13

The Future of the Clinical Science Movement: Challenges, Issues, and Opportunities

Robert W. Levenson
University of California, Berkeley

In the 14 years since Richard McFall wrote his Manifesto (McFall 1991), the clinical science movement has gained a great deal of momentum, and the term *clinical science* has achieved enviable brand name recognition. This can be seen in numerous ways, ranging from the subtle (a substantial number of university-based graduate programs in clinical psychology now refer to themselves as clinical science programs) to the profound (the clinical science model is now recognized by the field's primary accreditation body as being distinct from the older and more generic scientist-practitioner and practitioner-scientist models). The establishment of the Academy of Psychological Clinical Science (the Academy) in 1995, which largely grew out of McFall's vision, provided an umbrella organization for certifying that member predoctoral and internship training programs in clinical and health psychology embraced and adhered to the principles of the clinical science model. Currently, 45 graduate and nine internship programs have gained membership in the Academy, representing a veritable who's who of the top training programs in the United States and Canada. Increasingly, undergraduates considering

careers in scientific clinical psychology make use of the list of Academy programs in deciding which graduate schools to apply for, and graduate students do the same when applying for internship training. Clearly, clinical science has won the initial round in the battle for the hearts and minds of scientifically oriented clinical psychologists. Still, the war goes on and the ultimate fate of the clinical science movement is yet to be determined.

Throughout its history, clinical psychology has focused on a holy trinity of diagnosis, treatment, and etiology. In this chapter, I examine some of the challenges, issues, and opportunities the clinical science movement faces in each of these areas as well as in the general area of training. Many of these represent unsettled issues inherited from earlier times, but all have been shaped and made even more complex by the rapidly changing worlds of clinical science and practice.

DIAGNOSIS: IS THE *DSM* THE WRONG BLUEPRINT FOR CLINICAL SCIENCE?

The fruits of decades of work on diagnosis in clinical psychology and psychiatry are reflected in the various incarnations of the *Diagnostic and Statistical Manual* (*DSM*; American Psychiatric Association, 1994). Depending on one's viewpoint, the *DSM* can be situated somewhere on a bipolar scale anchored on the one end by "an evolving, increasingly reliable, progressively more objective basis for parsing pathology" and on the other by "a highly uneven, hopelessly politicized patchwork of dubious descriptions that identify some 'natural kinds' and many hopelessly heterogeneous diagnostic categories." Many clinical scientists view the *DSM* as a major impediment for building a true science of psychopathology (for thoughtful discussions of some of these issues, see Follette & Houts, 1996; Widiger & Clark, 2000). In this view, the heterogeneity of some diagnostic categories means that different investigators studying patients with the same diagnosis are, in fact, studying very different disease processes. This, of course, is a formula for disaster, especially when attempting to aggregate findings from different laboratories on what is ostensibly the same disorder. Clinical scientists have been actively involved for decades in attempts to perfect (or at least improve) the *DSM* in a number of different ways (Widiger, Frances, Pincus, Davis, & First, 1991).

One thrust of these efforts has been to modify criteria and clarify descriptions so that the reliability of diagnosis is improved (Nathan & Langenbucher, 1999; Williams et al., 1992). These clearly noble efforts

are reflected in the most recent versions of the *DSM* requiring much less inference and containing far fewer unoperationalized constructs than did earlier versions. However, as we all learned in introductory psychometrics, reliability establishes a ceiling on validity but it does not in any way ensure validity. The unsettling question remains: Are we getting better and better at assigning individuals to categories that are ultimately going to prove to be not very useful for understanding etiology, course, and treatment?

A second thrust (Hinshaw, 1987) has been in identifying subtypes of some *DSM* disorders (where observed heterogeneity is thought to reflect the existence of more that one disease process) and clusters of others (where putative distinctions between disorders are more apparent than real). In many ways, this has been an area in which clinical scientists have really shone, applying their considerable observational, psychometric, and empirical skills in the service of these efforts. Unfortunately, the application of findings from these efforts back into the *DSM* has been both slow and uneven. Thus, two parallel diagnostic universes now exist, one in which the *DSM* is the bible and the other in which research criteria are. This state of affairs is understandable, but provides a poor model for integration of science and practice.

