Professional Psychology: Research and Practice
2016, Vol. 47, No. 1, 77–83

0735-7028/16/$12.00 http://dx.doi.org/10.1037/pro0000034

On Possible Consequences of National Institute of Mental Health Funding for Psychotherapy Research and Training

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The National Institute of Mental Health (NIMH) has changed its funding priorities for psychotherapy-related research. With the introduction of Research Domain Criteria (RDoC), the focus has moved away from supporting randomized controlled trials (RCTs) to an emphasis on gathering primarily neurobiological data that are associated with observable and dimensionalized psychological problems, even as they occur across diagnostic categories. Among the general domains that are to be funded are negative and positive valence systems, cognitive systems, social processes, and arousal and regulatory systems. Moreover, each domain will be studied at different levels of analysis, such as genetic, molecular, neural circuitry, physiological, and behavioral. Offering an overview of the history of psychotherapy research and its funding as an historical context, this article discusses some of the implications of this shifting model, and considers the potential impact the current NIMH funding priorities may have on therapy-related research, the development of psychoactive medications, and the training of clinical psychologists as therapists.

Keywords: clinical psychology graduate training, psychotherapy research, psychotherapy training, RCTs, RDoC

After three decades of funding randomized controlled trials (RCTs) for *Diagnostic and Statistical Manual of Mental Disorders* (*DSM*) disorders, the National Institute of Mental Health (NIMH) has acknowledged the limitations of this model. Although RCTs have provided invaluable data supporting the efficacy of numerous psychosocial interventions for psychological disorders, they were limited in shedding light on the cause of these disorders. Given the descriptive and not etiological nature of the *DSM*, the heterogeneity existing within diagnostic categories, concerns about diagnostic inflation, and the system's essentially weak empirical foundation (see Frances, 2013; Frances & Widiger, 2012), the NIMH has shifted its funding priorities in a very different direction. According to the director of the NIMH, the primary focus will shift from RCTs to translational research (Insel, 2012). The goals of this research strategy are to uncover those factors that contribute to the etiology of psychological disorders, and hopefully to be able to develop an alternative to the current classificatory system—one that would be based on underlying cause rather than topographical similarity. The ultimate goal is to develop more effective treatments for the range of psychological disorders one is likely to encounter in clinical practice.

The acronym for this new, high priority initiative is called RDoC, which stands for "Research Domain Criteria." Instead of the current, categorical *DSM* classification, the goal of RDoC is to gather primarily neurobiological data that are associated with observable and dimensionalized psychological problems, even as they occur across diagnostic categories. Among the general domains that are to be funded are negative and positive valence systems, cognitive systems, social processes, and arousal and regulatory systems. Moreover, each domain will be studied at different levels of analysis, such as genetic, molecular, neural circuitry, physiological, and behavioral.

This shift in priorities will have important implications for the direction of treatment research, some of which have been discussed elsewhere (Hershenberg & Goldfried, 2015). The purpose of this article is to additionally consider the political and economic context underlying RDoC (e.g., the adoption of the biological model in psychiatry, the need for pharmaceutical companies to have biomarkers before developing any additional psychoactive drugs), and to discuss some of the possible consequences for training in clinical psychology, as well as future training in clinical psychology. The potential implications of the shift toward RDoC funding can be best understood within the historical context of the NIMH funding for psychotherapy-related research. In addressing this issue more directly, I begin with an overview of the history of psychotherapy research and its funding, move on to the implications of a shifting model from psychosocial to biological, and then consider the potential impact the current NIMH funding priorities have on therapy-related research, the development of psychoactive medications, and the training of clinical psychologists as therapists.

This article was published Online First June 15, 2015.

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THE AUTHOR IS GRATEFUL to Louis G. Castonguay, Joanne Davila, and Anita P. Goldfried for their very helpful comments on an earlier version of this article.

