THE FALL AND RISE OF EXPERTISE

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Something remarkable has happened this past year in professional psychology. It can give comfort to all our unassuming and quietly competent colleagues: the kinds of colleagues, mostly in private practice, who had little time for funded research into their work; colleagues who often did not like labels, diagnostic or otherwise; the kind we went to for our own therapy; people in whose hands we entrusted, with trepidation and increasing confidence, our own psychological well-being and even our lives.

Diplomates of the ABPP have cause to celebrate. Expertise is once again recognized by APA as essential to professional competence. For most readers of The Bulletin, the importance of expertise is too obvious to need mention. Without expertise there would be no need for an ABPP. All psychologists would be equal, as long as they had received training in manual-based interventions specific to certain diagnostic categories. It is not surprising that many of us have felt alarmed at the rising scientific status of psychological interventions that treat a psychologist's expertise as a confounding variable.

A brief recap. In 1993 Div. 12 proposed that clinical psychology demonstrate its treatment efficacy in a way that would be persuasive to our colleagues in psychiatry and medicine. There was good empirical reason to believe that we psychologists would knock our medical colleagues' socks off when it came to the treatment of conditions such as anxiety and depression. Ironically, however, Div. 12's Report endorsed the culture of medical and pharmaceutical hegemony. Psychological treatment research adopted medicine's definitions of psychopathology and its method of controlled trial comparisons as the measures of scientific credibility and therapeutic effectiveness. Treatment objectives became defined solely by the DSM; recruited 'subjects' were carefully screened to exclude "comorbidities," after which they were randomly assigned to different treatment modalities, or placebo groups or waiting lists as controls; therapists in outcome studies were trained to implement treatments according to precisely written treatment manuals. Div. 12 produced an initial list of "well established treatments," together with a list of "probably efficacious treatments," for certain disorders. Psychologists were encouraged to support treatment research that would either validate their approaches or would show them to be no better than placebo. In the latter case, those psychologists would need to be retrained in approved manual-based procedures.

Especially alarming for many of us was that this policy was explicitly meant to assist the health care insurance industry (ibid. p.1). Thus there was a realistic concern that ETVs (empirically validated treatments) might become the insurance industry's measure of professional legitimacy. There were even occasional hints that practicing within a tradition not listed for specific diagnostic categories might be incompatible with best practice and therefore make one vulnerable to malpractice charges. (Mercifully, I do not know of a case in which this actually happened.)

Expertise had not explicitly been rejected, but it had been effectively abolished as an undesirable treatment variable. As the Div. 12 Report stated: "Such standardization and precise definition of treatment through treatment manuals and other procedures reduce the methodological problems caused by variable therapist outcomes and lead to more specific clinical recommendations" (ibid. p. 1). Competence had been implicitly redefined as knowledge of the research literature, diagnostics, appropriate patient selection, and the precise implementation of manual-based treatments. Expertise was worse than irrelevant; it was unscientific and confounding.

The demise of expertise was one of the unintended consequences of clinical psychology's adoption of the medical model as its standard for measuring therapeutic efficacy. The pressure on our good colleagues was felt as an accusing professional atmosphere. We were on the defensive, such as when filling out those interminable insurance forms asking us what specific symptoms we planned to target over the course of the next ten sessions and what treatment model we planned to implement.

Many of our colleagues felt abandoned by APA. Its Template for Developing Guidelines (APA, 1995) effectively endorsed the assumption that practice guidelines should be based upon research in which controlled clinical trials formed the standard of professional legitimacy. But then, what would become of those who identified themselves as eclectic, multimodal, feminist, social constructivist, psychoanalytic independent, Jungian, Kleinian, existential or humanistic, and what would become of the various types of family therapists? How could one of my colleagues possibly make time in her practice so as to hustle an experiment that would put her approach--Jungian, with large doses of attachment theory and feminist sensibilities--on some approved list? What insurance check box would not be misleading?
Ironically, what treatment successes of hers, perhaps checked in the "Psychodynamic" box, would not further delegitimize her true orientation and skills. (And what failures would not be an undeserved black eye for mainstream psychodynamic therapists?)