Outside of the world of the *DSM*, an increasing number of clinical scientists have explicitly or implicitly abandoned the *DSM* entirely and are gravitating toward a transdiagnostic, symptom-oriented approach (Harvey, 2001; Harvey, Watkins, Mansell, & Shafran, 2004). The symptom-oriented approach to diagnosis is certainly not new, finding considerable favor during the heyday of behaviorism in clinical psychology (Wolpe, 1958). The transdiagnostic approach, however, is an important extension. The behaviorists focused on symptoms as an endpoint for diagnosis and treatment. In contrast, the transdiagnosticians attempt to leverage the observation that certain symptoms are manifest in a range of disorders (e.g., affect dysregulation, sleep disruption) into a deeper understanding of these symptoms as the basic building blocks of psychopathology and as promising loci for intervention.

Clinical science is at a decision point regarding the *DSM*. It can keep its scientific portfolio diverse by straddling the fence on these issues, or it can make a choice and invest its resources in ways that are most likely to lead to significant scientific discoveries and applied payoffs. For me, there are a number of reasons that argue in favor of investing most heavily in the transdiagnostic approach. The transdiagnostic approach allows clinical science to take advantage of the massive body of knowledge in nonclinical psychological science concerning measurement of, functions served by,

and underlying biological substrates for basic behavioral, cognitive, and social processes such as thinking, feeling, developing, remembering, deciding, attaching, individuating, affiliating, and so on. These are the precisely the processes that are most vulnerable to disruption in psychological disorders. Moreover, these disruptions often constitute the most debilitating aspects of mental illness for the patient, the patient's family, and society. Importantly, this kind of analysis is at the core of the translational research movement (National Institute of Mental Health, 2000), which envisions the application of advances in the most basic behavioral and social science to reducing the burden of mental illness.

The clinical science movement has a real opportunity to embrace these issues and take a bold leadership position in the debate over the future of the *DSM* in clinical research. Moreover, these issues have enormous implications for whether we will train clinical scientists for obsolescence or for producing and participating in significant, groundbreaking clinical research in the coming years.

TREATMENT: ARE EMPIRICALLY SUPPORTED TREATMENTS THE FUTURE?

Clinical science confronts several striking ironies in the realm of treatment. First there is the issue of empirically supported treatments. There is a long tradition of findings that treatment type doesn't really matter (tracing back to Smith & Glass, 1977). Complementing this are findings that the common, nonspecific aspects of treatment (placebo, expectancy, therapeutic alliance) account for most of the variance in outcome (Ahn & Wampold, 2001; Frank & Frank, 1991). However, there is a remarkable amount of effort currently being devoted to manualizing specific treatment protocols and amassing support for their inclusion in approved lists of empirically supported treatments (Chambless & Hollon, 1998). Among the most avid consumers of these empirically supported treatments are mental health care providers and insurers—who understandably want to use the most effective and efficient treatments—and clinical science training programs—who are committed to training their students in scientifically based clinical practice.

A second irony revolves around the question of who most needs psychological services and how to best deliver these services to them. A great deal of contemporary effort in treatment development and treatment evaluation in clinical science has focused on mounting well-controlled clinical trials of treatments conducted in university clinics. Implicit in these efforts has been the assumption that those treatments that are established as efficacious in the research clinic could then be exported into real-world community settings where they would be enthusiastically embraced by practitioners and reduce impairment in patients. However, when researchers began to investigate the relative effectiveness of these treatments administered in different settings, they found a dramatic reduction in effectiveness as treatments moved from the university research clinic into the community (Weisz, Donenberg, Han, & Weiss, 1995). There are many explanations for why this may be. For example, patients and therapists in university trials may not be representative of those in community settings (Weisz, Doss, & Hawley, 2005), and manualized treatments that are found to be effective in university-based trials may be too cumbersome for real-world use. The bottom line for clinical science is that we need to embrace the goal of developing treatments that work and can be administered not only in the rarefied atmosphere of university-based randomized clinical trials but also in real-world settings where the great majority of clinical services are delivered.

