COMMENTARY

Agenda for Child and Adolescent Psychotherapy Research

On the Need to Put Science Into Practice

ALAN KAZDIN'S empirical work and conceptual contributions have greatly enriched the study of child and adolescent dysfunction and treatment for many years. His research on the treatment of conduct disorder is a model of scientific elegance, and his thoughts on a research agenda for the field always warrant close attention. I certainly do agree with the author when he states that child and adolescent psychotherapy research should focus on understanding the mechanisms through which change takes place; that developmental psychopathology research and neuroscience findings should be used to inform treatment where possible; that judgments about treatment effects should be based on a broader range of outcomes than symptoms and diagnosis alone; and that a broad range of outcome-related questions should be addressed, including the identification of moderators that limit the range of treatment effects.

See also pages 829 and 839

Referring to these goals, which include understanding how and why therapy works, Kazdin[1] suggests that "there is reason for alarm" because "there are no clear signs that these limitations are being redressed by current studies." I am more optimistic on this front, because it appears to me that the changes proposed in the article are already underway. Single-case designs, clinical trial designs, dismantling tests, and comparative analyses have of course been with us for many years. But a review of more recent research reveals growing attention to moderators[2-4] and mediators of outcome.[5-8] Researchers, too, are beginning to heed the call to broaden outcome assessment by including functioning, environmental change, consumer perspectives, and system-use and service-use variables, as suggested by Hoagwood et al.[9] These research ideals are increasingly becoming a part of design meetings, journal manuscript reviews, and grant application review committee discussions. Thus, it seems to me that the field is moving in a positive direction—albeit more slowly than would be ideal—on the methodological issues Kazdin notes. But if I am mistaken about movement in the field, then Kazdin's call for change is certainly warranted and timely.

My primary concern is that the changes Kazdin proposes may not be fundamental enough to address what is arguably the most significant problem in child mental health care today. I will frame my comments as answers to the 2 key questions Kazdin[1] wisely posed: (1) What are the goals of child and adolescent psychotherapy research, and (2) what type of research is needed to obtain these goals? In my opinion, the most important goals are to develop treatments that are effectively used in clinical practice with referred children and adolescents, and to gain an understanding of their effective range as well as the causal processes by which the treatments achieve their effects with such youths. Unfortunately, I do not believe that the type of research done to date is leading us toward these goals, and I think that a kind of reform different from that proposed by Kazdin will be needed to attain them.

The problem we face can be summarized as follows: during the past 50 years, clinical trials have shown the beneficial effects of hundreds of specific child and adolescent treatments. Meta-analyses of the outcome evidence have revealed unadjusted mean effect sizes ranging from 0.71 to 0.88,[10-13] effects considered "medium" to "large" according to a widely used yardstick.[14] Task forces within the American Psychological Association (Washington, DC),[15,16] applying common review criteria, have identified more than 24 specific youth treatments as "empirically supported," and certainly more treatments would qualify today. Despite the ample base of evidence, virtually none of the empirically tested treatments has made its way into regular clinical practice in the United States. The tested treatments continue to be used almost exclusively in university research settings. And most of the 2.5 million American children seen in clinical practice each year receive interventions that have never been tested in a clinical trial. Researchers have given a party, but clinicians and families have stayed home.

What has produced this odd state of affairs? One cause, in my opinion, is that we have relied too heavily on a treatment development model designed for medical and pharmaceutical research, a model that may not generate the most clinic-ready psychosocial treatments or the most clinic-relevant data on how they work. In the medical-pharmaceutical model, treatment protocols are developed in the laboratory, then tested via an array of laboratory efficacy experiments, and finally, after extensive efficacy testing, brought into community-based clinical settings "to measure the public health impact."[17]

The medical-pharmaceutical model may not work well for psychotherapy because there is such a

broad gulf between laboratory conditions and clinical practice conditions, involving multiple variables that can interact with psychosocial treatment to affect outcome. Most efficacy research involves youngsters who were not spontaneously referred for treatment, but recruited from schools or through advertisements. They are symptomatic but may not meet diagnostic criteria. They are less likely to be severely disturbed or have serious comorbidities than referred youths, and their families are middle-class study volunteers rather than the highly stressed and often financially disadvantaged families seen in community treatment. These recruited individuals are treated in a university laboratory or a school (where no-shows rarely occur) by university faculty members or research assistants (who are accustomed to using treatment manuals) rather than by multicase clinical staff faced with productivity requirements and little experience with manualized treatment. These differences and numerous others between therapy in laboratory research and therapy in clinical practice may be a gap too pronounced to bridge as simply the final step in a long series of efficacy experiments. Perhaps the number of dimensions within which treatment needs to change to bridge the laboratory-to-clinic gap (eg, clients, families, settings, time and financial constraints, therapists, clinic characteristics) means that this complex task needs to be made an integral part of treatment development and testing from an early stage in the process.

But don't we need serial efficacy studies to understand the essential components, range of impact (moderators), and change mechanisms (mediators) of our treatments? This is a commonly held view, but it is not my own. It seems to me that, because the clients, therapists, and conditions of treatment in clinical settings differ in so many ways from those of laboratory efficacy tests, findings on component effects, the impact of various moderators, and the change processes that mediate treatment effects may well be different for recruited subjects in a laboratory study than for the referred clients treated in clinics, to whom we wish to generalize. We as clinical researchers need to ask ourselves whether the most valid answers to the questions how, why, and with whom our treatments work are really more likely to come from research with recruited samples seen in laboratory conditions, or from research tests placed within the context of genuine practice.

To support the search for treatments that will be effectively used in clinical practice, and to generate the most valid understanding of those treatments, my colleagues and I have proposed a Clinic-Based Treatment Development model[18-20] in which the development and testing processes are moved at an early stage into clinical practice contexts, where protocol development and refinement, outcome assessment, and efforts to answer the "how, why, and with whom" questions are all based. The guiding principle is that our most effective treatments and the most complete understanding of them will come through research embedded in the contexts where treatment most often happens. This is one perspective on Kazdin's useful questions regarding the goals of child and adolescent therapy research, and the type of research needed to attain these goals. Perhaps time will tell whether this perspective is a useful complement to views offered in Kazdin's insightful article.

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