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An Overview of the History of Psychotherapy Research

More than six decades ago, as a reflection of the paucity of psychotherapy research, it was possible to provide a review of the psychotherapy outcome literature within the confines of a 14-page chapter in the *Annual Review of Psychology* (Snyder, 1950). Shortly thereafter, in what many considered a provocative and influential article, Eysenck (1952) expressed his concerns about the lack of research evidence for the effectiveness of psychotherapy. Rather than basing the effectiveness of psychotherapy on the therapist's say-so, he argued that controlled research was sorely needed. As research-trained clinical psychologists began to practice therapy following World War II, there began several decades of outcome research to empirically demonstrate that therapy did indeed work.

Goldfried and Wolfe (1996) have suggested that outcome research on psychotherapy has gone through three generations. The very earliest work in this area—Generation I—took place in the 1950s and 1960s, and predominantly addressed the very general question "Does therapy work?" Little was done to specify the precise nature of the therapy (which was primarily psychodynamic and eclectic in nature), to differentiate one approach from another, to specify the types of clinical problems studied, or to be very precise in the specification of outcome measures. This has been referred to as the "uniformity myth." (Kiesler, 1966). Although Generation I represented a somewhat unsophisticated empirical attempt to grapple with a most complex question on which there was little or no empirical history, it was at least a start.

Generation II addressed a more specific question, namely "What specific interventions are effective with which specific clinical problems?" In doing so, it represented a major shift in therapy research—both conceptually and methodologically. It was in the 1960s and 1970s that behavior therapy introduced not only a new approach to psychosocial treatment, but also a methodology with which it could be studied. Unlike the psychodynamic and experiential approaches that preceded it, behavior therapy had its roots in basic research, not clinical practice, extrapolating findings from laboratory studies of classical and operant conditioning to the clinical setting. This extrapolation was then extended to *any* basic research findings (e.g., cognition, social learning) that might prove useful for clinical innovations (Goldfried & Davison, 1976).

Research on behavior therapy, and then on cognitive-behavior therapy, was generously funded by the NIMH and other federal funding agencies, partly because of its empirical foundation, but also because it developed numerous methodological advancements over Generation I. Thus, relevant control groups were used in comparative outcome studies, there was random assignment to treatment conditions, the number of sessions was held constant across conditions, and treatment manuals were developed to specify the nature of the intervention. The research made use of readily available undergraduates, and the "target behaviors" of the interventions consisted of such issues as anxiety in specific situations (e.g., test-taking anxiety), unassertive interpersonal behavior, and poor work habits.

As Generation II researchers were receiving generous research support from the NIMH—most often carried out by clinical psychologists—a dramatic change was occurring in the profession of psychiatry. For the most part, Departments of Psychiatry in medical schools at the time subscribed to a psychodynamic orientation to therapy, and were often looked down on by their more empirically minded colleagues in other departments, as well as by medical students considering residency training (Nielsen & Eaton, 1981; Yager, Lamotte, Nielsen, & Eaton, 1982). As a consequence, there was an exodus of dynamically oriented faculty from psychiatry departments in the 1970s and 1980s, to be replaced by faculty having a biological approach to treatment (Tasman, 1999). This adoption of the biological model in essence was a return to the age of psychiatry (late 19th and early 20th century) that was guided by Krapelin's categorization of mental disorders and the discovery of the biological etiology of specific disorders (e.g., general paresis). As will be noted below, this shift toward biological psychiatry was to have an important impact on the next generation of therapy research.

Generation III of psychotherapy outcome research, which began in the 1980s, adopted many of the methodological advancements made in the previous generation, adding such refinements as the requirement of having adherence and competency checks to evaluate the extent to which, and how competently, therapists followed the manuals. Reflecting the shift toward biological psychiatry, a subtle but very important difference was the use of the medical model in conceptualizing the goal of therapy and its empirical pursuit.