ETIOLOGY: WHAT IS THE APPROPRIATE LEVEL OF ANALYSIS?

Clinical psychology has lived through a number of paradigm shifts in psychology. Each model along the way—psychodynamic, behavioral, cognitive, and brain/neuroscience—has left its mark on our theories, methods, measures, and treatments. To an extent, nothing in clinical psychology is ever lost. Practitioners of psychodynamic, behavioral, cognitive, and pharmacologic treatments all still compete for patients in many urban centers. Similarly, in many hospital settings, projective tests and functional brain imaging are both used. Nonetheless, the heyday of discovery in the older paradigms has passed. In keeping with this view, when NIMH recently retargeted their grant portfolio toward research most likely to lead to breakthroughs in the treatment of severe psychopathology, biological and genetic models were enthusiastically endorsed while psychodynamic and classic behavioral paradigms were given short shrift (Levenson, 2005).

Although a great deal of the current excitement in mental health research centers on the brain, its neurotransmitters, and the neural circuits that can be traced through the methods of contemporary neuroscience, it is likely that the next big thing in psychopathology research will be genetics. The application of new molecular genetics methodologies

to the study of mental illness focuses on particular proteins that are involved in the basic processes thought to underlie normal and abnormal functioning. Unlike the quantitative genetics of the past, which utilized twin studies to establish the relative power of heredity and environment in the transmission of a number of *DSM* disorders, these new molecular methods show great promise for uncovering some of the mechanisms that underlie disease and health. Already, exciting findings are emerging using these new methods. For example, links have been established between genotypical variation related to catecholamine metabolism and deficits in performance on working memory tasks in schizophrenia (Goldenberg et al., 2003). Promising gene by environment interactions have also been elucidated such as one linking a polymorphism in a serotonin transporter gene to variation in the likelihood that life stress will lead to depressive symptomatology and suicide (Caspi et al., 2003). The implications of these kinds of findings for identifying risk for psychopathology and for designing future interventions are enormous.

What is the optimal level of analysis for clinical scientists wishing to work with modern functional brain imaging and molecular genetics? It seems clear to me that these new methods are better suited for building links to basic processes (e.g., attention, social interest, affect regulation) than to full-blown, often-heterogeneous *DSM* diagnostic categories (e.g., autism, hyperactivity, bipolar disease). If this view is correct, clinical scientists are going to need to become much more expert in basic psychological processes. It makes little sense to marry first-rate neuroscience and first-class genetics with second-rate psychology. For this reason, clinical science should be actively courting the participation of researchers from the nonclinical areas of psychology where there already is impressive expertise in conceptualization and measurement of basic behavioral, affective, social, and cognitive processes (but sadly, often little familiarity with psychopathology).

THE FUTURE OF THE CLINICAL SCIENCE MOVEMENT

Judgments of the ultimate importance of the clinical science movement will be based on the issues it raises, the ideas it promulgates, the science it produces, the scientists it trains, and its influence on the field. Since its inception, the clinical science movement has articulated a clear vision of a scientifically grounded clinical and health psychology and has established a standard for member graduate and internship programs that goes

far beyond merely talking the talk. These are its clearest victories to date. Some movements do not go beyond this point and, certainly, the clinical science movement could decide to rest on its laurels content in the knowledge that it spawned dialog, organizations, and had a significant influence on the field's consciousness, accreditation procedures, and market for its services and products. Hopefully the energy behind the clinical science movement has not been spent, for there are still many areas in which its potential has not yet been met. In this final section, I turn to some of the areas where the final judgment on the clinical science movement is still out and where future challenges and opportunities abound.