Energetic research and scholarship over the past ten years threw the clarity and mission of the purely medical model for clinical psychology into some disarray. Critiques of validity as an impossible standard softened the term to "empirically supported treatments." The notions of empirical support, and of treatment manuals, became increasingly flexible. While controlled clinical trials remained the gold standard of efficacy studies, case lore and clinical practice outcomes were slowly accepted as relevant and supportive. The definition of "manuals" broadened to include a range of psychoanalytic texts, such as Clarkin et al.'s (1999) book on the psychoanalytic treatment of borderline conditions. What I found particularly interesting was that these books were hardly manuals in any simple sense. To practice in accordance with these "manuals" requires years of training and probably one's own analysis.

A number of intellectual criticisms and research findings led to the decline of the EST movement and the return of expertise as a desirable variable in professional competence. Space precludes detailed argument, but here are some markers for the significant changes that had been occurring.

Different persons with the same DSM diagnosis need to be treated differently because of individual and cultural differences, including expectations about what kind of therapist or treatment might be helpful. Treatment goals are collaboratively formed, and are often adjusted as treatment proceeds. As Seligman (1995, p. 997) noted, therapy in the real world is of variable duration, with self-correcting improvements, and is aimed at improving the quality of life as well as symptom relief for patients who have multiple problems and who select their own therapists. Because of this, controlled clinical trials have insurmountable external validity problems.

Comorbidity and lack of diagnostic specificity are more common than not. The quest for diagnostic purity can be so far removed from clinical practice as to be utterly absurd. In one report of CBT for generalized anxiety disorder, an astonishing 450 of over 500 applicants were rejected from the study for not meeting the diagnostic criteria for GAD. Apparently the study showed treatment to be efficacious (Borkovec and Costello, 1993). But as Todd and Bohart (1999) comment: "One can ask how useful such information is to a practicing clinician given that nearly 90% of the clients referred for the study (presumably because they were anxious) were not included" (p. 459).

Treatment outcomes in controlled clinical trials tend to be disappointing. In one meta-analysis of 34 outcome studies for treatment of depression, panic disorder, and PTSD, fewer than 36% of potential study participants were included in the studies, the large majority being rejected because they had "comorbid conditions." More than half of the patients accepted into treatment dropped out. Of the fewer than half of the one third who were both selected and completed treatment, most continued to have mild symptoms after treatment and gains were only partly maintained after two years. Over 50% of these patients who had completed these courses of treatment went on to other therapies (Westen and Morrison, 2001). In other words, fewer than a half of a half of one third (<8.3%) did not pursue treatment elsewhere, and it is difficult to believe that those few were all fine. With outcomes like that most of us would have given up years ago.

Brand names for treatment approaches are poor descriptors for the processes that are effective in those approaches. In one major study (Ablon and Jones, 1998), it was shown that the sessions of effective CBT therapists met the defining templates for competent psychodynamic therapy, and that the sessions of most psychodynamic therapists met the defining templates for competent CBT sessions.

There is increasing evidence that adherence to treatment manuals is negatively correlated with treatment outcome. In the Ablon and Jones study (ibid.), those cognitive behavioral therapists who had adhered to their manuals were not effective. In a study of CBT for depression, better outcomes were found when therapists did not adhere rigidly to a manual (Castonguay et al, 1996). I once asked a group of committed EST enthusiasts whether any of them would choose a therapist because of his or her reputation for adhering strictly to a manual. The cognitive dissonance set up was funny to watch, but they did laugh.

The nonspecific factors—that is, factors shared by effective therapists of different orientations—are arguably more significant in treatment outcome than specific techniques (Wampold 2001). In particular, it is once again generally accepted that "empirically supported therapeutic relationships" (Norcross, 2002, 2004) are non-specific, complex, and necessary to positive treatment outcomes.