This sea change in policy was based on the predominance of a biological model of mental illness within the NIMH, which had been increasingly dominated by psychiatrists interested in pharmacotherapy and neuroscience (Goldfried & Wolfe, 1996). In essence, psychological problems were viewed as having its roots in underlying biological processes, such as chemical imbalances in the brain. Along with this biological orientation, the general approach to the treatment of psychological problems was viewed as paralleling the treatment of physical disorders. Thus, instead of focusing on procedures for changing problematic "target behaviors," such as ineffective interpersonal skills that might be responsible for a patient's anxiety, the medical model of treatment focused on symptoms, diagnosis, and treatment to relieve the overt symptoms. Further, outcome research was reconstrued as "clinical trials," the model used to document the efficacy of drugs. The increasing emphasis on the medical model led to a shift away from making a case formulation that would uncover the relevant dynamics/determinants that would guide treatment. All this occurred within the social and political context of responding to external pressure from Congress, third-party payers, and the public to document the effective treatment of "mental disorders."

Starting in the 1980s, the question addressed in this third generation was "Which treatment packages can best treat which diagnosable clinical disorders?" Research on RCTs was generously funded over the next three decades, including those involving psychodynamic and experiential as well as cognitive–behavioral interventions.

Impact of the Shifting Research Model

Although much has been learned by three decades of research with the Generation III model of therapy research, it nonetheless had its limitations (Castonguay, Barkham, Lutz, & McAleavy, 2013; Goldfried & Wolfe, 1996; Westen, Novotny, & Thompson-Brenner, 2004; Westen, Novotny, & Thompson-Brenner, 2005).

Specifically, the model shift brought with it a number of significant unforeseen consequences. It not only changed how we conducted research, but also how we began to think about clinical problems, especially through the medical lens—namely the need to *diagnose* and treat *disorders*. Moreover, the research methodology that called for delivering the treatment according to the specifications of the manual can at times limit clinical judgment and result in adverse outcomes (Castonguay, Goldfried, Wiser, Raue, & Hayes, 1996; Henry, Schacht, & Strupp, 1986). It should be noted that in the attempt to have manual-based treatments take into account individual differences, some manuals have made use of modules to tailor treatment to individual needs (Castonguay et al., 2004; Kendall, Chu, Gifford, Hayes, & Nauta, 1998).

With the NIMH requirement that the goal of treatment in RCTs be to reduce the symptomotology associated with the different diagnostic disorders—as opposed to more focal problems such as procrastination and unassertiveness—many of the findings resulting from research work carried out during Generation II have gone by the wayside, replaced by the focus on treating *DSM*-disorders. Another unfortunate consequence of this shift was that the priority to demonstrate that therapy worked caused research on therapy process—*how* therapy worked—to become underfunded (Goldfried & Wolfe, 1996). As noted above, the shift led to the view that the dynamics/maintaining variables/functional relationships among different cognitive, affective, and behavioral factors were less of a consideration in deciding on treatment. From the vantage point of the medical view of psychopathology, co-occurring problems were now viewed as reflecting "comorbidity"—the patient having more than one disorder in need of treatment. What was not considered within this model is that two clinical problems, such as anxiety and depression may co-occur because they are functionally related. Particularly surprising was that cognitive behavior therapy researchers readily adopted this paradigm shift, even though it went against the orientations' basic foundation that the focus needed to be on what patients "did," not what they "had" (Mischel, 1968). Moreover, the notion of comorbidity, along with random assignment to treatment conditions as opposed to treatment decisions based on case formulation, may very well have contributed to making the dissemination of Generation III research findings less appealing to practicing clinicians, as it did not fully inform them about how to deal with the individual patient.