Producing Science

Has the clinical science movement fundamentally changed the nature of research produced by clinical psychologists and has it spawned the kinds of breakthroughs in diagnosis, treatment, and etiology that the field, the funding agencies, and the public crave? These kinds of questions are always difficult, if not impossible, to answer. What we can say is that there are significant obstacles that stand in the path of faculty and students in clinical science programs who wish to produce significant science. Many of the top psychology programs in the country either do not have clinical science programs or have relatively small programs. Students and faculty in existing clinical science programs have to devote significant effort to clinical psychology coursework and applied clinical training that divert valuable time away from research. Gaining accreditation by APA and being competitive for internships are classic tail-wags-dog situations that can further dilute scientific efforts, reduce degrees of freedom in program design and individual curriculum choices, and add huge administrative overhead that further consumes time, energy, and resources.

Clinical science is highly time-consuming. Intervention studies take time, recruiting patient populations takes time, coding behavior takes time, and using paradigms that don't lend themselves to group testing takes time. Many clinical scientists end up moving toward using analog populations rather than "real" patients and mount relatively brief interventions with no or only brief follow-up. Other clinical scientists gravitate away from the study of clinical populations and interventions completely, instead studying basic psychological processes such as emotion, affiliation, and cognition in normal populations.

Another recent development worthy of note is that some of the most exciting work in clinical science is now being done by psychologists in nonclinical areas. Thus, there are a growing number of neuroscientists who are studying autism, dementia, and other developmental disorders (Amaral, Bauman, & Schumann, 2003) and a number of social and personality psychologists who are studying gene by environment interactions in depression, addictions, and other forms of mental illness (Caspi et al., 2003). From the vantage point of clinical science, it is encouraging to see researchers from nonclinical areas of psychology taking on these important problems; however, it is also important that clinical scientists stay involved in this kind of cutting-edge research.

Clinical scientists who drift away from research on clinical populations and nonclinical scientists who move into these areas are not problematical in and of themselves, especially if good science comes out of these efforts. However, to the extent that research and training choices of clinical scientists are being dictated in significant part by the competing demands associated with the mounting of traditional clinical psychology programs, it may be time to break this tie to the past. Clinical science programs may want to start anew and design themselves from the ground up in ways that conform to the ideals of the clinical science movement and the priority of producing important science that is relevant to mental health and illness.

Training

Clinical psychology in the coming decades is likely to see rapid changes in the realms of diagnosis, treatment, and etiology. It is difficult to argue against the general principle that we should be training clinical scientists who are master problem solvers rather than masters of the status quo. However, this still means striking a balance between training in the existing body of theories, methods, and techniques and training in how to develop new theories, new methods, and new techniques. Earlier sections of this chapter argue in favor of training in the new transdiagnostic, basic process-oriented approaches to diagnosis, in treatment development and evaluation, and in new neuroscience and genetics methodologies. Introducing this kind of training would clearly mean a reduction in emphasis on older theories, the *DSM* diagnostic system, and the current catalog of empirically supported treatments. As noted earlier, clinical science programs often find their training options constrained by past practices, by demands of accreditation, and by perceived demands of internship programs. In an ideal world, clinical science programs would be freed from these constraints and able to make pedagogical decisions solely in terms of what they believe would produce the best science and the best scientists.

