and endless discussions led to novel ideas, enduring friendships, and products (e.g., Agras et al. 1979; Kazdin & Wilson 1978a,b).

The time at the Center was the best and most intellectually stimulating year of my career. The daily regimen included long lunches, followed by long volleyball games, a quick shower, with iced tea afterward (if no sherry hour), some miscellaneous conversations with others in

residence, then a quick peek at my mail, and—my God, it was getting close to the time to go home—I had better wrap up. Nevertheless, this was my most productive year (e.g., four books and the usual cadre of journal articles), even though it was interrupted with monthly trips back to Penn State to see if my grant research was still going on and if my office had been turned into a time-out room for other faculty. The work with the colleagues I mentioned helped consolidate my interest in shifting my research to work with clinical dysfunction and patient populations.

John D. and Catherine T. MacArthur Foundation Research Network on Psychopathology and Development (1994–1998). A truly transformative experience was my participation in the MacArthur Foundation Research Network on Psychopathology and Development (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 days of solid meetings, discussion, and agenda. The overall goal was to make advances in understanding and developing novel assessments of child psychopathology evident in the early years of life. Critical issues were discussed, 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, tradeoffs in using various prevention strategies for childhood psychopathology, among other topics (e.g., Kazdin & Kagan 1994; Kraemer et al. 1997, 1999; Offord et al. 1998). The influence on my work went well beyond several publications. The network provided intensive educational experiences on content, methodologies, and conceptual views of diverse disciplines.

Medical School Jobs. I have spent approximately one-third of my career working in psychiatry in medical schools, including 10 years at the University of Pittsburgh and 4 years at Yale (see **Table 1**). At Yale, I was invited (by the university president and medical school dean) to move from the main campus and my position on the faculty of arts and sciences to the School of Medicine to serve as department chair and director of the Child Study Center, a department of mostly child psychiatrists but with other disciplines and professions (e.g., neuroscience, genetics, education, law, social policy) as well. The position required frequent interactions with chairs of other medical school departments. This was an intellectual and, through 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, working on medical school faculty ethics guidelines]. Also, as part of the hospital leadership (Yale-New Haven Hospital), 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 goal had always been to be a small fish in a large pond. I could see now that this had been achieved.)

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 presidency (2008) actually is a three-year period (elect, current, and past) of active positions involved in governing and representing the association. 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 viewpoint. The job entailed many activities, including conveying psychology's interests directly to members of the US 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 (Koss et al. 2011, White et al. 2011). The second theme, on anxiety and trauma in children, mounted a special task force (chaired by Annette LaGreca) with a charge to summarize and disseminate what is known and what treatments are available, and to develop and circulate user-friendly materials for the public, caregivers, policymakers, and agencies serving children and adolescents.

My 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 the US Congress and other policy makers, the public, students, and teachers) to bring the science together in readable formats for diverse audiences. I used my presidential address to convey how psychology might contribute to the mitigation of

climate change (Kazdin 2009b). Each of the themes included meetings of various groups and think tanks and programming in the annual convention, and drew on the APA resources and organizational units in communication, public interest, publications, and science.

Editing

For most of my career, I have been editing something, usually a journal. This began early in my career with two associate editorships (*Behavior Therapy*, 1975–1978; *Journal of Applied Behavior Analysis*, 1975–1976) and moved to editorships. By now I have been fortunate to have served as editor of six journals (*Behavior Therapy*, 1979–1983; *Journal of Consulting and Clinical Psychology*, 1985–1990; *Psychological Assessment*, 1989–1991; *Clinical Psychology: Science and Practice*, 1994–1998; *Current Directions in Psychological Science*, 1999–2004; and *Clinical Psychological Science*, 2012–present). As editor, one reads on topics one would never look at otherwise and sees new methods, measures, and 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 have actually generated studies.

I have served as editor of four book series. First, Benjamin Lahey and I coedited an annual series of books called *Advances in Clinical Child Psychology* (1977–1992). Each year we invited a diverse set of authors to write on research topics. The series produced 14 volumes, after which we turned this over to others. Second, I edited a series entitled *Developmental Clinical Psychology and Psychiatry* for Sage Publications (1983–1999). This series produced individually authored books on diverse topics of child and adolescent psychopathology. The series included 45 books total. Third, I edited a series, *Current Perspectives in Psychology*, for Yale University Press (2000–2008). I was chair of the Publications Committee at Yale University 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 invited authors to prepare books; 13 individually authored books were published before the series ended. Finally, I had a brief three-year run (2004–2007) 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 American Psychological Association and Oxford University Press (Kazdin 2000b). This presented 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. The *Encyclopedia of Psychology* was a seven-year, eight-volume effort and the longest book project on which I have worked.

Grant Funding

Most of the work I have done has required grants in light of the costs of 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 (R01s), Research Scientist Development and Research Scientist Awards, and MERIT (Method to Extend Research in Time) Awards] spanning more than 35 years as a principal investigator (PI). The challenges of procuring such funding now are much greater. My NIMH grant rejections now include gobs of accolades and mother-like praise but no funding. In prior years, praise was muted but funds would be provided. [My team of (non-evidence-based) therapists believes something in my applications might unwittingly reflect a “cry for praise” rather than “a cry for funds.” Actually, all of this more accurately reflects a “cry for a new team of therapists.”]