It should be noted that the inherent limitations of *DSM* diagnoses for informing treatment were noted by Allen Frances, chair of *DSM–IV*, during the second decade of Generation III. Indeed, in the introductory chapter to *DSM–IV*, Frances—a clinician as well as a researcher—cautioned that "making a *DSM–IV* diagnosis is only the first step in a comprehensive evaluation" (American Psychiatric Association, 1994, p. xxv). Given its descriptive and not etiological nature, *DSM–IV* had its clinical limitations, and Frances went on to caution that "the clinician will invariably require considerable information about the person being evaluated beyond that required to make a *DSM–IV* diagnosis" (p. xxv). Consistent with this view, the findings of recent surveys of practitioners using empirically supported treatments for *DSM*-diagnosed anxiety disorders have provided information about those clinically relevant mediators and moderators that are not typically taken into account in RCTs (Goldfried et al., 2014).

Similar concerns about the limitations of using *DSM* diagnoses as the focus of therapy research have been expressed others (e.g., Follette & Houts, 1996). The research underpinnings on which the *DSM* was based, as well as the questionable political/interpersonal factors associated with its development (e.g., the lack of transparency in the process, the existence of personal bias) caused some to question its reliability and validity (e.g., Kirk & Kutchins, 1992). Also questioned were the financial interests for its development that came from drug companies (Whitaker, 2010). From a clinical point of view, it was cautioned that "the field has gotten caught up in a research model that does not faithfully reflect clinical reality" (Goldfried & Wolfe, 1996, p. 1010). In particular, it was noted that the fact that patients with personality disorders are less likely to do as well in treatment for symptom reduction would indicate that therapy research should address "how certain personality characteristics contribute to the development of anxiety and depression" (p. 1010). Although such a consideration might have been consistent with the conceptualization of psychopathology during Generation II, it was not considered as an option in the use of RCTs for the treatment of *DSM*-disorders. Indeed, in a NIMH recommendation about outcome measures to be used in studying the treatment of panic disorder, only symptom measures were indicated, adding that it was not essential to assess personality variables, as they were not part of Axis I (Shear & Maser, 1994). It is the rare practicing clinician who ignores how the patient's personality characteristics can inform treatment. However, in many respects, this limitation may now be moot, as the current NIMH funding practices have moved RCTs to a lower priority.

RDoC: The Current NIMH Funding Priority

The goal of translational research, which is at the core of the RDoC funding priority, is to have findings from basic research in several domains (e.g., negative and positive valence systems, cognitive systems, social processes, and arousal and regulatory systems) used in developing treatments for psychological disorders. In many respects, it sounds very much like the basic epistemological philosophy noted above as the foundation for cognitive behavior therapy, where basic psychological research was used to develop psychological treatments. Indeed, RDoC has the potential for focusing on more specific problematic and possible etiological variables that can have important implications for the development of psychosocial treatments. Moreover, the potential identification of biomarkers can similarly have implications for the development of more effective psychoactive medications. At the same time, the priority of RDoC funding may have unforeseen implications for the future of clinical psychology. Each of these potential consequences is discussed below.

Implications of RDoC for Psychotherapy-Related Research

Psychotherapy researchers have been most productive in working within the Generation III model, conducting grant-supported studies on *DSM* diagnoses and their treatment. And although we may have become accustomed to this being the way research should be done, the question has been raised as to whether this has been the best way to advance the field. As funding is being diverted from RCTs to other areas, it may provide us with an opportunity to become free of the constraints associated with the *DSM*, returning to an aspect of the earlier therapy outcome mod-

el—Generation II—where the research addressed more focal, dimensional, and clinically relevant issues, such as interventions for emotional dysregulation, perfectionism, and unassertive interpersonal behavior. Thus instead of treating global syndromes, our research efforts, like that which often happens in clinical practice, might be addressed toward learning more about how to improve the psychological functioning of our clients—by focusing on more specific issues and their related interventions.