One aspect of pre-internship training that was once prominent and now seems to be in danger of being lost is exposure to a range of patients, especially to those with severe psychopathology. I recently conducted an informal poll of students in our clinical science program and learned that few had any significant exposure to schizophrenic patients prior to internship. Reasons for this are myriad, including sharp reductions in inpatient facilities, increasing training of clinical science students in-house or in facilities primarily devoted to outpatient care, increased use of pharmacologic treatments, and the disappearance of live patient case conferences. Exposure to patients suffering from severe psychopathology early in students' training can be a rich source of research hypotheses and can stimulate subsequent research. Moreover, students can learn how to work with families and mental health system gatekeepers to locate and recruit patients and can develop a comfort level with interacting with patients, both critical for patient-centric research. For many students, if the door into the world of patient research is not opened early, it will never be opened. In fact, I have argued recently that this kind of early exposure to patients should be part of the training of both clinical and nonclinical students (Levenson, 2004a, 2004b). If psychological science in general and clinical science in particular are going to stay relevant (and funded) in the years ahead, it is important that our students be trained in ways that makes them capable of and inclined toward conducting research with clinical populations (including those with the most severe psychopathologics).

Science and Practice: Bridging the Gap or Increasing the Schism?

One of the most sobering crises facing the clinical science movement is its relationship with clinical practice. In defining its own identity, the clinical science movement has often adopted harsh rhetoric criticizing current clinical practice, training in practice-oriented graduate programs, and the priorities of professional organizations that are devoted to the interests of practitioners. Practitioners have adopted their own harsh rhetoric about failures of clinical science to help real-world patients, the impracticality of manualized treatments, the superiority of clinical over

empirical evidence, and the priorities of professional organizations devoted to the interests of clinical scientists. Clinical scientists and clinical practitioners have taken opposing positions on a number of critical issues, including accreditation, prescription privileges, licensing, and the direction and leadership of professional organizations.

In reality, clinical practitioners and clinical scientists occupy overlapping niches, share common concerns, and work with many of the same issues. Clinical psychology will be severely diminished as a field and the mental health of the public will suffer if most of those who produce the science become estranged from most of those who proffer the treatments. Clearly there is a great deal of benefit to both groups if clinical practitioners respect and utilize the available science and if clinical scientists respect and utilize the experience that practitioners have working with clinical problems. There will always be tensions between scientists and practitioners, but in many other public health fields these groups are able to work together in much less adversarial ways. We need more collaboration between the groups rather than more isolation. There are ample opportunities to work together in research, in the application of research to practice, and in the training of scientists and practitioners. I think it is time for the clinical science movement to soften its rhetoric and take steps toward pursuing a rapprochement between these warring factions.

A CLOSING AND MORE PERSONAL THOUGHT

It is a great honor to be able to contribute to this Festschrift volume, which recognizes the far-reaching contributions of Dick McFall to clinical science. Our paths crossed early in my career when Dick was courted from the University of Wisconsin to come to Indiana University and direct its clinical psychology program. Of course, he did far more than that, building what has arguably become *the* national model for a clinical science training program. During the years when we were colleagues at Indiana, we collaborated on research, tackled clinical program issues, and talked often about lives and careers. After I left for Berkeley, I found myself on many occasions attending meetings and serving on committees with Dick. Initially, many of these revolved around his tireless efforts to build the clinical science movement and the Academy of Psychological Clinical Science. Later, we often served together as representatives of the movement he had built. In the past decade, there have been significant changes in the field of clinical psychology, numerous

crises have been averted, challenges met, and opportunities seized. Without Dick McFall's personal integrity, force of intellect, clarity of vision, and unbounded energy it is hard to imagine any of this coming to pass. For me, it has been a great privilege and joy to know and work with Dick McFall over these many years and to be able to consider him a colleague and friend.

REFERENCES

Ahn, H., & Wampold, B. E. (2001). Where oh where are the specific ingredients? A meta-analysis of component studies in counseling and psychotherapy. *Journal of Counseling Psychology, 48*(3), 251–257.

Amaral, D. G., Bauman, M. D., & Schumann, C. M. (2003). The amygdala and autism: implications from non-human primate studies. *Genes , Brain, and Behavior, 2*, 295–302.

American Psychiatric Association. (1994). *Diagnostic and statistical manual of mental disorders. Fourth edition (DSM–IV)*. Washington, DC: American Psychiatric Association.