Evidence from the insurance industry suggests that there is no correlation between theoretical orientation and treatment outcome, even controlling for diagnosis or severity of psychopathology (Brown, Dries & Nace, 1999). Good, indifferent, and bad therapists come in all colors. Differences in competence, measured in outcomes, are greater within treatment modalities than between modalities (Wampold, 2001, p. 212).
It should be noted that APA had been fairly responsive to these developments. Its revised Criteria for Developing Treatment Guidelines (APA, 2002) was significantly more subtle and sophisticated than its 1995 version. In many respects it astutely anticipates some of the themes of the Policy and Report that were produced in 2005.

In late 2004, incoming APA President, Ronald Levant, Ed.D., ABPP, hit the ground running with a Task Force reviewing the question of competence in evidence based practice in psychology (EBPP). This Task Force comprised scientists and practitioners of various theoretical persuasions, reflecting the diversity of the field. They were given the task of formulating a Policy and a Position Paper that would describe "the best possible care based on the best available evidence" (APA, 2005b, p.3). A provisional report was made available for comment in March 2005. The final Policy Statement on Evidence-Based Practice in Psychology was approved as APA Policy and the accompanying Report of the 2005 Presidential Task Force on Evidence-Based Practice was accepted by the Council of Representatives of APA in August 2005.

The Policy and the Report discuss three areas: the question of best available evidence, clinical expertise, and patients' characteristics. In my reading, clinical expertise is the pivot around which the questions of evidence and patient characteristics are organized.

It is up to the practicing clinical psychologist to evaluate different forms of evidence in the context of the patient's characteristics, circumstances, and values. This means that our good colleagues are advised not to submit their expertise to the results of controlled clinical trials, but to use their critical thinking and good judgment in their assessment of various sources of evidence as well as the needs and circumstances of each particular patient.

Expertise is operationally defined and discussed in considerable detail, and readers are encouraged to read the Policy and Report for themselves. I would simply like to highlight a few of the statements (all from APA, 2005b) that go to the heart of why we can celebrate—and why we can extend our thanks and congratulations to Ron Levant and his colleagues on the Task Force. We can start with their summary statement:

"Experts recognize meaningful patterns and disregard irrelevant information, acquire extensive knowledge and organize it in ways that reflect a deep understanding of their domain, organize their knowledge using functional rather than descriptive features, retrieve knowledge relevant to the task at hand fluidly and automatically, adapt to new situations, self-monitor their knowledge and performance, know when their knowledge is inadequate, continue to learn, and generally attain outcomes commensurate with their expertise" (p. 10).

"Psychological practice is a complex relational and technical enterprise that requires clinical and research attention to multiple, interacting sources of treatment effectiveness" (p. 8).

"Expert clinicians revise their case conceptualizations as treatment proceeds" (p. 11).

"The goals of therapy are developed in collaboration with the patient" (p. 11).

"Psychological practice, at root, an interpersonal relationship between psychologist and patient. Each participant in the treatment relationship exerts influence on its process and outcome, and the compatibility of psychologist and patient(s) is particularly important" (p. 12).

"Psychologists must attend to the individual person to make the complex choices necessary to conceptualize, prioritize, and treat multiple symptoms" (p. 16).

"Perhaps the central message of this task force report, and one of the most heartening aspects that led to it, is the consensus achieved among a diverse group of scientists, clinicians, and scientist-clinicians from multiple perspectives that [Evidence-Based Practice in Psychology] requires an appreciation of the value of multiple sources of scientific evidence" (p. 18).

"Treatment decisions should never be made by untrained persons unfamiliar with the specifics of the case" (p. 18).

It could not be clearer than that.

REFERENCES


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Academy Vice President Larry Schoefield (L) and Eugene D'Angelo (R) welcome Academy Executive Director Lynn Peterson to the board meeting in Denver and thank her for all her work on the academy's behalf.