The primary emphasis on RCTs over the past several decades has also led us to equate this form of evidence for psychosocial interventions as being the primary, if not the only form of therapy-relevant evidence (Beck et al., 2014; Castonguay & Oltmanns, 2013). Often ignored have been the results of research on psychopathology, as well as basic research findings on cognition, affect, and behavior. There was also a dramatic decrease in funding for psychotherapy process research, which was reduced by more than 60% in the late 1980s (Wolfe, 1993). With the unfortunate decrease in funding for process research during Generation III, we have tended to deemphasize the need for important information on *how* therapy works. The domains of research associated with RDoC, however, may help us to shift our empirical focus more to the clinically relevant mediators and moderators associated with psychosocial interventions (Hershenberg & Goldfried, 2015). For example, the focus on positive and negative valence systems can play an important role in learning more about psychosocial interventions, as it parallels what has been referred to as standards for self-reinforcement, whereby excessive perfectionism and self-criticism may be studied as an important mediator for such clinical problems as depression (Bandura, 1969). Moreover, the focus on cognitive systems and how they relate to different forms of psychopathology can have important implications for what we may learn about executive functioning as it relates to cognitively focused interventions.

Clinical psychologists, with their knowledge and expertise in psychopathology, are particularly well equipped to study the domains outlined in RDoC. Although it is hard to predict how long the new funding priority will continue, past funding initiatives lasted two decades for Generation II of psychotherapy research, and three decades for Generation III. And although many researchers will understandably be excited by conducting work in this new frontier, there nonetheless are several questions about which to be concerned. Will our knowledge of neuroscience research findings have anything to add to the guidelines for psychosocial intervention? Conceptually, does neuroscience have the potential for differentiating nuanced cognitive processes, such as the reevaluation of an unrealistic belief? What impact will the focus on the neuroscience of psychological processes have on the recognition of the therapy relationship in the change process? Of major concern is that this shift in the NIMH funding for research to uncover biological causes of psychological disorders may eclipse the funding for psychotherapy research.

Implications of RDoC for the Development of Psychoactive Drugs

As noted above, the goal of translational research is to have findings from basic research used in developing treatments for psychological disorders, or as it sometimes known, "from bench to bedside." It was also acknowledged above that RDoC has the potential for focusing on more specific problematic and potentially etiological variables that characterized the research done during Generation II (e.g., unassertiveness). However, there is a major difference. The key issue is whether the primary goal is to shed light on psychological or biological variables, namely: From whose bench to whose bedside? What has not been emphasized in the literature is the fact that the RDoC focus stems from the desire to develop more effective psychoactive drugs for the treatment of brain diseases (Insel, 2012). Indeed, the concept of translational research is part of a larger initiative within the practice of medicine itself, and the National Center for Advancing Translational Sciences (NCATS) was established within the National Institutes of Health (NIH) in 2011 to help facilitate "treatments and cures for disease" (http://www.ncats.nih.gov/). In addition to facilitating basic research to discover specific molecules and biomarkers that could be the "targets" on which a drug can act, NCATS has also been committed to developing partnerships among universities, pharmaceutical houses, and biotechnical companies. Insel has referred to this research initiative and its associated partnerships as "exciting opportunities for drug discovery and development" (Insel, 2012. p. 1). It most certainly can be argued that for some severe clinical disorders, important biomarkers may be uncovered, which can result in the development of more effective medications for the treatment of such disorders as schizophrenia and autism. However, of concern is that the funding for research on biomarkers may eclipse research on psychological variables and on psychotherapy research (Gaudiano & Miller, 2013; Kirmayer & Crafa, 2014).

The current RDoC initiative may be readily understood in light of the transition from Generation II to Generation III research, approaching psychological problems as one might medical disorders, as well as the shift within psychiatry from a psychosocial to a biological model. Of particular relevance, however, has been the more recent decision on the part of pharmaceutical companies to drastically cut back on their research efforts to develop new psychoactive drugs (van Gerven & Cohen, 2011). The uncertain likelihood of developing a new drug that will be more effective than a generic competitor, together with the current requirement that the results of all RCTs associated with drug development be made transparent, has made the investment of large sums of money in drug development too risky (van Gerven & Cohen, 2011). What pharmaceutical houses and biotechnical companies need are data on biomarkers on which to base future psychoactive drug development.