Caspi, A., Sugden, K., Moffitt, T. E., Taylor, A., Craig, I. W., Harrington, H., et al. (2003). Influence of life stress on depression: Moderation by a polymorphism in the 5-HTT gene. *Science, 301*(5631), 386–389.

Chambless, D. L., & Hollon, S. D. (1998). Defining empirically supported therapies. *Journal of Consulting & Clinical Psychology, 66*(1), 7–18.

Follette, W. C., & Houts, A. C. (1996). Models of scientific progress and the role of theory in taxonomy development: A case study of the DSM. *Journal of Consulting & Clinical Psychology, 64*(6), 1120–1132.

Frank, J. D., & Frank, J. B. (1991). *Persuasion and healing: A comparative study of psychotherapy* (3rd ed.). Baltimore, MD: John Hopkins University Press.

Goldberg, T. E., Egan, M. F., Gscheidle, T., Coppola, R., Weickert, T., Kolachana, B. S., et al. (2003). Executive subprocesses in working memory: Relationship to catechol-O-methyltransferase Val158Met genotype and schizophrenia. *Archives of General Psychiatry, 60*(9), 889–896.

Harvey, A. G. (2001). Insomnia: Symptom or diagnosis? *Clinical Psychology Review, 21*(7), 1037–1059.

Harvey, A. G., Watkins, E., Mansell, W., & Shafran, R. (2004). *Cognitive behavioural processes across psychological disorders: A transdiagnostic approach to research and treatment*. New York: Oxford University Press.

Hinshaw, S. P. (1987). On the distinction between attentional deficits/hyperactivity and conduct problems/aggression in child psychopathology. *Psychological Bulletin, 101*(3), 443–463.

Levenson, R. W. (2004a). Patients and impatience. *APS Observer*, pp. 5, 44.

Levenson, R. W. (2004b). Patients and impatience (Part II). *APS Observer*, pp. 5, 42.

Levenson, R. W. (2005). Basic research funding: An exercise in NIH-ilism. *APS Observer, 18*(2).

McFall, R. M. (1991). Manifesto for a science of clinical psychology. *Clinical Psychologist, 44*(6), 75–88.

Nathan, P. E., & Langenbucher, J.W. (1999). Psychopathology: description and classification. *Annual Review of Psychology, 50*, 79–107.

National Institute of Mental Health. (2000). *Report of the National Advisory Mental Health Council's Behavioral Science Workgroup: Translating behavioral science into action*. Bethesda, MD: Author.

Smith, M. L., & Glass, G. V. (1977). Meta-analysis of psychotherapy outcome studies. *American Psychologist, 32*(9), 752–760.

Weisz, J. R., Donenberg, G. R., Han, S. S., & Weiss, B. (1995). Bridging the gap between laboratory and clinic in child and adolescent psychotherapy. *Journal of Consulting and Clinical Psychology, 63*, 688–701.

Weisz, J. R., Doss, A. J., & Hawley, K. M. (2005). Youth psychotherapy outcome research: a review and critique of the evidence base. *Annual Review of Psychology, 56*, 337–363.

Widiger, T. A., & Clark, L. A. (2000). Toward DSM–V and the classification of psychopathology. *Psychological Bulletin, 126*(6), 946–963.

Widiger, T. A., Frances, A. J., Pincus, H. A., Davis, W. W., & First, M. B. (1991). Toward an empirical classification for the DSM–IV. *Journal of Abnormal Psychology, 100*(3), 280–288.

Williams, J. B., Gibbon, M., First, M. B., Spitzer, R.L., Davies, M., Borus, J., et al. (1992). The Structured Clinical Interview for DSM–III–R (SCID). II. Multisite test–retest reliability. *Archives of General Psychiatry, 49*(8), 630–636.

Wolpe, J. (1958). *Psychotherapy by reciprocal inhibition*. Stanford, CA: Stanford University Press.

V

PERSPECTIVES ON PSYCHOLOGICAL CLINICAL SCIENCE