Many other grants provided opportunities to evaluate the impact of psychiatric hospitalization on children, 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. Funding sources include the John D. and Catherine T. MacArthur Foundation, Laura J. Niles Foundation, Leon Lowenstein 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, the 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 (if 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 it is best to 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—which 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, Allan S. Bellack, Richard Bootzin, W. Edward Craighead, Susan Fiske, Richard Harrington, Donald Hartmann, Michel Hersen, Peter Jensen, Jerome Kagan, Ronald Kessler, David Kolko, David Kupfer, Mary Koss, Helena Kraemer, 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 on to very successful careers, including Andrew De Los Reyes, Russell T. Jones, Paul Marciano, Matthew Nock, Francheska Perepletchikova, and Linda Wilcoxon Craighead; others were members of my research team, for example, Debra Bass (Colbus), Michael Crowley, Karen Esveldt-Dawson, Jennifer Mazurick, Antoinette Rodgers, Roseann Sherick, Todd Siegel, Gloria Wassell, and Moira Whitley.

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.

REFLECTIONS AND FINAL COMMENTS

It is fitting to end with broader reflections than career moves, research programs, and experiences. To begin, the impact of one's work is a suitable topic and is one of great concern for promotion, career advancement, awards, and the like. I never considered my work to have impact or thought about this very much either way. It has been clear in psychology, from the beginning of my career, that there is an endless fire-hose gush of journal articles, most of which seem to be infrequently read, even if the studies are cited. Scores of findings within an individual study are lost. Thankfully, meta-analyses may dice, slice, and homogenize findings as part of a larger collection, but the impact of individual studies or program of research is hard to discern, with rare exception. The many measures of impact of one's work, now quantified for individual investigators and journals, are curious (to me) and are easily shredded on conceptual or methodological grounds. In my case, the impact of my research was on my nonempirical papers and my conceptualization and models of how to solve problems (e.g., reducing the burden of mental illness, developing effective treatments, studying mediators and mechanisms).

I have been interested in methodology in part because questions and answers in our empirical research depend heavily on how we go about studying the phenomena. Also,

methodology is the Esperanto of all of the sciences, and I am deeply interested in diverse sciences and their models and methods. Major scientific advances come from methodological advances and arguably even more than they do from theory (Greenwald 2012, Kazdin 2015b), although the theories of evolution and relativity deserve more than a passing curtsey as monumental exceptions. And yet methodology has its frustrations and oddities.

For example, my research has mostly been in the tradition of null hypothesis statistical significance testing and in clinical research that has a special source of frustration. Evaluating statistical significance at the end of a study when treatment is completed makes it too late to help patients who were participants and who did not respond particularly well. Means, mean differences, and standard deviations are important for characterizing groups but not usually for characterizing individuals. Also, statistical significance is well recognized to be a measure of sample size (see Kazdin 2015b), which invariably led me to wonder what I was doing by testing group differences. Effect size is a valuable addition to data analysis. Yet, in areas of my work (e.g., intervention) effect size is not a measure of importance or the applied impact of an effect, despite the constant confusion (see Kazdin 2014a).

Statistical significance was odd to me in other ways. To be more concrete, effects I obtained (statistically significant) in one study were not always replicated in another study using the same procedures, methods, research team, and population. Yes, there are good explanations for that, and as methodologists we often say, “Chance happens.” Incorrectly accepting or rejecting null hypothesis and nonreplication are inherent to the process. All that aside, it always seemed odd to me to be praying that some results will not show statistically significant differences (e.g., comparison of groups at pretest on demographic characteristics and pretreatment measures) and then switching the prayers so that other results come out as statistically significant (group differences at posttreatment or follow-up on some of the same measures). I am not against prayer or obviously not above praying for statistical significance. (In fact, I have a well-honed set of chants of which I am rather fond that draws from multiple religions.) Even so I find it odd praying for the opposite effects (first nonsignificance and then significance); it is like rooting for the horse to win part of the race but lose the other part.

The studies in which I used single-case designs (with individuals or with groups) gave me more confidence in what I was doing. Intervention effects fail and replication is not guaranteed any more than in group studies, but one can see the effects in real time (e.g., collection of data daily over time), can make changes to improve outcome, can replicate across situations, time, and clients—all of this while the study is going on. One can better see whether an intervention or experimental manipulation has had an effect on individuals and to what extent. Perhaps it is the real-time daily data rather than the type of design that gave me a better feeling for what I was doing.

The broader lesson of methodology (for me) is the important one and does not pit one type of methodology against another. Different methodological traditions (quantitative and qualitative research, single-case designs) and many variations within a given tradition have different yields and their own strengths and limitations. The methods are all needed to understand the phenomena in which we are interested and often provide different levels of analysis. This has made me argue for broader training of our researchers beyond the dominant quantitative tradition.

In providing an overview of my work, I have refrained from mentioning my personal life, or even my impersonal life now that I think of it. It is easy to argue that the personal and enduring influences are the main, albeit silent, “collaborations” for one’s work, and may only get fleeting and cursory recognition in unread dedications of unread books. It is fitting to end by noting that none of my work would have happened without good fortune and a remarkable family that made the work possible. 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. 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.

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Errata

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