Although the RDoC priority is presented as being preliminary, and makes passing reference to the importance of psychosocial treatments—especially cognitive behavior therapy—the primary focus is based on the premise that psychological problems are best understood as "disorders of brain circuits" (Insel, 2012, p. 3), and that they are therefore subcategories of medical disorders. Whether or not our knowledge of brain chemistry or other biological factors will help us to develop better drugs to treat psychological problems is clearly an open question. It has been vigorously argued that the reductionistic approach of RDoC is unlikely to succeed in capturing the nuances and varieties of psychological problems (e.g., Miller, 2010). Can a drug be developed that will clinically reduce a person's anxiety or depression that is due to ineffective work habits? Some who have been critical of using *DSM*-diagnoses to understand psychological problems as medical diseases, as well as

how the diagnostic system has been used by pharmaceutical companies to market drugs, have questioned the "unrealistic goal of transforming psychiatric diagnosis by somehow basing it on the exciting findings of neuroscience" (Frances, 2013, p. 171). And although there are undoubtedly genetic and other biological factors that influence psychological behavior, it is overly simplistic to ignore the fact that this is only part of what contributes to human functioning. As cognitive psychology has emphasized, both "top down" and "bottom up" perspectives are needed to fully understand a phenomenon, as its meaning can change as a result of the larger context in which it appears (e.g., the word "ball" has very different meanings in the context of a playground as opposed to a festival). The concern is that the RDoC initiative lacks the needed context in order to fully understand psychological problems.

At this point, whether the NIMH initiative will achieve its mission remains to be seen. For the field to make true progress, it will be important for the NIMH to encourage collaboration with both psychological researchers and practitioners, as they can provide the broader context within which biological factors can be understood. It is limiting to treat individuals as if they were psychologically disembodied organisms that are uninfluenced by psychosocial variables. Moreover, there is an accumulating body of evidence that psychological treatment can produce changes in the brain (Karlsson, 2011). However, regardless of whether the research associated with RDoC funding will be successful or not, there may important consequences for the future training of clinical psychologists.

Implications of RDoC for Training in Clinical Psychology

Over the years, the role of clinical psychologists has evolved from its original focus on research and assessment to the delivery of psychotherapeutic services (Freedheim, 2013; Norcross, Vandenbos, & Freedheim, 2011; Norcross & Karpiak, 2012). Before World War II, clinical psychologists in the United States were trained in research, and when they worked in applied settings, they functioned as clinical assessors. Because of the shortage of therapists to deal with psychologically disturbed returning veterans, clinical psychologists were pressed into service to adopt this role in the mid and late 1940s (Norcross et al., 2011). They did so with minimal training in therapy, which at the time was predominantly psychodynamic in orientation. It was not until the Boulder training conference that a curriculum was put in place to prepare clinical psychologists as both researchers and therapists (Raimy, 1950). The notion of training scientist-practitioners was exciting, as it allowed both vantage points to be brought to bear on the same phenomena. Clinical psychologists could read and evaluate the research literature relevant to practice, and could also offer clinical observations that could inform future research. Moreover, whatever limitations existed from one vantage point could be complemented by the other.

The field is currently undergoing some important changes, and the growing importance of research efforts that are consistent with RDoC priorities is likely to have significant implications for the role of the clinical psychologist. Indeed, there is reason to question whether the model of Ph.D. clinical psychology programs to train students to be therapists as well as researchers can continue to exist in the future.

As is already happening in clinical psychology programs, psychology departments, and universities as a whole, there is a very high priority on hiring faculty having expertise in neuroscience. Given the economic state of universities throughout the country, it is no surprise that hiring priority is given to individuals likely to bring in grant funding. With the NIMH emphasis on RDoC research, funding is increasingly likely to be in the area of neuroscience. As a result, it is also likely that we will see the shift in faculty interests within clinical psychology (as well as other areas of psychology) toward neuroscience, both in research and in the training of future clinical psychologists. Inasmuch as many of the graduate programs in clinical psychology are based on a mentorship basis, it is very likely that those applying for admission to clinical programs will be doing so in hope of doing work on neuroscience.

With this shift in research focus, there very well may be the need to change the clinical curricula to provide graduate students with the knowledge of neuroscience needed to obtain academic positions, to receive research funding, and to advance in their careers. Given the already heavy course load in existing in Ph.D clinical programs, it may not be practical to add still more requirements; something will need to be eliminated. In addition to needing to make space in the curricula for training students to do neuroscience research, at some point in the future, faculty having interest and competence in psychotherapy research and practice are likely to be replaced by those more interested in neuroscience. Hence, there may also be fewer faculty capable of teaching therapy courses.

It should be noted that there are other factors that have implications for changes in the clinical curriculum. In light of the fact that much of therapy is currently being conducted by masters level professionals (e.g., social workers, psychiatric nurses, counselors, and marriage and family therapists), it is not unreasonable to expect that psychotherapy courses and practicum in Ph.D programs might be reduced. With a growing trend for mental health professionals to learn about conducting therapy by means of treatment manuals—however limiting that might be in reality—it will be increasingly difficult to justify the amount of time needed to train Ph.D. clinical psychologists as therapists. Another factor that has implications for graduate training is a shifting emphasis as a result of the increasing demand to train clinical psychologists for prescription activities. Moreover, questions have also been raised as to whether the one-to-one model of conducting therapy is the best way to meet societal mental health needs, and that other models, such as those based on Internet and computer-based interventions, should be adopted.

This possible change in the clinical psychology curriculum is not unlike what happened in psychiatry departments when they shifted from an emphasis on psychotherapy to biological psychiatry. As noted earlier, starting in the 1970s and continuing through the 1980s, there was a mass exodus of psychodynamic faculty from psychiatry departments. Even the more empirically based behavioral and cognitive–behavioral movement in the field could not replace the growing emphasis on the interest in biological processes underlying psychological disorders. This increased emphasis on biological psychiatry led to "intense competition for curriculum time, producing a situation in which there is decreasing curriculum and supervisory time devoted to psychotherapy train-

ing at the same time that there are few faculty available to teach it" (Tasman, 1999, pp. 187–188).

Concluding Comment

Much of where we go as a field depends on what professional issues and questions are reinforced, and the NIMH has always played a key role in influencing the direction of clinical psychology. To the extent that funding priorities move research efforts in the direction of biological psychiatry, progress in psychotherapy research may become more difficult. And although research on those cognitive, affective, and behavioral processes underlying psychopathology are likely to be beneficial, research on such variables and their neurobiological correlates will result in the movement away from research on psychosocial interventions.

It is totally understandable why the NIMH and psychiatry would want to have the next several decades devoted to searching for biomarkers of mental diseases. Indeed, much may be learned as a result of research in this area, including the development of more effective psychotropic drugs. Still, there is reason for concern about the possible side effects—not of the drugs, but of the initiative itself. Research and grants clearly play an essential role in academic advancement and career development. As a result, there is concern not only for what may happen to psychotherapy research, but also for clinical training and the identity of Ph.D. clinical psychology programs.

Should these concerns eventually come to pass, the profession of clinical psychology would ironically come full circle. The Ph.D. in clinical psychology started out primarily as a research degree, and it was the need for more therapists during World War II that expanded the role of psychologist in the United States to be that of therapist as well as researcher. A related irony is that the profession of psychiatry has already come full circle, starting with Freud the neurologist, moving to the development of the talking cure, and returning to neuroscience.

Although predictions about the future of the profession can be notoriously inaccurate, I nonetheless believe that raising concerns about the direction in which we may be headed professionally are justified. Regardless of intent, and regardless of its likelihood of success, the RDoC initiative will guide funding priorities for the foreseeable future, with potentially dramatic consequences for research and training in clinical psychology.

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Received March 4, 2015
Revision received May 20, 2015
Accepted May 20, 2015 ■